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Long-run Effects of Local Government Mergers on Educational Attainment and Income*

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Abstract

Local government mergers are an important policy issue in many countries, yet empirical evidence of the effects of merging and of local government size on the production and quality of local public services is scarce. We use the spatial and temporal variation in forced mergers between cities and their surrounding local governments in Norway to provide quasi-experimental evidence of the effect on long-run student outcomes. We find that the mergers increase students' educational attainment by about 0.1 years and income by about 4%, suggesting that mergers improve long-run student outcomes through increased school productivity.

I. Introduction

Reforms to consolidate local governments are contentious issues in many countries, and such reforms are currently on the political agenda in countries like Norway and Finland.[1] Yet the effect of such reforms on the production and quality of local public services is uncertain. On the one hand, increasing local government size could increase public service quality for a given amount of available resources through economies of scale. On the other hand, local governments may be less able to meet the needs of public service users as the population increases and becomes more heterogeneous, thereby reducing average quality. Ultimately, the relationship between local

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[1]Reforms merging local governments 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 Finland (Saarimaa and Tukiainen, 2015).

government size and the quality of public sector services is an empirical question, where mergers provide a quasi-experimental framework to estimate causal relationships.

In this paper we investigate whether increasing local government size has a positive effect on students' long-term outcomes through increasing the quality of an essential public service provided by local governments; compulsory schooling. In the 1980s and 1990s, the Norwegian central government enforced mergers between cities and their surrounding local governments, resulting in increased local government size while schools and catchment areas remained unchanged. We exploit these mergers to estimate the effect on student income and educational attainment, using a difference-in-differences approach with school fixed effects. These outcome variables are likely to reflect the multi-dimensional property of educational production.

There are several advantages of our approach. First, we investigate forced mergers, excluding some endogeneity issues. The central government may have more knowledge than local actors on the expected benefits of a merger, and can overcome coordination problems preventing voluntary mergers. Finding positive effects of the merger suggests that enforcement or strong incentives may be necessary to achieve efficiency gains from consolidation.

Second, the mergers were carried out at different times; 1988, 1992 and 1994. Partly due to the strong local resistance to the merger process, the central government decided that mergers would no longer be enforced after 1994, preventing any potential mergers that were next in line. This creates some randomness both in the selection and timing of mergers.

Third, city local governments merged with their surrounding local governments, with the former city local government becoming the administrative centre in the new local governments. Similar mergers took place in the 1960s, providing a natural comparison group. Also, there is reason to believe that merging could have different consequences for the cities and for the surrounding local governments. The mergers often met with strong local resistance in the surrounding local governments and several referenda gave very little support for the planned mergers. If this resistance reflected correct anticipation of the future effects of a merger on service production, the effect on school quality in the schools located in former surrounding local governments should be negative. School identifiers allow us to explore possible heterogeneous effects of mergers across premerger local governments.

We find that the mergers significantly increase student income in adulthood by around 4%, while the effect on educational attainment measured by years of education is generally positive and around 0.1. The income effect is driven by students enrolled in schools located in premerger local governments surrounding the former city, suggesting that surrounding local governments did not have correct anticipation of negative future merger effects. The finding is consistent with the hypothesis that students enrolled in schools in former surrounding local governments took advantage of potential gains in existing administrative quality in the former cities, although further research is needed to confirm this interpretation. Our findings also imply that it can be beneficial to use enforcement to overcome merger coordination problems at the local government level.

The paper is organized as follows. Section II presents the literature, section III describes the institutions and the data and section IV presents the identification and the model specification. Section V presents the main results, section VI analyses potential alternative mechanisms, and finally, section VII concludes.

II. Literature

A rich literature models the relationship between jurisdiction size and output. The decentralization theorem (first formulated by Oates, 1972 and also presented by Musgrave and Musgrave, 1973; Atkinson and Stiglitz, 1980) 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.

Tiebout (1956) showed that optimal allocation of private and public goods can be achieved 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 is central mechanisms for reaching the Tiebout equilibrium. The public choice tradition, where the public sector acts as an agent ('Leviathan') with the objective of maximizing revenues extracted from the private sector, also views fiscal decentralization as beneficial (Brennan and Buchanan, 1980). In this perspective decentralization of taxation and production decisions creates competition between local jurisdictions and leads to enhanced economic efficiency and taming of the 'Leviathan'. Both of these mechanisms suggest that enforcing mergers of local jurisdictions would lead to a less efficient production of local services.

A more recent literature explicitly models the political process at both the central and local government level (see Oates, 2005 for an extensive review), allowing for varying levels of outputs across jurisdictions in a centralized regime. Lockwood (2002) and Besley and Coate (2003), for instance, model the centralized outcome as a vector of local outcomes determined by locally elected representatives, where decentralization has benefits in terms of reduced corruption, waste and poor governance compared to a centralized regime, and potential losses due to spillovers between jurisdictions and scale effects in the production of local services. 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, finding 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 US local governments that a trade-off between size and heterogeneity exists.

While some studies confirm the existence of economies of scale in most local government services, other studies find that they only exist up to a certain size, or find no correlation between cost and size (Gyimah-Brempong, 1987; DeBoer, 1992; Solé-Ollé and Bosch, 2005; Duncombe and Yinger, 2007; Breunig and Rocaboy, 2008). However, local authorities typically produce a variety of services and scale effects might differ across services. Additionally, most of the existing empirical literature has concentrated on the effects of scale on fiscal outcomes, such as expenditure and taxes.

Fewer empirical studies exist on the relationship between the size of a political jurisdiction and local public outcome and quality. Barankay and Lockwood (2007) build explicitly on the fiscal federalism literature and provide evidence from Swiss cantons that educational attainment is higher with more decentralized provision of educational services. Galiani, Gertler and Schargrodsky (2008) study the effect of transferring federal schooling to provincial control, and find an overall positive effect on student test scores. Salinas and Solé-Ollé (2018) study the transfer of autonomy in Spain from the federal government to the regional governments during the 1980s in a difference-in-differences framework and find similar results on early dropout from schools. Falch and Fischer (2012) estimate the effect of public sector spending decentralization by utilizing a panel of international student achievement tests, and the results suggest that decentralization is beneficial to student performance. Heinesen (2005) analyses the association between school district size and educational attainment using Danish administrative register data and finds that educational attainment is higher for students from larger districts.[2] 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 positive returns to size.

Closely related to our paper are studies that exploit school district consolidation reforms to study student outcomes. Berry and West (2010) attempt to address endogeneity concerns by exploiting the variation in the timing of school district consolidation across the United States and 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 local government consolidation reform of 2003 and finds positive effects of consolidation, among other things on the share of matriculation exam recipients.

Other studies have looked at the effect of school consolidation. While not directly related to school district or local government size, the topic of the present paper, school consolidation may be one channel whereby local government mergers can affect student outcomes, as indicated by Berry and West (2010). Beuchert *et al*. (2018) exploit exogenous variation in school consolidations in Denmark and find that school consolidations have negative effects on student achievement in the short run, which are most 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.[3]

Studies of the effect of local government mergers on non-educational outcomes are also methodologically related to this paper. Moisio and Uusitalo (2013) investigate the

[2]A limited literature, finding mixed evidence, has studied the association between student performance and school district size in a traditional educational production framework using OLS models. Andrews, Duncombe and Yinger (2002) review five studies from the United States that estimate the returns to school district size using student test scores as the dependent variable; two studies find a negative association, one finds a positive association, and the two last studies have mixed findings.

[3]The literature on school consolidation also includes Abdulkadiroğlu, Hu and Pathak (2013); De Haan, Leuven and Oosterbeek (2016); Barrow, Schanzenbach and Claessens (2015); Brummet (2014).

impact of voluntary municipal mergers in Finland in the 1970s on local public expenditure in Finland using a matching approach, and find that per capita expenditure increased. Saarimaa and Tukiainen (2015) study voluntary municipal mergers in Finland in 2009 using a difference-in-differences methodology and find that free-ride incentives create increased debt and spending. Lastly, Reingewertz (2012) studies the Israeli local government consolidation reform of 2003 using a difference-in-differences approach and finds that the consolidation reduced municipal expenditure without lowering the level of services.

III. Institutions and data

School system

Norwegian compulsory schooling consists of primary and lower secondary education[4] and is provided free of charge by multipurpose local governments. Less than 1.5% of students were enrolled in private schools in the empirical period.[5] Most students continue on to upper secondary education, which is divided into a 3-year long academic study track and different vocational study tracks. After a major reform in 1994, vocational study tracks typically last for 4 years (including 2 years of apprenticeship training). Acceptance to upper secondary school is based on the grades achieved at the end of lower secondary education. However, all students have been guaranteed admission to upper secondary education since 1994. Public upper secondary schools are owned and run by county governments.

It is not possible to fail a class in primary or lower secondary education during the empirical period, meaning that all students finish compulsory education on time at age 16.[6] Education is comprehensive with a common curriculum for all students and there is no tracking in compulsory education. The cutoff between grades is age at January 1.

Local governments

Norway had 422 local governments in 2018, located in 19 different counties. After a wave of mergers, there are 356 local governments in 11 counties in 2021. Local governments range in size from about 200 inhabitants (Utsira) to about 690,000 inhabitants (Oslo). The mean and median numbers of inhabitants in 2020 were 15,077 and 5,163 respectively (Statistics Norway, 2021). Norwegian local governments are multipurpose institutions, providing a large number of services, such as day care and

[4]The school starting age was 7 years until 1997 when it was reduced from 7 to 6 years such that today's 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.

[5]During this period, private schools were mainly specific religious schools and not a realistic alternative for the great majority of families.

[6]In some cases, students do not start primary education at the expected age, meaning 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 enrolment by 1 year. In addition, some older students return to improve their grades, and immigrants are often over-aged at graduation.

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. Compulsory education is one of the core responsibilities of local governments, illustrated by its budget share of 43% on average for the period 1980–90. The corresponding budget shares for child care, health care, culture and infrastructure are 4%, 18%, 6% and 17%, respectively, see Borge, Brueckner and Rattsø (2014).

Local government mergers

In our empirical analysis we explore eight enforced local government mergers occurring from 1988 to 1994, reducing the number of local governments from 454 to 435, summarized in Table 1.[7] The local government mergers were carried out as a result of two official Norwegian reports charged with recommending local government mergers surrounding cities (Norwegian Ministry of Local Government and Labor (1986, 1989), known as Buvik I and Buvik II, respectively).

In order to interpret the empirical analysis, it is important to describe the context for the mergers in greater detail. Historically, the local public sector in Norway has been divided into a large number of small local governments. In 1960 there were more than 700 local governments in the country. An important feature of the Norwegian system is that changes in local government borders and splits and mergers of local governments must be approved by the national parliament. Thus, the central government has always played an important role in the design of the local government structure. During the 1960s, the government initiated and implemented a large merger reform reducing the number by nearly 40% and as a result the number of local governments was 454 in the period 1977–87.[8]

The reform in the 1960s covered local governments in both rural and urban areas of the country. For central areas around the cities, the general principle was to merge many small city local governments with surrounding local governments. However, after some years it become apparent that the mergers implemented in central areas during the 1960s were not sufficient. This was particularly true for the county of Vestfold, where the city local governments of Horten, Tønsberg and Larvik were not merged with surrounding local governments and experienced problems with placement of businesses, housing and public infrastructure. These city local governments had made many unsuccessful attempts at merging voluntarily with surrounding local governments. In the 1980s, the Ministry of Local Government and Labor appointed a committee to look into potential mergers in Vestfold county and in the Buvik I report, the committee recommended specific mergers around these city local governments. The recommended Horten merger was unanimously passed by Parliament, while the

[7]During the period 1994–2018, there were 12 additional voluntary mergers, bringing the number of local governments down to 422 in 2018.

[8]An extensive description of the historical development of local government structure in Norway is given in Norwegian Ministry of Local Government (1992).

TABLE 1

Local government mergers

Year	*New local government*	*Local governments merged*	*Population year prior to merger*	*Population with higher education year prior to merger*
1988	Tønsberg	*Tønsberg*	8,893	15.7%
		Sem	21,942	14.2%
1988	Larvik	*Larvik*	8,036	9.7%
		Stavern	2,538	12.9%
		Tjølling	7,876	13.9%
		Brunlanes	8,137	12.4%
		Hedrum	10,446	9.2%
1988	Horten	*Horten*	12,993	13.6%
		Borre	9,095	13.9%
1992	Sarpsborg	*Sarpsborg*	11,826	13.8%
		Varteig	2,199	10.2%
		Skjeberg	14,295	8.2%
		Tune	18,288	11.0%
1992	Arendal	*Arendal*	12,478	19.7%
		Moland	8,148	11.8%
		Øestad	8,679	11.4%
		Tromøy	4,711	16.6%
		Hisøy	4,026	22.5%
1992	Hamar	*Hamar**	16,351	21.8%
		Vang	9,103	13.7%
1992	Hammerfest	*Hammerfest*	6,909	16.7%
		Sørøysund	2,341	9.1%
1994	Fredrikstad	*Fredrikstad*	26,539	16.1%
		Borge	11,959	10.6%
		Rolvsøy	5,947	10.0%
		Kråkerøy	7,445	17.0%
		Onsøy	12,923	12.2%

Note: Local governments in italics are the city local governments, defining the name of the new local governments. *Hamar also merged with a small part of Ringsaker where the population prior to the merger was 224.

recommended mergers for Tønberg and Larvik were passed by a parliamentary majority. All mergers were implemented on 1 January 1988.

Other city local governments with similar problems were identified while working on the Vestfold mergers, and the same committee was asked to discuss similar mergers for the city local governments in other counties. The Buvik II report recommended five additional mergers, and all were mainly implemented as recommended during the next 5 years. The mergers for Sarpsborg in the county Øtfold, Arendal in the county Aust-Agder and Hammerfest in the county Finnmark were implemented on 1 January 1992. The merger for Fredrikstad in the county Østfold was implemented on 1 January 1994. For Hamar, the recommendation was to merge with Vang, Løten and a small part of Ringsaker. The resistance in Løten was so strong that they were able to remain independent by a marginal vote in their favour in Parliament. The Hamar merger took place on 1 January 1992.

The mergers often met with strong resistance in the affected surrounding local governments.[9] Because of this resistance and the fear of potential future political unrest, the Parliament decided in 1995 that local governments should no longer be merged against their will, after which no further local governments merged until 2002.

Table 1 shows the complete list of local governments affected by the mergers, with the city local governments in italics. In all cases, the administrative centre in the merged local government was located in the premerger city. Although all of the mergers consist of city local governments merging with surrounding local governments, the numbers of inhabitants in the city and surrounding local governments are quite similar, so it was typically not the case that a large city absorbed much smaller neighbouring local governments. The city and surrounding local governments differ more when it comes to the level of education, with the city local government in most cases having a higher educational level prior to the mergers.

Data

We use Norwegian register data from Statistics Norway covering all individuals born in the period 1965–84 and leaving secondary education in the period 1981–2000. The data contain unique identifiers that allow us to combine detailed individual information, including which lower secondary school they attended. The main outcome variables are years of education and income. Years of education is measured by degrees obtained by 2011. For higher education, the measures for bachelor's degree, master's degree and PhD are 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.[10] The youngest individuals in the data are 27 years of age when education is measured and 25–26 years of age when income is measured. Outcome variables are measured in the same way as Falch, Sandsør and Strøm (2017). The individual register data also include various socioeconomic statistics, summarized in Table A1.

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

We restrict the sample to students turning 16 the year they graduate from lower secondary school. The cohort leaving school in 1990 lacks school identifier information, and is therefore not included in the analysis. Students with missing information on income or years of education are excluded from the analysis. Table A4 reports the observations lost due to these restrictions.

[9]Some local governments organized referendums before the proposed mergers. In the local governments of Onsøy, Rolvsøy, Borge, Kråkerøy, Øyestad and Vang, less than 10% voted for a merger.

[10]We use the pension-qualifying income as reported in the tax registry. This income measure is not top coded and includes labour income, taxable sick benefits, unemployment benefits, parental leave payments and pensions, see Black, Devereux and Salvanes (2013, p. 132).

IV. Identification and model specification

We use a difference-in-differences approach where we compare the change in student outcomes across cohorts that finished compulsory education before and after a local government merger (treatment group) with the similar change for students located in local governments that did not experience a merger (comparison group). A major issue with this approach is how to define a credible comparison group. In the mergers we study, city local governments were merged with their surrounding local governments since they were considered to be too small in terms of area available for building houses and necessary transport infrastructure. Thus, the mergers were not directly related to characteristics of the education sector. The comparison group should reflect the outcomes of students in the treated local governments had the local governments not been merged, and should enable us to control for the impact of both observed and unobserved student and local characteristics related to the outcomes.

As our baseline comparison group, we choose the local governments that were the result of city governments that merged with their surrounding local governments in connection with the large consolidation reform in the 1960s, more than 20 years before the mergers included in our analysis took place. One argument for this choice is that the committee appointed to consider mergers in the 1980s (Buvik II), explicitly referred to the principle behind the mergers of city and surrounding local governments implemented in the 1960s reform. Moreover, to bolster the arguments for their recommendations, the committee (p. 24 in the report) provided a list of local governments that were the result of similar mergers between city and surrounding communities implemented in the 1960s reform. The list of local governments consists of 38 mergers from all over the country and is a natural point of departure when defining a comparison group similar in characteristics to the local governments merged in the 1980s and 1990s. The sample using this group of governments as the comparison group is denoted 'Previous mergers'. It includes 36 local governments (8% of all non-treated local governments) and their geographical location is illustrated in Figure 1a.[11]

In order to judge the robustness of the results, we also present results for two alternative comparison groups. The first alternative group is defined as all non-treated city governments that existed in 1987 (the year before the first treatment), and all local governments bordering these city governments within the same county.[12] This comparison group includes 207 local governments (49% of all non-treated local governments) and represents local governments in a geographical area that could potentially have been merged if the Parliament had not made the decision in 1995 to stop the merging process that started in the 1980s and early 1990s. The sample using this comparison group is denoted 'Potential mergers' and the geographical location of the local governments in this group is illustrated in Figure 1b. The second alternative

[11]From the list of 38 mergers, we exclude the local government of Ålesund because it was de-merged in the 1970s and the city of Fredrikstad because it was merged with more local governments in 1994 and is thus in the treatment group.

[12]For Oslo, all bordering local governments are included regardless of county since Oslo is a county on its own.

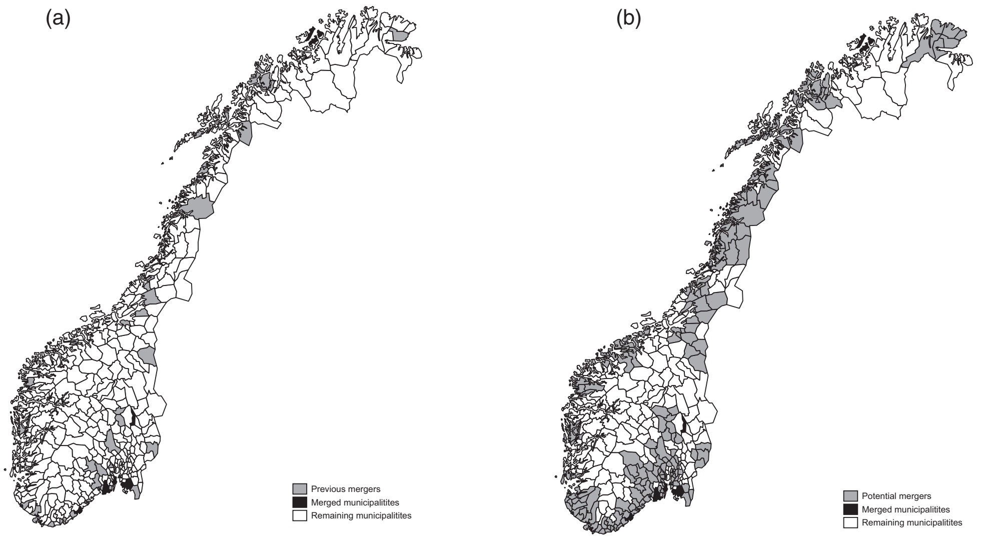

Figure 1. Comparison groups
Note: The figures display the 435 municipalities that existed after the mergers studied in this paper took place.

comparison group is all non-treated local governments, and this sample is denoted 'All local governments'. For all samples, the sample of each treated local government 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 comparison groups.

The model we estimate can be expressed as

$$Y_{ist} = \alpha_t + \beta_s + \gamma d_{st} + X'_{ist}\delta + \varepsilon_{ist} \tag{1}$$

where Y_{ist} is the outcome for individual i in school s at time t. 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 education the year they turn 16. Cohort fixed effects control for temporal shocks that affect all local governments equally. β_s is school fixed effects, which can be included because the school structure was not affected by mergers, at least not in the short term. School fixed effects control for time-invariant unobserved differences across schools, irrespective of whether the school is located in merging school districts or not. X_{ist} is socioeconomic characteristics at the individual level, and includes individual characteristics (immigrant status, gender and birth month) and parental characteristics (parental education and employment status). The socioeconomic characteristics are measured at age 16. Standard errors, ϵ_{it}, are clustered at the (merged) local government level. d_{st} is the consolidation variable for local government s in year t (denoted merger in tables) and γ is the coefficient of interest. If the change in outcomes from the premerger period to the postmerger period is different in the merged local governments than in the non-merged local governments, then γ will be significantly different from zero. The standard identification assumption is parallel trends in the outcome in treated and non-treated units in the unobserved case of no treatment in the empirical period.

TABLE 2

Effect of mergers on years of education

	(1)	(2)	(3)	(4)	(5)	(6)
Panel (a): Average effect						
Merger	0.0987**	0.0792*	0.0823*	0.0913*	0.0404	0.0443
	(0.0453)	(0.0458)	(0.0471)	(0.0477)	(0.0432)	(0.0418)
Panel (b): Short- versus long-term effects						
Short-exposure effect	0.100**	0.125***	0.126***	0.126***	0.125***	0.129***
	(0.0489)	(0.0411)	(0.0417)	(0.0421)	(0.0437)	(0.0441)
Medium-exposure effect	0.0831	0.0915*	0.0913*	0.0885	0.0604	0.0615
	(0.0496)	(0.0535)	(0.0537)	(0.0558)	(0.0546)	(0.0545)
Long-exposure effect	0.0930	0.0867	0.0867	0.0850	0.0416	0.0437
	(0.0559)	(0.0656)	(0.0667)	(0.0696)	(0.0652)	(0.0638)
Premerger effect	−0.00785	0.0102	0.0145	0.0107	0.0411	0.0390
	(0.0693)	(0.0512)	(0.0533)	(0.0562)	(0.0565)	(0.0567)
R-squared	0.006	0.193	0.191	0.179	0.176	0.175
Observations	343,287	343,287	343,287	343,287	767,454	1,036,154
Time/age FE	Yes	Yes	Yes	Yes	Yes	Yes
Soc. char.	No	No	No	Yes	Yes	Yes
Local gov. FE	No	Yes (44)	No	No	No	No
School FE	No	No	Yes (358)	Yes (358)	Yes (920)	Yes (1,402)
Comparison group	Previous mergers	Previous mergers	Previous mergers	Previous mergers	Potential mergers	All local governments

Notes: Years of education is measured as degrees obtained by 2011. Standard errors clustered at the (merged) local government level in parentheses, ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. The comparison groups are 'Previous mergers' in columns (1)–(4), 'Potential mergers' in column (5) and 'All local governments' in column (6). Panel (a) shows the average treatment effect. Panel (b) shows the results of an event study that excludes the year before treatment, splits the treatment effect into short-term exposure (0–2 years) medium-term exposure (3–5 years) and long-term exposure (6–9 years) and includes a premerger effect (−10 to −2 years). Socioeconomic characteristics include birth month, gender, immigration status, parental education and parental employment status.

In addition to the main model, we conduct an event study analysis that shows the evolution of the outcomes relative to treatment. We define indicator variables for 3-year periods before and after treatment, leaving out the year prior to the merger.[13] Each estimate indicates how the outcome variable changes in the time interval relative to the excluded year. The three postmerger periods are labelled short-, medium- and long-term exposure to treatment in Tables 2 and 3. The event study investigates

[13]The event study model can be expressed as:

$$Y_{ist} = \alpha_t + \beta_s + \gamma_1 d_{st}^{Pre(10-8)}{}_{st} + \gamma_2 d_{st}^{Pre(7-5)} + \gamma_3 d_{st}^{Pre(4-2)} + \gamma_4 d_{st}^{Post(0-2)} + \gamma_5 d_{st}^{Post(3-5)} + \gamma_6 d_{st}^{Post(6-9)} + X'_{ist}\delta + \varepsilon_{ist}$$

The prevariables are dummy variables that take the value of one for treated observations 10 to 8 years, 7 to 5 years and 4 to 2 years prior to treatment. Similarly, the postvariables are dummy variables that take the value of 1 for observations 0 to 2 years, 3 to 5 years and 6 to 9 years after treatment. Using year intervals instead of yearly dummy variables is similar to the approach used by Salinas and Solé-Ollé (2018) and Bottan and Perez-Truglia (2015).

TABLE 3

Effect of mergers on income

	(1)	(2)	(3)	(4)	(5)	(6)
Panel (a): Average effect						
Merger	0.0432***	0.0455***	0.0460***	0.0436***	0.0296**	0.0206*
	(0.0142)	(0.0137)	(0.0132)	(0.0132)	(0.0123)	(0.0119)
Panel (b): Short- versus Long-term effects and premerger effect						
Short-exposure effect	0.0203	0.0190	0.0202	0.0141	0.0104	0.00757
	(0.0141)	(0.0144)	(0.0140)	(0.0141)	(0.0141)	(0.0140)
Medium-exposure effect	0.0299*	0.0309**	0.0307**	0.0319**	0.0239*	0.0183
	(0.0153)	(0.0153)	(0.0147)	(0.0145)	(0.0136)	(0.0133)
Long-exposure effect	0.0554**	0.0584**	0.0593**	0.0565**	0.0373	0.0263
	(0.0230)	(0.0233)	(0.0232)	(0.0231)	(0.0227)	(0.0217)
Premerger effect	−0.00948	−0.0117	−0.0118	−0.0120	−0.00686	−0.00384
	(0.0085)	(0.0081)	(0.0095)	(0.0097)	(0.0089)	(0.0086)
R-squared	0.055	0.055	0.053	0.109	0.108	0.107
Observations	323,847	323,847	323,847	323,847	724,561	981,126
Time/age FE	Yes	Yes	Yes	Yes	Yes	Yes
Soc. char.	No	No	No	Yes	Yes	Yes
Local gov. FE	No	Yes (44)	No	No	No	No
School FE	No	No	Yes (358)	Yes (358)	Yes (920)	Yes (1,402)
Comparison group	Previous mergers	Previous mergers	Previous mergers	Previous mergers	Potential mergers	All local governments

Notes: Income is measured as the log of average pension-qualifying income for the years 2009 and 2010. Standard errors clustered at the (merged) local government level in parentheses, ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. The comparison groups are 'Previous mergers' in columns (1)–(4), 'Potential mergers' in column (5) and 'All local governments' in column (6). Panel (a) shows the average treatment effect. Panel (b) shows the results of an event study that excludes the year before treatment, separates the treatment effect into short-term exposure (0–2 years) medium-term exposure (3–5 years) and long-term exposure (6–9 years) and includes a premerger effect (−10 to −2 years). Socioeconomic characteristics include birth month, gender, immigration status, parental education, and parental employment status.

whether it is reasonable that the parallel trends assumption holds by comparing the evolution of the outcomes before treatment in the treatment group relative to the comparison group. Significant pretreatment coefficients suggest that the parallel trend assumption might be violated.

V. Results

Main results

The results of estimating equation 1 are presented in Table 2 for years of education and 1 for income. Panel (a) estimates average effects, while panel (b) estimates nonlinear effects. The first four columns present the results for the 'Previous mergers' sample while the last two columns present the results for the alternative comparison groups 'Potential mergers' and 'All local governments', as described in section IV. Column (1) includes time/age fixed effects, column (2) adds local government fixed effects, column (3) instead adds school fixed effects and column (4) adds socioeconomic characteristics. Columns (5) and (6) have the same specification as column (4).

Panel (a) of Table 2 shows that the effect of local government mergers on years of education is significantly positive across all specifications, at least at the 10% level, but is small in economic magnitude. A merger increases education by about 0.1 years compared to the comparison group consisting of previously merged local governments.

For income, the results in panel (a) of Table 3 show that there is a positive average effect across model specifications. A merger significantly increases income by about 4% in the models using previous mergers as the comparison group. This effect is close to the return of an additional year of education in the Norwegian labour market (Barth and Roed, 1999) and is thus not trivial. Taken together, these results indicate that only a small part of the income effect can be explained through increased years of education among the treated students.[14]

Columns (5)–(6) in Tables 3 and 2 present the results for models using the alternative comparison groups. As these are very different from our preferred comparison group, we do not expect the results to be the same. However, results that point in the same direction will indicate robustness of our main results. For both education and income, the point estimates are smaller in the alternative models, and with a similar percentage-wise decrease for both of the alternative comparison groups. For income, effects are still significantly positive at conventional levels. The effects on income are 3% ($p < 0.05$) and 2% ($p < 0.10$) in the samples of potential mergers and all local governments respectively. For education, the effects using the alternative comparison groups are insignificant, which follows from the significance being lower than for income in the main specification.

Figure 2a,b present the results from the event study described in section IV. We observe that the point estimates prior to the merger are close to zero and not statistically different from zero, except for the first pretreatment period for income, which supports the common trends assumption. If there is an effect of the merger through schools, then we would expect the effect to be larger for later cohorts who have been in school for several years after the merger. For years of education, only the short-term exposure effect (0–2 years) is statistically significant, although the point estimates remain about the same for the medium (3–5 years) and long-term (6–9 years) exposure effects. For income, however, there is an exposure time effect. The estimates are also reported in panel (b) of Tables 2 and 3, where all pretreatment variables are

[14]Goodman-Bacon (2018) argues that the standard difference-in-differences (DID) estimator can be interpreted as a weighted combination of all possible two-group, two-period difference-in-differences estimators. In our case, this consists of the combination of three estimators: 'early- versus late-treated', 'late- versus early-treated' and 'treated versus never treated'. The weights are proportional to group sizes and the variance in the treatment dummy in each group. A method for empirically assessing the contribution of each of the components to the overall DID estimator is also provided (Goodman-Bacon, Goldring and Nichols, 2019). As the implementation of the decomposition method requires a balanced panel design, we aggregated the individual data to school level. The decomposition exercise shows that the treated versus never treated component is the main contributor (more than 95%) in our estimation, which comes as no surprise, since the sample sizes of the other groups are very small. Detailed results from the Goodman-Bacon decomposition are available upon request.

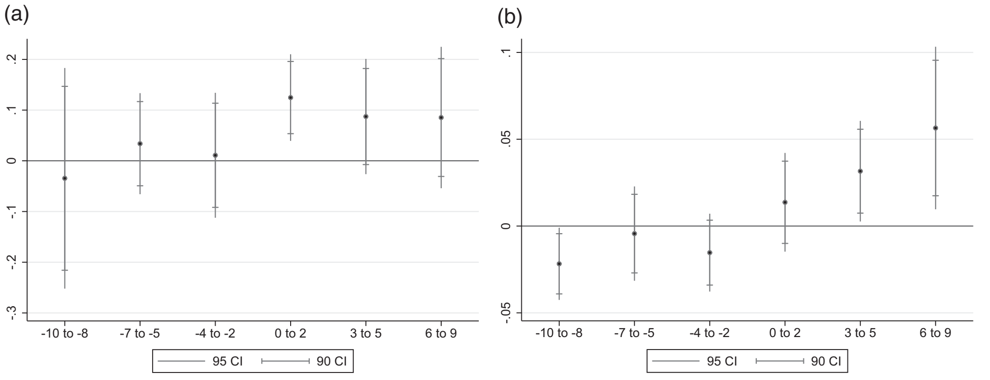

Figure 2. Event study analysis
Notes: Results of the event study analysis with 90% and 95% confidence intervals. Years of education is measured as degrees obtained by 2011. Income is measured as the log of average pension-qualifying income for the years 2009 and 2010. The *x*-axis indicates years relative to treatment year. 0 is the first year of treatment and −1 is the excluded year. Standard errors clustered at (merged) local government level. Estimation includes time/age fixed effects, school fixed effects and socioeconomic characteristics. Socioeconomic characteristics include birth month, gender, immigration status, parental education, and parental employment status. Estimates are also reported in panel (b) of Tables 2 and 3, where all pretreatment variables are combined into a premerger effect. 0–2, 3–5 and 6–9 are the short-, medium- and long-term exposure effects of treatment.

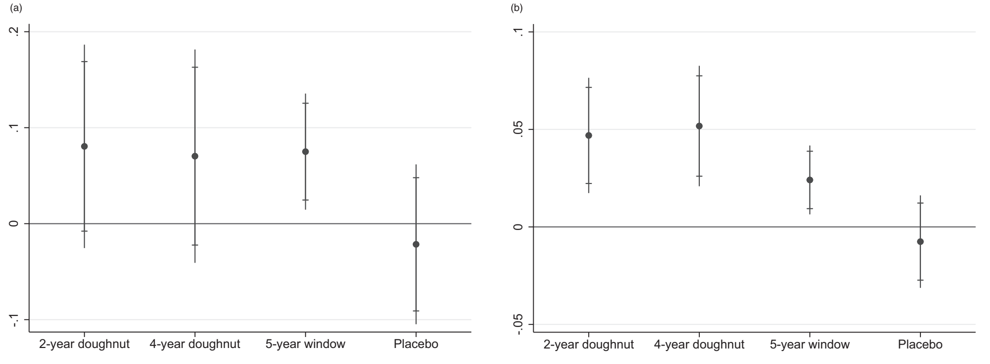

Figure 3. Robustness
Notes: Results of estimating equation (1) with 90% and 95% confidence intervals. Years of education is measured as degrees obtained by 2011. Income is measured as the log of average pension-qualifying income for the years 2009 and 2010. All regressions include time/age fixed effects, socioeconomic characteristics and school fixed effects. Standard errors clustered at (merged) local government level. Socioeconomic characteristics include birth month, gender, immigration status, parental education and parental employment status. The 2-year and 4-year doughnuts drop the 1+/− and 2+/− years surrounding the merger. The 5-year window reduces the estimation window to 5+/− years surrounding the merger. Placebo reform runs the specification as if the merger had occurred 4 years previously and only includes the years before the merger occurred. The sample corresponds to the 'Previous mergers' sample.

combined into a single premerger effect. The results for the alternative comparison groups for both pretreatment and post-treatment estimates reflect the results found for the 'Previous mergers' comparison group.

Robustness analyses

Figure 3a,b present the estimation results of various robustness analyses. All estimations include socioeconomic characteristics and school fixed effects and should be compared to column (4) of Tables 2 and 3.

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 an effect due to the merger. There might also be some anticipatory effects of the merger which would affect the cohorts leaving lower secondary education just before the merger. Excluding observations just around the time of the merger eliminates such concerns. The first row in the figures, the 2-year doughnut, presents the treatment effect when the last cohort not affected as well as the first cohort affected by the merger ($t - 1$ and t) are excluded from the sample, creating a 'doughnut hole'. The second row in the figures, the 4-year doughnut, presents the treatment effect when the doughnut hole is expanded to include four years ($t - 2$, $t - 1$, t and $t + 1$). For education, the magnitude of the effect is similar to that in the main model specification, but becomes insignificant. For income, results remain strongly significant for both specifications. The estimated effect is 4.7% for the 2-year doughnut and 5.2% for the 4-year doughnut.

Another potential issue is the length of the estimation window. In the main model specification in Tables 2 and 3, the estimation window covers cohorts up to 10 years before and after the reform. The third row in Figure 3a,b shows the treatment effect when the estimation window is reduced to +/−5 years. If there is an effect of the merger through schools, then we would expect the effect to be larger for later cohorts. For years of education, the result is very similar to the previous model specification, and significant at the 5% level. This corresponds to the results found for the short-term effects in Table 2. For income, the estimate is somewhat lower when the shorter window is used. The point estimate for income is reduced to 2.4%, but is still significant at the 1% level.

The last row presents the results of a placebo specification where we pretend that the merger happened 4 years prior to the actual merger. The estimation only includes premerger years for the treated local governments. A significant estimate in this specification would challenge our common trends assumption. The estimates for both income and years of education are insignificant and close to zero, reflecting the results found for the premerger effects in Tables 2 and 3. The pretreatment effects in panel (b) of Tables 2 and 3 and Figure 2a,b are consistent with the parallel trends assumption being satisfied.

Alternative outcomes

The main results show that there are positive effects of local government mergers on traditional long-run outcomes: educational attainment and income. In order to obtain a more nuanced understanding of the effects, this section considers a range of alternative educational, labour market and family-related individual outcomes.

We first investigate the effects on alternative educational outcomes. This is of interest because the effect on years of education can explain only a small part of the

TABLE 4

Effect of mergers on educational outcomes

	(1) Started academic track	(2) Completed upper secondary	(3) Completed upper secondary academic track	(4) Completed higher education	(5) Completed STEM education
Merger	0.0106	0.00669	0.0140	0.00804	0.00260
	(0.0153)	(0.0120)	(0.0172)	(0.0094)	(0.0023)
Observations	343,494	343,494	343,494	343,494	343,494
R-squared	0.154	0.099	0.147	0.160	0.043
No. schools	365	365	365	365	365

Notes: All regressions include time/age fixed effects, socioeconomic characteristics and school fixed effects. The sample corresponds to the 'Previous mergers' sample. Standard errors clustered at (merged) local government 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.

overall effect on income. Second, we investigate the effects on employment, migration and family-related outcomes. For descriptive statistics on the outcome variables in this section, see panels C and D of Table A2.

Alternative educational outcomes

Table 4 attempts to shed light on the level and type of education that are affected by the mergers. The outcome in column (1) is an indicator for whether the individual started the academic track in upper secondary education immediately after completing lower secondary education at age 16. The outcome in column (2) is an indicator for whether the individual completed upper secondary education (irrespective of study track) by the age of 21, while column (3) indicates whether the individual graduated from the academic track. In columns (5) and (6), the outcomes are indicators of whether the individual completed higher education or completed a higher education degree in a STEM field[15].

Although all estimates in Table 4 are positive and therefore in the same direction as years of education, none of the estimates are significant. Students affected by a merger are not significantly more likely to start the academic track, complete upper secondary education, complete higher education or complete an education in a STEM field. These decisions do not seem to be driving the income effect.

Alternative labour market and family outcomes

Table 5 presents the results of regressions with a range of labour market and family outcomes as dependent variables. These variables are typically correlated with income. The outcomes are whether the individual is registered as employed in 2011 (column 1), whether the individual is registered as married by 2010 (column 2), whether the individual has had at least one child by 2008 (column 3), whether the individual is

[15]STEM refers to the academic disciplines science, technology, engineering and mathematics. The Norwegian standards for education grouping (Statistics Norway, 2021) are used for the completed STEM education measure. The highest completed higher education degree has a NUS2000 field of study code equal to 5 for STEM education.

TABLE 5

Effect of mergers on labour market and family outcomes

	(1) Employed	(2) Married	(3) Children	(4) Single parent	(5) Welfare benefits	(6) Mover
Merger	0.0167***	−0.00796	−0.00561	−0.0245***	−0.00511*	0.0269**
	(0.0034)	(0.0061)	(0.0102)	(0.0061)	(0.0026)	(0.0108)
Observations	343,494	316,171	343,494	121,580	343,494	275,042
R-squared	0.010	0.127	0.216	0.046	0.014	0.024
No. schools	365	365	365	365	365	357

Notes: All regressions include time/age fixed effects, socioeconomic characteristics and school fixed effects. The sample corresponds to the 'Previous mergers' sample. Standard errors clustered at (merged) local government 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.

registered as being a single parent at least once between 1996 and 2008 (column 4), and whether the individual received welfare benefits in 2011 (column 5).

The last column in Table 5 investigates whether individuals are more likely to move after mergers. One hypothesis is that the combination of a positive effect on income and only a modest effect of years of education is a result of treated students being more likely to move. With higher mobility they are more likely to live in regions with higher income. We define movers as individuals registered as working in a different local government in 2011 than the postmerger local government in which the student completed compulsory schooling.

The results in Table 5 show that individuals affected by the merger are more likely to be employed, less likely to be single parents and less likely to receive welfare benefits. These results are in line with our findings for income. The result for 'employed' is especially interesting. The income effect appears to a large extent to be driven by individuals who are more likely to have a job. The last column shows that individuals affected by the mergers are more likely to move. As evident from panel D of Table A2, movers have higher salaries than non-movers, so this may also explain part of the income effect.

Heterogeneity analyses

So far, we have estimated the average effects on education, income and other labour market and family outcomes. However, the average effects may hide important effect heterogeneity across individuals with different characteristics as well as among students located in premerger city and surrounding local governments. This section provides an analysis of possible heterogeneity along these dimensions.

Heterogeneity by individual characteristics

Table 6 presents results when the sample is stratified by parental education level, parental employment status, gender and immigrant background. 'Parental education high' is defined as at least one parent having a higher education degree, while 'Parental education low' is defined as none of the parents having such a degree.

TABLE 6

Individual level heterogeneity

	(1) Parental educ. high	(2) Parental educ. low	(3) Girls	(4) Boys	(5) Immigrant	(6) Non-immigrant	(7) Both parents working	(8) One or no parent working
Years of education								
Merger	−0.0139	0.112*	0.111**	0.0569	0.260	0.0777	0.0847**	0.0874*
	(0.0567)	(0.0574)	(0.0476)	(0.0521)	(0.3255)	(0.0490)	(0.0389)	(0.0502)
Observations	103,986	239,301	168,552	174,735	4,873	318,974	105,362	218,485
R-squared	0.004	0.004	0.012	0.004	0.041	0.052	0.076	0.045
No. schools	346	364	354	362	264	357	348	358
Income								
Merger	0.0419***	0.0369***	0.0451***	0.0431**	0.257**	0.0436***	0.0410**	0.0488***
	(0.0120)	(0.0116)	(0.0116)	(0.0169)	(0.1031)	(0.0136)	(0.0176)	(0.0136)
Observations	98,190	225,657	159,025	164,822	4,873	318,974	105,362	218,485
R-squared	0.105	0.035	0.040	0.074	0.041	0.052	0.076	0.045
No. schools	342	358	351	357	264	357	348	358

Notes: Years of education is measured as degrees obtained by 2011. Income is measured as the log of average pension-qualifying income for the years 2009 and 2010. All regressions include time/age fixed effects and school fixed effects. The sample corresponds to the 'Previous mergers' sample and should be compared to Panel (a), column (3) of Tables 2 and 3. Standard errors clustered at (merged) local government level in parentheses, $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

The effect for years of education is larger for girls and for students with low-educated parents. However, neither of the differences across sub-samples is statistically different at conventional levels. For income, it is evident that there are no significant differences in the effect of mergers between students with respect to parental education, parental employment status and gender. The income effect is positive for both immigrants and non-immigrants, although the estimate is much larger for immigrants (the difference is significant at the 10% level). The analysis provides some indication that the effect of years of education is driven to a large extent by girls and students with low-educated parents, while the income effect holds for all subgroups. The results also indicate that mergers had large positive effects on immigrants.

Heterogeneity by premerger school location

A unique feature of our data set is that we can distinguish between schools in the cities and schools in the surrounding local governments both before and after the merger. One hypothesis is that the strong resistance in the premerger surrounding local governments before the mergers was implemented reflected a real concern that mergers would reduce the quality of public services, including education, in these local governments. However, a completely different hypothesis is that mergers would improve school quality in the surrounding governments through the benefit of being part of a city local government with a high capacity leading to efficient school administration. Table 7 displays the results using two comparison groups: previous mergers and potential mergers. Columns (1) and (4) repeat the previous results of Tables 2 and 3 for readability.

In columns (2) and (3), treated city and surrounding schools are compared to all schools in the 'Previous mergers' comparison group. For students in schools in the premerger city local governments, the point estimates are small (−0.01 for years of education and 0.9% for income) and far from statistically significant. For students in schools from the premerger surrounding local governments, the point estimates are significant at the 1% level. The effect on years of education is 0.14 while the effect on income is 6%. Clearly, the main results are driven by students in schools located in the local governments surrounding the city local governments before the mergers.

A benefit of using the 'Potential mergers' sample is that we can distinguish between city and surrounding local governments in the comparison group as well as in the treatment group. In column (5) treated city schools are compared with non-treated city schools in the comparison group and in column (6) treated surrounding schools are compared to non-treated surrounding schools in the comparison group. Again, for the cities the point estimates for the outcome variables are small (−0.04 for years of education and 0.7% for income) and statistically insignificant, while the point estimates are positive for surrounding local governments. The effect for years of education is 0.06 and less precisely estimated (t-value of 1.58) while the effect on income is 3% and statistically significant at a 1% level.

Even though the effects are smaller than in the models that use previous mergers as the comparison group, and smaller than the benchmark model in Table 3, the results in this section clearly show that the average effects are driven by students attending schools in surrounding local governments. This is consistent with the hypothesis that

TABLE 7

Effect of mergers by premerger school location

	(1) All schools	(2) City schools	(3) Surrounding schools	(4) All schools	(5) City schools	(6) Surrounding schools
Years of education						
Merger	0.0913*	−0.0138	0.140***	0.0404	−0.0438	0.0584
	(0.0476)	(0.0596)	(0.0448)	(0.0432)	(0.0601)	(0.0384)
Observations	343,287	302,596	324,297	767,454	436,543	330,865
R-squared	0.179	0.180	0.178	0.176	0.177	0.174
No. schools	365	318	346	929	469	462
Sample	Previous mergers			Potential mergers		
Income						
Merger	0.0436***	0.00943	0.0593***	0.0296**	0.00767	0.0292***
	(0.0132)	(0.0220)	(0.0118)	(0.0123)	(0.0218)	(0.0104)
Observations	323,847	285,336	306,073	724,561	410,230	314,291
R-squared	0.109	0.110	0.110	0.108	0.107	0.109
No. schools	358	313	340	920	462	461
Sample	Previous mergers			Potential mergers		

Notes: Years of education is measured as degrees obtained by 2011. Income is measured as the log of average pension-qualifying income for the years 2009 and 2010. All regressions include time/age fixed effects, socioeconomic characteristics and school fixed effects. The sample in columns (1)–(3) corresponds to the 'Previous mergers' sample. In columns (2) and (3), treated city and surrounding schools are compared to all schools in the comparison group. The sample in columns (4)–(6) corresponds to the 'Potential mergers' sample. In columns (5), treated city schools are compared to non-treated city schools in the comparison group. In column (6), treated surrounding schools are compared to non-treated surrounding schools in the comparison group. For both samples, 40/46 observations are dropped as school identity only exists in post-treated municipalities and city/surrounding school status can therefore not be verified. Standard errors clustered at (merged) local government 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.

schools in the surrounding local governments benefited from being part of a city government with high capacity for providing efficient school administration. However, other types of data not available to us are needed to confirm this interpretation.

VI. Alternative mechanisms

In this section, we investigate alternative mechanisms behind the results. In particular we consider the extent to which the effect on individual student outcomes might be related to changes in resource allocation across schools as well as among sectors within local governments, or systematic changes in demographic composition and teacher quality.

School level resources

Available information on resources at school level is restricted to the average class size. Although recent evidence in Falch *et al.* (2017) shows that class size does not affect long-run educational and earnings outcomes in Norway, it is nevertheless of interest to investigate whether the average effects, as well as the divergent effects in

TABLE 8

Effect of mergers on class size

	(1) All schools	(2) City schools	(3) Surrounding schools	(4) All schools	(5) City schools	(6) Surrounding schools
Merger	−0.0759	−0.711	0.182	−0.0536	−0.968	0.431
	(0.4138)	(0.5723)	(0.5513)	(0.3849)	(0.6386)	(0.5124)
Observations	4,174	3,683	3,944	10,891	5,307	5,584
R-squared	0.162	0.166	0.168	0.172	0.138	0.183
Sample	Previous mergers			Potential mergers		

Notes: All regressions include year fixed effects and yearly average socioeconomic characteristics at school level. Standard errors clustered at (merged) local government 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. The sample in columns (1)–(3) corresponds to the 'Previous mergers' sample. In columns (2) and (3), treated city and surrounding schools are compared to all schools in the comparison group. The sample in columns (4)–(6) corresponds to the 'Potential mergers' sample. In column (5), treated city schools are compared to non-treated city schools in the comparison group. In column (6), treated surrounding schools are compared to non-treated surrounding schools in the comparison group.

premerger city and surrounding local governments, can be explained by systematic changes in class size due to the mergers. If the earnings and employment effects could be explained by class size, we would expect a reduction in class size in schools located in merged governments as well as a reduction in class size in premerger surrounding schools and not in premerger city schools.

Our measure for class size is the same as that in Falch *et al*. (2017). Table 8 displays the results where estimations include one observation per school and year. As in section 'Heterogeneity analyses', the previous merger sample compares city and surrounding schools to all other schools, while the potential merger sample compares treated city schools to comparison city schools and treated surrounding schools to comparison surrounding schools. See panel E of Table A2 for descriptive statistics on class size.

We find no statistically significant effects of mergers on class size, and this holds for schools in both cities and surrounding areas. The point estimates in Table 8 are small. In addition, the point estimates are negative in city schools and positive in surrounding schools and thus the opposite to what might be expected if changes in class size were to explain the different income and education effects found in the two types of geographical areas. The largest point estimate in absolute terms is found in column (5), which implies that a merger reduces class size by 0.9 students in the cities.

Local government expenditure, demographics and teacher quality

Next, we investigate whether variables at the local government level are affected by mergers. Since these outcomes cannot be disaggregated to the pre-existing local governments for the postmerger period, we use the local government after a merger has taken place as the unit of observation. This means that all variables in premerger local governments are aggregated to this level and measured yearly. The small number of treated units should therefore be kept in mind when interpreting the results in this section.

One hypothesis is that the total budget or the share of the budget allocated to compulsory education increased in the merged local governments. Another hypothesis is that the merged local governments were able to attract teachers of higher quality than the comparison local governments. A final hypothesis is that the total population, the school-age population and the number of schools, changed systematically in a different way in merged and comparison local governments.

Data on expenditure are extracted from official statistics on local government accounts from Statistics Norway, while the numbers of schools and 16-year olds are constructed from our own register data. We use the share of certified teachers as an indicator of teacher quality, a measure previously used by Bonesrønning, Falch and Strøm (2005) and Falch, Johansen and Strøm (2009). See panel F of Table A2 for descriptive statistics for these variables.

In Table 9, the first four columns display outcomes related to log expenditures. Panel (a) presents results for models without fixed effects, while Panel (b) includes local government fixed effects, which is the unit of observation. The outcomes are total local government expenditure (column 1), total per capita expenditure (column 2), total school expenditure (column 3) and total school expenditure per student (column 4). All estimated effects are small, and none of the estimates are significantly different from

TABLE 9

Effect of mergers on local government expenditure, teacher quality and school consolidations

	(1) Total exp. (log)	*(2) Per capita total exp. (log)*	*(3) School exp. (log)*	*(4) Per student school exp. (log)*	*(5) Share of teachers w/o teacher certification*	*(6) Lower secondary schools (log)*
Panel (a)						
Merger	0.125	−0.0178	0.0993	−0.0372	−0.00775	0.0964
	(0.0967)	(0.0184)	(0.0808)	(0.0275)	(0.0085)	(0.1034)
Treated	0.295	0.00902	0.255	0.0114	−0.00250	0.162
	(0.1868)	(0.0385)	(0.1876)	(0.0357)	(0.0109)	(0.2265)
R-squared	0.470	0.875	0.402	0.663	0.274	0.272
Loc. gov. FE	No	No	No	No	No	No
Panel (b)						
Merger	0.003	−0.012	−0.006	−0.019	−0.008	−0.027
	(0.0137)	(0.0134)	(0.0325)	(0.0283)	(0.0077)	(0.0327)
R-squared	0.971	0.965	0.808	0.831	0.183	0.165
Loc. gov. FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	820	820	820	820	820	820

Notes: The sample corresponds to the 'Previous mergers' sample. Estimations include yearly observations for each (merged) local government, average socioeconomic characteristics at local government level and time/age fixed effects. Estimations in panel (b) also include local government fixed effects. Standard errors clustered at the (merged) local government 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.

zero. For example, the point estimate in column (2) indicates that local government per capita expenditure decreased by 1.8% and the point estimate in column (4) indicates that school expenditure per student decreased by 3.7%. This suggests that changes in the total budget or in the allocation of the budget among local government sectors cannot explain the main results obtained above.

In column (5), the outcome is the share of teachers without teacher certification according to the Norwegian rules. It is not immediately obvious that teacher certification really matters for educational achievement. However, local governments in Norway are allowed to fill vacant teacher positions with persons without teacher certification only if there is no certified teacher among the job applicants. Thus, as also argued in Bonesrønning *et al*. (2005) and Falch *et al*. (2009), it is reasonable to believe that local governments that are unattractive to teachers have a higher share of non-certified teachers. The estimated merger effect on this variable is small and statistically insignificant, indicating that our results do not seem to be driven by increased teacher quality in merged local governments.

The last column in Table 9 investigates the effect on the number of lower secondary schools. Mergers might have led to school consolidations, and larger schools might improve school quality. The effect has the expected negative sign, but is small. The point estimate indicates a reduction of 9.6%. Given that the average number of schools is 5.6 in the treated postmerger local governments, the point estimate implies that the merger led to one school closing for every second merger. However, the effect is clearly insignificant (*t*-value of 0.93).

Lastly, we investigate changes in different measures of the population as a result of mergers. In Table 10 the dependent variables are the log of total population (column 1), the size of the school-age population (column 2) and the number of 16-year olds (column 3). All estimates are small and insignificant. For example, the point estimate in column (1) indicates that a merger increases the total population by 14%, but with a *t*-value of 1.49. Overall, there is no evidence of systematic demographic changes resulting from the mergers.

VII. Conclusion

The optimal size of local political jurisdictions and the potential effects of mergers are important issues in the debate on public sector productivity. However, limited empirical evidence exists on the effect of mergers on productivity in key public services. We exploit spatial and temporal variation originating from central government enforced mergers between a city and surrounding local governments in Norway in the 1980s and 1990s to provide evidence of the effects of mergers on students' long-term outcomes. Using a difference-in-differences strategy, we estimate that a merger increased income in adulthood by approximately 4%, while the effect on educational attainment measured by years of education is generally positive and around 0.1. Moreover, we find that the mergers increased the probability of employment and worker mobility. These average effects on outcomes in adulthood hide important heterogeneity. We show that the positive effects are driven by students attending schools in premerger local governments surrounding the cities, despite the fact that

TABLE 10

Effect of mergers on local government demographics

	(1) Total population (log)	(2) School-age population (log)	(3) 16-year-olds (log)
Panel (a)			
Merger	0.142	0.136	0.0943
	(0.0950)	(0.0919)	(0.0974)
Treated	0.286	0.243	0.284
	(0.2024)	(0.2011)	(0.1976)
R-squared	0.430	0.408	0.399
Lo. gov FE	No	No	No
Panel (b)			
Merger	0.015	0.013	−0.030
	(0.0136)	(0.0261)	(0.0362)
R-squared	0.498	0.780	0.601
Loc. gov FE	Yes	Yes	Yes
Observations	820	820	820

Notes: The sample corresponds to the 'Previous mergers' sample. Estimations cover yearly observations for each (merged) local government, average socioeconomic characteristics at municipality level and time/age fixed effects. Estimations in Panel (b) also include local government fixed effects. Standard errors clustered at local government 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.

mergers were very unpopular among politicians and inhabitants in these surrounding governments prior to the mergers. This suggests that enforced mergers by central governments can improve efficiency by overcoming local coordination problems.

While it is always difficult to generalize from evidence based on specific policy interventions in specific countries, the results in the paper lend support to the view that local government mergers can actually increase the productivity of schools. The external validity is, however, likely restricted to specific aspects of the Norwegian institutional system and the characteristics of the mergers; local governments with multi-purpose responsibilities, populations of 3,000 to 50,000, and cities that expand into neighbouring local governments.

What can explain these findings? We show that students attending schools in the treated local governments prior to the merger did not experience better outcomes. This indicates that the effect on income is not the result of some general improvement in labour market conditions generated by the merger, but rather suggests improved school quality. The combination of substantial income effects and smaller and more imprecisely estimated effects on educational attainment further suggests that mergers may improve non-cognitive skills. However, the lack of data on cognitive and non-cognitive skills implies that we are not able to confirm this interpretation directly.

As we do not find that mergers affect class size or other measures of expenditure, the positive effect cannot be explained by monetary priorities. The results therefore support the broader hypothesis that students attending schools in areas surrounding premerger cities benefited from existing school administrative and leadership competencies in the former city local governments, particularly if these competencies were able to improve students' non-cognitive skills.

Appendix

TABLE A1

Descriptive statistics

	Treated			*Comparison previous mergers*		
	Mean	*(SD)*	N	*Mean*	*(SD)*	N
A. Outcome variables						
Log of income 2009–10	12.7	(0.75)	56,245	12.7	(0.79)	267,600
Years of education	14	(2.55)	59,635	14.1	(2.58)	283,652
B. Socioeconomic characteristics						
Girl	0.49	(0.50)	59,635	0.49	(0.50)	283,652
Mother's education: Upper secondary school	0.49	(0.50)	59,635	0.48	(0.50)	283,652
Mother's education: Bachelor's degree	0.16	(0.37)	59,635	0.17	(0.38)	283,652
Mother's education: Master's degree +	0.010	(0.10)	59,635	0.017	(0.13)	283,652
Mother's education: Unknown	0.0069	(0.083)	59,635	0.0095	(0.097)	283,652
Father's education: Upper secondary	0.52	(0.50)	59,635	0.50	(0.50)	283,652
Father's education: Bachelor's	0.15	(0.35)	59,635	0.15	(0.36)	283,652
Father's education: Master's +	0.064	(0.24)	59,635	0.088	(0.28)	283,652
Father's education: Unknown	0.021	(0.14)	59,635	0.023	(0.15)	283,652
First generation immigrant	0.009	(0.09)	59,635	0.013	(0.11)	283,652
Second generation immigrant	0.004	(0.06)	59,635	0.0044	(0.066)	283,652
Only mother working	0.17	(0.37)	59,635	0.17	(0.37)	283,652
Only father working	0.16	(0.37)	59,635	0.15	(0.36)	283,652
Both parents working	0.31	(0.46)	59,635	0.32	(0.47)	283,652
Birth month	6.26	(3.33)	59,635	6.35	(3.33)	283,652

Notes: Descriptive statistics for the estimation sample for years of education. Treated covers all individuals from local governments that underwent a merger. Comparison previous mergers covers city and surrounding local governments that merged in the 1960s.

TABLE A2

Descriptive statistics

	Treated			*Comparison previous mergers*		
	Mean	*(SD)*	N	*Mean*	*(SD)*	N
C. Other educational outcome variables						
Started academic track	0.45	(0.50)	59,635	0.46	(0.50)	283,652
Graduated upper secondary	0.70	(0.46)	59,635	0.70	(0.46)	283,652
Completed upper secondary from academic track	0.42	(0.49)	59,635	0.46	(0.50)	283,652
STEM education	0.072	(0.26)	59,635	0.076	(0.27)	283,652
D. Other labour and family outcome variables						
Employed	0.79	(0.41)	59,635	0.80	(0.40)	283,652
Married	0.48	(0.50)	55,266	0.44	(0.50)	260,904
Children	0.24	(0.43)	59,635	0.23	(0.42)	283,652
Single parent	0.29	(0.45)	21,403	0.29	(0.46)	100,177
Social assistance	0.032	(0.18)	59,635	0.032	(0.17)	283,652
Mover	0.564	(0.50)	47,256	0.496	(0.50)	227,785
Income mover	12.9	(0.56)	26,584	12.9	(0.62)	112,567

(*Continued*)

TABLE A2

(*Continued*)

	Treated			*Comparison previous mergers*		
	Mean	*(SD)*	N	*Mean*	*(SD)*	N
Income non-mover	12.8	(0.50)	20,544	12.8	(0.55)	114,412
E. School characteristics						
Class size	25.23	(2.71)	57,415	25.42	(3.09)	258,759
F. Local government characteristics						
Total population (log)	10.3	(0.55)	136	9.99	(0.84)	684
School-age population (log)	8.15	(0.54)	136	7.88	(0.82)	684
16-year olds (log)	5.95	(0.56)	136	5.65	(0.81)	684
Total expenditure (log)	20.2	(0.53)	136	19.8	(0.87)	684
Total per capita expenditure (log)	9.85	(0.28)	136	9.85	(0.28)	684
School expenditure (log)	18.8	(0.47)	136	18.6	(0.78)	684
Per student school expenditures (log)	10.7	(0.20)	136	10.7	(0.18)	684
Share of teachers without teacher certification	0.026	(0.04)	136	0.034	(0.04)	684
Lower secondary schools (log)	1.66	(0.40)	136	1.41	(0.76)	684

Notes: Descriptive statistics for the estimation sample for years of education. Treated covers all individuals from local governments that underwent a merger. Comparison previous mergers covers city and surrounding local governments that merged in the 1960s. All local government characteristics, except share of teachers without teacher certification, are measured as logs.

TABLE A3

Descriptive statistics – alternative samples

	Treated			*Comparison potential mergers*			*Comparison all local gov.*		
	Mean	*(SD)*	N	*Mean*	*(SD)*	N	*Mean*	*(SD)*	N
A. Outcome variables									
Log of income 2009–10	12.7	(0.75)	56,245	12.7	(0.79)	668,313	12.7	(0.77)	924,876
Years of education	14	(2.55)	59,635	14	(2.57)	707,819	13.9	(2.54)	976,519
B. Socioeconomic characteristics									
Girl	0.49	(0.50)	59,635	0.49	(0.50)	707,819	0.49	(0.50)	976,519
Mother's education: Upper secondary	0.49	(0.50)	59,635	0.48	(0.50)	707,819	0.48	(0.50)	976,519
Mother's education: Bachelor's degree	0.16	(0.37)	59,635	0.17	(0.37)	707,819	0.16	(0.37)	976,519
Mother's education: Master's +	0.010	(0.10)	59,635	0.017	(0.13)	707,819	0.015	(0.12)	976,519
Mother's education: Unknown	0.007	(0.08)	59,635	0.012	(0.11)	707,819	0.010	(0.10)	976,519
Father's education: Upper secondary	0.52	(0.50)	59,635	0.49	(0.50)	707,819	0.49	(0.50)	976,519
Father's education: Bachelor's	0.15	(0.35)	59,635	0.15	(0.35)	707,819	0.14	(0.34)	976,519
Father's education: Master's +	0.064	(0.24)	59,635	0.082	(0.27)	707,819	0.073	(0.26)	976,519

(*Continued*)

TABLE A3

(*Continued*)

	Treated			*Comparison potential mergers*			*Comparison all local gov.*		
	Mean	*(SD)*	N	*Mean*	*(SD)*	N	*Mean*	*(SD)*	N
Father's education: Unknown	0.021	(0.14)	59,635	0.025	(0.16)	707,819	0.023	(0.15)	976,519
First-generation immigrant	0.009	(0.09)	59,635	0.015	(0.12)	707,819	0.013	(0.11)	976,519
Second-generation immigrant	0.004	(0.06)	59,635	0.008	(0.09)	707,819	0.006	(0.08)	976,519
Only mother working	0.17	(0.37)	59,635	0.17	(0.37)	707,819	0.17	(0.37)	976,519
Only father working	0.16	(0.37)	59,635	0.15	(0.36)	707,819	0.15	(0.35)	976,519
Both parents working	0.31	(0.46)	59,635	0.33	(0.47)	707,819	0.33	(0.47)	976,519
Birth month	6.26	(3.33)	59,635	6.35	(3.33)	707,819	6.35	(3.33)	976,519

Notes: Descriptive statistics for the estimation sample for years of education. Treated covers all individuals from local governments that underwent a merger. Comparison potential mergers includes all non-merged city local governments and their bordering local governments in 1987. Comparison all local governments covers all non-merged local governments.

TABLE A4

Data reduction

	Previous mergers		*Potential mergers*		*All local gov.*	
	Obs.	*Reduc.*	*Obs.*	*% Reduc.*	*Obs.*	*% Reduc.*
1. Sample 1981–2000 (without 1990)	373,132		825,733		1,109,469	
2. 16 years old when completed lower secondaryschool	351,459	5.81%	777,043	5.90%	1,046,188	5.70%
3. 10 +/− years around merger	343,562	2.25%	768,072	1.15%	1,036,919	0.89%
5. Non-missing years of education	343,287	0.08%	767,454	0.08%	1,036,154	0.07%
5. Non missing log of income	323,847	5.74%	724,561	5.66%	981,126	5.38%

Notes: School identifier data are missing for 1990. 19,715, 43,511 and 55,793 observations have zero income for previous mergers, potential mergers and all local governments, respectively, and are excluded from the analysis as we use the logarithmic value of income.

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