Municipality mergers*

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Abstract

To merge municipalities is an important policy issue in many countries, yet empirical evidence on the effect of municipality size on the production and quality of local public services is scarce. We use the spatial and temporal variation in forced municipality merges in a difference-in-differences approach to provide quasi-experimental evidence of the effect of municipality size on school output, measured by student educational attainment and income in adulthood. We find that municipality mergers increase student income by 2-3%, while the effect on educational attainment is less clear.

Keywords: school district, quasi-experiment, educational attainment, income
JEL codes: I2, H7

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1 Introduction

The size and number of local governments is an important policy question. Municipal amalgamation reforms and consolidation of school districts are hot issues in many countries and such reforms are currently on the political agenda in countries like Norway and Finland.[1] While fiscal decentralization is generally believed to be beneficial for society as suggested by the decentralization theorem formulated by Oates (1972), common arguments for amalgamation reforms are based on economics of scale, that increased school district size implies reduced expenditure per pupil. However, the size effect on output quality is not obvious. Expenditure reduction may come at the cost of reduced quality of services provided by the local units. On the one hand, larger local units may decrease local autonomy at the provider level (school, day care institution or homes for elderly). If the population becomes more heterogeneous as a result, the larger local governments might be less able to meet the needs of the heterogeneous users of public services. On the other hand, it is possible that larger local governments will have more professional administration and management of resources and so increase output quality for a given amount of resources available. For example, the probability of hiring professional and able school administrators may be higher in large than in small school districts. Ultimately, the relationship between local government size and output quality can only be resolved by empirical studies.

Below we investigate the effect of municipal size on educational output in terms of student educational attainment and earnings in adulthood using rich data from administrative registers in Norway. To provide credible evidence, we explore the spatial and temporal variation in municipal size from enforced municipality mergers taking place in Norway in the 1980's and 1990's in a difference-in-differences approach. Using outcomes in terms of educational attainment and earnings has several advantages when studying the relationship between municipality size and output quality. First, educational services in terms of compulsory schooling is provided by all municipalities, small and large. The users are well defined (children age 7-16) and to the extent that private schooling is not an option, services are solely provided by the local public sector. Second, educational attainment and earnings in adulthood may be more relevant measures of education output than test scores often used in estimates of education production functions as these broader measures are more likely to reflect the multi-dimensional property of educational production.

[1] Municipal merger reforms have been implemented in a number of countries including Canada (Dafflon, 2013), Denmark (Hansen, 2014), Sweden (Hinnerich, 2009; Jordahl and Liang, 2010), Israel (Reingewertz, 2012) and to some extent in Finland (Saarimaa and Tukiainen, 2015).

Third, we can control for individual socioeconomic characteristics in the analysis. Lastly, we are able to use a school fixed effects strategy. To the extent that municipality mergers did not lead to school consolidation, we can compare students before and after the merger attending the same schools.

Causal evidence of the output-size relationship is hard to obtain for a number of reasons. The size of a local unit measured by the number of inhabitants as an explanatory variable in traditional expenditure or output equations is clearly endogenous since fiscal variables and the production and quality of local public services affect migration decisions. An obvious alternative is to explore municipality or school district mergers in a quasi-experimental framework. However, to the extent that mergers are voluntary, endogeneity issues are still a concern. For municipalities to merge voluntarily, they not only need to find that the benefits outweigh the costs, they also must overcome any political coordination problems. Central authorities might have more knowledge about the expected benefits of a merger and can overcome coordination problems by enforcing the merger, making these mergers especially interesting to study. Using large structural reforms induced by the central government as the reform in Sweden in the 1950's or the reform in Norway in the 1960's can potentially offer better identifying opportunities. However, such large structural reforms often occur in combination with other reforms in the provision of local public services making it difficult to disentangle the impact of the different reform elements.[2] This paper uses forced mergers from a period without other large national structural reforms in the provision of local services and therefore offers a better opportunity to isolate the effect of mergers on municipal output.

The mergers we study were enforced by the central authorities based on recommendations from two official Norwegian reports (Norwegian Ministry of Local Government and Labor, 1986, 1989).[3] The mergers were former city municipalities merging with surrounding municipalities, having two main benefits. First, it creates a natural comparison group of city and surrounding municipalities. Second, there is reason to believe that merging could have different consequences for the city and surrounding municipalities. The mergers were often met by large local resistance in the municipalities surrounding the city and several referenda gave very little support

[2]For example the large reduction in the number of municipalities in Norway in the 1960's coincided with substantial changes in the education system (extension of mandatory school years from 7 to 9, a new curriculum and a new tracking system in the new compulsory lower secondary school, see Aakvik, Salvanes, and Vaage (2010). Similarly, the 1952 reform in Sweden which drastically reduced the number of municipalities coincided with extension of mandatory school years from 7 to 9, see Meghir and Palme (2005).

[3]All recommended mergers were carried out except in the case of the city municipality Hamar, where the merge met such large resistance from Løten municipality that they managed to remain independent.

for merger plans. If this resistance reflected correct anticipations of future merger effects on service production, the effect on output and quality in schools located in former surrounding municipalities could be negative. The rich individual by school by municipality data available to us, makes it possible to test this hypothesis.

Partly because of the large local resistance in the merger process, central authorities decided to no longer enforce mergers after the last merger was carried out in 1994. Although the municipalities chosen to merge are not random, the timing of the mergers might be. Also, there might have been municipalities that were next in line when the central authorities decided to abandon enforced mergers. This creates some randomness to the selection and timing and further strengthens our analysis.

This paper estimates the effect of school district size through municipal mergers using a difference-in-differences approach with a school fixed effects strategy. Municipality mergers are found to significantly increase student income in adulthood by 2-3%, while the effect on educational attainment is generally positive, but not precisely estimated. To enhance the understanding of possible mechanisms behind this important result, we further investigate possible heterogeneous effects by school location and the effect of mergers on school characteristics and fiscal variables, using the same difference-in-differences approach but with municipalities as the unit of analysis.

Our results clearly show that the income effect is driven by students enrolled in schools in pre-merger municipalities surrounding the former city. The effect on students enrolled in schools located in the pre-merger city is numerically very small and far from significant. Thus, the hypothesis that former surrounding municipalities resisted merger because of correct anticipations of negative future merger effects on service production and quality is not supported by the empirical results. Rather the evidence suggests the opposite. Output and quality as measured by our variables increased in these former surrounding municipalities. The former cities became administrative centers in the new municipalities. The finding is consistent with the hypothesis that students enrolled in schools in former surrounding municipalities took advantage of potential gains in existing administrative quality in the former cities, although further research is needed to confirm this interpretation.

We also find that the merger reduced total municipal expenditure per capita by nearly 5% which is qualitatively consistent with the evidence in Reingewertz (2012) although numerically smaller. The effect on expenditure per student (6-15 years old) is also negative but not statistically significant. This suggests that the positive student income effect in adulthood cannot be explained by increased total budgets in merged municipalities or budget reallocation in favor of the education sector.

Finally, we find that the number of lower secondary schools, the number of persons aged 7-16 and overall teacher quality measured by the share of teachers without a teacher certification at the municipality level is not significantly affected by the merger. Thus, we tentatively conclude that systematic changes in the number of schools, cohort size and teacher quality cannot explain the income effect.

The paper is organized as follows. Section 2 presents a review of the literature on the optimal size of local public authorities and relevant empirical studies. Section 3 describes the institutions and data while the identification and model specification are presented in Section 4. Section 5 presents the main results of the difference-in-differences estimation of municipality mergers on log income and years of education. Section 6 presents various robustness checks and Section 7 presents a discussion of mechanisms. Section 8 concludes.

2 Theoretical background and empirical literature

2.1 Theoretical background

The first generation fiscal federalism literature, represented by Oates' seminal contribution (Oates, 1972), formulated what is called the decentralization theorem. This theorem states that public services which are local in nature should be produced and financed at the local level because these entities can meet the demands of the local population in the least costly way.[4] Moreover, from a different perspective, Tiebout (1956) showed that an optimal allocation of private and public goods can be reached when households sort themselves across jurisdictions according to their preferences for local services and local taxes. Endogenous formation of a large number of jurisdictions and household mobility are central mechanisms to reach the Tiebout equilibrium.

The early theoretical contributions have been extended and challenged by authors taking political issues into account. On the one hand, authors in the public choice tradition, represented by the seminal contribution by Brennan and Buchanan (1980), also view fiscal decentralization as beneficial, but for a very different reason. In their view, the public sector acts as an agent ("Leviathan") with the objective of maximizing revenues extracted from the private sector. In this perspective decentralization of taxing and production decisions creates competition between local jurisdictions and leads to enhanced economic efficiency and taming of the "Leviathan". In both the Tiebout and the public choice model, enforced mergers of local jurisdic-

[4]This view is also presented in Musgrave and Musgrave (1973) and Atkinson and Stiglitz (1980).

tions could lead to a less efficient production of local services.

The second generation fiscal federalism literature has extended the original approach in Oates (1972) with an explicit modelling of the political process both at the central and local government level (see Oates (2005) for an extensive review). While the first generation literature assumes that central provision requires a uniform level of public output, recent authors allow for varying levels of outputs across jurisdictions in a centralized regime. For example, Lockwood (2002) and Besley and Coate (2003) model the centralized outcome as a vector of local outcomes determined by locally elected representatives. In their framework, decentralization has additional benefits in terms of reduced corruption, waste and poor governance compared to a centralized regime. These benefits must be weighed against potential losses due to spillovers between jurisdictions and scale effects in the production of local services.[5] Alesina and Spolaore (1997) explicitly consider jurisdictions with heterogeneous populations and argue that there is a trade-off between the benefits of large political jurisdictions and the costs of heterogeneity in large populations. They find that the democratic process leads to an inefficiently large number of jurisdictions (countries). Alesina, Baqir, and Hoxby (2004) take a similar approach and provide empirical evidence from U.S. municipalities, school districts and special districts that a trade-off between size and heterogeneity exists. They find a negative relationship between local government size and racial and income heterogeneity while no relationship is found between size and religious or ethnic heterogeneity.

2.2 Empirical literature

The theoretical models discussed above, suggest that gains from decentralization of public service production to a large number of jurisdictions must be balanced against potential economies of scale. While some studies confirm the existence of economies of scale in most municipal services,[6] other studies find that they only exist up to a certain size,[7] or find no correlation between costs and size.[8] However, local authorities have many services and optimal size may differ according to service. Most of the existing empirical literature has concentrated on scale effects on fiscal outcomes, such as expenditures and taxes. Oates (1985) provides an empirical test of the hypotheses that more decentralization reduces the size of government and

[5]Other papers in this literature are Besley and Case (1995), Ellingsen (1998) and Coate and Knight (2007).

[6]Kraus (1981); Duncombe and Yinger (2007); Razin (1999); Callan and Thomas (2001); DeBoer (1992); Farsi, Fetz, and Filippini (2007)

[7]Reiter and Weichenrieder (1997); Solé-Ollé and Bosch (2005); Breunig and Rocaboy (2008)

[8]Gyimah-Brempong (1987); Derksen (1988)

the tax burden as predicted by the public choice view represented by Brennan and Buchanan (1980). He finds no clear evidence that countries with more decentralized government structure have lower total public expenditure. Zax (1989) using data from U.S. local governments finds mixed evidence. While the size of multipurpose local governments like municipalities is negatively associated with measures of fiscal decentralization, the opposite seems to be the case for single-purpose governments like school districts. While potential effects on fiscal variables are interesting, knowledge of the relationship between local public output and quality, and size of political jurisdictions is warranted, but few empirical studies exist on this relationship.

One recent study, building explicitly on the fiscal federalism literature and providing evidence on the effect of decentralization on public output, is Barankay and Lockwood (2007). Using panel data for Swiss cantons, they find that educational attainment is higher in cantons with more decentralized provision of educational services measured by the share of education expenditures in a canton provided at the county level.

A small literature has also studied the effects of school district size on school output in a traditional educational production framework. The evidence on the effect of district size on student performance in this literature is mixed. Driscoll, Halcoussis, and Svorny (2003) use data from California to estimate an educational production function with test scores as output and find a negative effect of district size on test scores. Andrews, Duncombe, and Yinger (2002) review five studies from the United States that estimate the returns to school district size using test scores as the dependent variable. Of these, Walberg and Fowler (1987) and Ferguson (1991) find a negative effect of district size on test scores, Sebold and Dato (1981) and Baum (1986) find no or positive effects of district size, while Ferguson and Ladd (1996) find positive effects of district size. Kiesling (1967), Niskanen (1998) and Jacques, Brorsen, and Richter (2000) all find negative effects of district size on test scores.[9]

Test scores could be misleading as a measure of quality of school outputs, as they are possible to manipulate (Angrist, Battistin, and Vuri, 2015) and only measure cognitive skills, while non-cognitive skills might also be important for future outcomes (Kautz, Heckman, Diris, ter Weel, and Borghans, 2014). Both arguments suggest that analyses of long-run outcomes in terms of educational attainment and income provide the most credible evidence of the effect of district size (Driscoll, Halcoussis, and Svorny, 2003). Heinesen (2005) analyzes the effect of size of school district on educational attainment using Danish administrative register data and

[9]See also Fox (1981)

finds that educational attainment is higher for students from larger districts, i.e. districts with population above 15,000.

A problem with the studies above is that smaller and larger districts differ in characteristics that are not well measured. Over time, highly effective schools and districts may attract more students which will generate a bias towards finding increasing returns to size. Berry and West (2010) attempt to address this concern by exploiting the variation in the timing of consolidation across the United States to estimate the effect of changing school and district size on student outcomes. They find that larger districts have some modest gains with respect to returns to education but that these gains are outweighed by the harmful effect of larger schools. Reingewertz 2012 uses a difference-in-differences methodology to study the Israeli municipality consolidation reform of 2003 and finds positive effects of consolidations, among other things on the share of matriculation exam recipients. Gordon and Knight (2008) use school district consolidations to examine the effect of whole-grade sharing and consolidation of school districts on pupil-teacher ratio, enrollment, drop-out, revenues, and local expenditures, and their findings suggests an absence of efficiency gains from consolidations.

Other studies have looked at the effect of school consolidation on student outcomes. While not directly related to school district or municipality size, school consolidation may be one channel whereby municipality mergers can affect student outcomes. Beuchert, Humlum, Nielsen, and Smith (2015) exploit exogenous variation in school consolidations in Denmark to analyze their impact on student achievement and find that school consolidations have negative effects in the short run that are more pronounced for the students experiencing a school closure. Berry and West (2010) find that students educated in states with small schools have higher returns to education and complete more years of schooling.[10]

The methodology in this paper is similar to that of many other papers studying the impact of municipality mergers on various outcomes. Saarimaa and Tukiainen (2015) use a difference-in-differences methodology to investigate the free riding behavior in relation to voluntary municipal mergers and find that stronger free riding incentives create increased debt and spending. Reingewertz (2012) uses a difference-in-differences methodology to study the Israeli municipality consolidation reform of 2003 and finds that municipality consolidation reduced municipal expenditures without lowering the level of services. Moisio and Uusitalo (2013) investigates the impact

[10]See also Kuziemko (2006); Schwartz, Stiefel, and Wiswall (2013); Abdulkadiroğlu, Hu, and Pathak (2013); de Haan, Leuven, and Oosterbeek (2014); Humlum and Smith (2015); Barrow, Schanzenbach, and Claessens (2015); Engberg, Gill, Zamarro, and Zimmer (2012); Brummet (2014); Liu, Zhang, Luo, Rozelle, and Loyalka (2010).

of municipal mergers on local public expenditures in Finland. Rather than use a difference-in differences methodology, they use matching to compare pairs of merged municipalities to similar pairs of unmerged municipalities. The municipalities mergers they study are voluntary municipalities, and this method attempts to control for the non-random selection of municipalities that chose to merge.

3 Institutions and data

3.1 School system

Compulsory education is one of the core responsibilities of the Norwegian municipalities. The relative importance of the education sector in municipality activity is illustrated by its budget share of 43% on average for the 1980-1990 period, while the corresponding shares for child care, health care, culture and infrastructure is 4%, 18%, 6% and 17% respectively, see Borge, Brueckner, and Rattsø (2014). Schooling is provided free of charge and only a very small fraction of children enroll in private schools. Compulsory education in Norway consists of primary school and lower secondary school, and ends the year students turn 16 years of age.[11] Most students continue on to upper secondary education, which is divided into a three-year long academic study track and different vocational study tracks. After a major reform in 1994, vocational study tracks typically last for four years (including two years of apprenticeship training). Acceptance to upper secondary school is based on the grades achieved in grade 10. However, all students have been guaranteed admission to upper secondary education since 1994.

There is no possibility to fail a class in primary or in lower secondary education during the empirical period, which implies that all students finish compulsory education on time.[12] Education is comprehensive with a common curriculum for all students and there is no tracking. The cutoff between grades is birth at January 1.

[11] During the empirical period, the school starting age was 7 years. In 1997 the school starting age was reduced from 7 to 6 years such that today primary education consists of grades 1-7 (ages 6-13) and lower secondary education consists of grades 8-10 (ages 14-16). We refer to grades 8-10 as lower secondary education throughout the paper.

[12] In some cases, students do not start primary education at the expected age, which implies that they finish lower secondary education at a higher age. If a child is not considered to be mature enough, the parents together with the school and psychologists can postpone enrollment one year. In addition, some older students return to improve their grades, and immigrants are often over-aged at graduation.

3.2 Municipalities

Norway currently has 428 municipalities located in 19 different counties. Municipalities range in size from 206 inhabitants (Utsira) to 647,676 inhabitants (Oslo). The mean and median number of inhabitants are 12,027 and 4,674 respectively (Statistics Norway, 2015). Norwegian municipalities are multipurpose institutions, providing a large number of services, such as day care and care for the elderly, in addition to primary and lower secondary education. There are usually several primary schools within each school district, but many small school districts only have one lower secondary school.

Municipality mergers

Historically, the local public sector in Norway has been divided into a large number of small municipalities and in 1957 there were more than 700 municipalities in the country. An important feature of the Norwegian system is that changes in municipality borders and splits and mergers of municipalities must be approved by the central government. Thus, the central government has always played an important role in the design of municipality structure. During the 1960's the government initiated and implemented a large merger reform reducing the number by nearly 40 percent and as a result the number of municipalities was 454 in 1982.[13]

In our empirical analysis we explore eight enforced municipality mergers occurring from 1988 to 1994 which reduced the number of municipalities from 454 to 435.[14] The municipality mergers were carried out as a result of two Official Norwegian Reports charged with recommending municipality mergers surrounding cities (Norwegian Ministry of Local Government and Labor (1986, 1989), known as Buvik I and Buvik II respectively).

The mergers in the 1960's merged many city municipalities with surrounding municipalities, but in some cases, it was argued that the mergers had not gone far enough. This was particularly true for the county of Vestfold. The city municipalities of Horten, Tønsberg and Larvik were not expanded in the 1960's and experienced problems with placement of businesses, housing, and public infrastructure generally. The city municipalities had made many attempts at merging with surrounding municipalities without success.

In the 1980's, the ministry of Local Government and Labor decided it was nec-

[13] An extensive description of the historical development of municipality structure in Norway is given in Norwegian Ministry of Local Government (1992)

[14] After 1994, there have been 7 additional voluntary mergers bringing the number of municipalities down to 428.

essary to find a solution for these city municipalities and appointed a committee to look into potential mergers in Vestfold county. The committee published the Official Norwegian Report, Norwegian Ministry of Local Government and Labor (1986), recommending specific mergers around the city municipalities of Horten, Tønsberg and Larvik. The recommended Horten merger was implemented without resistance, while the recommended mergers for Tønberg and Larvik were passed with a majority in the Parliament. All mergers were implemented January 1, 1988.

Other city municipalities with similar problems were identified while working on the Vestfold mergers, and the committee was asked to look into potential mergers for the city municipalities of Sarpsborg and Fredrikstad in the county of Østfold, Arendal in the county of Aust-Agder, Hamar in the county of Hedmark and Hammerfest in the county of Finnmark. This resulted in the second Official Norwegian Report, Norwegian Ministry of Local Government and Labor (1989). The mergers for Sarpsborg, Arendal and Hammerfest were implemented as recommended January 1, 1992 while the recommended merger for Fredrikstad was implemented as recommended January 1, 1994. As for Hamar, the recommendation was that Hamar merge with Vang, Løten and a part of Ringsaker. The resistance in Løten was so large that they were able to remain independent by a marginal vote in their favor. Hamar, Vang and parts of Ringsaker merged January 1, 1994.

The mergers were often met with large resistance by affected municipalities,[15] and in 1995 the Parliament decided municipalities should no longer be merged against their will, after which no further municipalities merged until 2002.

Table 1 shows the complete list of municipalities affected by the mergers with city municipalities in italics. In all cases, the city municipality was chosen to have the new administrative center. Although all of the mergers are city municipalities merging with surrounding municipalities, we see that the number of inhabitants in the city and surrounding municipalities are quite similar, so it is not necessarily the case that a large city is absorbing much smaller neighboring municipalities.

[15]Some municipalities organized referendums before the proposed mergers. In Onsøy, Rolvsøy, Borge, Kråkerøy, Øyestad and Vang municipality, less than 10% voted for a merger.

Table 1: Municipality mergers

Year	New municipality	Municipalities merged	Population year prior to merger
1988	Tønsberg	*Tønsberg*	8,893
		Sem	21,942
1988	Larvik	*Larvik*	8,036
		Stavern	2,538
		Tjølling	7,876
		Brunlanes	8,137
		Hedrum	10,446
1988	Horten	*Horten*	12,993
		Borre	9,095
1992	Sarpsborg	*Sarpsborg*	11,826
		Varteig	2,199
		Skjeberg	14,295
		Tune	18,288
1992	Arendal	*Arendal*	12,478
		Moland	8,148
		Øyestad	8,679
		Tromøy	4,711
		Hisøy	4,026
1992	Hamar	*Hamar*	16,351
		Vang	9,103
1992	Hammerfest	*Hammerfest*	6,909
		Sørøysund	2,341
1994	Fredrikstad	*Fredrikstad*	26,539
		Borge	11,959
		Rolvsøy	5,947
		Kråkerøy	7,445
		Onsøy	12,923

3.3 Data

The Norwegian register data from Statistics Norway cover all individuals born in 1965-1984 leaving secondary school during 1981-2000. The data contain unique identifiers that allow us combine detailed individual information including which school they attended in lower secondary school. The main outcome variables are years of education and income. Years of education is measured by degrees obtained in 2011. In higher education that is bachelor degree, master degree, and PhD, with 16, 18, and 21 years of education, respectively. Income is measured as the log of average pension qualifying income for the years 2009 and 2010. The youngest individuals are 27 years of age when education is measured and 25-26 years of age when income is measured.

The individual register data include information on gender, birth month and immigration status.[16] We also have information on parental education[17] and parental employment status[18] the year the individual turns 16, the year the individual leaves lower secondary school. Descriptive statistics are presented in Table 2.

We define the first cohort affected by the merger as the cohort leaving lower secondary school the year of the merger. As the mergers occurred January 1st, this cohort is potentially affected by the reform for half a year. All subsequent cohorts are affected for an additional year.

There are two main samples in the analysis. In the first sample, "All municipalities", merged municipalities are compared to all other municipalities in Norway. In the second sample, "Potential mergers", merged municipalities are compared to all other potential municipality mergers. These are defined as all city municipalities that existed in 1987, the year before the first merger, and all municipalities bordering the city municipalities within the same county.[19] The sample includes 211 municipalities (46% of all municipalities) displayed in Figure 1. For both samples, the sample of merged municipalities includes a window of 10+/- years around the merger year. This time period is shortened for each merger either due to data only being available from 1981 or due to the data ending in 2000. All available years are included for the non-merged municipalities.

We restrict the sample to students turning 16 the year they graduate from lower secondary school. The cohort leaving school in 1990 has missing information on school identifies, and is therefore not included in the analysis. Students with missing information on income or years of education are excluded from the analysis. Table A1 reports the observations lost due to these restrictions in the "All municipalities" and in the "Potential mergers" sample.

[16]Immigration status is divided into first and second generation immigrant, where first generation immigrants are born abroad and have both parents born abroad, while second generation immigrants are born in Norway and have both parents born abroad.

[17]Parental education is categorized as the highest completed education by one of the parents. The categories included are upper secondary education (High school), Bachelor's degree, Master's degree or PhD, and unknown education, with less than upper secondary education being the reference category.

[18]Indicators for only mother working, only father working, and both parents working are included, with the reference category being no parent working.

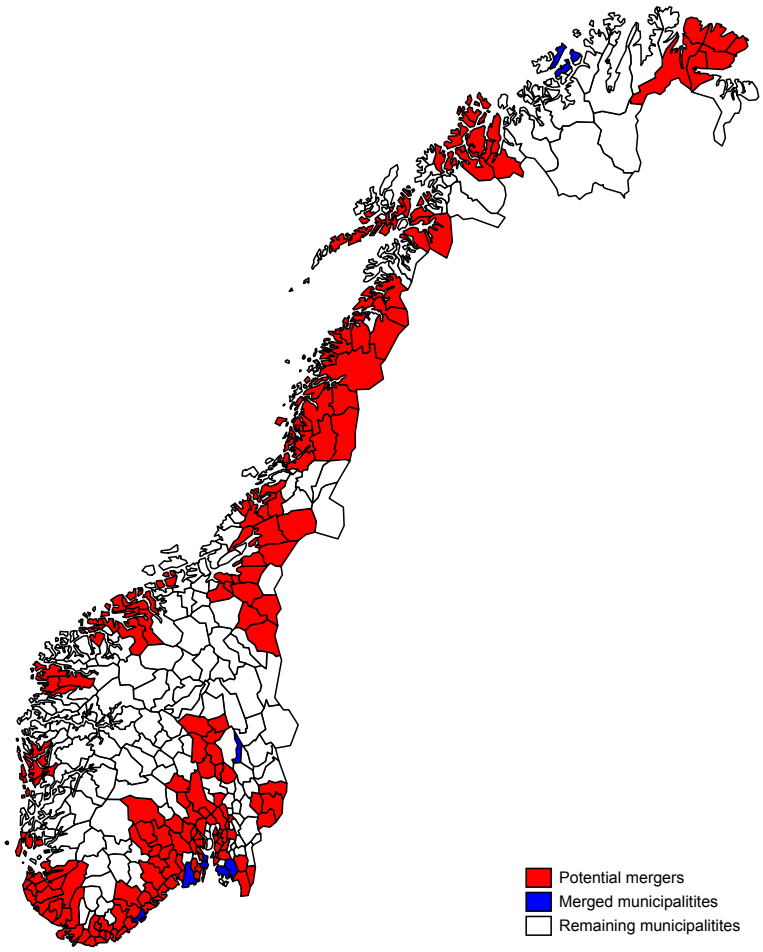
[19]For Oslo, all bordering municipalities are included regardless of county since Oslo is both a municipality and a county.

Table 2: Descriptive statistics

	Treated			Comparison all municipalities			Comparison potential mergers		
	mean	(sd)	N	mean	(sd)	N	mean	(sd)	N
A. Outcome variables									
Log of income 2009-2010	12.7	(0.75)	56245	12.7	(0.77)	924876	12.7	(0.79)	668313
Years of education	14	(2.55)	59635	13.9	(2.54)	976519	14	(2.57)	707819
B. Socioeconomic characteristics									
Girl	0.49	(0.50)	59635	0.49	(0.50)	976519	0.49	(0.50)	707819
Parental education: High School	0.56	(0.50)	59635	0.54	(0.50)	976519	0.53	(0.50)	707819
Parental education: Bachelor	0.21	(0.40)	59635	0.2	(0.40)	976519	0.2	(0.40)	707819
Parental education: Masters +	0.066	(0.25)	59635	0.077	(0.27)	976519	0.085	(0.28)	707819
Parental education: Unknown	0.028	(0.16)	59635	0.032	(0.18)	976519	0.034	(0.18)	707819
First generation immigrant	0.009	(0.09)	59635	0.013	(0.11)	976519	0.015	(0.12)	707819
Second generation immigrant	0.004	(0.06)	59635	0.006	(0.08)	976519	0.008	(0.09)	707819
Only mother working	0.17	(0.37)	59635	0.17	(0.37)	976519	0.17	(0.37)	707819
Only father working	0.16	(0.37)	59635	0.15	(0.35)	976519	0.15	(0.36)	707819
Both parents working	0.31	(0.46)	59635	0.33	(0.47)	976519	0.33	(0.47)	707819
Birth month	6.26	(3.33)	59635	6.35	(3.33)	976519	6.35	(3.33)	707819
C. Municipality characteristics (log)									
Total population	10.3	(0.55)	136	8.45	(1.02)	8001	8.8	(1.11)	3921
School aged population	8.15	(0.54)	136	6.39	(1.03)	8001	6.75	(1.11)	3921
16-year olds	5.95	(0.56)	136	4.17	(1.04)	8001	4.53	(1.11)	3921
Total expenditures	20.2	(0.53)	136	18.5	(0.90)	7999	18.8	(1.03)	3920
Per capita total expenditures	9.85	(0.28)	136	10	(0.39)	7999	9.97	(0.38)	3920
School expenditures	18.8	(0.47)	136	17.3	(0.88)	8000	17.6	(0.96)	3920
Per student school expenditures	10.7	(0.20)	136	10.9	(0.28)	8000	10.8	(0.27)	3920
Teachers without teacher certification	1.68	(0.94)	127	1.41	(0.94)	6825	1.55	(1.01)	3329
Lower secondary schools	1.66	(0.40)	136	0.55	(0.64)	7981	0.72	(0.73)	3903

Note: Descriptive statistics corresponding to the estimation sample for years of education. Treated includes all individuals from municipalities experiencing a merger. Comparison all municipalities includes all non-merged municipalities. Comparison potential mergers includes all non-merged city municipalities and their bordering municipalities in 1987. All municipality characteristics are measured in log. Errors in reporting school and total expenditures reduce N for these variables. For teachers without teacher certification and lower secondary schools, N is reduced due to observations with 0.

Figure 1: Potential mergers and merged municipalities



4 Identification and model specification

The merges are investigated using a difference-in-differences model estimated by OLS. *Treat* is equal to one if the individual graduated from a lower secondary school located in a municipality that merged sometime between 1981 and 2000. This includes all municipalities in Table 1. *Post* is equal to one in the time period after the merger for the cohorts thought to be affected by the merger. α_t is a cohort specific constant term and corresponds to age at graduation as we restrict our sample to students graduating from lower secondary school the year they turn 16. The cohort specific constant term consumes the separate effect of the variable *Post*.

This model can be expressed as

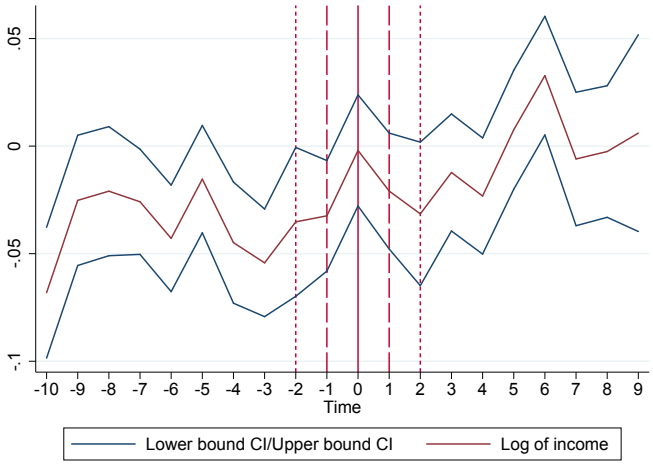
$$Y_{it} = \alpha_t + \beta Treat_i + \gamma Treat_i \times Post_t + X_{it}^{'}\delta + \epsilon_{it} \tag{1}$$

where i indexes individual and t indexes cohort. X indicates the socioeconomic characteristics of the individual, and includes individual characteristics (immigrant status, gender and birth month) and parental characteristics (parental education and employment status). Socioeconomic characteristics are measured the year the individual leaves lower secondary school. Standard errors, ϵ_{it}, are clustered at the school level.

We want to compare the outcomes of students in treated municipalities before and after the merger to students in non-treated municipalities before and after the merger. $Treat_i \times Post_t$ is our variable of interest, and γ captures this effect. If the change in outcomes from the pre-merger period to the post-merger period is significantly different in the merged municipalities than in the non-merged municipalities, then γ will be significantly different from 0. If γ is significant and positive, this indicates that the merger has a positive effect on outcomes and the opposite if γ is significant and negative.

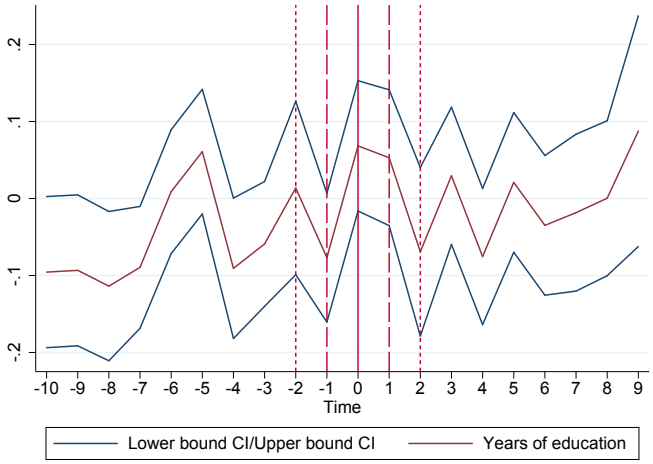
Figures 2 and 3 investigate whether the parallel trends assumption holds. The treatment (the mergers) occurred in different years in different municipalities. The figures present log of income (Figure 2) and years of education (Figure 3) relative to the control municipalities in the "Potential mergers" sample. Log of income and years of education in treated municipalities are compared to the non-treated municipalities in the same year for each individual observation. The red lines present the mean values while the blue lines present the 95% confidence interval. Time indicates the time period relative to the treatment year where the treatment year is time=0.

Figure 2: Trend in the relative log of income



Note: Trend in the relative log of income with 95% confidence interval. Time indicates time relative to treatment year, with 0 being the first year of treatment (solid red line). In Table 4 observations between the long dashed lines are dropped from estimations in column (2) and observations between the short dashed lines are dropped from estimations in column (4).

Figure 3: Trend in the relative years of education



Note: Trend in the relative years of education with 95% confidence interval. Time indicates time relative to treatment year, with 0 being the first year of treatment (solid red line). In Table 4 observations between the long dashed lines are dropped from estimations in column (2) and observations between the short dashed lines are dropped from estimations in column (4).

Both figures show some variation in the relative measures. However, the figures do not show a clear pre-treatment trend, which supports the parallel trends assumption. Relative log of income increases after the mergers indicating that income is increasing in treated municipalities relative to non-treated municipalities after the

merger. The pattern is not as clear for relative years of education, and it is unclear whether the mergers increased years of education.

5 Results

We run three versions of Equation (1). In the first, we exclude socioeconomic characteristics, in the second we include socioeconomic characteristics, and in the third version we add school fixed effects. Adding school fixed effects allows us to control for time-invariant unobserved differences between individuals from different schools. Results with the sample "All municipalities" are presented in columns (1)-(3) of Table 3. Results with the sample "Potential mergers" are presented in columns (4)-(6). The top panel displays results for log income while the bottom panel displays results for years of education.

For log income, estimates show that municipality mergers have a positive effect on income. After the merger, income increases by about 2-3 % in the merged municipalities compared to the non-merged municipalities. With the "All municipalities" sample, the effect is approximately 2%. The effect increases to 3% when the comparison group consists of the sample "Potential mergers". For years of education, the estimates are positive for years of education (about 0.05), but they are not significant at conventional levels when including school fixed effects. This is true for both samples, where the t-value is equal to 1.6 in the "All municipalities" sample and 1.3 in the "Potential mergers" sample.

Both samples confirm the same results. We believe the "Potential mergers" sample to be the best suited for this difference-in-differences specification. In Sections 6 and 7, estimates are reported using the "Potential mergers" sample along with time/age fixed effects, socioeconomic characteristics and school fixed effects.

Table 3: Effect of mergers on log income and years of education

	All municipalities			Potential mergers		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Dependent variable: Log income						
Treat*Post	0.0205**	0.0182*	0.0206**	0.0310***	0.0269**	0.0298***
	(0.0102)	(0.0103)	(0.0102)	(0.0104)	(0.0104)	(0.0103)
Treat	-0.0327***	-0.0279***		-0.0360***	-0.0291***	
	(0.0075)	(0.0063)		(0.0077)	(0.0064)	
Observations	981,126	981,126	981,126	724,561	724,561	724,561
R-squared	0.049	0.107	0.106	0.051	0.108	0.106
No. of schools			1,402			920
B. Dependent variable: Years of education						
Treat*Post	0.0514	0.0535*	0.0461	0.0572*	0.0500	0.0417
	(0.0325)	(0.0303)	(0.0316)	(0.0332)	(0.0308)	(0.0320)
Treat	-0.0214	-0.00433		-0.0518	0.0104	
	(0.0575)	(0.0329)		(0.0590)	(0.0336)	
Observations	1,036,154	1,036,154	1,036,154	767,454	767,454	767,454
R-squared	0.007	0.168	0.150	0.007	0.170	0.150
No. of schools			1,413			929
Time/age FE	Yes	Yes	Yes	Yes	Yes	Yes
Soc. Char.	No	Yes	Yes	No	Yes	Yes
School FE	No	No	Yes	No	No	Yes

Note: Standard errors clustered at the school level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Socioeconomic characteristics include birth month, gender, immigration status, parental education, and parental employment status.

6 Robustness checks

This section presents results for various model specifications. The results are presented in Table 4 and all should be compared to column (6) of Table 3.

First we investigate whether results are sensitive to excluding the two biggest cities from our sample, Bergen and Oslo. Results are reported in column (1) of Table 4. Both cities, along with their bordering municipalities are excluded from the estimation which reduces the sample by 26%. The estimate for log income is reduced from 3% to 2% but is still significant. For years of education, the estimate increases from 0.04 to 0.06 and is significant at the 10% level (t-value of 1.8).

Next, the years right before and after the merger are removed from the estima-

tion, creating a "donut hole". The first cohort affected by the merger is only in school for 6 months after the merger. This might not be sufficient time to expect there to be an effect. Also, there could be some anticipatory effects of the merger which would affect the cohorts leaving lower secondary school just before the merger. Removing the observations just around the time of the merger removes such concerns.

Column (2) reports the results when removing the one observation before and one after (time= -1 and time= 0). Column (3) reports results when two years are removed before and after the merger (time= -2 and time= 1 are also removed). The long dashed lines in Figures 2 and 3 correspond to the 2-year "donut hole" (column (2)) while the short dashed lines correspond to the 4-year "donut hole" (column (3)). In both specifications the results remain strongly significant for log income. The estimate is 3% for the 2-year "donut hole" and 3.5% for the 4-year "donut hole". For education, results are insignificant.

Another concern is the length of our estimation window. In our main results, the estimation window is 10 years before and after the reform (when possible). Column (4) estimates the results when reducing this window to 5 years. This reduces the point estimate to 1.7% for income with a t-value of 1.7. For each year following the merger, the cohort leaving lower secondary school has spent an additional year in a post-merge school. If there is an effect of the merger through schools, then we would expect this effect to be larger for later cohorts. It is therefore expected that this estimate is somewhat lower. For years of education, the results are very similar to column (6) of Table 3.

Finally, we run a placebo reform. In this specification, we pretend that the merger happened 4 years before and only include pre-merger years for the treated municipalities. A significant estimate in this specification would challenge our common trends assumption. For both log income and years of education, estimates are insignificant. The estimate is -1.2% for income with a t-value of 0,984. For years of education, the estimate is -0.04 with a t-value of 1.27.

Table 4: Robustness checks

	(1) No big cities	(2) 2 year «donut hole»	(3) 4 year «donut hole»	(4) 5-year window	(5) Placebo reform
A. Dependent variable: Log income					
Treat*Post	0.0203**	0.0309***	0.0345***	0.0166*	-0.0123
	(0.0103)	(0.0114)	(0.0124)	(0.0098)	(0.0125)
Observations	537,513	717,809	712,854	561,246	465,775
R-squared	0.108	0.106	0.106	0.088	0.086
No. of schools	696	920	920	889	870
B. Dependent variable: Years of education					
Treat*Post	0.0585*	0.0241	0.00954	0.0451	-0.0423
	(0.0326)	(0.0347)	(0.0358)	(0.0316)	(0.0332)
Observations	567,327	760,317	755,087	594,036	497,141
R-squared	0.150	0.150	0.150	0.150	0.145
No. of schools	703	929	929	896	873

Note: All regressions include time/age fixed effects, socioeconomic characteristics and school fixed effects. No big cities drops the city municipalities Oslo and Bergen along with their bordering municipalities. 2 and 4 year "donut hole" drop the 1+/- and 2+/- years surrounding the merger. 5-year window reduces the estimation window to 5 +/- years surrounding the merger. Placebo reform runs the specification as if the merger occurred 4 years earlier and only includes years before the merger occurred. The sample corresponds to the "Potential mergers" sample. Standard errors clustered at the school level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Socioeconomic characteristics include birth month, gender, immigration status, parental education, and parental employment status.

7 Mechanisms

What are the mechanisms behind the main results? First, we investigate if results differ depending on whether the student attends a school in a city or a surrounding municipality. Next, we investigate whether municipality characteristics change before and after the merger using the (merged) municipality as the unit of analysis.

7.1 City vs. surrounding schools

A unique feature of our data set is that we can separate between city municipality schools and surrounding municipality schools both before and after the merger. This allows us to study the effect for students attending city school and surrounding schools separately.

Table 5 displays the results. Column (1) the same specification as column (6) of Table 3. In column (2), only students from city schools are included in the analysis. This includes students in city municipalities that experience a merger and students in city municipalities in the "Potential mergers" comparison group. The point estimate for log income is small (0.7%) and the results are nowhere close to being significant. The point estimate for years of education is negative and not significant.

In column (3), only students from surrounding schools are included in the analysis. This includes students in surrounding municipalities that experience a merger and students in surrounding municipalities in the "Potential mergers" comparison group. The point estimate for log income is 3%, and is highly significant, while the point estimate for years of education is 0.06 and not significant at conventional levels (t-value of 1.58). This shows that the results are driven by students from surrounding schools.

Table 5: Mechanisms – city vs. surrounding schools

	(1) All schools	(2) City schools	(3) Surrounding schools
Dependent variable: Log income			
Treat*Post	0.0298***	0.00710	0.0293***
	(0.0103)	(0.0181)	(0.0110)
Observations	724,561	410,248	314,421
R-squared	0.106	0.105	0.108
No. of schools	920	462	461
Dependent variable: Years of education			
Treat*Post	0.0417	-0.0448	0.0616
	(0.0320)	(0.0414)	(0.0391)
Observations	767,454	436,567	331,013
R-squared	0.150	0.151	0.149
No. of schools	929	469	462

Note: All regressions include time/age fixed effects, socioeconomic characteristics and school fixed effects. The sample corresponds to the "Potential mergers" sample. Standard errors clustered at the school level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Socioeconomic characteristics include birth month, gender, immigration status, parental education, and parental employment status.

7.2 Municipality effects

Lastly, we investigate whether municipality characteristics change in merged municipalities relative to non-merged municipalities after the merger. Total population

and school aged population are from Statistics Norway while the number of schools and 16-year olds are constructed from our data. Expenditure measures are from municipality accounts and the share of certified teachers is a measure previously used by Bonesrønning, Falch, and Strøm (2005) and Falch, Johansen, and Strøm (2009).

Table 6 displays results where estimations include one observation per (merged) municipality and year. In Column (1), the outcome is the log of total population in the municipality. Columns (2) and (3) include the school aged population and the 16 year olds respectively. All estimates are insignificant. There is no evidence of demographic changes resulting from the mergers.

Table 6: Mechanisms – Municipality characteristics, population

	(1)	(2)	(3)
	Total population (log)	School aged population (log)	16 year-olds (log)
Treat*Post	0.00631	-0.00249	-0.0654
	(0.0365)	(0.0434)	(0.0476)
Treat	1.507***	1.407***	1.454***
	(0.2192)	(0.2128)	(0.2065)
Observations	4,057	4,057	4,057
R-squared	0.058	0.056	0.060
Time FE	Yes	Yes	Yes

Note: The sample corresponds to the "Potential mergers" sample. The estimation includes one observation per (merged) municipality and year. Standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

In Table 7, the first four columns display outcomes related to expenditures, measured in log. The first is total expenditures, the second is per capita total expenditures, the third is school expenditures and the fourth is per student school expenditures. The only significant estimate is the per capita total expenditures where merged municipalities have 3.8% lower expenditures after the merger compared to non-merged municipalities. This result is qualitatively consistent with the evidence in Reingewertz (2012) although numerically smaller. The effect on expenditure per student (6-15 years old) is also negative but not statistically significant. This suggests that the positive student income effect in adulthood cannot be explained by increased total budgets in merged municipalities or budget reallocation in favor of the education sector.

Table 7: Mechanisms – Municipality characteristics, population

	(1) Total exp. (log)	(2) Per capita total exp. (log)	(3) School exp. (log)	(4) Per student school exp. (log)	(5) Teachers w/o teacher certification (log)	(6) Lower secondary schools (log)
Treat*Post	-0.0317	-0.0384**	-0.0274	-0.0262	0.0273	-0.0281
	(0.0314)	(0.0169)	(0.0351)	(0.0261)	(0.2392)	(0.0312)
Treat	1.405***	-0.102*	1.276***	-0.131**	0.143	0.943***
	(0.1759)	(0.0536)	(0.1741)	(0.0543)	(0.2761)	(0.1549)
N	4,056	4,056	4,056	4,056	3,456	4,039
R-squared	0.134	0.537	0.068	0.254	0.048	0.053
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: The sample corresponds to the "Potential mergers" sample. The estimation includes one observation per (merged) municipality and year. Errors in reporting school and total expenditures reduce N compared to Table 7 for these variables. For teachers without teacher certification and lower secondary schools, N is reduced due to observations with 0. Standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

In column (8), the outcome is teachers without teacher certifications. The variable teachers without teacher certification is a reasonable measure of teacher quality (Bonesrønning, Falch, and Strøm, 2005; Falch, Johansen, and Strøm, 2009). Our results do not seem to be driven by increased teacher quality. The last column is the number of lower secondary schools in the municipality. There is no evidence of a change in the number of lower secondary schools as a result of the merger.

8 Conclusion

We use the spatial and temporal variation in municipality merges in a difference-in-differences approach to provide quasi-experimental evidence of the effect of municipality size on school output measured by student educational attainment and income in adulthood. Municipality mergers are found to increase student income in adulthood by 2-3%, while the effect on educational attainment is generally positive, but not so precisely estimated.

Our results are consistent with the hypothesis that student enrolled in schools in former surrounding municipalities took advantage of potential gains in existing administrative quality in the former cities. The income effect is driven by students enrolled in schools in premerger municipalities surrounding the former city, not by students enrolled in premerger city schools. However, further research is needed to confirm this interpretation.

We also find that the merger reduced total municipal expenditure per capita by

nearly 5% which is qualitatively consistent with the evidence in Reingewertz (2012) although numerically smaller. The effect on expenditure per student (6-15 years old) is also negative but not statistically significant. This suggests that the positive student income effect in adulthood cannot be explained by increased total budgets in merged municipalities or budget reallocation in favor of the education sector. Finally, we find that the number of lower secondary schools, the number of persons aged 7-16 and overall teacher quality measured by the share of teachers without a teacher certification at the municipality level is not significantly affected by the merger. Thus, we tentatively conclude that systematic changes in the number of schools, cohort size and teacher quality cannot explain the income effect.

When deciding whether to merge municipalities, proponents argue that larger municipalities increase efficiency, while opponents argue that the population is further removed from their elective officials. The results from this paper suggest that municipality mergers can have positive effects on school outputs measured by years of education and income in adulthood, lending support to the proponents of municipality mergers.

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A Appendix

Table A1: Data reduction

	All municipalities		Potential mergers	
	Observations	% Reduc.	Observations	% Reduc.
1. Sample 1982-2000 (without 1990)	1105383		823700	
2. Non-missing municipality	1103880	0,14 %	822197	0,18 %
3. 16 years old when graduating from lower secondary school	1044816	5,35 %	775671	5,66 %
4. 10 +/- years around merge	1036919	0,76 %	768072	0,98 %
5. Non missing years of education	1036154	0,07 %	767454	0,08 %
5. Non missing log of income	981126	5,38 %	724561	5,67 %

Note: Data on the school identifier is missing in 1990. 55,789 and 43,508 observations have zero income for all municipalities and potential mergers respectively. They excluded from the analysis because we use the logarithmic value of income.