REDUCED SCHOOL AUTONOMY AND STUDENT PERFORMANCE

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Abstract

In my thesis I evaluate the effect of the 2013 school centralization reform in Hungary on student performance. In 2013, local decision making rights of Hungarian state schools were transferred to one government controlled central institution. The centralization did not affect schools maintained by other actors (e.g. churches and foundations), so these schools can be used as a control group to assess the effect of the policy. Using a panel dataset of schools from 2008 to 2015, I estimate that reducing school autonomy had an overall small negative effect on student achievement. Despite its declared goal of reducing inequalities between schools, the centralization did not have a more positive effect on student performance in disadvantaged schools. The effects are small and their statistical significance is sensitive to specification.

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Introduction

Promoters of decentralization of decision making argue that local decision-makers understand and can adjust to heterogeneous local demands efficiently (Hanushek and Woessmann, 2013). Others point out that increasing local autonomy makes it easier to hold decision-makers accountable, reduces bureaucratic burdens of central decision-making, and gives room to innovations. (Bjørnskov et al, 2008). However, decentralized decision-making can also lead to problems. It is not guaranteed that local actors pursue the same goals as the state (Hanushek and Woessmann, 2013), and they may lack the technical expertise needed to provide public services (Galiani et al, 2013).

In the context of education, decentralization of decision-making is a widespread policy tool, and studies mainly show that increasing local autonomy affects student performance positively (Falch and Fisher, 2011). However, Hanushek and Woessmann (2013) show that the effect of decentralization is heterogeneous: negative in poor countries, and positive in rich ones. They find that increasing school autonomy makes sense in countries where GDP per capita is higher than $8,000-$20,000 (depending on whether increasing autonomy affects school budget, content or personnel decisions).

In my thesis, I estimate the effect of reduced school autonomy on student performance in Hungary. Based on its income, Hungary falls into a country category where the expected effect of changing local autonomy based on Hanushek and Woessmann's results is not obvious. But there is an opportunity to estimate the effect of school autonomy on student outcomes in a within-country context. In a 2013 education reform, the Hungarian government centralized decision-making of schools. Decision rights of schools and local governments were transferred to one centrally administered institution, the Klebelsberg Institution Maintenance Centre (KLIK by its Hungarian

abbreviation). There is now a window to detect a causal relationship between the centralization policy and student performance as the decision-making became centralized only for schools previously maintained by local governments but did not change for others (e.g. schools maintained by churches, foundations or universities).

These schools serve as a control group in my analysis. To estimate the effect of reduced school autonomy on student performance, I use individual fixed effects models on a panel dataset containing standardized test results of all Hungarian schools between 2008 and 2015.

I show that the reduction of school autonomy had an overall small negative effect on student performance both in primary and secondary schools. The estimates are very close to zero, and not statistically significant in all specification, but a substantial positive effect of the policy can be ruled out. I also show the effect to be the same or even less negative for schools which are located in poor settlements. This is an important result, because the declared goal of the centralization was to reduce inequalities by helping schools whose maintainers could not finance them stably.

The structure of the thesis is the following: the next section summarizes the Hungarian situation regarding school autonomy before and after the centralization policy. In Chapter 2 I give a detailed description of the dataset. Chapter 3 presents the models and results, and I conclude in Chapter 4.

Chapter 1 – School autonomy and finance in the Hungarian education system, before and after the centralization

1.1 School autonomy and finance before 2013

Before 2013 the vast majority of Hungarian public schools were maintained by local governments, which financed schools mainly from a normative aid received from the central state budget based on headcounts. The normative aid was not enough to cover all educational expenses of local governments, so it was complemented by targeted central budget aids and by the local governments' own budget. For schools maintained by churches and for private schools the state budget provided the same amount of normative aid as for state schools, and for church maintained institutions it supplemented it by the amount the local governments spent on average per students (Balogh and Halász, 2004).

Local governments had the right to open and close schools. They could also form partnerships with other settlements to share the responsibilities of maintaining the schools, or could transfer maintenance to the county. They were also responsible for appointing school principals, considering the recommendation of the teacher board. The yearly budget of schools, compiled and proposed by the school principal, was approved by the local government after a bargaining period. (OECD, 2007).

After the school budget was approved, the principal could make the financial decisions of the school freely within the conditions of central rules. The principal was responsible for the employment and remuneration of teachers, but the minimum wage she had to pay was regulated centrally. This meant that principals had some flexibility in paying teachers, as they could give premiums or pay overwork. The principal employed non-teaching school staff as well, e.g. janitors

and cleaners. The equipment needed for teaching and daily operations were also financed from the school's own budget (Györgyi, 2015; Balogh and Halász, 2004).

The principal could also use the school's own resources. In addition to funds from the maintainer, public educational institutions could have their own income mainly from letting out their rooms, winning tenders or operating a school foundation through which parents could support the school (OECD, 2007).

Thus on one hand, the system before 2013 had the positive features we expect from decentralized systems, as local governments and principals knowing the local needs could make decisions to respond to them. On the other hand, the regional differences in the financial situation of schools were large. Local governments in richer settlements (typically cities) could provide much more resources to schools, and the gap between rich and poor settlements regarding the money spent per pupil grew further in the 2000's (Varga and Hermann, 2011).

1.2 School autonomy since 2013

As a declared response to these large inequalities the government decided in the 2011 National Education act to reorganize this system (Balogh, 2015). In the beginning of 2013 the government established a central entity, KLIK, which became the maintainer of previously municipality maintained public schools. KLIK operates through 198 study districts, which function as mediators between schools and the central leadership. Strategic decisions are made at the headquarters, and are executed by the study districts (Györgyi, 2015).

Since 2013, public schools do not have their own budget and financial decisions are made by KLIK. The center is also responsible for opening and closing schools, appointing and employing school principals, employing teachers, and obtaining teaching equipment. In settlements with less than 3000 inhabitants KLIK automatically became responsible for maintaining school buildings

and paying non-teaching school staff as well, so no responsibilities stayed at local governments. In larger settlements, local governments could decide to keep the responsibilities of maintaining the building, but according to Györgyi (2015) most of them decided not to do so (however there is no data on this).

Centralization has made daily operations of schools more difficult than before. As every expense of the school has to be approved by KLIK, the bureaucratic burden of teachers and school principals increased considerably (Györgyi, 2015).

In the new system, principals still have the right to lead professional work. However, as they cannot fire, hire or reward teachers, they lack the financial instruments to do this. In addition, schools' revenues have to be transferred to the central budget, so schools lack the incentive to generate their own revenue. An exception is revenue from school's foundations, which can be kept by the school, but this probably is a very small amount of money[1]. In 2001, 24% of state schools had their own revenue from foundations, the average yearly budget was near 600 thousand forints (Balogh and Halász, 2003).

The autonomy of schools and teachers were reduced regarding professional decisions as well. From 2014 they could only choose textbooks that were on a list provided by KLIK. In addition, the amount by which KLIK supported textbook purchases was maximized in 12,000 HUF per student. As this amount was well below the usual amount schools spent on textbooks, in practice this meant that schools either did not order all the textbooks they needed (for example they ordered the course books but not the workbooks), or they ordered the books from the state publisher. These books are much cheaper than the textbooks of private publishers; however, experts and teachers deem these

[1] The 2011 National Education Act does not go into detail, just states that schools foundations can work to support operations of the schools. According to some school websites, foundation revenue is typically spent on school events, and to support disadvantaged students' participation in summer camps.

books to have low quality (Zubor, 2014). While the pedagogical program of schools could also be modified by KLIK in principle, Györgyi (2015) found no indication that this ever happened.

1.3 Control group

The abovementioned changes did not affect schools maintained by other actors like churches, foundations, ethnic minority governments or universities[2]. The maintainers of these schools continued to receive the normative state support and could manage their schools as before.

Regardless of the maintainer, the core curriculum and expected student outcomes (like secondary school leaving exams) are regulated centrally, so education in non-state maintained institutions should be similar than that of state institutions. Thus, it could make sense to use these schools as a control group in the analysis. On the other hand, church maintained schools, which take up the majority of control schools, experienced large changes in the observed period before the centralization policy as well, and this makes them a less suitable as controls.

In 2002, Hungary spent 5.8% of its GDP on public education, since then, this amount gradually decreased to 4.2% in 2013[3] . Though the number of students decreased in the period as well, the decrease in the spending cannot be accounted for demographical reasons only: the amount of money spent per pupil also decreased between 2002 and 2011 (Hajdu et al, 2015).

I did not find exact data on the amount of resources that state and non-state schools receive, but there are some information on church maintained schools. The central aid support received by church maintained institutions from 2004 was as high as the full expenses in public schools (including central budget and municipality spending), and this amount took up about 80% of their

[2] Besides the creation of KLIK, other educational reforms took place as well, which affected every school regardless of their maintainer. For example, school leaving age was reduced to 16 years, there were changes in the National Core Curriculum and the content regulation of local curriculums, the supervisory and promotion system of teachers changed, and the government restructured the textbook market. These changes – as they affected every school – do not affect my estimates of the centralization.

[3] See the website of the World Bank: http://data.worldbank.org/indicator/SE.XPD.TOTL.GD.ZS?locations=HU

expenses in 2012 (Hajdu et al, 2015). So it is sure that on average more money was spent per pupil in church maintained institutions than in public schools before the centralization reform.

It is possible that the gap in the resources of public and church maintained schools grew further since the centralization: according to press releases, KLIK is underfinanced and inefficient, while church maintained institutions were reported to receive additional funds. In this case, estimating a negative effect might not show the effect of the centralization but the effect of public schools receiving decreasing funds compared to church maintained institutions.

The number of church maintained schools increased in the period as Table 1 in Chapter 2.1 will show. The growth is also well-documented is by Hermann and Varga (2016). They show that church maintained primary schools are disproportionately present in poor areas, but in these areas they teach students with a relatively good social status. If the proportion of disadvantaged students grew in state maintained schools independent of the centralization, my estimate of the treatment effect would be biased. Hermann and Varga also show that the proportion of disadvantaged students who study in church maintained secondary schools grew considerably between 2010 and 2015. This would bias my estimates in the opposite direction.

I try to tackle the issue of changing student composition by controlling for the proportion of disadvantaged pupils in the school. I also check parallel trends before the policy. In addition, treatment effect is estimated for primary and secondary schools as well. As different dynamics are at work for these schools, finding the same effect for them would support that the estimates are not not driven by policy-independent changes in student ability.

Chapter 2 – Data

2.1 Dataset

I am using school level panel data from 2008 to 2015, from the National Assessment of Basic Competencies (NABC). NABC measurements is conducted in every May on the same day in all Hungarian schools in 6^{th} 8^{th} and 10^{th} grades, and is answered by all attending students. Reading and mathematics competencies are measured, and data on schools and students' background are collected as well. The tests are conducted and evaluated in standardized circumstances (Balázsi et al, 2016).

Though mathematics and reading test scores are available separately, I use their mean as the response variable to avoid checking too many outcomes.

In the Hungarian school system one institution can have multiple establishments. For example one institution can have two different buildings in a town with different teachers and leaders, and there can be substantial differences among the establishments. So as it is more suitable to use data on the establishment level, I use the establishment as the unit of observation, and I refer to it with the word "school".

The dataset contains information on 5102 schools, of which 3813 have a valid 6^{th} grade test score at least for one year. Among these, the vast majority (3704 schools) offer only primary school training, the remaining schools offer 8 year secondary schools trainings. For the analysis of primary schools these schools are not included, even if they offer primary school education well. 1630 secondary schools offer one or more of 4, 6 or 8 year grammar school programs, vocational secondary and vocational training programs. To estimate the effect on primary schools 6^{th} grade results, on secondary schools 10^{th} grade results are used. Schools teaching 6^{th} graders and 10^{th} graders are almost disjoint, only 4% of all observations come from schools which have test results

for both grades[4]. Test results of 8th graders are not used in the analysis, as these are outcomes of either primary or secondary schools, so this again helps to avoid testing multiple outcomes.

Table 1 shows the number of all schools by maintainer and year. In 2013, KLIK does not appear yet, though it already existed and started to operate. So in the analysis I regard every school which was maintained by KLIK in 2014 as if it was maintained by KLIK in 2013 as well. The majority of schools were maintained by local governments before the policy. All local governments and partnerships of local governments stopped their maintenance activity according to the database. Schools maintained by an "other state entity" are all agricultural, food industry or forestry secondary schools (both before and after the centralization). So it appears that for agricultural secondary schools the maintenance were transferred from local governments to the Ministry of Agriculture[5]. The majority of them were maintained by county local governments before.

Table 1 reveals that the number of church and minority local government maintained schools increased in the period. In the dataset according to their IDs, all previously local government maintained schools' management were transferred to the KLIK. So in theory, self-selection of schools out from the policy, should not be a problem as it was compulsory for local government managed schools to transfer their maintenance to KLIK. However, in the dataset the number of church and ethnic or national minority maintained schools grew in 2013, so it possible that some schools decided that instead of the KLIK they want to be maintained by other entities, and the schools that opted out might have different potential outcomes than the schools who did not, which could bias my estimates. But the number of schools like this cannot be very large, and the number

[4] When estimating the effect for primary schools I do not include secondary schools that have 8 year old training, and thus have 6th grade results. So this 4% means schools have primary school training, and some other (vocational, vocational secondary, 4 year grammar or 6 year grammar) training

[5] There is no exact data in the database, these schools are coded to be maintained by a „state entity". Their names reveal though that these are agricultural, forestry or food industry schools. I did not find exact information about what happened to them in 2013, but on some of their websites it is indicated that they are maintained by the Ministry of Agriculture

of church maintained schools grew throughout the period, not just in the year before KLIK started to operate, so self – selection out from the policy is probably not a large issue.

Table 1 – Number of schools in different years by maintainer

Maintainer	2008	2009	2010	2011	2012	2013	2014	2015
KLIK	0	0	0	0	0	0	2,753	2,743
Town local govt. or partnership of local govts.	3,121	3,038	2,984	2,896	2,842	2,715	0	0
County	167	181	168	166	170	154	0	0
Other state entity	22	20	19	18	16	28	66	68
National or ethnic minority local gov.t	7	7	7	7	10	10	25	29
University	33	35	36	35	34	34	35	34
Church	258	268	282	287	342	472	500	515
Foundation, firm or other	237	231	232	227	238	216	207	201
Total	3,845	3,780	3,728	3,636	3,652	3,629	3,586	3,590

In fixed effects specifications, where treatment status before the centralization is used as well, a school is defined to be part of the treatment group before the policy, if KLIK started to manage it in 2013. Table 2 shows that the number of schools in the treatment and control groups by year for primary and secondary schools. The number of untreated schools decrease because schools that stopped operating before 2013 cannot be part of the treatment group.

Table 2 – Number of treated and untreated primary and secondary schools by year

Treatment status	2008	2009	2010	2011	2012	2013	2014	2015	Total
Untreated primary schools	1,073	899	785	724	602	568	472	472	5,595
Treated primary schools	1,669	1,779	1,868	1,871	1,991	2,012	2,095	2,097	15,382
All primary schools	2,742	2,678	2,653	2,595	2,593	2,580	2,567	2,569	20,977
Untreated secondary schools	583	527	515	493	478	457	422	433	3,908
Treated secondary schools	562	604	628	627	660	669	689	678	5,117
All secondary schools	1,145	1,131	1,143	1,120	1,138	1,126	1,111	1,111	9,025

As Table 1 and Table 2 revealed, schools close and start to operate in the period. Table A1 in the Appendix shows that about half of schools operated throughout the period the whole period. I run the models not just on the whole sample of schools, but on this subsample as well.

In the models I control for some time-variant features of the schools, which can affect the average test results. The most important determinant of pupil performance in Hungary is his or her family background (Sinka and Horn, 2013). Because of this, I use the proportion of students with unemployed parents, and the proportion of students whose parents have a university degree as control variables. I also control for the proportion of students with special educational needs. These variables come from a questionnaire answered by the school principals during the NABC. These are very crude measures, as principals are asked to provide information on the student composition of whole school, not just the tested classes. Also, the estimates of the principals are very imprecise, the distribution of these measures jump at round numbers like 5 and 10.[6]

When the effect of reduced school autonomy is estimated on secondary school results, the school's type is controlled for as well. Vocational secondary schools, vocational training schools and grammar schools differ to a large extent in Hungary, so controlling for them can reduce standard errors of the estimates.

To be able to measure the effect of KLIK separately for schools located in poor and rich settlements, I constructed a proxy for settlement status based on 2011 settlement level census data on unemployment rate, the number of businesses per capita, and the percentage of tenants receiving social aid. After applying principal component analysis on these variables, I calculated the factor scores based on the first principal component. Having a high factor score indicates high unemployment rate, high proportion of people receiving social aid, and low number of businesses

[6] These could have been measured better by using the student questionnaires of the NABC

per capita. I divided settlements to three equal groups based on their factor score. In the analysis I coarsened the two poorer groups together, as the number of observations was low for the poorest group, especially when analyzing secondary schools.

2.2 Descriptive Statistics

Table 3 shows the average 6th grade NABC test results for treatment and control schools on different subsamples. Looking at the raw average test scores one year before and three years into the policy helps to imagine what happened. The test scores grew from 2012 to 2015 both for control and treatment schools. However, on the total sample, for treatment schools the average test results increased by about 8 points, and for control schools it increased by 25 points. In poor settlements there appears to be a larger difference: 8 points increase in the treatment schools and 33 points increase in control schools. For rich settlements the increase between the two time points is about 10, but a bit larger for control schools. So these numbers suggest that the centralization policy had a negative effect, and that it had a more negative effect in schools located in poor settlements.

The number of observations in each cell for the tables in this section is available in the Appendix (Table A1), and it shows that each cell has at least 70 observations, which indicates that common support is ensured in the models.

Table 3 – Primary school test results in 2012 and 2015 in the treatment and the control group

	Year	Full sample	Rich Settlements	Poor settlements
Treatment	2012	1453	1497	1413
	2015	1461	1505	1421
Control	2012	1456	1494	1424
	2015	1481	1507	1457

Table 4 shows 10th grade test results for control and treatment secondary schools. Between 2012 and 2015 test results of treatment schools decreased by 17 points in the full sample. The decrease was about this large in poor and rich settlements as well. In the meantime, results of

control schools slightly grew in the period, by 8 points. So Table 4 also suggests a negative effect of the policy.

Table 4 Secondary school test results in 2012 and 2015 in the treatment and the control group

	Year	Full sample	Rich settlements	Poor settlements
Treatment	2012	1617	1646	1565
	2015	1602	1631	1549
Control	2012	1561	1588	1513
	2015	1570	1601	1515

As at the secondary school level there are three types of trainings, it is worth checking separately how the test results changed for the different types. In Table A3 in the Appendix, we see that the test scores of treatment grammar schools decreased, while test scores of control grammar schools increased. In vocational secondary schools, test scores decreased in both groups, but decreased more in the treatment group. In vocational training schools test scores decreased in treatment schools and stagnated in control schools. So for all three types, the numbers are in accordance with a negative effect of reduced school autonomy,

Figures 1 to 4 show the evolution of the test results between 2008 and 2015. In 2008 and 2009 scores were measured in a different scale, so the test scores are standardized to have a mean of 0 and standard deviation of 1 every year. Thus the figures visualize how the difference between treatment and control schools evolved in the period.

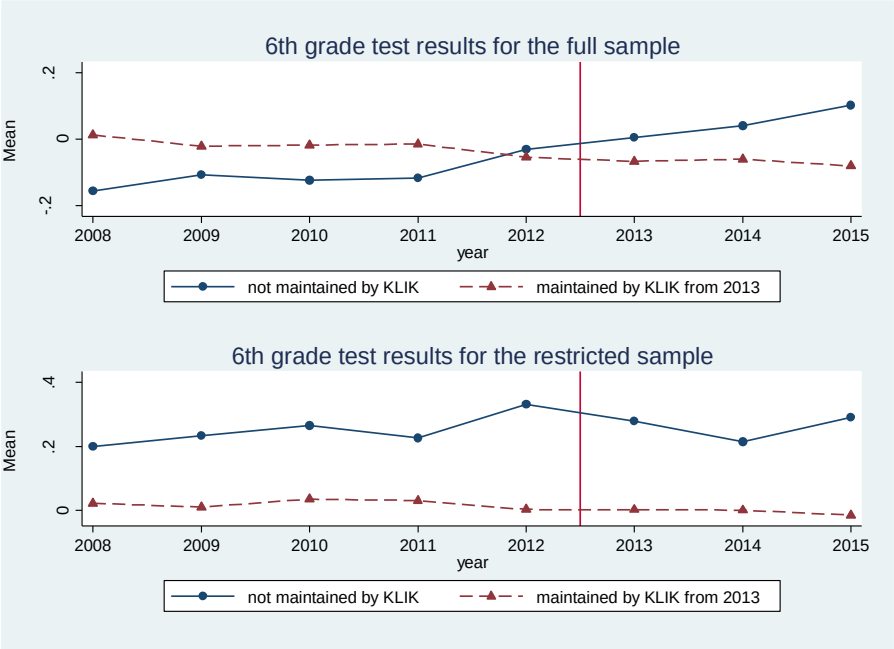
Test results are always plotted on the full sample, and also on a restricted sample, where only schools that existed throughout the period are included. It is useful to look at the balanced and unbalanced panel as well: results on all schools show how the difference between treatment and control schools changed, while results on the balanced panel visualize better how the test results changed within schools. These figures can be very different because schools opened and closed in the observed period. For example, imagine the following situation: the test results of treatment and

control schools are the same in the beginning of the period, and they remain totally unchanged in every school. In 2013 though, low performing non-state schools close, and instead new state-maintained schools start to operate. As a consequence, average test results become higher for control schools and lower for treatment schools, and this difference shows up on the figure that plots results for the whole sample. But as the abovementioned change does not affect within-school changes, and so estimates in fixed effects specifications, these graphs can be misleading. Plotting test results only on the balanced sample better represents visually that no change had happened within schools.

Figure 1 reveals that the average test results of schools whose maintainer changed to KLIK in 2013 got worse between 2008 and 2015 compared to control schools. The difference between them gradually became larger. However this does not mean that the differences within schools followed the same pattern.

To be able to see better what happened within schools, in the second panel of Figure 1 only schools that existed throughout the period are included. This shows a relatively stable difference between control and treatment primary schools, which might have grew after 2011. The figure reveals that control and treatment school results might not have moved together before the policy: it seems that control schools results improved in 2012.

Figure 1 – The evolution of standardized test results of treatment and control primary schools



For secondary schools it seems more obvious that something happened in 2013. Schools that started to be maintained by KLIK had better test results on average than other schools in the beginning of the period, but their results got closer to each other after 2013. On the restricted sample, in 2015 non-KLIK schools scored higher than KLIK schools for the first time.

Figure 2 - The evolution of standardized test results of treatment and control secondary schools

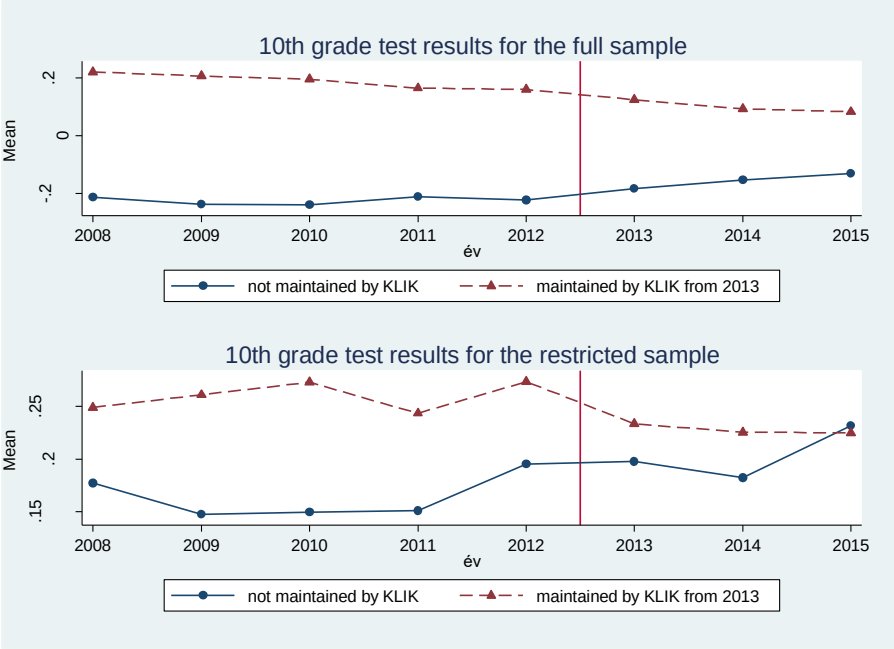


Figure 3 shows primary school test results in the treatment and control schools separately for schools located in rich and poor settlements. In rich settlements, there are large changes in the difference between the test results even before the policy. In poor settlements, average results of control schools gradually became better, but as results on the restricted sample reveal, this probably does not mean large within school changes.

Figure 3 - The evolution of standardized test results of treatment and control primary schools in rich and poor settlements

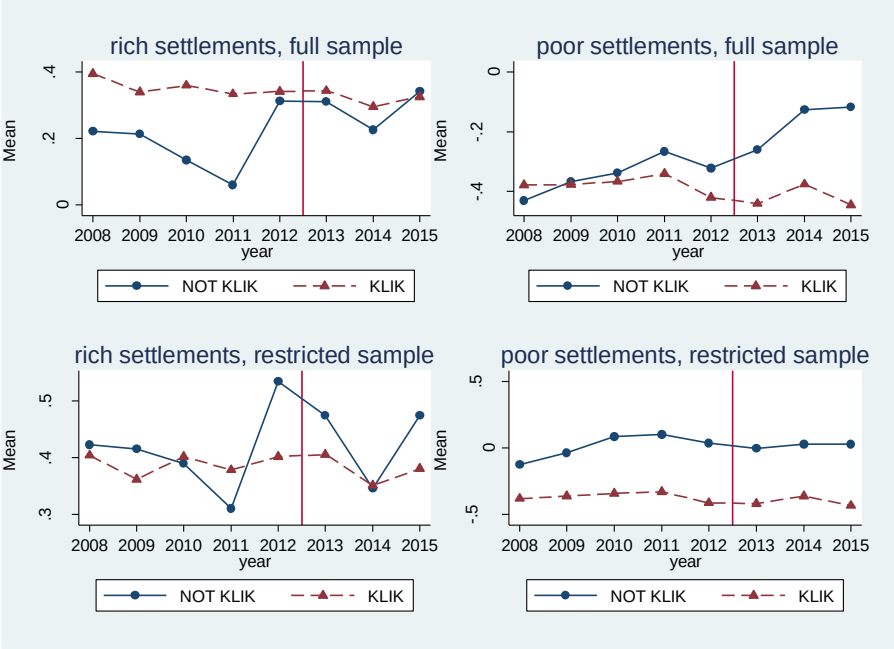
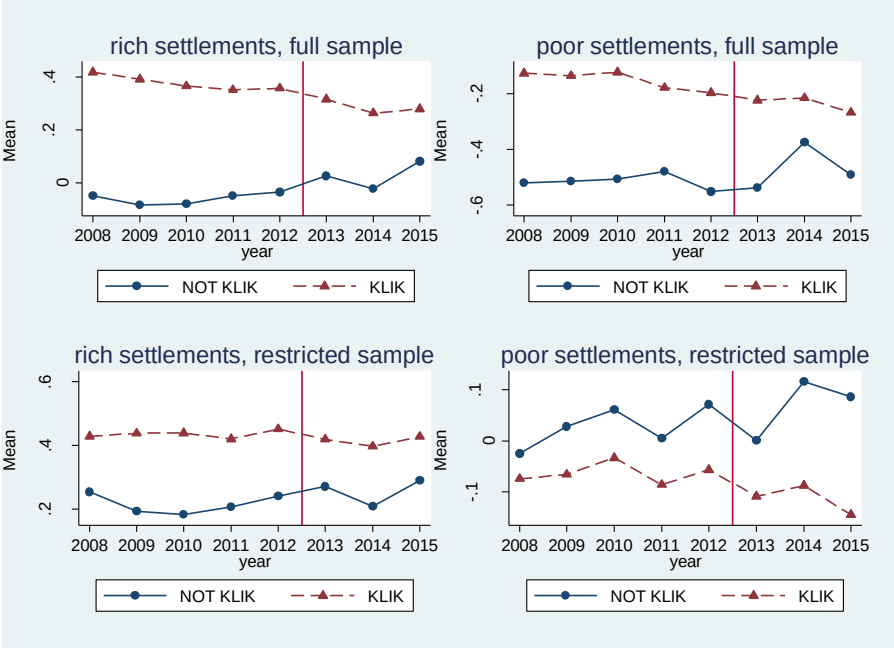


Figure 4 shows the same plots for 10th grade results the picture seems more clear. Both in rich and poor settlements test results move together until 2013, while after it control schools seem to improve a bit compared to treatment schools.

Figure 4 - The evolution of standardized test results of treatment and control secondary schools in rich and poor settlements



To sum up, the descriptive statistics are consistent with a zero or negative effect of the policy. However, it is a concern that pre-treatment results of control and treatment primary schools do not move together. In the next chapter I estimate the effect of reduced school autonomy with fixed effects models, and check the parallel trends assumption.

Chapter 3 – Models and Results

3.1 The overall effect of the centralization

The main specification is an individual fixed effects model which exploits the panel nature of the data, and assumes that the effect of centralization for schools in rich and poor settlements is the same:

$$Y_{it} = \beta_1 klik_{it} + \mu_i + \nu_t + X'_{it}\gamma + \varepsilon_{it}$$

Here, Y_{it} is the average NABC test score for school i in year t. The dummy $klik_{it}$ is set to one if the school was managed by KLIK in year t, so β_1 is the coefficient of interest. School fixed effects (μ_i) and time fixed effects[7] (ν_t) are included as well, along with time-variant controls in some specifications. The controls are proportion of pupils whose parents are unemployed, the proportion of pupils whose parents have a college degree, and the proportion of students with special needs in the school. For 10th grade test results the type of the trainings offered by the school are controlled for as well.

β_1 measures how the difference between state schools and non-state schools changed after the policy. In order for β_1 to show the average treatment effect, the key assumption is that there are no important time-variant omitted variables, so that KLIK-maintained and non KLIK-maintained schools' test results would have moved together if the centralization did not happen. This assumption is not satisfied if along with the centralization policy for public schools, conditions for non-state schools changed as well. If, for example, financial conditions of church maintained schools improved since the centralization reform, and this had a positive effect on their test results,

[7] As Y_{it} is standardized to have 0 mean in the whole sample every year, including time fixed effects is not very important, as they are about 0 anyway. I included time fixed effects to control for time trends even when there are tiny differences between average yearly test results (e.g. because some observations are lost due to a missing value on one of the controls).

β_1 would be negative even if the centralization itself had zero effect on the test results. As discussed in section 1.3, this might be a real concern, so my estimate of β_1 is probably biased downward.

Another unobserved time-variant factor that could bias the estimates is student ability. This might be important if student ability changed in the observed period systematically differently for state and non-state schools. Time-varying student composition can be a spillover effect of the policy: more education-conscious families may try to get their kids out from KLIK maintained schools. In models where controls are not included this effect is incorporated in β_1, but this is not a problem as this can really be regarded as an effect of the policy. On the other hand, student composition might have changed even in the absence of the policy, which could bias my estimates. I try to tackle this issue by including proxies for student ability in the school (proportion of pupils whose parents are unemployed, the proportion of pupils whose parents have a college degree, and the proportion of students with special needs in the school).

Table 4 shows the model estimates: for primary schools (Model (1) and (2)) there is a small negative effect of 6-8% of the standard deviation of the test results, which is statistically significant at the 10% level, and at the 5% level when controls are included. Table 4 shows that reducing school autonomy had a small negative effect on student performance in secondary schools as well. The magnitude of the effect is similar to the effect measured for primary schools: -0.05 standard deviations when controls are included and -0.06 when they are not. Standard errors are clustered at the school level in all models to account for the serial correlation in the error term.

So the main specification shows that the centralization policy was not successful in improving student performance. The fact that the estimates are similar for regressions with and without controls for student ability support that the results are not driven by time varying student composition.

Table 4 – The effect of KLIK on student performance

VARIABLES	(1)	(2)	(3)	(4)
	6th grade scores		10th grade scores	
Klik	-0.06*	-0.08**	-0.06***	-0.05***
	(0.03)	(0.03)	(0.02)	(0.02)
Observations	20,977	19,575	9,025	8,000
R-squared	0.00	0.00	0.00	0.02
Number of schools	3,704	3,652	1,630	1,567
Controls		YES		YES
School FE	YES	YES	YES	YES

Standard errors (in parentheses) are clustered at the school level: *** p<0.01, ** p<0.05, * p<0.1

3.1.1 Checking parallel trends

For pre-treatment years, it is possible to support the assumption of no omitted time-variant variables by measuring the "effect" of the centralization before the actual policy change. To do this, the following model is used:

$$Y_{it} = \sum_{j=2009}^{2015} \beta_j treatment_i \times I(year = j) + \mu_i + \nu_t + X'_{it}\gamma + \varepsilon_{it}$$

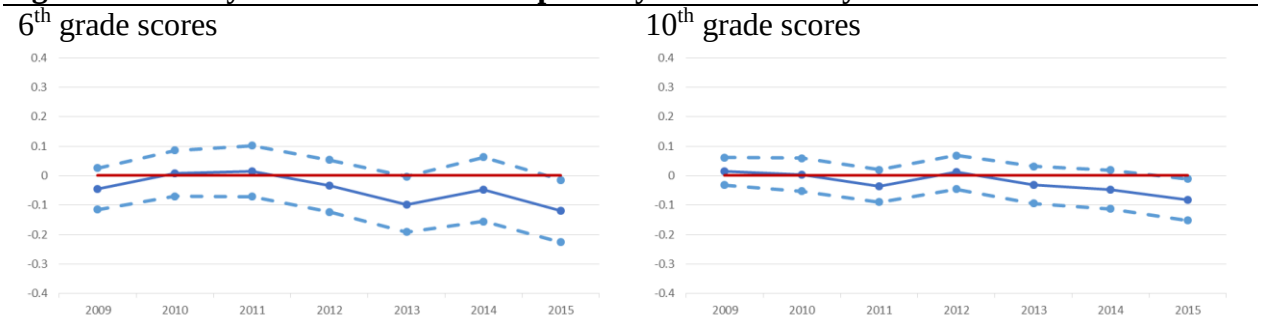
where the variable $treatment$ is set to 1 for all time periods to schools which were maintained by KLIK after the policy, and set to 0 to all other schools. β_j measures the treatment effect in year t, compared to the base year (2008). If β_j-s are 0 up until the year of the policy, it shows that before the policy control and treatment schools moved together, so there is hope that they would have moved together after 2012 as well if the policy had not happened. This model is also useful, because if there is an effect of the centralization, it should be the smallest in 2013 and the largest in 2015, and this can be checked with it.

Figure 5 shows the treatment effect separately for different years: there is no statistically significant effect in pre-treatment years compared to 2008 as base year, which supports that there are no important omitted variables. However, there is no clear effect in post-treatment years either.

6th grade students in public schools in 2015 spent nearly half of their studies in schools managed by the KLIK. So if the policy really had a small negative effect on student performance, as indicated by Table 1, one would expect that there is a larger negative effect in the second, and an even larger effect in the third year of the centralization. But no such patter arises: the effect in 2013, the first year of the policy, is more negative than the effect in 2014. The 2013 and 2015 effects are statistically significantly different from 0 (-0.12 standard deviations in 2015), but the upper bounds of the confidence interval are still close to 0.

For secondary schools the effect is the most negative (-0.08 standard deviations) in 2015 and it is only statistically significant that year. So the effects are get a bit larger in absolute value as more years passed since the centralization, but they still stay very small.

Figure 5 – Yearly treatment effects for primary and secondary schools



95% confidence intervals are shown with dashed lines. Controls are included and standard errors are clustered at the school level

3.2 The effect of the centralization in poor and rich settlements

Even if the overall effect of the policy is zero or negative, it is possible that it helped disadvantaged schools, which was its declared goal. Schools located in poor settlements before the centralization probably had a worse financial situation than schools in rich settlements, where the local government was able to provide more resources to its schools. To answer the question if the centralization had a different effect on disadvantaged schools, in the next model I let the effect of

KLIK to be different for schools which are located in rich and poor settlements. The model is the following:

$$Y_{it} = \beta_1 klik_{it} + \beta_2 klik_{it} \times poor_i + \mu_i + \nu_t + X'_{it}\gamma + \varepsilon_{it}$$

where $poor$ is a dummy equal to 1 for poor settlements (as defined in section 2.1), so β_1 measures the effect for rich settlements, $\beta_1 + \beta_2$ shows the effect in poor settlements.

Table 5 shows that the effect for schools located in rich settlements is negative but small and insignificant at the 5% level. For poor settlements, the effect is 0.02 standard deviations more negative than the effect in rich settlements, but this difference is statistically not different from 0. The coefficients are very similar to secondary schools. So Table 5 shows that the policy was not able to decrease inequalities, as the effect of KLIK was not more positive for schools located in poor towns than the effect in rich settlements.

Table 5 – The effect of the centralization on 6th grade test results in poor and rich settlements

	(1)	(2)	(3)	(4)
VARIABLES	6th grade scores		10th grade scores	
Klik	-0.05	-0.06*	-0.06***	-0.05**
	(0.03)	(0.03)	(0.02)	(0.02)
klik * poor	-0.03	-0.02	-0.01	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)
Observations	20,760	19,368	9,020	7,999
R-squared	0.00	0.00	0.00	0.02
Number of schools	3,665	3,613	1,626	1,566
Controls		YES	YES	YES
School FE	YES	YES		YES

Standard errors (in parentheses) are clustered at the school level: *** p<0.01, ** p<0.05, * p<0.1

Split sample regressions show an even stronger result. Now the model in 3.1,

$$Y_{it} = \beta_1 klik_{it} + \mu_i + \nu_t + X'_{it}\gamma + \varepsilon_{it},$$

is used separately for schools located in rich and poor settlements. In this specification, all coefficients can be different for schools in rich and poor settlements, not just the coefficient on the

treatment variable. For primary schools, Table 6 shows a negative but insignificant effect for schools in rich settlements, and a larger negative treatment effect for schools in poor settlements of about -0.1 standard deviations. For 10th graders there is a statistically significant negative effect for schools in rich settlements, and depending on the specification a statistically significant or insignificant negative effect for schools in poor settlements. These results are again inconsistent with a positive effect, and for 6th grade results they affirm that the effect was even more negative in disadvantaged schools.

Table 6 – The effect of KLIK on 6th grade test results in poor and rich settlements using split sample regressions

	6th grade scores				10th grade scores			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Rich settlements		Poor settlements		Rich settlements		Poor settlements	
klik	-0.02	-0.03	-0.10**	-0.11**	-0.06***	-0.06***	-0.07*	-0.03
	(0.04)	(0.04)	(0.05)	(0.05)	(0.02)	(0.02)	(0.04)	(0.04)
N	9,818	8,938	10,942	10,430	5,774	4,995	3,246	3,004
R^2	0.00	0.00	0.00	0.00	0.007	0.020	0.009	0.028
Number of schools	1,629	1,597	2,036	2,016	1,004	963	622	603
School FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls		YES		YES		YES		YES

Standard errors (in parentheses) are clustered at the school level: *** p<0.01, ** p<0.05, * p<0.1

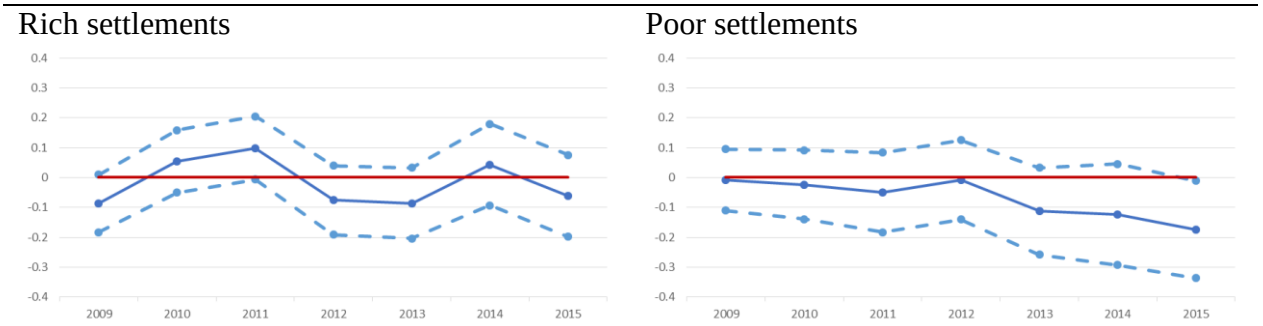
3.2.1 Checking parallel trends for rich and poor settlements

Yearly treatment effects are checked separately for schools located in rich and poor settlements as well. The model

$$Y_{it} = \sum_{j=2009}^{2015} \beta_j treatment_i \times I(year = j) + \mu_i + \nu_t + X'_{it}\gamma + \varepsilon_{it}$$

is used separately on schools in different locations. For primary schools the parallel trends assumption is not satisfied for test results of schools in rich settlements, but seems to hold for schools in poor settlements. In poor settlements, the effect is the most negative in 2015.

Figure 6 – Yearly treatment effects for rich and poor settlements in primary schools

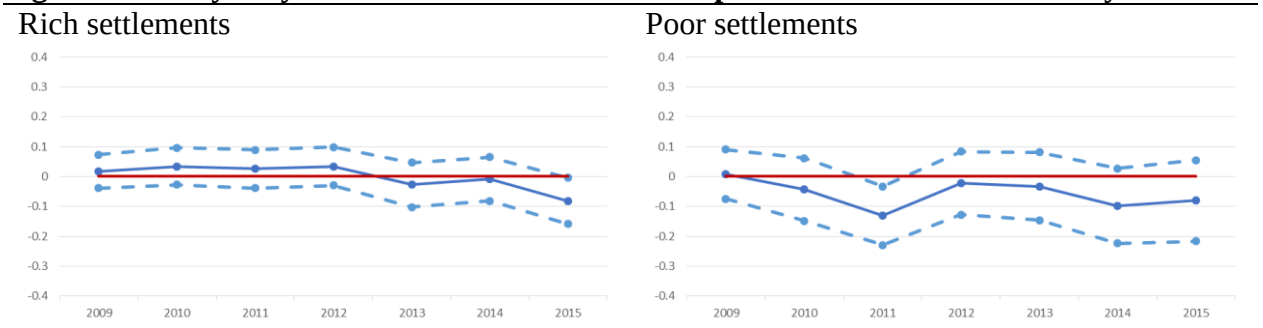


N=8938, number of schools=1597 N=10430, number of schools=2016

95% confidence intervals are shown with dashed lines. Controls are included and standard errors are clustered at the school level

Figure 7 shows that for 10th grade test scores, parallel trends assumption seems to hold better for schools in rich settlements but not really for poor settlements. The 2015 effect estimated to be negative both for schools located in rich and poor settlements, but it is not statistically different from zero for the latter.

Figure 7 – The yearly treatment effects for rich and poor settlements in secondary schools



N=4995, # of schools=963 N=3004, # of schools=603

95% confidence intervals are shown with dashed lines. Controls are included and standard errors are clustered at the school level

So regressions allowing the treatment effect to vary by the status of the school's location show that the overall negative effect found in the main specification is not driven by a negative effect on advantaged and a positive effect on disadvantaged schools. The treatment effects for primary and secondary schools located in poor settlements are the same or even more negative than the effects for schools in rich settlements. Checking placebo treatment effects in pre-treatment years revealed that parallel trends assumption might not hold though.

3.3 Results on the balanced panel

I check the robustness of the results by estimating the above models on a balanced panel, where only schools are included which existed throughout the period between 2008 and 2015. In this restricted sample, about half of the schools, and one third of the observations are lost, as a lot of schools closed and started to operate in this period. Still, this specification is appealing, because the effect can be measured on schools which existed both before and after the centralization. This is useful because the effect of the policy on a newborn or a dying school may be different than on schools that have existed for long. Finding similar effects on this subsample to the ones on the whole sample could show that a results are not driven by some strange dynamics in closing and opening schools.

Results on the balanced panel, are similar to what we saw on the unbalanced panel. The effect of the centralization policy is negative, but very close to 0, and for primary schools it is statistically insignificant. The effect is estimated to be between -0.04 and -0.07 depending on school level and specification.

Table 7 – Treatment effects on the balanced panel

VARIABLES	(1) 6th grade	(2) 6th grade	(3) 10th grade	(4) 10th grade
klik	-0.04	-0.04	-0.07***	-0.06***
	(0.04)	(0.04)	(0.02)	(0.02)
Observations	14,722	13,664	6,231	5,553
R-squared	0.00	0.00	0.01	0.02
Number of schools	1,868	1,859	798	784
School FE	YES	YES	YES	YES
Controls		YES		YES

Standard errors (in parentheses) are clustered at the school level: *** p<0.01, ** p<0.05, * p<0.1

Table 8 and Table 9 show again a similar result to the result on the whole sample: the policy was not successful in helping disadvantaged schools, the effect of the centralization on schools located in poor settlements is not positive, and in some specifications it is even more negative than the effect in rich settlements.

Table 8 – Treatment effect

	6th grade scores		10th grade results	
VARIABLES	(1)	(2)	(3)	(4)
klik	-0.02	-0.04	-0.06***	-0.05**
	(0.04)	(0.04)	(0.02)	(0.02)
klik *poor	-0.03	-0.02	-0.02	-0.02
	(0.03)	(0.03)	(0.02)	(0.02)
Observations	14,579	13,526	6,231	5,553
R-squared	0.00	0.00	0.01	0.02
Number of schools	1,850	1,841	798	784
School FE		YES	YES	YES
Controls		YES		YES

Standard errors (in parentheses) are clustered at the school level: *** p<0.01, ** p<0.05, * p<0.1

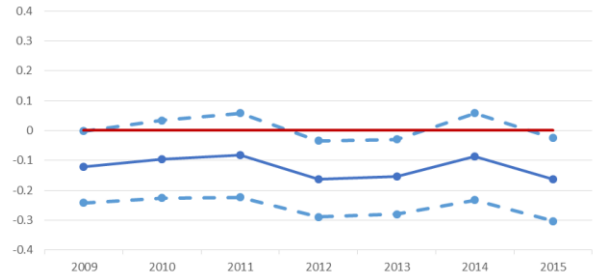
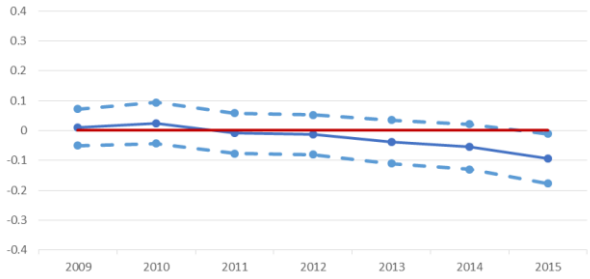
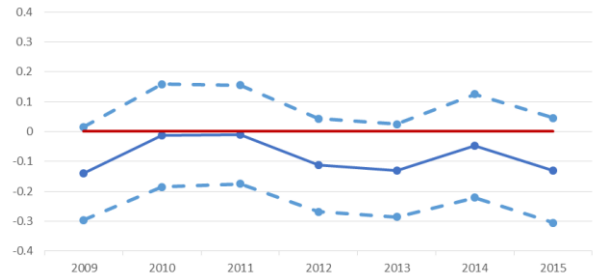
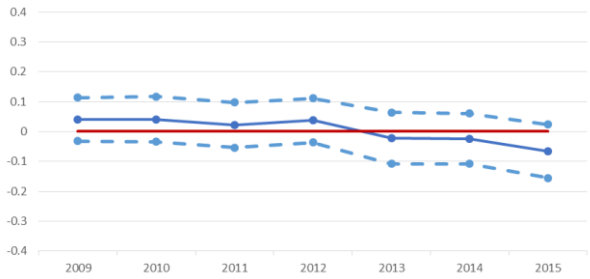
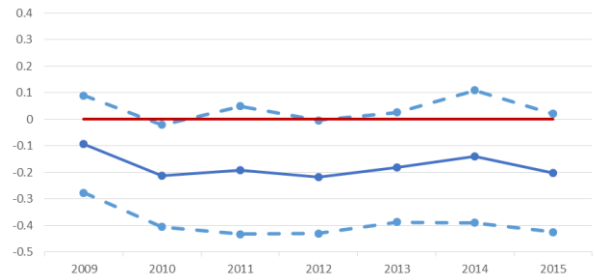
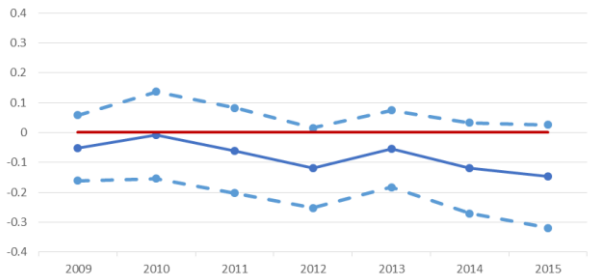
Table 9 – different effect by municipality, split sample regression

	6th grade scores				10th grade scores			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Rich settlements		Poor settlements		Rich settlements		Poor settlements	
klik	-0.04	-0.05	-0.06***	-0.06***	-0.06***	-0.07***	-0.07*	-0.06
	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)	(0.04)
Observations	7,602	6,907	4,175	3,626	5,774	4,995	3,246	1,927
R-squared	0.00	0.00	0.00	0.01	0.00	0.02	0.01	0.04
Number of schools	968	960	533	522	1,004	963	622	262
School FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls		YES		YES		YES		YES

Standard errors (in parentheses) are clustered at the school level: *** p<0.01, ** p<0.05, * p<0.1

Figure 8 shows yearly treatment effects on all schools in the balanced panel, and on schools located in rich and poor settlements. Figures show that the parallel trends assumption seems more valid for secondary schools. For 6th graders all yearly effects are estimated to be under 0. Still, the effect is estimated to be the most negative in 2015, which is agin inconsistent with a positive effect of the centralization.

Figure 8 – Yearly treatment effects on the balanced panel

Primary schools	Secondary schools
Full restricted sample	
	
N=13664, Number of schools=1859	N=5553, Number of schools=784
Schools located in rich settlements	Schools located in rich settlements
	
N=6907, Number of schools=960	N=3626, Number of schools=522
Schools located in poor settlements	Schools located in poor settlements
	
N=6619, Number of schools=881	N=1927, Number of schools=262

95% confidence intervals are shown with dashed lines. Controls are included and standard errors are clustered at the school level

Results on the balanced panel are similar to the ones on the unbalanced panel. So it is supported that reducing school autonomy had an overall small negative effect and that that there is no positive effect for disadvantaged schools either.

Chapter 4 – Conclusion

In my thesis I found that reducing school autonomy in 2013 had a negative effect on student performance in Hungary, but this effect was not substantial. The estimates were not statistically significant in all specifications, and close to zero in all specifications. A treatment effect larger than 0.03 standard deviations could be ruled out in all specifications for primary schools, and for secondary schools all results were inconsistent with an effect larger than -0.01.

The fact that the estimates were negative and about the same magnitude both for primary and secondary schools, and that the estimates were the most negative after three years into the policy, supported that this really is an effect of the policy, and not just noise. Finding a similar effect of the policy for primary and secondary schools, and with and without controlling for student background suggest that this negative effect is not driven by changes in student composition within schools. However, it can happen that the effect I found is driven by other unobserved variables, for example changes in the financial situation of church maintained schools.

Though the centralization policy intended to help disadvantaged schools, the effect on schools located in poor settlements was shown to be the same or even more negative than on schools in rich settlements. For disadvantaged primary schools an effect larger than 0.02, for disadvantaged secondary schools an effect larger than 0.03 could be ruled out. This shows that reducing school autonomy was not successful in reducing inequalities between schools.

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Appendix

Table A1 – Average test results and number of schools by the number of years they existed in the period

Number of years that the school existed in the period	Average NABC standardized result	Number of nonmissing observations	Number of primary schools with at least one non-missing observation	Average NABC standardized result	Number of nonmissing observations	Number of primary schools with at least one non-missing observation
	Primary schools			Secondary schools		
1	-0.33	411	411	-0.68	214	214
2	-0.37	566	289	-0.53	262	132
3	-0.23	660	229	-0.70	243	85
4	-0.22	1321	344	-0.44	562	146
5	-0.23	856	177	-0.68	300	64
6	-0.31	1072	184	-0.42	466	80
7	-0.24	1369	202	-0.42	747	111
8	0.04	14722	1868	0.23	6231	798

Table A2 - Number of observations in different subgroups

			Total	On restricted sample	Rich settlements	Poor settlements
Primary schools	Treatment	2012	1991	1605	939	1030
		2015	2097	1605	976	1098
	Control	2012	602	236	272	324
		2015	472	240	229	239
Secondary schools	Treatment	2012	660	539	424	236
		2015	678	533	434	244
	Control	2012	478	241	305	172
		2015	433	239	271	161
Grammar schools	Treatment	2012	308	276	193	115
		2015	307	274	194	113
	Control	2012	251	159	169	81
		2015	237	164	158	79
Vocational secondary schools	Treatment	2012	414	324	261	153
		2015	422	322	267	158
	Control	2012	247	107	153	93
		2015	225	107	134	88
Vocational training schools	Treatment	2012	460	353	281	179
		2015	471	347	286	185
	Control	2012	319	128	187	131
		2015	279	124	158	121

Table A3 Secondary school test results in 2012 and 2015 in the treatment and the control group by school type

		Year	Full sample	Rich settlements	Poor settlements
Grammar schools	Treatment	2012	1698	1730	1645
		2015	1694	1725	1642
	Control	2012	1634	1651	1599
		2015	1644	1662	1610
Vocational secondary schools	Treatment	2012	1576	1596	1544
		2015	1560	1583	1521
	Control	2012	1542	1561	1511
		2015	1535	1560	1496
Vocational training schools	Treatment	2012	1561	1588	1518
		2015	1542	1571	1498
	Control	2012	1508	1533	1471
		2015	1509	1544	1463