Pawel Bukowski and Martyna Kobus

The threat of competition and public school performance: evidence from Poland

Article (Accepted version) (Refereed)

Original citation:
Bukowski, Pawel and Kobus, Martyna (2018) The threat of competition and public school performance: evidence from Poland. Economics of Education Review. ISSN 0272-7757
DOI: 10.1016/j.econedurev.2018.09.004

Economics of Education Review 67 (2018) 14–24

Contents lists available at ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

The threat of competition and public school performance: Evidence from Poland

Paweł Bukowski*,[a], Martyna Kobus[b]

[a] Centre for Economic Performance, London School of Economics and Political Science, Houghton Street, London WC2A 2AE, United Kingdom
[b] Institute of Economics, Polish Academy of Sciences, Nowy Świat 72, 00-330 Warsaw, Poland

ABSTRACT

Theoretical literature on whether school competition raises public school productivity is ambiguous (e.g. MacLeod & Urquiola, 2015) and empirical evidence is mixed (e.g. Hsieh and Urquiola, 2006). Moreover, competition might itself be an outcome of changes in productivity (e.g. Hoxby, 2003). We provide evidence for the negative effect of the *threat* of competition on students' test scores in elementary public schools in Poland. The identification strategy uses the introduction of the amendment facilitating the creation of autonomous schools in Poland in 2009 as an external shock to the threat of competition. We focus on the short run in which there is only a limited set of actions available to schools' principals. For the total sample we find no effect, however, for more competitive urban educational markets, we report a drop in test scores in public schools following the increased threat of competition. This negative effect is robust to the existence of autonomous schools prior to the amendment and to the size of public schools. It does not result from a pre-existing or concurrent trend either. We exclude student sorting and adjustments in schools' expenditures as potential channels.

1. Introduction

Disconnection between educational expenditures and student achievement (Mayer & Peterson, 1999), as well as between standard measures of school quality and student achievement (Hanushek, 2003) has turned economists' attention to the incentive structure of public schools (Betts, 1995; Hoxby, 2003). There is a substantial disagreement in the literature on how market-like incentives can impact on public school performance. For those who argue that following increased competition public schools should improve their quality (Hoxby, 2003), the basic argument is that more productive schools would drive students away from their current school. This process would continue until higher-quality schools dominate the whole educational market or lower-quality schools respond to competitive pressure. This would be true if public schools reacted to competition by increasing productivity. McMillan (2005), MacLeod and Urquiola (2013) argue, however, that schools may find it optimal to exert lower effort if the losses from the smaller market share are more than offset by the saving in effort cost. In other words, the theoretical literature does not provide a clear sign for the productivity changes in public schools due to increased competition.

Empirical evidence is also not strong, as acknowledged in a recent edition of the Handbook of the Economics of Education (Epple, Romano, and Zimmer, 2016, p. 199), and faces methodological challenges. Most importantly, it suffers from identification problems. Usually competitors do not locate randomly, whereas ideally an exogenous variation in the size of competition is needed. The *actual* competition is endogenous with respect to market characteristics and actions of existing competitors (e.g. entry deterrence). Therefore, as pointed out by Hoxby (2003, p. 32), it is the *threat* of competition that matters.

In this paper we extend the empirical literature by providing evidence on the negative effect of the threat of competition from community schools on public school performance in Poland. Community schools are more autonomous than public schools with respect to teachers' hiring, their salaries, and collecting external funds, but they have to follow a nationwide curriculum. As an identification strategy we use the amendment to the Education Act introduced in March 2009, which facilitated the transformation of existing small public schools to community schools, but only for schools that have 71 and fewer students. The higher the percentage of students in schools with enrolment below 71, the more large public schools in the area are exposed to the threat of competition. The Polish reform is thus a source of exogenous variation in competitive pressure.

Using year 2009 as a breakthrough date in a difference-in-differences technique, we find that the higher threat of competition caused by the aforementioned reform has significant negative impact on the performance of urban public schools.[1] A one standard deviation

* Corresponding author.
E-mail addresses: p.bukowski@lse.ac.uk (P. Bukowski), mkobus@inepan.waw.pl (M. Kobus).

[1] Urban areas are more competitive educational markets than rural areas in Poland, due to a denser school network, lower transportation costs, and a more educated population.

https://doi.org/10.1016/j.econedurev.2018.09.004
Received 27 October 2017; Received in revised form 18 September 2018; Accepted 19 September 2018
Available online 25 September 2018

increase in the treatment intensity leads to around -.03 standard deviation change in the outcome.[2] The effect is similar for public schools that are larger (more than 150 and 300 students) and becomes stronger for schools that already have a community school in their neighbourhood and may thus be more aware of the consequences of the reform. We show that the effect does not result from a pre-existing or concurrent trend. The aggregate effect could be driven by changes in student population, changes in available resources or changes in productivity (i.e. residual effect) (Epple et al., 2016; Hsieh & Urquiola, 2006). We exclude student sorting and adjustments in school expenditures as potential channels.

To understand how reform increases competitive pressure, we must understand how it changes the incentives of local government and large public schools. These are related to how education is financed. Most of the schools in Poland are financed by the central government through subsidies. The subsidy from central government is tied to the pupil (the money goes with him/her). In theory this should be sufficient to cover all expenditures on education, excluding investments and pre-school education. In practice, it covers only around 50–70% of the costs (Herbst, Herczyński, & Levitas, 2009; Instytut Badań Edukacyjnych, 2011) and the rest is covered by local governments. Because of fixed costs, smaller schools yield higher cost per student and local governments have an incentive to close them down and switch pupils to bigger entities. Such a decision, however, might be not politically feasible. Parents object to closures because of a higher distance to a new school and the school's role in the cultural and social life of a local community. Community school is an attractive option for the local government, because with such schools the government does not have to finance expenditures that exceed the amount of the central subsidy. The school itself is more flexible than a public school in regulating its costs, because it is not forced to abide by collective bargaining agreements concerning teachers' wages (i.e. roughly 80% of the costs) and can acquire external funding. Due to lower costs, local politicians are incentivized to consider the transformation of all public schools into community schools.[3]

These changed incentives on the part of the local government create a new regime for large public elementary schools. For them, the liquidation of a small school is beneficial, because they can then capture students from a liquidated school and thus receive more funding. In the case of a handover they lose these potential funds, but now there is also a new type of competitor, with more flexibility in cost-rationalization and financing, which can steal students from them. Principals of large public schools are thus motivated to influence local politicians to close endangered schools and block the entry of new schools. It is unlikely that principals would form a coalition with parents and transform their school into a community school, because they are more free in their decisions when they respond to a local politician than directly to parents. Therefore, the ensuing number of liquidations and community schools is endogenous and subject to the degree of competition. There is also a potential heterogeneity in schools' response to the new type of competition.

Apart from the unique possibility for analyzing exogenous variation in the threat of competition that Polish reform enables, the Polish case is interesting for several other reasons. It is a transition country whereas most evidence comes from highly developed economies (mostly the United States and Scandinavia). It has experienced substantial gains in PISA scores, rising from 470 points in 2000 to 518 in 2012, placing Poland fifth in Europe and eleventh in the world. It significantly transformed its education system in the last decade.

Polish community schools differ from such schools in other countries (Heers, Van Klaveren, Groot, Maassen van den Brink et al., 2011). In Poland, they are not targeted at low or high-income students, as the main reason for their establishment is cost-rationalization. They operate like regular schools, but they are given substantial autonomy in management, which makes them similar to charter schools in the USA or autonomy schools in the UK. The current body of evidence finds modest effects of the impact of charter schools on the academic performance of public schools (Bettinger, 2005; Booker, Gilpatric, Gronberg, & Jansen, 2008). Clark (2009) analyses the British reform and finds no evidence of spillover effects of schools with increased autonomy on the neighbouring schools. Eyles and Machin (2015) study the introduction of academy schools in the UK and report positive effects on other schools. In other study, Cremata and Raymond (2014) find greater effects when the impact comes from higher-quality charter schools. This may be related to why we find a greater effect in urban areas, in which schools are generally better. Consistent with our results, using a variety of estimation approaches Imberman (2011) argues that charter schools may have negative a impact on public schools. Hoxby (2003) also exploits changes in the threat of competition and finds a positive reduced-form effect on student performance. We find a negative effect, and, in addition, we show that it was not driven by sorting of student and financial resources.[4]

Another strand of literature focuses on large scale voucher reforms in Chile and Sweden and exploits changes in private enrolment. In 1981 Chile introduced nationwide school choice by providing vouchers to students, resulting in 20 a percentage point increase in the private enrolment rate. Hsieh and Urquiola (2003) find that the main effect of this expansion is school stratification. This is consistent with the predictions of the reputational model by MacLeod and Urquiola (2009). If a school's reputation depends on both productivity and on the ability of its students, private schools have the incentive to boost their reputation by cream skimming the best students rather than by raising productivity. On the contrary, if schools cannot select on ability (e.g. they must select students via lotteries), then their model implies that school choice will unambiguously raise school performance and student outcomes. It seems that this is what happened in Sweden following the 1991 reform that led to the creation of independent municipality fund schools (Böhlmark & Lindahl, 2015). In the short run, on which we concentrate, school behaviour might be different than predicted by MacLeod and Urquiola (2009), because some options are not available, for instance, firing unproductive teachers. Often what remains available to school principals is either efficiency changes, that is, incentivising teachers to work harder, or boosting their school's prestige. We find no evidence for increasing teachers' salaries or investment in the infrastructure. Anecdotal evidence suggests that principals may deter the entry of community schools by shifting their attention from tasks oriented at the performance of students, to those which are visible to parents (e.g. school trips).

The paper is organised as follows. In Section 2 we describe the education system in Poland with particular emphasis on community schools and the reform. In Section 3 we present empirical strategy and data. Sections 4 and 5 contain, respectively, results and robustness checks. In Section 6 we analyse and discuss possible channels of transmission. The last section concludes.

[2] This magnitude corresponds to the effect of an increase of classroom size by one student in Israel or Sweden (Angrist & Lavy, 1999; Fredriksson, Öckert, & Oosterbeek, 2012) or by three students in California (Jepsen & Rivkin, 2009).

[3] For example, the mayor of Hanna in eastern Poland gave all public schools in his municipality to parental associations (Grabek, 2013).

[4] There is also related literature on the effects of decentralization in the US (Hoxby, 2000; Rothstein, 2007). This, however, provides choice between public school districts rather than between private and public schools, so this literature answers a different question and so far has produced mixed results. Similarly for the effects of private voucher-induced competition on public school performance in the USA (Abdulkadiroğlu, Angrist, Hull, & Pathak, 2016; Figlio & Hart, 2014; Hoxby, 2002).

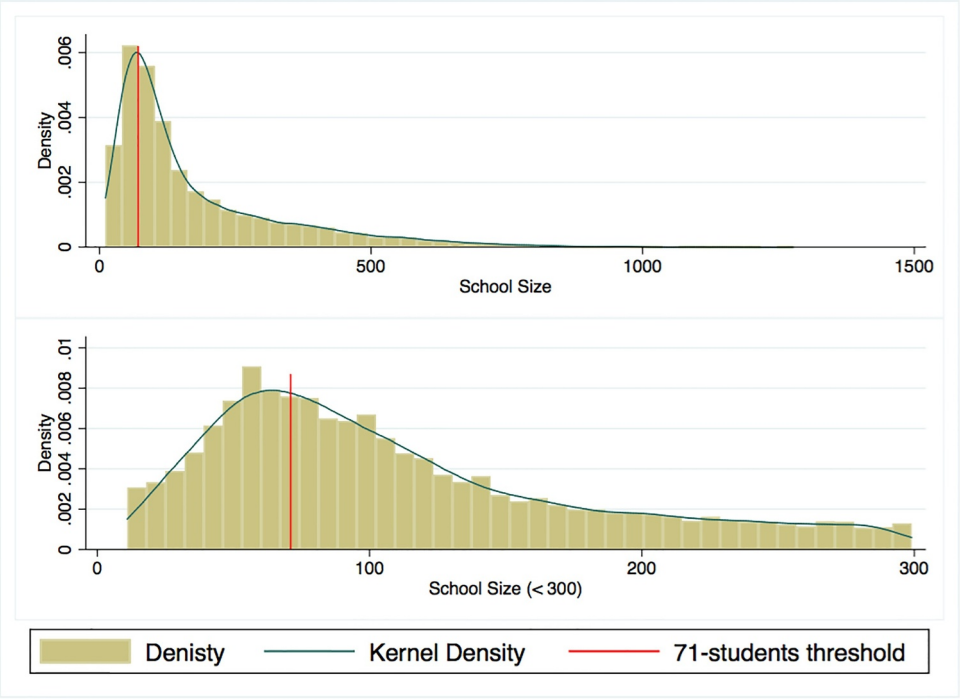

Fig. 1. The Distribution of School Size in 2008 Note: The plot presents the distribution of school size in 2008. The upper panel is for all schools, the bottom panel only for schools smaller than 300 students.

2. Community schools in Poland

Polish comprehensive education is compulsory and consists of six years of elementary school (ISCED 1), which is followed by three years of lower secondary school *gimnazjum* (ISCED 2). Elementary school and *gimnazjum* usually serve the same community of students, but they are separate entities, with different managerial and teaching bodies. After finishing the comprehensive part, student may finish their education or continue in academic, mixed or vocational higher secondary school (ICED 3). Admission to elementary school and *gimnazjum* is based on catchment areas. During comprehensive education, students are examined by two standardized, externally graded and obligatory examinations: a low-stake exam after elementary school (6th grade) and a high-stake exam after *gimnazjum* (9th grade).[5] The school averages from these exams are published in various unofficial school rankings. The only official measure of school quality is the school-level educational value added, but it is only available for *gimnazja*.

The Polish education system, and the Polish administrative system in general, is considered to be very decentralized (Herbst et al., 2009). All public elementary and secondary schools (*gimnazja*) are governed by local governments, and the role of central government is limited to educational financing and imposing a core curriculum. Local governments are free to open and close new schools, hire teachers, principals and redistribute money among schools. The subsidy from central government in practice covers between 50% and 70% percent of the costs in various municipalities (Herbst et al., 2009; Instytut Badań Edukacyjnych, 2011). They have to cover the rest, which makes closing and merging schools an attractive alternative. This, however, is often politically costly for local policymakers. The creation of community schools allows them to avoid such political risks, and local governments do not need to contribute their own revenues. They only transfer the subsidy from the central authority.

Community schools have to follow a nation-wide curriculum, but are more autonomous than public schools. They can manage the composition of the teaching body more flexibly, which is important given that teacher wages are almost 80% of the costs of public schools. In particular, teachers employed in regular public schools sign their contracts with their corresponding local government, but the lower band of their wages is set by the central authority (in agreement with the teachers' unions).[6] Moreover, they have several benefits, for instance, the right to a year-long sick leave, which is exceptional among occupational groups financed from the central budget. Conversely, the specifics of their contracts with the community school are negotiated at a school level and their wage is regulated by the free market. It is also important that they have better opportunities than public schools in terms of acquiring external funds (e.g. from the European Union). Finally, parental involvement in community schools is higher than in public schools since community schools are often led by parents' associations, so the principal - agent problems are less severe.

Before 2009 a school could be taken over by the association only after it had been liquidated first, that is, a new school had to be set up. Liquidation was a complicated process and many activists and politicians saw the need for an improvement of this procedure. The first official project was introduced in June 2008 by the ruling party, Civic Platform (PO) and it immediately ignited public debate. The opposition was trying to block the reform and the teachers' unions organised the nation-wide campaign against schools' handover "Do not let our school get ruined". After almost a year of ongoing debate, the amendment to the Education Act 1991 was finally introduced in March 2009. It allows the takeover of schools without putting them into liquidation when the school's enrolment is smaller than 70 pupils.[7] This number has no specific meaning. Fig. 1 shows the distribution of school size in Poland;

[5] The 9th grade exam serves as a basis for admission into higher secondary education (ISCED 3).

[6] In 2015 the minimum monthly gross wages ranged from 1513 PLN (340 EUR) to 3109 (700 EUR). Additionally, local municipalities have to make sure that the average *total* gross salary for each teacher's rank within municipality is at least as large as specified in *Karta Nauczciela*. In 2015 these averages ranged from 2717 PLN (612 EUR) to 5000 PLN (1126 EUR).

[7] The old way of a handover through liquidation was left as an option, though.

it shows no bunching around the value 71. The mean size is 192 and the median is 125. The distribution is relatively dispersed, with the standard deviation of 172. There are still some legal problems with the handover (e.g. the takeover has to be announced around 11 months in advance), but the 2009 amendment is considered a very important step in introducing community schools into the Polish system.

There are currently 11,398 public elementary schools and 949 elementary schools run by associations. Not all such schools result from handovers, but this is now the main channel through which community schools are established. Indeed, the amendment accelerated handovers significantly. While in 2009 there were 30 handovers, in 2011 the number was 89 and in 2012–244. In the year 2010, only 19 handovers took place, but this is due to local elections, which discourage any changes to the school network (Herczynski & Sobotka, 2013). Altogether, between 2008 and 2012, 446 schools were handed over to non-public associations, 84.9% of which were elementary schools.

3. Empirical strategy and data

The amendment facilitated the handing over of schools with less than 71 pupils. The existence of such schools is likely to be correlated with unobserved characteristics of *gminas* and therefore simple regression of outcome on the proportion of small schools will be biased. Yet, as long as these characteristics do not change over time, we can control for them by focusing on changes. We exploit the introduction of the amendment to the Education Act 1991 from March 2009 as a breakthrough point in difference-in-differences technique. In other words, we claim that schools that have low exposure to treatment, that is, an almost unchanged threat of competition following the reform, act as a control group for schools that face a bigger threat. If schools with low exposure faced bigger threats, then our results would give a lower (in absolute terms) bound estimate of the true effect. The threat of competition is measured as the fraction of students in a given municipality who attend a school with 70 or less pupils. The outcome variable is the result of standardised and obligatory nation-wide external exams taken by students at the end of elementary school in schools with more than 70 students. We use a set of controls, in particular, we control for the population size of the municipality, which to some extent determines the structure of the school network. Our goal is to capture the effect of the threat of competition, not the threat of being transformed into a community school.

We follow Card (1992) and use the following panel fixed effect model:

$$S_{igt} = \beta(T_g \times After_t) + \delta X_{gt} + \mu_i + \mu_t + \epsilon_{igt}, \quad (1)$$

where S_{igt} is an average test score in a public elementary school i located in a municipality g at time t. T_g is a fraction of students in a municipality g who attended elementary public school with less than 71 pupils in the

Table 1
Descriptive statistics (2008).

	N	Mean	SD	Min	Max	*Corr(x, T)*
			Unit of observation: Municipality			
Panel A: Total sample						
Standardized 6th grade exam	2477	−.12	0.54	−1.92	1.69	−0. 010***
Unemployment rate (in %)	2477	7.6	3.8	1	25	0.262***
Expenditures per capita (th.)	2477	2.547	1.067	1.606	4.5560	−0.01***
Population(th.)	2477	15	51	1.35	1710	−0.0003***
Population age 0–18 (in %)	2477	21	2.48	11	33	0.004***
Population density (th. per km^2)	2477	0.244	1.28	0.005	59.5	−0.010***
Educational expenditures per capita (th.)	2477	0.929	0.183	0.454	2.846	0.04***
Kindergarten attendance (in %)	2471	51	18	3	120	−0.167***
Sec. school gross enrollment (in %)	2465	97	16	3	206	−0.116***
Number of elementary-school students	2477	910	2177	64	66,450	−1e-05***
Students in schools <71 (in %)	2477	9.4	12.3	0	78.8	N/A
Community schools (in %)	2477	3.2	9.1	0	80	−0.010
Panel B: Rural only						
Standardized 6th grade exam	2229	−0.17	0.53	−1.93	1.69	−0.004
Unemployment rate (in %)	2229	7.7	3.8	1	25	0.171**
Expenditures per capita (th.)	2229	2.523	1.084	1.606	4.5560	−0. 01***
Population(th.)	2229	8.3	4.76	1.3	23.7	−0.006***
Population age 0–18 (in %)	2229	22	2.36	11	33	0.0005
Population density (th. per km^2)	2229	0.12	0.24	0.005	3.8	−0.101***
Educational expenditures per capita (th.)	2229	0.931	0.179	0.454	2.846	0.04***
Kindergarten attendance (in %)	2223	49	17	3	120	−0. 136***
Sec.school gross enrollment (in %)	2217	96	16	3	206	−0.085***
Number of elementary-school students	2229	557	319	64	1999	−7e-06***
Students in schools <71 (in %)	2229	10	13	0	79	N/A
Community schools (in %)	2229	3	9.2	0	80	0.010
Panel C: Urban only						
Standardized 6th grade exam	248	0.27	0.39	−0.86	1.35	−0.002
Unemployment rate (in %)	248	6	2.6	1	15	0.092
Expenditures per capita (th.)	248	2.768	0.871	1.786	8.663	−0.003
Population(th.)	248	79	145	23.8	1710	−0.00001
Population age 0–18 (in %)	248	19	2.1	12	27	0.002**
Population density (th. per km^2)	248	1.37	3.8	0.043	59.5	−0.0007
Educational expenditures per capita (th.)	248	0.917	0.222	0.559	1.554	0.0002
Kindergarten attendance (in %)	248	72	10	31	99	−0. 043***
Sec. School gross enrollment (in %)	248	109	11	68	165	−0. 049***
Number of elementary-school students	248	4079	5949	837	66,450	0
Students in schools <71 (in %)	248	1.6	2.5	0	11	N/A
Community schools (in %)	248	4.8	8	0	50	−0.031

Notes: All variables are defined at the municipality level, except the Standardized 6th Grade Exam, which is defined at the school level and we calculate the weighted average for each municipality. The last columns present correlations with the treatment variable at the municipality level, which is defined as a share of students attending schools smaller than 71 students. *** denotes significance at the 1% level, ** at the 5% level and * at the 10%.

year 2008. A higher value of this variable means a bigger threat of competition after the introduction of the 2009 amendment. The time dummy $After_t$ switches on for observations after the introduction of the amendment, that is, years 2009–2012 ("before" means years 2005–2008). μ_t are year-specific effects, X_{gt} are time-variant characteristics of *gminas*, μ_i are school fixed effects. Finally, ϵ_{git} is the error term.

The parameter of interest is β. As mentioned in the Introduction, theoretical literature is not clear about its sign, but the initial presumption was that it is positive, that is, we expect that a higher threat of competition induces public schools to improve their outcomes or that it induces the flow of students between schools that drives the result. Moreover, reading the current evidence, we expect the effect to be heterogeneous in sub-samples. In particular, the effect should be stronger in more developed and dense educational markets. Therefore we run separate analyses for both urban and rural areas, as there are substantial differences in Poland between these markets (Jakubowski & Kozińska-Bałdyga, 2005).

Our sample consists of 9846 publicly funded elementary schools with enrolment above 70 students (robustness checks restrict the sample to bigger entities). The data on the outcome variable come from the Central Examination Commission. The elementary school exam score is generally irrelevant for further education, however, it is obligatory and is considered by the local authority in the school evaluation. Students are examined in language and maths skills. The maximum score is 40. Information on school enrolment in 2008 comes from System Informacji Oświatowej (the System of Educational Information). This registry contains a rich set of characteristics for all schools in Poland, including spending, infrastructure and changes to the school network (handovers and closures). The characteristics of municipalities are taken from the Central Statistical Office of Poland. We could obtain information on school size for only 70% of observations. For the rest we approximate the number of students by multiplying the number of students who took the test by the number of grades (i.e. six). Imputations do not affect the results.

Table 1 presents the municipality-level descriptive statistics for the pre-treatment baseline (2008) and their cross correlations with the treatment variable. We define urban areas as municipalities at and above the 90th percentile of the distribution of municipality population size in 2008 (around 23 thousand people), and rural areas below the 90th percentile. In general, rural areas in Poland are significantly poorer and less developed than urban areas. 4.8% of schools in the urban areas are led by communities, while in the total and rural samples the share is around 3%. There is, however, a much higher percentage of students going to such schools in rural areas - 10% of all students (s.d. 12.6pp) - than in urban areas - 1.6% (s.d. 2.5pp). The treatment variable is positively correlated with the unemployment rate and educational expenditures, and negatively with total expenditures, population level and density, kindergarten and secondary school enrolment and with number of students. Overall, the higher the fraction of smaller schools in the area, the worse the municipality's characteristics. This is partially explained by the fact that there are more small schools in less populated areas, which are also poorer.

4. Results

We start by substantiating the claim that the reform increased the threat of competition. To this end, we estimate (1), but with the number of handovers as a dependent variable. Table 2 Columns (1) and (2) show that one standard deviation (σ) increase in the treatment variable leads to on average 0.02 more episodes of school handover. We now focus on the main results. Table 3 (column (1)) shows the results of panel fixed effect estimations, controlling for the year-specific effects but without the additional covariates. Because the estimator exploits the variation from within an observation, the unobservable and observable time - invariant characteristics of schools and *gminas* cancel out. The impact of the variable of interest is negative but insignificant. It becomes larger in absolute terms when one adds educational covariates (i.e. a municipality's gross enrolment in pre-school education, secondary education ratio and expenditures on education per capita) and covariates describing the general economic condition of a municipality (i.e. population size, unemployment rate and total expenditures per capita) (Table 3 column (2)). One σ increase in the fraction of small-school students causes a drop in the exam score on average by 0.010σ. The effect is heterogeneous in different areas (Table 3). In rural municipalities the effect is smaller than in the full sample and statistically insignificant. For the urban sub-sample the effect is, in absolute value, around three times bigger than in the full sample and significant at 1% level. One σ increase in the treatment variable causes a drop in the exam score on average by 0.028σ. The change in the value of treatment from minimum to maximum causes a decrease in the exam score by 0.26σ. Adding covariates does not change the magnitude of the results. In the total sample, only the coefficients of unemployment rate, population density and secondary school enrolment are significant. They are slightly negative. Population density is highly significant and positive in the rural sample.[8]

Table 2
The 2009 reform and changes in the school network.

Dependent variable:	*No. of school handovers*		*No. of school closures*	
	(1)	(2)	(3)	(4)
	Unit of observation: Municipality-Year			
Panel A: Total sample				
Treatment effect ($\Delta\sigma$)	0.022***	0.021***	0.028***	0.031***
	(0.005)	(0.005)	(0.007)	(0.007)
Observations	9908	9881	9908	9881
Number of municipalities	2477	2475	2477	2475
Mean of dep. variable	0.023	0.022	0.061	0.061
Panel B: Rural only				
Treatment effect ($\Delta\sigma$)	0.022***	0.020***	0.030***	0.032***
	(0.005)	(0.005)	(0.008)	(0.008)
Observations	8916	8889	8916	8889
Number of municipalities	2229	2227	2229	2227
Mean of dep. variable	0.022	0.022	0.061	0.062
Panel C: Urban only				
Treatment effect ($\Delta\sigma$)	0.029	0.034	−0.043	−0.045
	(0.023)	(0.025)	(0.060)	(0.061)
Observations	992	992	992	992
Number of municipalities	248	248	248	248
Mean of dep. variable	0.026	0.026	0.053	0.053
General characteristics	No	Yes	No	Yes
Educational characteristics	No	Yes	No	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes

Notes: Robust and clustered at the municipality level standard errors are reported in parentheses. Table presents the coefficient of interaction between the treatment variable and the dummy *After* from regression (1). General covariates include: unemployment rate, log total municipality expenditure per capita, population, population density and a share of people aged 0–18. Educational covariates include: kindergarten attendance, secondary school gross enrolment ratio and log municipality educational expenditures per capita. The dependent variables are the number of episodes of school handover (columns (1) and (2)) or school closure (columns (3) and (4)). *** denotes significance at the 1% level, ** at the 5% level and * at the 10%.

The results suggest that the introduction of the amendment changes the performance of urban schools. Why does the effect differ between urban and rural areas? Compared to the rural market, it is larger and more competitive with a denser school network and lower costs of transportation. Population density in urban areas is on average 1370 persons per square kilometre versus 120 in rural areas; the ratio of elementary schools per square kilometres equals 0.28 in the urban areas versus 0.06 in the rural areas. Finally, there is only 5% of tertiary

[8] We did not include the results of full regressions in the tables. These results are available on demand.

Table 3
Main results.

	Baseline		Pre-reform Community school		Schools > 150	Schools > 300
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Standardized 6th grade exam					
	Unit of observation: School-Year					
Panel A: Total						
Treatment effect ($\Delta\sigma$)	−0.007	−0.010	−0.013	−0.028*	−0.010	−.011
	(0.008)	(0.008)	(0.014)	(0.016)	(0.007)	(0.007)
Observations	64,964	64,488	17,297	16,928	33,975	16,606
Number of schools	9846	9838	2731	2731	4952	2424
Panel B: Rural only						
Treatment effect ($\Delta\sigma$)	−0.007	−0.005	−0.026	−0.023	−0.004	0.0004
	(0.009)	(0.009)	(0.022)	(0.023)	(0.009)	(0.012)
Observations	44,705	44,594	5697	5693	17,997	5788
Number of schools	6713	6705	887	887	2590	834
Panel C: Urban only						
Treatment effect ($\Delta\sigma$)	−0.026***	−0.028***	−0.020**	−0.022**	−0.028***	−0.031***
	(0.010)	(0.010)	(0.009)	(0.009)	(0.008)	(.0008)
Observations	20,259	19,894	11,600	11235	15,964	10,818
Number of schools	3133	3133	1844	1844	2360	1590
General characteristics	No	Yes	No	Yes	Yes	Yes
Educational characteristics	No	Yes	No	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust and clustered at the municipality level standard errors are reported in parentheses. Table presents the coefficient on the interaction term between the treatment variable and the dummy *After* from regression (1). General covariates include: unemployment rate, log total municipality expenditure per capita, population, population density and a share of people aged 0–18. Educational covariates include: kindergarten attendance, secondary school gross enrolment ratio and log municipality educational expenditures per capita. The dependent variable is the average of the standardized 6th grade exam score at the school level. *** denotes significance at the 1% level, ** at the 5% level and * at the 10%.

education degree holders in rural areas compared to 12% in urban areas.

5. Robustness

We carry out the following robustness checks. First, we take into account that the results might be different for schools that had a community school in their neighbourhood before the reform. Second, we check whether the effect differs for larger schools (more than 150 and 300 students) showing that the results are not because of the *direct* effect of a higher risk of handover. Third, we check whether we do not confound the treatment effect with the effect of concurrent trends that are underway in the municipality. Finally, we analyse the key assumption in the identification strategy, namely, of the same pre-treatment trends in the treated and control municipalities.

Public schools which have a community school in their vicinity might be more aware of the impact of such schools on their situation, and thus of the potential consequences of the reform. In addition, as shown in Tables 1 and 3, in the urban areas there are more community schools and the negative treatment effect is stronger. Is it possible then that the negative effect is driven by the pre-reform presence of community schools? To assess this, we limit our sample to only public elementary schools located in *gminas* in which there was at least one community school in the year 2008. There are now 2731 schools in our dataset. The effect becomes stronger in all samples (Table 3 columns (3) and (4)). One standard deviation increase in the treatment intensity leads to a 0.02–0.03σ drop in the test score. For urban schools, the magnitude of the results is a bit smaller than in the unrestricted sample, but still large and significant. As for the second robustness check, given the heated debate on the amendment before its official introduction and the fact that the threshold was chosen rather ad hoc, there is a possibility that schools with enrolment just above 71 students were in fact facing the risk of handover. This might have caused a specific reaction of these schools to the reform. Since there is a negative correlation in the sample between a school size and the treatment intensity, our results could be driven by this effect. Therefore, we now focus only on public elementary schools that have more than 150 students, which is close to the median size.[9] In the case of schools that have over 150 students the results are virtually the same as in the baseline specification. For schools over 300 students, the effect becomes slightly greater in absolute terms for the urban areas, more than 0.03σ (Table 3 columns (5) and (6)).

An example of a concurrent trend confounding the treatment effect is when places with higher exposure to treatment also face worse economic conditions, which may also contribute to the negative result. To assess this, we run a placebo test using the results of the secondary school exam as a dependent variable. Secondary school handovers are extremely rare, thus the amendment was unlikely to affect secondary schools. The exam takes place at the end of the 9th grade, three years after the elementary school test, and is also obligatory, standardized and externally graded. This analysis is performed at the municipality level. The results show the positive and strong effect of the reform, significant at the 10% level in the total and rural samples (Table 4 columns (1) and (2)). One standard deviation increase in the treatment intensity leads to around 0.027σ increase in the score. For the urban schools the effect is greater, almost 0.04σ, but not significant. If anything then, the potential concurrent trends might bias upward our main results and the reported negative effect might be a conservative estimate.

The key assumption in difference-in-differences technique is that *gminas* with different exposure to treatment have the same pre-treatment trends in outcomes. We run an "event study" using a generalized version of (1), where we allow the treatment effect to vary each year. Technically, we replace the interaction term between treatment and the single dummy for all periods after the reform, with individual interactions between treatment and year fixed effects. We estimate the following model:

[9] In Poland the marginal cost of additional students becomes flat for schools bigger than 150 students Jakubowski (2006), which implies that closures of such schools may be less profitable for the local government.

Table 4
Results at the municipality level.

	Placebo: 9th grade exam		*Sorting: 6th grade exam*	
	(1)	(2)	(3)	(4)
	Unit of observation: Municipality-year			
Panel A: Total sample				
Treatment effect ($\Delta\sigma$)	0.025*	0.026*	− 0.008	− 0.009
	(0.014)	(0.014)	(0.012)	(0.012)
Observations	17,294	17,272	17,337	17,301
Number of municipalities	2472	2472	2477	2475
Panel B: Rural only				
Treatment effect ($\Delta\sigma$)	0.027*	0.029*	− 0.005	− 0.002
	(0.015)	(0.015)	(0.013)	(0.013)
Observations	15,558	15,538	15,601	15,567
Number of municipalities	2224	2224	2229	2227
Panel C: Urban only				
Treatment effect ($\Delta\sigma$)	0.040	0.034	− 0. 059***	− 0. 061***
	(0.025)	(0.025)	(0.017)	(0.017)
Observations	1736	1734	1736	1734
Number of municipalities	248	248	248	248
General characteristics	No	Yes	No	Yes
Educational characteristics	No	Yes	No	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes

Notes: Robust and clustered at the municipality level standard errors are reported in parentheses. Table presents the coefficient on the interaction term between the treatment variable and the dummy *After* from regression (1). General covariates include: unemployment rate, log total municipality expenditure per capita, population, population density and share of people aged 0–18. Educational covariates include: kindergarten attendance, secondary school gross enrolment ratio and log municipality educational expenditures per capita. Columns (1)–(2), the dependent variable is the average of the standardized 9th grade exam score at the municipality level. Columns (3)–(4), the dependent variable is the average of the standardized 6th grade exam score at the municipality level. *** denotes significance at the 1% level, ** at the 5% level and * at the 10%.

$$S_{igt} = \sum_{t=2006}^{2011} \beta_t (T_g \times \mu_t) + \delta X_{gt} + \mu_i + \mu_t + \epsilon_{igt}. \quad (2)$$

The notation is similar as above (see Section 3). Note that the year 2005 is a reference point, thus, for instance, β_{2006} is the effect of the exposure to the treatment in 2006 relative to 2005. The reform was introduced in 2009, therefore if the common trend assumption is correct, the interactions prior to 2009 should be jointly insignificant. Fig. 2 plots the β_t coefficients against time (x-axis) along with confidence intervals, for the total and urban samples. There are no clear trends in coefficients for the pre-treatment years and the F-test cannot reject the null hypothesis that the interaction terms are jointly insignificant. At the same time there is a clear brake in the reported interactions for the years after the reform.

Another way to test the key assumption is by showing that there is no correlation between the treatment intensity and time trend for the period before the reform. To this end, we estimate the following model on the pre-reform sample (2005–2008):

$$S_{igt} = \beta (T_g \times Trend_t) + \gamma Trend_t + \delta X_{gt} + \mu_i + \mu_t + \epsilon_{igt}. \quad (3)$$

Under the parallel trend assumption $\beta = 0$ and indeed almost all reported coefficients are close to zero and statistically insignificant (Table 5). In two specifications in the total sample (Table 5 columns (1) and (3)), we report a small, positive and marginally significant correlation, which actually suggests that we might underestimate the absolute value of the treatment effect when looking at the full sample.

Finally, we run an augmented model, which identifies the treatment effect without relying on the parallel trend assumption. We follow Angrist and Pischke (2014), Jaeger, Joyce, and Kaestner (2018) and include interaction terms between municipality fixed effects and time trends into our baseline regression. This way we directly control for all municipality-level trends potentially affecting the outcome. We estimate the following regression

$$S_{igt} = \beta (T_g \times After_t) + \delta X_{gt} + \sum_i \gamma_i (\mu_g \times Trend_t) + \mu_i + \mu_t + \epsilon_{igt}, \quad (4)$$

where μ_g is municipality fixed effects. Note that we effectively include almost 2500 additional variables into the regression and thus we might have a low-power problem. Alternatively, we run a model where, instead of municipality-specific time trends we include covariate-specific time trends. Thus we can partially control for differential municipality-level trends, but avoid a low-power problem as we add only 10 new variables. The model takes the following form

$$S_{igt} = \beta (T_g \times After_t) + \delta X_{gt} + \sum_x \gamma_x (X_{g2008} \times Trend_t) + \mu_i + \mu_t + \epsilon_{igt}. \quad (5)$$

Controlling for covariate-specific trends does not affect the magnitude of the estimates (Table 6 columns (1) and (2)). The only difference, compared to the main results (Table 3), is that the effect of the threat of competition becomes significant for the whole sample. When municipality-specific trends are taken into account (Table 6 columns (3) and (4)) treatment effects are sizebly larger than in the baseline model. For the whole sample, one standard deviation increase in the treatment intensity leads to a 0.1σ drop in the test score. The same holds for rural and urban sub-samples, but the estimates are not precise, possibly due to a low-power problem. More negative effects (Table 5), might indicate that the pre-treatment trends bias the results upward in the previous specifications. To summarize, we test the parallel trend assumption in three different ways and in each test we find that the estimated negative effect of the threat of competition is not driven by the differential pre-treatment trends in outcome.

6. Channels

In the previous sections we provide evidence for the negative effect of increased competitive threat. As mentioned in the Introduction, it is typically difficult to disentangle whether this effect is driven by the flows/self-selection of students between schools (Hsieh & Urquiola, 2006), or by changes in schools' productivity. Sorting has a direct impact through the composition of students and an indirect impact through peer effects. It is rather unlikely that the effect observed in the year 2009 is driven by self-selection, as this would imply that parents decide to send their kids to a different school only for the last year of their elementary education and they decide so based only on the information that the reform will be introduced nation-wide. However, such relocations are more likely in the next few years, therefore we analyse sorting empirically the best we can with the available data.

We should first distinguish between changes in student flows prior to the reform and following the reform. The former could drive the result if, for example, small public schools had a disproportionate number of high-ability students, and as their schools closed before the reform, they moved to large public schools increasing their school-level average performance. The reform stopped this flow as it became harder to close a small school. Consequently, one should observe a negative effect of the reform on large schools' performance in affected areas. That such pre-reform flows drive the result is excluded by tests on parallel trend assumption in Section 5. Furthermore, such a scenario is also inconsistent with Fig. 3, which shows that small schools had disproportionately more *low*-ability students.

We now focus on student flows after the reform. While the reform was aimed at saving small schools, because of the strategic behaviour of large school principals, the reform could have in fact led to more closures. If students from small schools are on average under-performers, then their relocation to larger schools might explain the reported negative effect. However, for urban schools on which we report

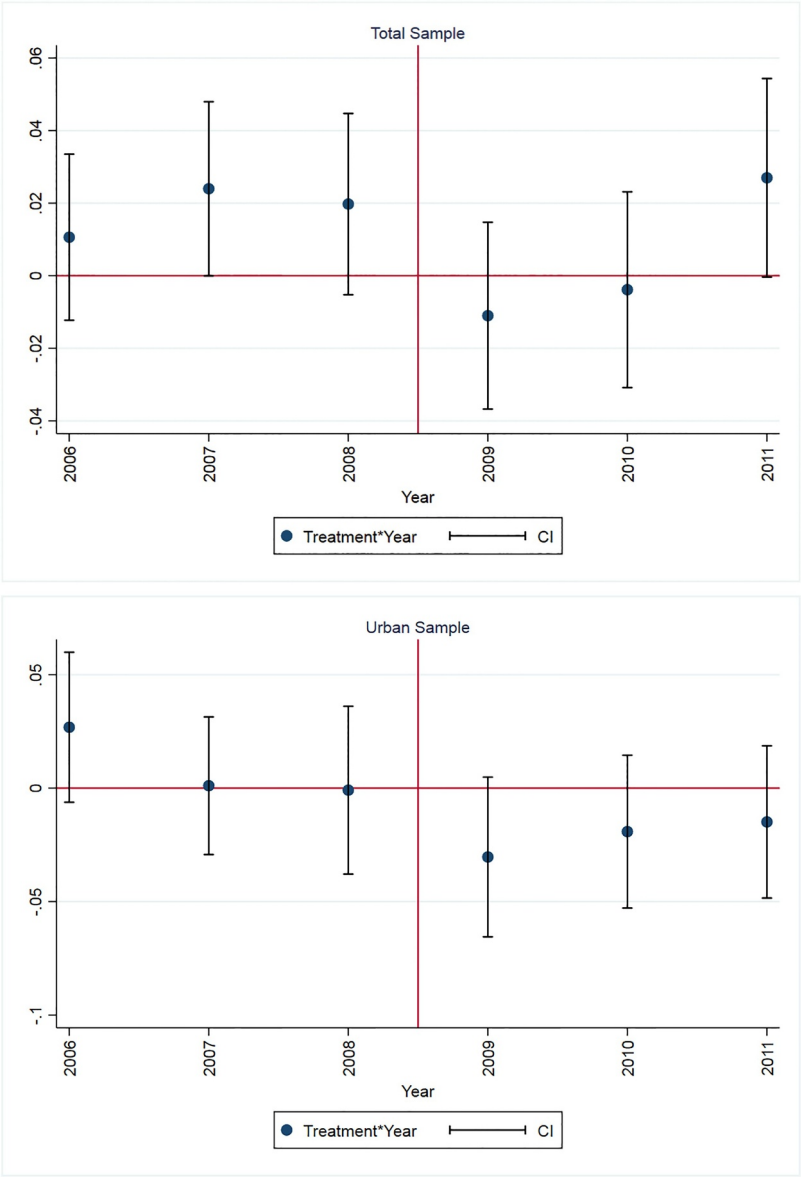

Fig. 2. Event Studies Note: The plot presents the estimates and the 95% confidence intervals of the interaction term between the treatment variable and the year fixed effects. The coefficient were obtained from a regression of the school-level 6th grade standardised exam scores on the set of interactions and the time fixed effects. The confidence intervals calculated using clustered and robust standard errors.

the negative impact of the threat of competition, the effect of the reform on the number of school closures is negative and insignificant (Table 2 columns (3) and (4)), which is inconsistent. Changes around the extensive margin thus appear unlikely. Obviously, the negative effect could also be explained by high-ability students switching from large public schools to the community schools, or low-ability students going in the opposite direction, relative to before the reform. We do not have individual level data to test this, so we follow the approach of Hsieh and Urquiola (2006). That is, we measure aggregate change in outcomes at the municipality level. If the documented effect is due to the flow of students within a municipality, there should be no effect on the municipality average score as a dependent variable. Note that, while this approach controls for student composition, it cannot net out peer effects. The results are very similar to the baseline school-level regressions (Table 4 columns (3) and (4)), except that there is now an even more negative effect of the threat of competition in urban areas. One standard deviation increase in the treatment intensity causes a 0.06σ drop in the average municipality exam score. All in all, we exclude student sorting as a channel of transmission of our main result.

We now further unpack school's productivity, that is, changes in school's resources, organization and management in reaction to bigger competition threat. The data (available from the schools' registry *System Informacji Oświatowej*) allow us to test whether the reform is related to changes in the share of spending devoted to renovations and teacher salaries, and on the size of green and sport areas per student. We estimate (1) with these indicators as dependent variables. We find no evidence for the impact of higher threat on these variables (Table 7). Either this channel is not important or principals re-direct resources towards other activities which we are unable to check. There is some anecdotal evidence that principals invest in activities that advertise their school to parents whose children attend a small school. They thus deter the entry of a community school. For example, in Kożuchowo (Kloc, 2012) there were plans for a local small school to be handed over. The principal of the large public school (which would take over students in the case of liquidation) was trying to persuade parents that

Table 5
Treatment effect and pre-reform trend (2005–2008).

	Dependent variable: standardized 6th grade exam			
	(1)	(2)	(3)	(4)
	Unit of observation: School-year			
Panel A: Total sample, 2005–2008				
Treatment x trend ($\Delta\sigma$)	0.008*	0.005	0.007*	0.004
	(0.004)	(0.004)	(0.004)	(0.004)
Observations	37,403	37,037	37,370	37,005
Number of schools	9757	9757	9749	9749
Panel B: Rural only, 2005–2008				
Treatment x trend ($\Delta\sigma$)	0.002	0.003	0.002	0.003
	(0.005)	(0.005)	(0.005)	(0.005)
Observations	25,778	25,777	25,745	25,745
Number of schools	6667	6667	6659	6659
Panel C: Urban only, 2005–2008				
Treatment x trend ($\Delta\sigma$)	−0.002	−0.003	−0.002	−0.004
	(0.006)	(0.006)	(0.005)	(0.006)
Observations	11,625	11,260	11,625	11,260
Number of schools	3090	3090	3090	3090
General characteristics	No	Yes	No	Yes
Educational characteristics	No	No	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes

Notes: Robust and clustered at the municipality level standard errors are reported in parentheses. Table presents the coefficient on the interaction term between the treatment variable and the time trend, estimated for the years before 2009. General covariates include: unemployment rate, log total municipality expenditure per capita, population, population density and a share of people aged 0–18. Educational covariates include: kindergarten attendance, secondary school gross enrolment ratio and log municipality educational expenditures per capita. The dependent variable is the average of the standardized 6th grade exam score at the school level. *** denotes significance at the 1% level, ** at the 5% level and * at the 10%.

his school was better than the newly established community school: "[...] for the parents of students [from the small school] he organised attractive and competitive curriculum. Additional sport and language classes, after-class activities, school trips, cafeteria, bus transportation of students, safety were the main points. This offer was passed over to all parents." (Kloc, 2012, p. 19).

7. Conclusions

Using the Polish education reform of 2009, we provide evidence that competitive effects caused by the threat of establishing more autonomous schools are negative, significant, robust and appear mainly in urban areas. One standard deviation increase in the treatment intensity leads to around -.03 standard deviation change in student performance. This magnitude is not small, it corresponds to the effect of an increase of classroom size by one student in Israel or Sweden (Angrist & Lavy, 1999; Fredriksson et al., 2012) or by three students in California (Jepsen & Rivkin, 2009). The reduced-form results contrast with the current body of evidence, which tend to find either positive or null effects of school competition (Epple et al., 2016), with a notable exception of a negative effect reported by Imberman (2011). One reason why our study is unique is that, we capture an exogenous change in the *threat* of competition. This way, we can account for the fact that actual entries into educational markets may be endogenous to school conduct or reactions of existing schools. In particular, principals might have an incentive to block the entry of a new school. The entry deterrence might affect the performance of students, however, in a different way than actual competition. Our paper is similar to Hoxby (2003), which also exploits changes in the threat of competition, but also finds a positive reduced-form effect on student performance.

Table 6
Municipality-specific time trends.

	Dependent variable: standardized 6th grade exam			
	(1)	(2)	(3)	(4)
	Unit of observation: School-year			
Panel A: Total sample				
Treatment effect ($\Delta\sigma$)	−0.017**	−0.015*	−0. 104***	−0.103***
	(0.008)	(0.008)	(0.034)	(0.034)
Observations	64,488	64,488	64,488	64,488
Number of schools	9838	9838	9838	9838
Panel B: Rural only				
Treatment effect ($\Delta\sigma$)	−0.005	−0.005	−0.059	−0.058
	(0.009)	(0.009)	(0.039)	(0.039)
Observations	44,594	44,594	44,594	44,594
Number of schools	6705	6705	6705	6705
Panel C: Urban only				
Treatment effect ($\Delta\sigma$)	−0. 03***	−0.029***	−0.067	−0.066
	(0.010)	(0.010)	(0.044)	(0.044)
Observations	19,894	19,894	19,894	19,894
Number of schools	3133	3133	3133	3133
General characteristics	Yes	Yes	Yes	Yes
Educational characteristics	Yes	Yes	Yes	Yes
Time fixed effects	No	Yes	No	Yes
School fixed effects	Yes	Yes	Yes	Yes
Covariates (in 2008) x trend	Yes	Yes	No	No
Municipality FE x trend	No	No	Yes	Yes

Notes: Robust and clustered at the municipality level standard errors are reported in parentheses. Table presents the coefficient on the interaction term between the treatment variable and the dummy *After* from regression (1). General covariates include: unemployment rate, log total municipality expenditure per capita, population, population density and a share of people aged 0–18. Educational covariates include: kindergarten attendance, secondary school gross enrolment ratio and log municipality educational expenditures per capita. In addition, each regression includes either the interaction term between covariates (in 2008) and time trend or municipality-specific time trends. The dependent variable is the average of the standardized 6th grade exam score at the school level. *** denotes significance at the 1% level, ** at the 5% level and * at the 10%.

We focus on the short run in which there is only a limited set of actions available to schools' principals. The negative effect can be driven by the outflow of good students, adjustment in available resources or negative change in productivity (i.e. residual effects). We exclude student sorting and adjustments in schools' expenditures as potential channels, and conclude that the threat of competition might have a negative effect on school productivity. More research is needed to fully understand the mechanisms at play. The anecdotal evidence suggests that when the decisions are made in the short run, school principals may use simple marketing actions to attract parents, such as school trips. These activities might shift the attention of teachers and students away from learning. Therefore, the promotion of performance-based school rankings or an accountability system might alleviate the short-run negative impact of the threat of competition.

Apart from the unique possibility to analyse exogenous variation in the threat of competition that the Polish reform enables, the Polish case is interesting for other reasons. The Economist (2013) wrote "Poland has made some dramatic gains in education in the past decade. Before 2000 half of the country's rural adults had finished only primary school. Yet international rankings now put the country's students well ahead of Americas in science and maths (the strongest predictor of future earnings), even as the country spends far less per pupil. What is Poland doing right? And what is America doing wrong?". In other words, by studying the determinants of student performance in countries like Poland, we can also learn how to improve education systems in highly developed economies.

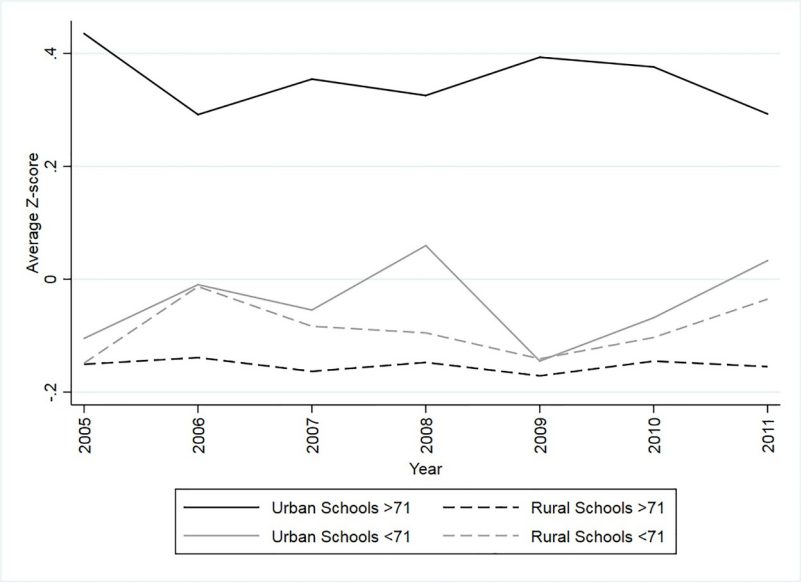

Fig. 3. The Average School Performance Note: The plot presents the average standardized (z-score) exam score for schools larger than 71 students, smaller than 71 students, in a breakdown by urban and rural areas.

Table 7
School spending.

Dep. variable:	Share of renovation costs (in%)		Share of teacher salaries (in%)		Greenareas per student (in m^2)		Sport areas per student (in m^2)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Unit of observation: School-year			
Panel A: Total sample								
Treatment effect ($\Delta\sigma$)	1.132	1.367	0.148	0.069	0.205	0.215	0.0003	-0.011
	(2.586)	(2.512)	(0.001)	(0.001)	(0.259)	(0.274)	(0.128)	(0.134)
Observations	45,639	45,371	45,454	45,187	45,596	45,331	45,596	45,331
Number of schools	9191	9183	9193	9185	9197	9189	9197	9189
Mean of dep. variable	75.84	75.886	64.8	64.8	22.945	22.991	13.484	13.504
Panel B: Rural only								
Treatment effect ($\Delta\sigma$)	2.362	2.558	− 0.011	−.042	.161	.165	−.121	−.114
	(2.975)	(3.004)	(0.001)	(0.001)	(0.300)	(0.308)	(0.147)	(0.150)
Observations	31,498	31,405	31,379	31,286	31,493	31,402	31,493	31,402
Number of schools	6311	6303	6313	6305	6319	6311	6319	6311
Mean of dep. variable	77.196	77.344	64.7	64.7	26.867	26.873	15.86	15.86
Panel C: Urban only								
Treatment effect ($\Delta\sigma$)	0.458	0.227	0.134	0.061	− 0.157	− 0.180	−0.344	−0.305
	(2.612)	(2.625)	(0.002)	(0.002)	(0.198)	(0.184)	(0.362)	(0.346)
Observations	14141	13,966	14,075	13,901	14,103	13,929	14,103	13,929
Number of schools	2880	2880	2880	2880	2878	2878	2878	2878
Mean of dep. variable	72.818	72.607	65.2	65.2	14.188	14.238	8.18	8.191
General characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Educational characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust and clustered at the municipality level standard errors are reported in parentheses. Table presents the coefficient on the interaction term between the treatment variable and the dummy *After* from regression (1). General covariates include: unemployment rate, log total municipality expenditure per capita, population, population density and share of people aged 0–18. Educational covariates include: kindergarten attendance, secondary school gross enrolment ratio and log municipality educational expenditures per capita. *** denotes significance at the 1% level, ** at the 5% level and * at the 10%.

Acknowledgment

We thank Sascha O. Becker, Roman Dolata, John S. Earle, Torberg Falch, David Figlio, Jan Herczyński, Gábor Kézdi, Małgorzata Kłobuszewska, Sergey Lychagin, Stephen Machin, Sandra McNally, Aneta Sobotka, Mateusz Żółtak; the participants of seminars at the Centre for Economic Performance at LSE, Central European University and The Educational Research Institute in Warsaw; the participants of The Workshop on Educational Governance in Trondheim, the 6th RGS Doctoral Conference in Economics in Bohum and EALE Conference in Warsaw for their comments and suggestions. We are grateful to the anonymous reviewers for their careful reading and excellent comments. Martyna Kobus' participation in the project was supported by the Polish Ministry of Science and Higher Education under a "Mobility Research Award" (contract No. Nr 893/MOB/2012/0). All opinions expressed are those of the author and have not been endorsed by the Ministry. All errors are ours.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.econedurev.2018.09.004

References

Abdulkadiroğlu, A., Angrist, J. D., Hull, P. D., & Pathak, P. A. (2016). Charters without lotteries: Testing takeovers in new orleans and boston. *American Economic Review, 106*(7), 1878–1920.

Angrist, J. D., & Lavy, V. (1999). Using maimonides' rule to estimate the effect of class size on scholastic achievement. *The Quarterly Journal of Economics, 114*(2), 533–575.

Angrist, J. D., & Pischke, J.-S. (2014). *Mastering'Metrics: The path from cause to effect.* Princeton University Press.

Bettinger, E. (2005). The effect of charter schools on charter students and public schools. *Economics of Education Review, 24*, 133–147..

Betts, J. R. (1995). Does school quality matter? evidence from the national longitudinal survey of youth. *The Review of Economics and Statistics, 77*(2), 231–250.

Böhlmark, A., & Lindahl, M. (2015). Independent schools and long-run educational outcomes: Evidence from Sweden's large-scale voucher reform. *Economica, 82*(327), 508–551.

Booker, T., Gilpatric, S., Gronberg, T., & Jansen, D. (2008). The effect of charter schools on traditional public school students in texas: Are children who stay behind left behind? *Journal of Urban Economics, 64*, 123–145..

Card, D. (1992). Using regional variation in wages to measure the effects of the federal minimum wage. *ILR Review, 46*(1), 22–37.

Clark, D. (2009). The performance and competitive effects of school autonomy. *Journal of Political Economy, 117*(4), 745–783.

Cremata, E. J., & Raymond, M. E. (2014). The competitive effects of charter schools: Evidence from the district of columbia. *Association for Education Finance and Policy Conference Working Paper*.

Economist, T. (2013). Best and brightest. economist.com[Online; posted 21-August-2013].

Epple, D., Romano, R., & Zimmer, R. (2016). Charter schools: A survey of research on their characteristics and effectiveness. *Handbook of the Economics of Education,* 139–208.

Eyles, A., & Machin, S. (2015). *The introduction of academy schools to England's education* Centre for Economic Performance Discussion Paper No. 1368.

Figlio, D., & Hart, C. (2014). Competitive effects of means-tested school vouchers. *American Economic Journal: Applied Economics, 6*(1), 133–156.

Fredriksson, P., Öckert, B., & Oosterbeek, H. (2012). Long-term effects of class size. *The Quarterly Journal of Economics, 128*(1), 249–285.

Grabek, A. (2013). Oswiata bez karty nauczyciela. Online; posted 20-December-2013, http://www.rp.pl/artykul/19,1074030-Oswiata-bez-Karty-nauczyciela.html?p=2.

Hanushek, E. A. (2003). *The economics of schooling and school quality. Vol. 159.* Edward Elgar Pub.

Heers, M., Van Klaveren, C., Groot, W., Maassen van den Brink, H., et al. (2011). *Community schools unfolded: A review of the literature* TIER working paper series, 12(04).

Herbst, M., Herczyński, J., & Levitas, A. (2009). *Finansowanie oświaty w polsce: Diagnoza, dylematy, możliwości.* Wydawnictwo Naukowe Scholar.

Herczynski, J., & Sobotka, A. (2013). *Siec szkol podstawowych i gimnazjow w Polsce 2007–2012 - Draft version of the reportTechnical Report.* Instytut Badan Edukacyjnych.

Hoxby, C. M. (2000). Does competition among public schools benefit students and taxpayers? *American Economic Review, 90*(5), 1209–1238.

Hoxby, C. M. (2002). *How school choice affects the achievement of public school students.* Hoover Press, Stanford Choice with Equity

Hoxby, C. M. (2003). *School choice and school productivity. could school choice be a tide that lifts all boats? The economics of school choice*. University of Chicago Press287–342.

Hsieh, C.-T., & Urquiola, M. (2003). When schools compete, how do they compete? an assessment of Chile's nationwide school voucher program. National Bureau of Economic Research Working Paper No. 1008.

Hsieh, C.-T., & Urquiola, M. (2006). The effects of generalized school choice on achievement and stratification: Evidence from Chile's voucher program. *Journal of Public Economics, 90*(8), 1477–1503.

Imberman, S. (2011). Achievement and behavior in charter schools: Drawing a more complete picture. *Review of Economics and Statistics, 93*, 416–435..

Instytut Badań Edukacyjnych (2011). Raport o stanie edukacji 2010.

Jaeger, D., Joyce, T., & Kaestner, R. (2018). Does reality tv induce real effects? on the questionable association between 16 and pregnant and teenage childbearing. *Journal of Business and Economic Statistics* forthcoming Vol.

Jakubowski, M. (2006). *Efektywność instytucji zarządzających lokalnymi dobrami publicznymi na przykładzie inicjatywy Mała Szkoła.* WNE Uniwersytet Warszawski Ph.D. thesis.

Jakubowski, M., & Kozińska-Bałdyga, A. (2005). ÂGmałe szkoły–polska odpowiedź na wybrane problemy wiejskiej oświaty. *Wieś i Rolnictwo, 3*, 2005.

Jepsen, C., & Rivkin, S. (2009). Class size reduction and student achievement the potential tradeoff between teacher quality and class size. *Journal of Human Resources, 44*(1), 223–250.

Kloc, K. (2012). *Konflikty w procesie racjonalizacji sieci szkółTechnical Report.* Ośrodek Rozwoju Edukacji.

MacLeod, W. B., & Urquiola, M. (2009). Anti-lemons: school reputation and educational quality. National Bureau of Economic Research Working Paper No. 15112.

MacLeod, W. B., & Urquiola, M. (2013). *Competition and educational productivity: Incentives writ large.* University of Chicago Press Education Policy in Developing Countries.

MacLeod, W. B., & Urquiola, M. (2015). Reputation and school competition. *American Economic Review, 105*(11), 3471–3488.

Mayer, S. E., & Peterson, P. E. (1999). *Earning and learning: How schools matter.* Brookings Institution Press.

McMillan, P. (2005). Competition, incentives, and public school productivity. *Journal of Public Economics, 89*, 1131–1154.

Rothstein, J. (2007). Does competition among public schools benefit students and taxpayers? comment. *American Economic Review, 97*(5), 2026–2037.