Scale economies can offset the benefits of school competition[1]

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Abstract

For a given size of an educational market, more school choice and competition in the form of more suppliers, means that suppliers will on average serve fewer pupils. This implies a trade-off between scale and competition which has been largely ignored in the economics of education literature. We study this trade-off using a large school consolidation reform in the Netherlands that decreased the supply of schools by on average 15 percent, but where the reduction in the supply of schools varied considerably across municipalities. We find that reducing the number of schools by 10 percent *increases* pupils' achievement by about 3 percent of a standard deviation. We present evidence that in our setting scale effects dominate the effects of choice and competition. More generally, our results illustrate that ignoring scale effects can lead to substantial bias in general equilibrium estimates of choice and competition.

JEL-codes: I21, I22, H75, D40

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1 Introduction

If more choice and competition is induced by an increase in the number of suppliers, and if the size of the market is fixed, then suppliers will on average operate on a smaller scale. This is the classical trade-off between market power and scale economies of anti-trust economics (Williamson, 1968).

In education markets this trade-off is particularly apparent in the context of school consolidation. Berry and West (2008) and Kenny and Schmidt (1994) for example, study the massive school consolidation that took place in the United States in the last century. From 1930 to 1970, as 120,000 schools and 100,000 districts disappeared, average school size increased from 87 to 440 students. The increase in scale was thus accompanied by a decrease in choice and competition. Although Berry and West note that the size effects they estimate also include potential competition effects, they do not disentangle the two.

Just like potential competition effects are contained in the estimated scale effects of the consolidation literature, potential scale effects may also confound the estimates of studies investigating the general equilibrium effects of school choice and competition. Hoxby (2000) for example exploits variation in the supply of local school districts to estimate competition effects. By keeping population size of a metropolitan area constant however, varying the number of school districts will also vary school district size.

The main contribution of this paper is to explicitly address this trade-off between scale and competition, which has been largely ignored in the economics of education literature. We do this using a school consolidation reform in the Netherlands which led to a large reduction in the supply of schools over a short period of time in the 1990s. This can be seen in Figure 1, which shows the number of primary schools in the Netherlands by year for the period from 1990 to 2004.

The Dutch education system is particularly suited for studying issues related to school choice and competition because parents have the possibility to react to supply

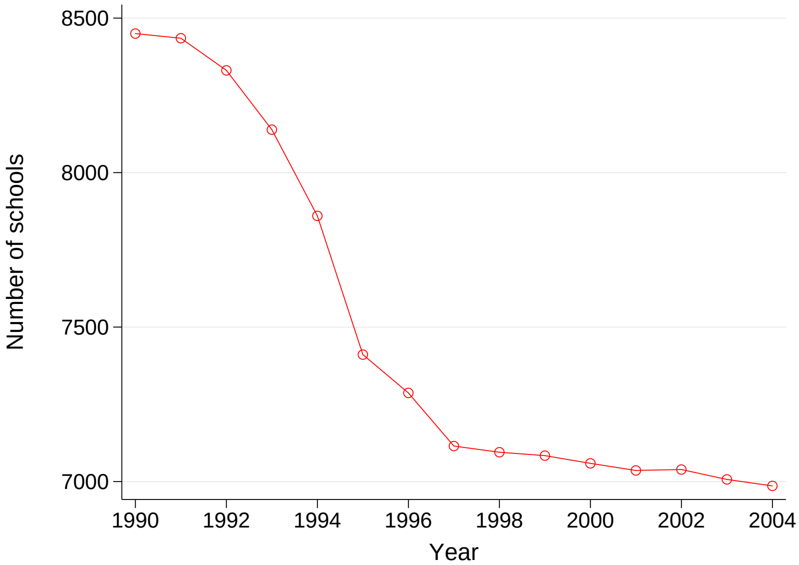

Figure 1. Number of primary schools in the Netherlands 1990−2004

shocks. Since 1917 the primary school system in the Netherlands is a universal voucher system, and parents have the possibility to enroll their child in the school of their choice, irrespective of where they live and how much they earn. In this voucher system all schools, publicly or privately operated, are funded by the central government through a "money follows pupil"-mechanism. Schools only receive this funding if the number of enrolled pupils is above a minimum required school size, which is set by the national government and varies across municipalities.

We use the number of schools in a municipality as measure of school choice and competition.[1] A change in the number of schools in a municipality changes the number of schools from which parents can choose in their own municipality. A change in the number of schools in a municipality also changes the number of competitors with which the (remaining) schools in a municipality have to compete.

The school choice literature has struggled to find exogenous supply side variation to estimate competition effects. We argue below that we can credibly identify the effect of the supply of schools on pupils' achievement, because the school consolida-

[1]Our results are, however, not dependent on this choice and competition measure. In the appendix we report results based on the Herfindahl index instead of the number of schools.

tion was triggered by a policy that revised minimum required school size rules. In our 2SLS analysis we instrument changes in the number of schools in a municipality with the change in the minimum required school size due to the change in rules, while controlling for municipality and time fixed effects.

Our analysis focuses on two cohorts of pupils: the last cohort of pupils who finished primary school before the policy was announced and the first cohort of pupils who enrolled in primary school when the policy was fully implemented. The results indicate that the consolidation reform, which decreased the supply of schools by 15%, increased pupil performance on average by 4% of a standard deviation. School segregation did not change as a result of the change in the supply of schools.

This positive effect of school consolidation appears to be driven by a *positive* effect on achievement of a *reduction* in school choice as measured by the number of schools. However, as school choice decreased, school size increased. In the second part of our analysis we therefore disentangle choice and scale effects. We report results that assume exogeneity of school size, and also results where we instrument school size with municipality level fertility shocks. The latter approach aims to address the main source of endogenous school size, namely within municipality sorting. While exploiting very different variation, both approaches deliver nearly identical results. Once we take scale into account the effect of school choice drops substantially, is close to zero and no longer statistically significant. At the same time there are significant positive returns to scale which drive the positive consolidation effects. These results highlight the potential trade-off between the benefits of choice and competition on the one hand and scale economies on the other hand. More generally, our results illustrate that ignoring scale effects can lead to substantial bias in general equilibrium estimates of choice and competition.

The remainder of the paper continues as follows. The next section discusses how our contribution fits into the existing literature on school choice and competition. Section 3.1 provides information about the Dutch education system, thereby focusing on existing mechanisms for parents to exercise choice and for schools to respond to that. Section 3.2 describes the details of the change in the minimum school size rule that we use as our source of exogenous supply-driven variation in school choice and competition. Section 4 introduces the data and Section 5 provides details of our estimation strategy. Section 6 presents and discusses the results and Section 7 summarizes and concludes.

2 Related research

In this section we briefly discuss how the research in this paper is related to different strands of the literatures on school choice and competition, and school consolidation.

Most studies deal with choice programs that expand choice for a small group of students. Examples are vouchers programs (Rouse, 1998; Angrist et al., 2002; Peterson et al., 2003; Krueger and Zhu, 2004; Angrist et al., 2006), charter schools (Bettinger, 2005; Hoxby and Rockoff, 2005; Bifulco and Ladd, 2006; Hanushek et al., 2007; Abdulkadiroglu et al., 2009; Imberman, 2010), or programs that generate changes in the choice set as in Cullen et al. (2006); Lavy (2010). These studies typically examine the impact of the program on the achievement of the students that make use of it. The impact on other students (the peers that are left behind and the new peers) is usually ignored, as are the effects through more competitive pressure on schools (see Ladd, 2002; Neal, 2002, for a discussion). Compared to the partial equilibrium effects that these studies estimate, our analysis looks at the average impact on all pupils in a municipality where the degree of school choice and competition has changed.

Closer to our research are studies that examine the general equilibrium effects of system-wide variation in school choice and competition. The evidence from these studies is mixed. Hoxby (2000) looks at the impact of Tiebout choice in American public education on various indicators of achievement by exploiting variation in the number of school districts across metropolitan areas induced by variation in natural boundaries. She reports significantly positive effects of school choice and competition on achievement. To measure the effects of unrestricted choice on educational outcomes in Chile, Hsieh and Urquiola (2006) use the differential impact across municipalities that the provision of vouchers had on private enrollment. They find no evidence that choice improved average educational outcomes. However, they do find evidence that the program increased sorting, as the best public school students left for the private sector. Böhlmark and Lindahl (2008) use a similar approach to assess the impact of a voucher reform that was implemented in Sweden in 1992. While they find moderately positive short-term effects of an increase in the private school share, they fail to find any impact on medium or long-term educational outcomes. Gibbons et al. (2008) use discontinuities generated by admissions district boundaries and find that performance gains from greater school competition among English primary schools are limited. Finally, Card et al. (2010) use variation in the fractions of Catholics and of new homes across local areas in Ontario to find that competitive pressure has a significantly positive impact on test score gains. Our study differs from these previous papers on three accounts. First, unlike previous studies this paper explicitly addresses scale economies resulting from a change in supply. Second, this paper exploits an arguably more credible source of exogenous variation in the supply of schools than most previous studies. Finally, while most existing studies start from a situation with limited choice, the starting point in this paper is a situation with a lot of choice being in place already for a long time.

Since the reform that we exploit in this paper led to school mergers, our work is also related to studies that deal with consolidation of schools or school districts (Berry, 2006; Berry and West, 2008; Brasington, 1999, 2003). These studies are typically interpreted as providing evidence on scale economies. Andrews et al. (2002) review the literature that is concerned with scale economies in education and conclude that "there is little convincing evidence in the United States on how consolidation actually affects school districts in the long-run." Kuziemko (2006) is interested in school size effects, and notes the lack of consensus in the literature concerning the relation between scale and achievement, and explains this by "the empirical weakness that the existing papers share", namely omitted variable bias. The next section explains how we aim to circumvent this trap. Moreover, the consolidation studies focus on scale effects and ignore the effects of changes in competitive pressure, whereas this study deals with effect of competition and scale in tandem.

3 Institutional context

3.1 The Dutch education system

Since the beginning of the 20th century the Dutch system of primary education has many similarities to the voucher-system later proposed by Friedman. A key principle is “freedom of education”. This has two components: Parents can freely choose the school for their child, and there is the freedom to start new schools and to organize the teaching in schools.

Parents’ freedom to choose a school is not restricted by where they live (there are no school catchment areas), or how much they earn. With the exception of a few cases of orthodox religious schools, primary schools do not select pupils. Parents can therefore enroll their children in the school of their choice. Currently there are about 7,000 primary schools in the Netherlands. For most pupils the nearest primary school is within walking distance. For about 59 percent of the pupils the nearest school is less than 500 meters from their home and 89 percent of the pupils live less than 1 kilometer away from the nearest primary school (Bunschoten, 2008).

About two thirds of pupils is enrolled in publicly-funded private schools. The main difference between public schools and publicly-funded private schools is that the latter are governed by a private school board and the first are governed by the municipality. Historically most publicly-funded private schools were founded on the basis of religious beliefs (mainly Protestantism and Catholicism), but such schools can also be based on pedagogical principles such as Montessori, Dalton, or Jenaplan.

Both public and publicly-funded private schools receive funding from the central government through a "money follows pupil"-mechanism. The funding of a school is thus based on the number of pupils enrolled. There is no additional funding from local government agencies, and schools that receive funding through this voucher system are not allowed to charge school fees.[2] Privately funded primary schools are virtually non-existent in the Netherlands.

To be eligible to operate under the voucher system, schools have to satisfy two requirements. The first comes in the form of quality standards. The government sets a number of broadly stated core objectives which state what skills and knowledge pupils should have at the end of (primary) school. Whether these core objectives are achieved is checked by the educational inspectorate, which monitors schools for compliance with laws and regulations.

The second requirement concerns the number of pupils at a school. To start a new school the number of pupils enrolled in the school should within a specified period after the start-up exceed a certain threshold. For existing schools a different set of minimum school size rules applies, which are in general lower than the rules for new schools. The minimum required school sizes are set by the central government but vary between municipalities. This is described in more detail in the next section.

The Dutch system is more regulated than the universal voucher system proposed by Friedman and others. In addition to the above, there are regulations in place concerning firing of teachers, and teacher wages are set by a collective wage agreement. Schools have, however, full discretion when it comes to their organization and the teaching methods they employ. Schools are also not for profit. This does however not imply that there are no incentives for schools to increase quality in order to attract more pupils. School funding depends on the number of pupils, in

[2]Schools are allowed to ask for a voluntary fee to finance extra-curricular activities such as, for example, a yearly school trip. Schools cannot exclude pupils whose parents do not pay this voluntary fee from the regular school program. They can, however, exclude these pupils from the extras.

addition the wages of school principals also depend on the number of pupils that is enrolled in the school.

Schools can grow or shrink from year to year, albeit within the limits of the speed with which they can adjust their capacity. If schools are oversubscribed, they typically follow a first-come, first-served rule. Applying this rule is facilitated by the system of rolling admissions. Children in the Netherlands are allowed to start school the day they turn 4 years old, and are required to start school the day they turn 5 years old. This system prevents that a large group of children applies to a school at the same time and is then informed that the school has no places available. Also, this system of rolling admissions provides more flexibility for schools to adjust their capacity.

3.2 Minimum school size rules

Primary schools in the Netherlands must comply with a minimum school size rule in order to be eligible for funding. Before 1994 funding ceased if a school had fewer pupils than the number required under the Primary Education Act for three school years in a row (Staatsblad 1986, 256, WBO).[3] The minimum required school size depended on the number of inhabitants in the municipality in which the school was located, according to the following step function:

$$\text{min\_size}(pop_{mt})_{t_{rule}} = \begin{cases} 50 & \text{if } pop_{mt} < 25,000 \\ 75 & \text{if } 25,000 \leq pop_{mt} < 50,000 \\ 100 & \text{if } 50,000 \leq pop_{mt} < 100,000 \\ 125 & \text{if } pop_{mt} \geq 100,000 \end{cases} \qquad t_{rule} < 1994 \tag{1}$$

where pop_{mt} is the population size of municipality m in year t. So for example, if a school was located in a municipality with $30,000$ inhabitants and had less than 75

[3] A school year starts on August 1 of a given year and ends on July 31 the following year.

pupils for three consecutive years, the funding was stopped at the beginning of the next school year in case of a privately-run school or was closed down in case of a publicly-run school.[4]

In the 1980's there were many small schools, and there were concerns about their ability to provide education of sufficient quality (Ministry of Education, 1990). Moreover, the funding system was such that, in addition to the vouchers, each school also received a lump-sum transfer. Many small schools were thus more expensive than a smaller number of large schools. For these reasons the minimum school size rules were revised in the beginning of the 1990's. On July 11, 1992 a new minimum school size rule was published in the weekly newsletter that is sent to all schools. Although the new rule was published in 1992, the old rule applied until January 1, 1994. The new minimum school size rule was no longer based on the number of inhabitants of the municipality, instead the new rule was based on the pupil density of the municipality as follows

$$\text{min\_size}(d_{mt})_{t_{rule}} = \frac{d_{mt}}{0.25 + 0.0045 d_{mt}} \qquad t_{rule} \geq 1994 \tag{2}$$

where d_{mt}, pupil density in municipality m in year t, is defined as the number of inhabitants between 4 and 11 years old divided by the size of the municipality in square kilometers.

Figure 2 shows scatter plots of the old and new minimum school size rules. The first panel shows a scatter plot of the old and new rules against the number of inhabitants. The dots connected by the line show the old minimum school size rule, and each dot represents a municipality. All municipalities with less than 25,000 inhabitants have a minimum school size of 50, at 25,000 there is a jump to 75, at 50,000 there is a jump to 100 and all municipalities with more than 100,000 inhabitants have a minimum school size of 125. The crosses show the new minimum

[4]If a privately-run school stops receiving funding from the government this means in practice that it has to close down. The only source of funding is the funding of the government since schools are not allowed to charge school fees.

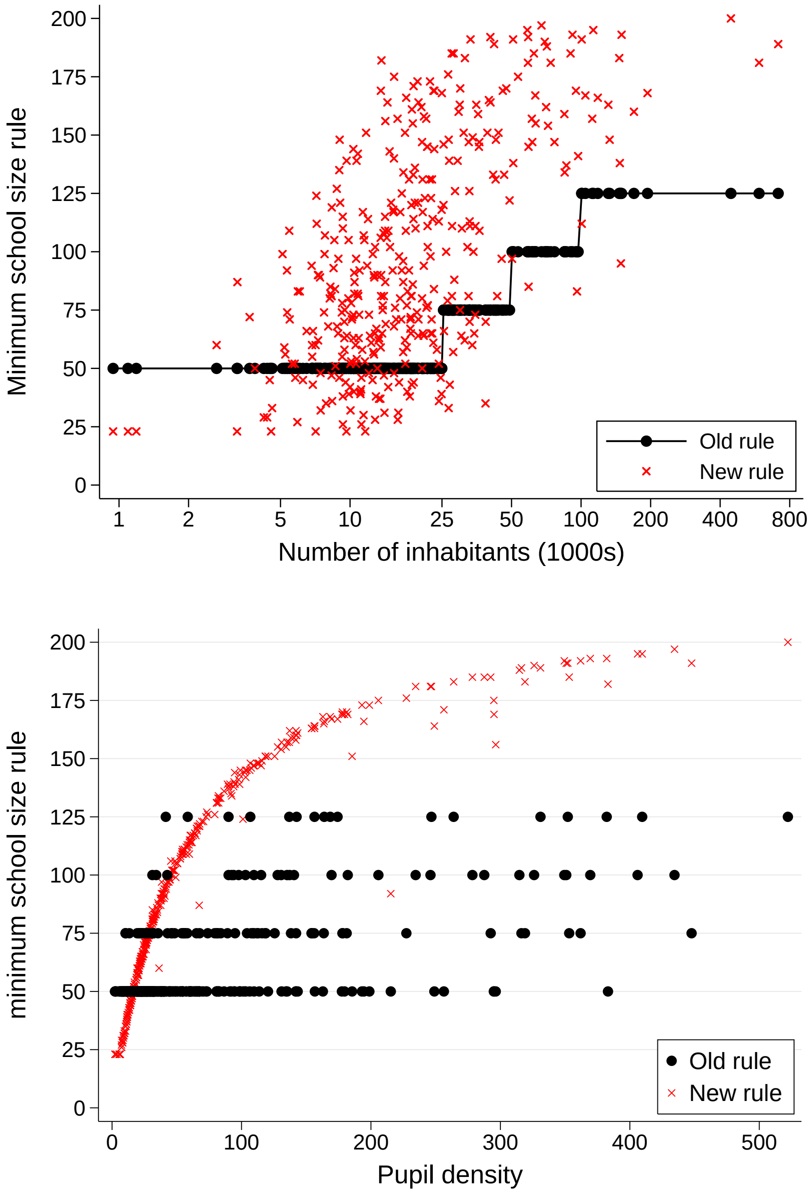

Figure 2. Old and new rules by number of inhabitants and pupil density in 1992

school size rule, the new minimum school size ranges from 23 pupils to 200 pupils. As can be seen in the first panel there is substantial variation in the new minimum school size between municipalities with the same number of inhabitants, and thus the same minimum school size before the reform.

The second panel in Figure 2 shows the old and new rules by pupil density. The new minimum school size rule, indicated by the crosses, shows the relation with pupil density.[5] Municipalities with the same pupil density have the same minimum school size after the reform but, as the dots show, the old minimum school size was often very different for municipalities with the same pupil density.

Although the new rule was introduced in 1994, there was a grace period of two years. Consequently, no schools were forced to close down or stopped receiving funding in the school years 1994/1995 and 1995/1996. From 1996 onward all schools with a number of pupils below the minimum school size for two school years (either consecutive or with one year in between) were either closed down (in case of public schools), or stopped to receive funding (in case of private schools) from the beginning of the following school year.

On average the minimum required school size increased due to the reform. Figure 3 shows the average minimum school size by year as well as the average number of schools in a municipality by year. The vertical axis on the left shows the average number of schools and the vertical axis on the right shows the average minimum school size. Until 1993 the average minimum school size was just above 60 pupils. In 1994, after the implementation of the law, the average minimum school size jumped to about 100. At the same time the average number of schools declined. In 1991 municipalities had on average 16.5 schools, but after 1992 the number of schools started declining until 1997 when it stabilized around an average of 13.5 schools per municipality. In total the number of schools went down from 8,362 schools in 1992

[5] There are some "outliers" which are due to the fact that if the pupil density was more that 500 it was set at 500 and when the size of the municipality was smaller than 10 km^2 it was set at 10.

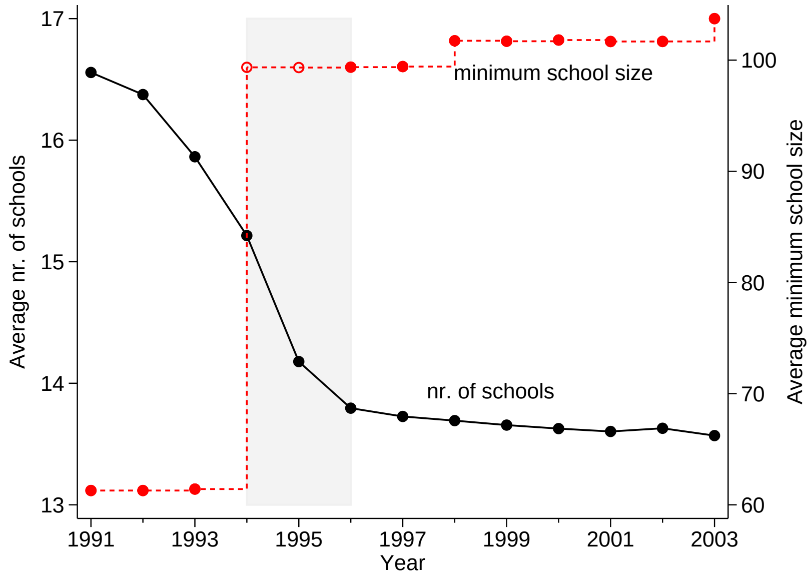

Figure 3. Average number of schools and minimum school size rules by year

to 7,100 schools in 1997, a decline of 15% within a period of five years.[6]

Most schools that were below the new rule in 1994, merged with another school instead of being closed down on August 1, 1996. Of the 8,362 primary schools in 1992, 2,293 schools were part of a merger in the five years between 1992 and 1997. Most of these mergers were real mergers and not administrative mergers as is reflected by the fact that the number of school locations declined to 7,163 in 2003.[7]

4 Data

We use data from various sources. As outcome variable we use standardized test scores. At the end of primary school pupils take a nationwide test developed by the national institute for educational testing and measurement. This test determines for

[6]The reform affected private and public schools similarly. We do not have access to schools' denomination in our micro data, but from aggregate statistics we know that the share of public schools remained approximately constant between 1992 and 1997, 35% vs. 33.5%.

[7]There were not only changes in the minimum school size rules for existing schools, but the minimum school size rules for new schools also changed due to the reform. Before the reform the minimum school size for new schools was $\frac{8}{5}$ times the minimum school size for existing schools. After the reform the minimum school size for new schools is $\frac{10}{6}$ times the minimum school size for existing schools with a minimum of 200. In principle this gives us a second instrumental variable. In practice, however, the two instruments are too highly correlated for the second instrument to give any leverage.

a large part the type of secondary school a pupil will attend after primary school. Although the test is not compulsory, most pupils take the test. The test consists of multiple choice questions that deal with language, arithmetic/mathematics, information processing and (optional) world orientation. The test is administered on three days in February and at the end of the last day the answer sheets are sent to the testing institute where they are marked. The results for each pupil are sent back to the school. The score is based on the number of correct answers for language, arithmetic/mathematics and information processing. We standardized the scores by year, so that results can be interpreted in terms of standard deviation units of the annual test score distribution.

School level data, such as information about school size and the share of minority pupils, are obtained from the Dutch Ministry of Education. Data at the municipality level are obtained from Statistics Netherlands. The minimum school size rules are collected from Het Staatsblad (1986, 1993) that publishes (changes in) laws and from Gele Katern, a newsletter for schools.

In the analysis we compare two cohorts of pupils. The first cohort is the one that finished primary school in 1992, just before the change in the number of schools. The second cohort is the one that enrolled in primary school just after the large reduction in the number of schools and who were therefore not directly affected by the school mergers. This is the cohort of pupils who finished primary school in 2003.

Some municipalities merged during our observation period. Because a municipality merger can lead to changes in pupil density it will trigger changes in the minimum required school size. A merger between municipalities however also leads to other changes related to local governance. This means that even if mergers between municipalities might give another source of variation in the supply of schools, it is unclear whether this variation is exogenous. We therefore only consider the municipalities that were not part of a merger between 1992 and 2004.[8] About 20%

[8]We take 2004 as end date because the school year 2003 starts in August 2003 but ends in June 2004

Table 1. Summary statistics

	1992		2003	
	mean	SD	mean	SD
Test scores				
Standardized score	-0.02	1.01	-0.02	1.01
N	71,283		111,226	
Municipality Characteristics				
Number of schools	17.3	21.1	14.4	18.2
Average school size	162.5	46.5	216.5	74.9
Minimum required school size	62.2	21.1	101.1	47.6
Number of pupils (×1000)	3.0	4.7	3.2	5.2
Number of inhabitants (×1000)	31.9	59.9	34.2	62.5
Share minority pupils (%)	5.2	6.3	6.3	6.6
Number of jobs (×1000)	8.4	20.6	9.9	24.0
Number of disability benefits (×1000)	1.7	3.4	2.0	3.8
N	345		345	

of the municipalities in 2004 are a result of a merger, the analysis will thus be based on the remaining 80% of the municipalities.

Table 1 reports summary statistics separately for the years 1992 and 2003. The bottom panel of the table shows the substantial changes that took place in the average number of schools and the average minimum school sizes. The numbers of inhabitants and pupils increased increased by 7 and 10 percent, respectively. The top part of the table shows that the number of pupils that took the test increased much more than the number of pupils. This is not problematic for the analysis as long as the change in test-taking pupils is unrelated to the changes in the minimum school sizes rules. The results in Table A in the Appendix show that the change in the share of test-takers (ratio of test-takers to number of 11-year-olds in municipality) is not significantly related to the change in the minimum required school size.

5 Empirical approach

Before we present the empirical results, we first spell out which empirical specifications we estimate and on which identifying assumptions these are based.

We are interested in the effect (δ) of the number of schools in municipality m in year t (s_{mt}) on the test scores of pupil i in that municipality in that year (y_{imt}). We postulate the following relationship:

$$y_{imt} = \delta \cdot \ln(s_{mt}) + x'_{mt}\beta + \lambda_m + \mu_t + \varepsilon_{imt} \quad (3)$$

where we use the logarithm of the number of schools, $\ln(s_{mt})$, because the effect of a given change in the number of schools is likely to be very different in a municipality with 4 schools than in a municipality with 40 schools. δ is therefore the effect of a 100% change in the number of schools on pupil test scores. We include municipality fixed effects λ_m, and time-varying controls x_{mt}. In our reference specification x_{mt} will consist of the number of inhabitants, the number of pupils, the share of minority pupils, the number of disability benefits and the number of jobs to reduce the residual variance and proxy for changes in the socioeconomic conditions in a municipality over time.[9] The idiosyncratic error term ε_{imt} is allowed to be clustered at the municipality level. The year fixed effects μ_t control for changes over time which are common across municipalities, such as education policies which are implemented nationwide.

Since equation (3) includes municipality fixed effects λ_m, it is already an improvement over a cross-sectional regression of test scores on the number of schools in a municipality. This latter approach will produce biased estimates if municipalities with more or fewer schools are systematically different. The fixed effects specification removes such systematic differences and only exploits within municipality

[9] Data on the number of jobs and the number of disability benefits are only available at the municipality level from 1995 onwards. We therefore use the values observed 1995 as a proxy for the number of jobs and the number of disability benefits in 1992.

changes in the number of schools.

Changes in the number of schools within a municipality may however be due to changes in unobserved municipality characteristics. For example, a change in the composition of the population of the municipality might change the demand for schools, and in addition have a direct impact on pupil test scores, leading to omitted variable bias. We will therefore use an instrumental variable approach with the following first stage

$$\ln(s_{mt}) = \gamma \cdot \ln(z_{mt}) + x'_{mt}\alpha + \eta_m + \tau_t + \nu_{imt} \tag{4}$$

where the instrument is the (log) minimum required school size based on the number of inhabitants and pupil density in 1992:

$$z_{mt} = \begin{cases} \text{min\_size}(pop_{m,92}), & t = 1992 \\ \text{min\_size}(d_{m,92}), & t = 2003 \end{cases}$$

Note that z_{m03} is the *predicted* minimum required school size based on baseline characteristics. Our instrument will therefore not pick up changes in the number of inhabitants or pupil density over time. Because our specification includes municipality fixed effects, we essentially instrument the change in the number of schools in a municipality between 1992 and 2003 with the predicted change in the minimum required school size due to the change in rules.

The variation in our instrument comes from differences between municipalities in the ratio of pupil density and the number of inhabitants in 1992. If these municipalities also differ in other (unobserved) characteristics this will be captured by the municipality fixed effects. Our identifying assumption is thus that the change in a municipality's minimum required school size and the change in the average residual achievement of pupils in that municipality are mean independent.[10]

[10]Whereby the change in minimum school size and the change in average residual pupil achieve-

Most previous studies that estimate general equilibrium effects of school choice and competition rely on conditional independence assumptions for identification. Hsieh and Urquiola (2006) and Böhlmark and Lindahl (2008) for example, assume that the variation in the entry of private schools across areas is supply driven conditional on time trends and covariates. Card et al. (2010) assume that, conditional on the joint share of Protestants and Catholics, the share of Catholics has no direct impact on outcomes. In contrast, Hoxby (2000) exploits supply side variation generated by natural boundaries in an instrumental variable setup. In all these cases the supply side variation that is exploited in the estimation needs to be orthogonal to demand side factors that are correlated with outcomes. One advantage of our setup is that since the rules are set at the national level, changes in minimum school size rules are by definition not directly generated by (changes in) the demand side. Something which is more difficult to rule out when exploiting school entry and exit, or cross-sectional variation in supply.

If changes in the (pupil) population of a municipality are systematically related to the change in rules this will invalidate our instrument. We test whether this assumption is valid by estimating equation (4), but where we replace the dependent variable by i) the number of pupils, ii) the total number of inhabitants, and iii) the share of ethnic minority pupils. If the coefficient on the instrument is statistically significant, then changes in the minimum required school size are confounded by changes in the underlying population. Table 2 shows however that this is not the case; changes in the number of pupils, total number of inhabitants and share of ethnic minority pupils between 1992 and 2003 are not systematically related to changes in minimum required school size.[11]

ment are measured as deviations from a nation wide trend, since we include year fixed effects in our specification.

[11] Another scenario under which the exclusion restriction might fail is if closeness to the minimum required school size gives an incentive to schools to perform and if, in addition, the share of schools in a municipality that are close to the norm is correlated with the instrument. As a robustness check we include the share of schools in a municipality that are at most 25 pupils (about 10 percent of average school size) away from the norm in equation 3. The coefficient for this variable is not significantly different from zero. Moreover, the estimated coefficient of ln(number of schools)

Table 2. Mobility and the change in rules

Dependent variable	ln(number of inhabitants)	ln(number of pupils)	share ethnic minority pupils
ln(minimum required school size)	-0.0004	-0.0089	-0.0007
	(0.0133)	(0.0181)	(0.0024)
Year fixed effects	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes

Note: Sample: 182,509 observations over 345 municipalities. Standard errors in parentheses are clustered at the municipality level. Control variables: the number of people on disability benefits, and the number of jobs in the municipality. Trend in test scores between 1992 and 2003 are allowed to differ between municipalities with number of inhabitants of respectively (0-25000), (25000-50000), (50000-100000) and (100000 or more).

Although changes in (pupil) population are unrelated to the changes in minimum school size rules, our identifying assumption could be further weakened if we could condition on municipality-specific time trends. This is however not feasible because we only have observations from two years. To relax the assumption of a common trend for all municipalities in the form of μ_t, we allow the year fixed effect to vary across four groups of municipalities of different size.[12]

In the next section we will start with presenting the difference-in-difference results without using the change in minimum required school size as instrument for the change in the number of schools. We will show results with and without control variables. Subsequently we will present our instrumental variables results. Since one may wonder whether pupils and schools in the southern part of a large city are affected by the number of schools in the northern part of the city, we will also present estimates whereby we exclude the 20 biggest municipalities, those with more than 100,000 inhabitants (in 2003).

remains the same.

[12]Municipalities are divided into 4 categories based on the number of inhabitants: (0-25000), (25000- 50000), (50000- 100000) and (100000 or more). Trends in test scores are allowed to differ between these four categories.

Table 3. Difference-in-differences results

	(1)	(2)	(3)
ln(number of schools)	-0.05	-0.15**	-0.08
	(0.08)	(0.06)	(0.06)
Year fixed effects	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes
Control variables	-	Yes	Yes
Allowing for different trends large and small municipalities	-	-	Yes

Note: Dependent variable is standardized test score. Sample: 182,509 observations over 345 municipalities. Standard errors in parentheses are clustered at the municipality level. Control variables: ln(nr. pupils), ln(nr. inhabitants), municipality share of ethnic minority pupils, the number of people on disability benefits, and the number of jobs in the municipality. Trend in test scores between 1992 and 2003 are allowed to differ between municipalities with number of inhabitants of respectively (0-25000), (25000-50000), (50000-100000) and (100000 or more).

6 Results

6.1 Difference-in-differences estimates

We start with presenting fixed-effect results which can be interpreted as naive difference-in-differences estimates which ignore the possible endogeneity of the change in the number of schools in a municipality. Column 1 of Table 3 shows a regression of pupil test scores on the number of schools in the municipality including municipality and year fixed effects, thereby controlling for (unobserved) differences between municipalities that are constant over time and for changes over time that are constant across municipalities.[13] The result in column 1 indicates that a 10% reduction in the number of schools is associated with an increase in test scores of about 0.5% of a standard deviation, which is very small and not significantly different from zero.

Changes in the number of schools that are correlated to changes in other municipality characteristics affecting pupil test scores will lead to omitted variables bias. Column 2 therefore shows the results when controlling for time-varying municipal-

[13]See Appendix Table B1 for results using the Herfindahl index as measure of school choice and competition.

ity characteristics. The coefficient in column 2 is negative and larger in absolute value than the coefficient in column 1 and is significantly different from zero at a five percent significance level. Column 3 shows results where trends in test scores between 1992 and 2003 are allowed to differ between municipalities with a different number of inhabitants. The coefficient in column 3 is still negative but smaller in absolute value compared to the estimate in column 2 and no longer significantly different from zero.

Table 3 shows that the results are sensitive to the inclusion of control variables, and indicates that these results might suffer from endogeneity problems. It is therefore important to rely on exogenous variation in the number of schools in order to obtain a consistent estimate of the effect of the supply of schools on pupil test scores.

6.2 Instrumental variable estimates

To address endogeneity problems we isolate the change in the number of schools which is due to the reform, by using the change in minimum school size rules as instrument as outlined above.

Figure 4 shows a scatter plot of the percentage change in the number of schools against the percentage change in the minimum school size rule. On average the reduction in the number of schools was 15% but, as can be seen in the figure, there was substantial variation across municipalities. Some municipalities had no change in number of schools while other municipalities faced a reduction in the number of schools of 50%. The linear fit of the change in the number of schools on the change in rules in Figure 4 illustrates the strong negative relation of the first-stage reported in Table 4.

The first column of the top panel of Table 4 shows the result of the first-stage which regresses the logarithm of the number of schools on the logarithm of the (predicted) minimum school size rule including municipality and year fixed effects and controlling for changes in a number of municipality characteristics. We see that

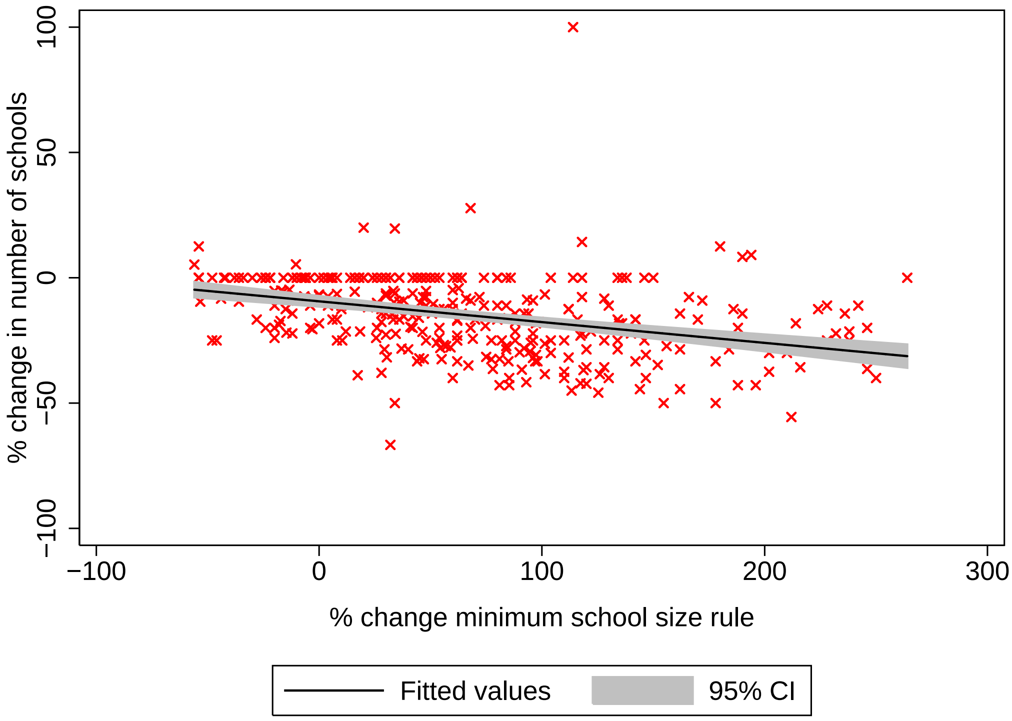

Figure 4. Change in rules and schools

a 100% increase in the minimum school size rule leads on average to a reduction in the number of schools of 20%, which is significant at the 1 percent level and has a partial F-statistic of 91.4. The effect of the change in rules on the change in schools is therefore sufficiently strong to avoid weak instrument problems.

The second-stage estimate in column 1 of Table 4 shows that, once we instrument for the (log) number of schools in a municipality, a 10% reduction in the number of schools increases test scores on average by 2.6% of a standard deviation.[14] This effect is larger in absolute value than the estimates in Table 3, and significantly different from zero at a 5 percent significance level. The second column in Table 4 shows that excluding the 20 biggest municipalities (those with more than 100,000 inhabitants) from the analysis does not affect our findings.

We find that a reduction in the supply of schools has a small positive impact on pupil performance. The reform led to a 15% decrease in the number of schools.

[14]Equivalent first stage and 2SLS results based on the Herfindahl index instead of the number of schools are presented in Appendix Table B2.

Table 4. Main results: first and second stage

	(1)	(2)
First-stage:		
ln(minimum school size)	-0.20^{***}	-0.18^{***}
	(0.02)	(0.02)
Partial F-statistic	91.4	94.7
Second-stage:		
ln(number of schools)	-0.26^{**}	-0.26^{*}
	(0.11)	(0.14)
Excluding biggest 20 municipalities	-	Yes
Nr municipalities	345	325
Nr observations	182,509	130,097

Note: Dependent variable in second stage is standardized test scores. Standard errors are clustered at the municipality level. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level. All regression include municipality fixed effects, year fixed effect, control variables: ln(nr. pupils), ln(nr. inhabitants), municipality share of ethnic minority pupils, the number of people on disability benefits,and the number of jobs in the municipality. Trends are allowed to differ between municipalities with number of inhabitants of respectively (0-25000), (25000- 50000), (50000- 100000) and (100000 or more).

The estimates therefore imply that the school consolidation reform increased pupil performance by 4% of a standard deviation.

At face value our result seems to be at odds with the theoretical arguments for school choice and competition: With more school choice it should be easier for parents to find the school that matches their preferences and the needs of their child. In addition more schools should lead to more competition and a resulting increase in school quality. On the basis of these two mechanisms one would expect that the decrease in the supply of schools would have had a negative impact on pupil performance. In the next two subsections, we examine to what extent this finding can be attributed to changes in segregation and school size.

6.3 Segregation

We first investigate whether the decline in the supply of schools, due to the reform, affected sorting of pupils among schools. For each primary school we do not only know the number of pupils attending the school but also the number of pupils in each of the following three categories; 1) pupils with low educated migrant parents, 2) pupils with low educated Dutch parents and 3) all pupils that do not fall in the first two categories. Given this division of pupils by socioeconomic status we can calculate a relative heterogeneity index as in Urquiola (2005). Urquiola investigates the effect of school choice on sorting by investigating the impact of the number of school districts on the (racial/educational) heterogeneity of a school district relative to the heterogeneity of the metropolitan area in which the district is located. This measure of heterogeneity is defined as $H = 1 - \sum_{r=1}^{R} S_r^2$ where R is the number of groups and S_r is the share of group r in the population. On the basis of the division into the groups defined above we can calculate the heterogeneity index for each school and for the municipality in which the school is located. By taking the ratio of the two we obtain a measure of relative heterogeneity.

There is one practical issue that we need to address, which is that the definition

Table 5. Effect of the number of schools on sorting

	# Groups in Heterogeneity Index			
	3		2	
Summary statistics	*Mean*	*S.D.*	*Mean*	*S.D.*
Heterogeneity index school	0.36	0.20	0.12	0.15
Heterogeneity index municipality	0.44	0.14	0.17	0.14
Relative heterogeneity index	0.84	0.57	0.86	1.23
Results				
ln(number of schools)	-0.09		0.15	
	(0.08)		(0.14)	
Partial F-statistic first stage	117.3		116.9	
Nr. observations (schools)	11,403		11,391	

Note: Dependent variable is school heterogeneity (relative to municipality heterogeneity). Estimates come from 2SLS regressions. Standard errors in parentheses are clustered at the municipality level. All regression include municipality fixed effects, year fixed effect, control variables: ln(nr. pupils), ln(nr. inhabitants),the number of people on disability benefits, and the number of jobs in the municipality. Trends are allowed to differ between municipalities with number of inhabitants of respectively (0-25000), (25000- 50000), (50000- 100000) and (100000 or more).

of the second category (pupils with low educated Dutch parents) changed between 1992 and 2003. In 1992 all children with at least one parent that had at most the lowest level of secondary education were included in this category. In 2003 pupils were only included in the second category when both parents had at most the lowest level of secondary education. Since this change in the definition of the second category applied to all schools in all municipalities in the Netherlands this should be captured by the year fixed effect and therefore not affect the results. As an additional robustness check we calculate the (relative) heterogeneity index on the basis of two groups; 1) pupils with low educated migrant parents and 2) all other pupils. The index based on this division is not affected by the change in the definition of the second category.

Table 5 shows 2SLS results of the effect of the number of schools on the two measures of relative heterogeneity, using the minimum school size rule as instrument. The result shows that there is no significant impact of the change in the supply of

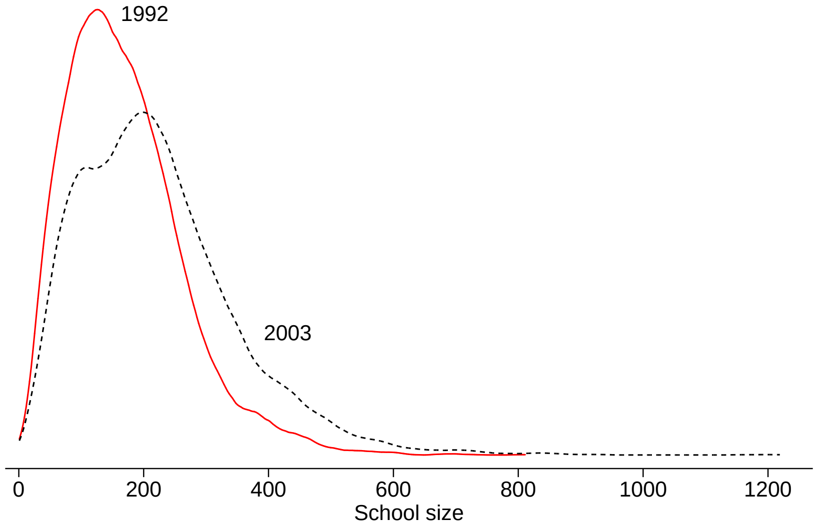

Figure 5. Kernel density of school size in 1992 and 2003

schools on sorting of pupils in terms of socioeconomic status. The estimates are small and not significantly different from zero. This indicates that sorting cannot explain our findings.

6.4 Economies of scale

The change in the supply of schools was accompanied by changes in school size. This can be seen from the kernel densities of school size for the years 1992 and 2003 in Figure 5. Average school size increased from 162 pupils in 1992 to about 216 pupils per school in 2003. The increase in average school size can explain our findings if there are economies of scale. In this subsection we provide two pieces of evidence supporting this. First, we present results from a survey among school principals suggesting that increases in school size could be beneficial for some processes relevant for pupils' performance. Second, we extend our previous 2SLS regressions by including school size, and find that the effect of the number of schools in a municipality is no longer significantly different from zero.

In the year of the announcement of the change in the minimum school size rule (1992), a survey was conducted which asked principals of 177 primary schools,

Table 6. Associations between school size and school characteristics

Dependent variable:	ln(school size)	
Share of time the principal spends on teaching	-0.139***	(0.023)
School has at least one full time director	0.233***	(0.044)
Share of classes that contain pupils from multiple grades	-0.575***	(0.045)
School has a remedial teacher	0.181**	(0.052)
School is involved in extracurricular parent-pupil activities	-0.020	(0.053)
School has agreement with parents about:		
- parents attending parent-teacher meetings	-0.002	(0.062)
- discussing the school report of the pupils	0.002	(0.023)
- time spend on the different subjects	0.087	(0.061)
- minimum goals that pupils should achieve	-0.035	(0.066)

Note: Robust standard errors in parentheses. ** significant at the 5% level, *** significant at the 1% level. Results are based a survey among principals of 177 schools in 1992.

among other things, about the organization of teaching in their schools and about the schools' contacts with parents.[15] In Table 6, we report results from regressions of these organizational features and school-parent contacts on the size of the school. Each row comes from a separate regression. The results show that larger school size is associated with (i) less teaching by the principal, (ii) a higher probability of having at least one full time director, (iii) fewer classes with pupils from multiple grades, and (iv) a higher probability of having a remedial teacher. At the same time a larger school is not associated with less involvement of the parents with the school (as indicated in the second half of the table). These findings are consistent with the view that just before the reform was implemented, increases in school size could increase the efficiency of the teaching process without harming parental involvement with the school.

To disentangle the effect of the change in school choice and competition and the effect of the change in school size, we augment equation (3) with school size:

$$y_{imt} = \delta \cdot \ln(s_{mt}) + \psi \cdot \ln(\text{school size}_{imt}) + x'_{mt}\beta + \lambda_m + \mu_t + \varepsilon_{imt} \tag{5}$$

[15] This survey is part of a larger project that collected data from primary school pupils, their parents and teachers; the Landelijke Evaluatie Onderwijsvoorrangsbeleid. We only use information from the representative sample of schools.

where ψ is the scale effect.

We start out by presenting estimation results that treat school size as exogenous. Endogeneity of school size is however potentially a concern. If good schools attract more students then this could create a spurious relationship between size and performance. In other words: Are large schools large because they are good? Or are they good because they are large? To address this concern we therefore also present results where we use an instrument for school size.

We expect the major source of endogeneity of school size to come from sorting across schools within a municipality. We therefore use variation in the number of pupils at the municipality level as instrument for school size, while we continue to control for (the log of) the number of inhabitants. The advantage of this instrument is that it varies at the municipality level over time. It should therefore address concerns about within municipality sorting at a given point in time. For our instrument to be valid we require shocks to the population share of pupils over time to have no independent effect on achievement.[16] In addition variation in the number of pupils in a municipality should not be related to the instrument that we use for changes in the supply of schools. We do not need to worry about this because, as was shown in Table 2, neither changes in the number of pupils nor changes in the share of minority pupils are significantly related to changes in minimum required school size.

The first column in Table 7 reports the estimates of the baseline specification but excludes the number of pupils from the list of covariates which was included to gain precision in the specifications in Table 4. It is reassuring to see that this does not affect the results. It also confirms the exogeneity of the number of pupils, our main instrument for the number of schools. Column 2 then shows the results when the logarithm of school size is included in the specification. The coefficient on the logarithm of the number of schools is reduced by half of its original value and is no longer significantly different from zero. The coefficient on the school size variable

[16]We control for changes in the composition of the pupil population by including changes in the share of minority pupils in a municipality over time as control variable.

on the other hand indicates that test scores increase with school size.

The final column reports the estimates where (log) school size is instrumented with the (log) number of pupils in the municipality. Since the specification includes municipality fixed effects, changes in school size are essentially instrumented with changes in the population share of pupils. As can be seen in the table, we have good first-stages for both endogenous variables. The estimated size effect is slightly larger than when we assume exogeneity of school size in column 2, and is significant at the 5 percent significance level. In the final column, the coefficient on the logarithm of the number of schools is further reduced (in absolute size) to only one third of the value of the coefficient in column 1 and stays insignificant. These results are consistent with the reasoning that the negative effect of the reduction in the supply of schools is entirely explained by positive effects of an increase in school size, and that choice and competition effects are small and negligible.[17]

The result that choice and competition effects are small and negligible may be due to the specific setting of primary schools in the Netherlands in which there is already a relatively high degree of choice and competition. What is probably not attributable to our specific setting is the finding that ignoring scale effects leads to an underestimation of the effects of choice and competition. This could for example explain why Hsieh and Urquiola (2006) find no effect on average educational outcomes of the voucher program in Chile where more than 1,000 private schools entered the market while the number of public schools remained constant. It is an open question whether correcting for the reduced average school size would have led to a positive effect of the increase in choice and competition.

The potential importance of taking scale economies into account when studying the effects of school choice and competition does not only hold for the study of Hsieh and Urquiola but for all studies where the increase in school choice and competition

[17]An alternative explanation for our findings is that due to the consolidation weak schools disappeared and that many of these disappearing schools were small because they were weak. This does not drive our results. We regressed achievement in 1992 on a dummy for closure after 1992 (controlling for fractions of disadvantaged students and school size) and find no “effect”.

Table 7. Scale versus competition effects

	(1)	(2)	(3)
ln(number of schools)	-0.25**	-0.11	-0.08
	(0.11)	(0.12)	(0.13)
ln(school size)		0.15***	0.18**
		(0.02)	(0.09)
Instrument ln(number of schools)	Yes	Yes	Yes
Instrument ln(school size)	n/a	No	Yes
F statistic 1st stages:			
ln(number of schools)	92.6	92.7	54.3
ln(school size)			53.6

Note: Dependent variable is standardized test scores. Sample: 182,509 observations over 345 municipalities. Standard errors in parentheses are clustered at the municipality level. ** significant at the 5% level, *** significant at the 1% level. Control variables: ln(nr. inhabitants),the number of people on disability benefits, the number of jobs in the municipality, and municipality share of ethnic minority pupils. Trend in test scores between 1992 and 2003 are allowed to differ between municipalities with number of inhabitants of respectively (0-25000), (25000- 50000), (50000-100000) and (100000 or more).

is induced by (or results in) an increase in the supply of school (districts). Our results illustrate that ignoring scale effects can lead to wrong conclusions regarding the effects of choice and competition. Finally, a symmetric argument applies to studies of scale economies that ignore potential effects of choice and competition.

7 Conclusion

In this paper we have analyzed the impact of variation in the number of schools in a municipality on pupils' achievement. Variation in the number of schools in a municipality causes variation in school choice and competition. The setting of our analysis is primary education in the Netherlands. This setting is very different from the settings of previous papers that looked at the impact of school choice on achievement. While in most countries school choice is limited, primary education in the Netherlands is characterized by a large amount of choice. Parents can freely choose the school of their children and all primary schools are publicly funded through a system in which money follows pupils.

We exploit variation in the number of schools at the level of municipalities induced by a change in the minimum school size rule. Before the change the minimum school size in a municipality was determined by the population size, after the change it was determined by pupil density. Some municipalities were more affected by this change than others. We find a strong effect of the change in the minimum school size on the number of schools in a municipality.

We find a significantly *negative* effect of the number of schools in a municipality on pupils' achievement. A reduction in the number of schools of 10 percent increases test scores on average by 3 percent of a standard deviation. Hence, more school choice (and competition) is – in the setting of primary education in the Netherlands – detrimental for achievement. Our preferred explanation for this counter-intuitive result comes from the fact that a reduction of the number of schools in a municipality mechanically implies an increase in average school size. The reform reduced the number of small primary schools in the Netherlands. If we include school size in the achievement equation, the negative effect on the number of schools is small and not statistically significant, and we find nearly identical estimates in an instrumental variable estimation. Information from a survey among principals conducted just prior to the reform is consistent with this: larger schools are associated with more efficient teaching practices while parental involvement does not vary significantly with school size.

Our results call attention to a trade-off that is usually ignored in the school choice and competition literature. If more choice and competition is induced by an increase in the number of suppliers, and if the size of the market is fixed, each supplier will on average serve fewer pupils. Our results show that scale effects can offset the benefits of school competition. The case that we examined in this paper bears some resemblance with the discussion in introductory economics textbooks about the distinction between perfect competition and monopolistic competition. Under monopolistic competition firms operate at a point of their average cost curve tangent to their demand curve. At this point the firm's supply is lower than the amount at which average costs are minimized. The below minimum average costs are usually interpreted as the price customers have to pay for increased product variety. In our setting, pupils paid in the form of lower achievement to attend a smaller school, on average located closer to where they live.

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Appendix A: The effect of minimum school size on the share test-takers in a municipality

Table A: Change in minimum school size rule and the change in share test-takers

	(1)	(2)	(3)
ln(minimum school size rule)	0.006	0.006	0.004
	(0.029)	(0.029)	(0.029)
Year fixed effects	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes
Control variables	-	Yes	Yes
Allowing for different trends large and small municipalities	-	-	Yes
Nr municipalities	345	345	345
Nr observations	690	690	690

Note: Dependent variable: ratio of test participants and the number of 11 year-olds in municipality. Standard errors in parentheses are clustered at the municipality level. Control variables: ln(nr. pupils), ln(nr. inhabitants), number of disability benefits, number of jobs and municipality share of ethnic minority pupils.Trend in test scores between 1992 and 2003 are allowed to differ between municipalities with number of inhabitants of respectively (0-25000), (25000- 50000), (50000-100000) and (100000 or more).

Appendix B: Results based on the Herfindahl index

Table B1: Difference in differences results

	(1)	(2)	(3)
Herfindahl index	0.485	0.211	0.123
	(0.341)	(0.254)	(0.268)
Year fixed effects	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes
Control variables	-	Yes	Yes
Allowing for different trends large and small municipalities	-	-	Yes
Nr municipalities	345	345	345
Nr observations	182509	182509	182509

Note: Dependent variable is standardized test scores. Standard errors in parentheses are clustered at the municipality level. * significant at the 10% level. Control variables: ln(nr. pupils), ln(nr. inhabitants) and municipality share of ethnic minority pupils. Trend in test scores between 1992 and 2003 are allowed to differ between municipalities with number of inhabitants of respectively (0-25000), (25000- 50000), (50000- 100000) and (100000 or more).

Table B2: First stage and 2SLS results

	(1)	(2)
First stage		
ln(minimum school size)	-0.018***	-0.019***
	(0.003)	(0.003)
Partial F-statistic	40.8	34.7
Second stage		
Herfindahl index	-2.973**	-2.496*
	(1.377)	(1.368)
Excluding biggest 20 municipalities	-	Yes
Nr municipalities	345	325
Nr observations	182509	130097

Note: Standard errors are clustered at the municipality level. ** significant at the 5% level, *** significant at the 1% level. All regression include municipality fixed effects, year fixed effect, control variables: ln(nr. pupils), ln(nr. inhabitants), number of disability benefits, number of jobs, municipality share of ethnic minority pupils and trends are allowed to differ between municipalities with number of inhabitants of respectively (0-25000), (25000- 50000), (50000- 100000) and (100000 or more).

Table B3: Scale versus competition effects

	(1)	(2)	(3)
Herfindahl index	-2.898**	-1.319	-1.076
	(1.373)	(1.377)	(1.748)
ln(schoolsize)		0.148***	0.170
		(0.016)	(0.108)
Instrument ln(# schools)	yes	yes	yes
Instrument ln(schoolsize)	n/a	no	yes
F statistic 1st stages:			
Herfindahl index	40.3	38.0	20.4
ln(schoolsize)			53.6
Nr municipalities	345	345	345
Nr observations	182509	182509	182509

Note: Standard errors in parentheses are clustered at the municipality level. ** significant at the 5% level, *** significant at the 1% level. Control variables: ln(nr. inhabitants),number of disability benefits, number of jobs and municipality share of ethnic minority pupils. Trend in test scores between 1992 and 2003 are allowed to differ between municipalities with number of inhabitants of respectively (0-25000), (25000- 50000), (50000- 100000) and (100000 or more).