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Abstract

We study the effects of municipal mergers using novel geocoded data on public sector jobs and local politicians' place of residence. We find that municipal fiscal outcomes evolve in the same way on average in the merged municipalities as in the control group of hypothetical mergers that did not take place. However, these findings hide substantial heterogeneity within the mergers. The mergers led to highly unequal political representation between the merging municipalities. The municipalities that were politically marginalized in the post-merger council experience a substantial reduction in local public jobs in administration and health and social care sectors relative to the municipalities that were more strongly represented.

Key words: Difference-in-differences, municipal mergers, political representation

JEL classes: C23, D72, H72, H75, H77

1 Introduction

Policy-makers often see municipal mergers as an effective way of realizing economies of scale in municipal service provision. Other perceived benefits from larger local jurisdictions include the internalization of interjurisdictional spillovers and an increased fiscal capacity to sustain spending or revenue shocks. With these goals mind, municipal merger reforms have been implemented over time in a vast number of countries. For example, Blom-Hansen et al. (2016) report that since the 1950's extensive merger reforms have taken place in 28 developed countries.

The optimal size of local jurisdictions has also been on the research agenda in economics and political science for decades. The take-away from this literature is more nuanced than that of the policy-makers. It is now well understood that the optimal size involves trade-offs as larger size may come with a number of offsetting costs. First, some interjurisdictional spillovers, such as yardstick competition, may actually be beneficial (Besley and Case 1995; Grossman et al. 2017). Second, multi-purpose jurisdictions, such as municipalities, produce many different public services and the optimal jurisdiction size varies across these different services (Miceli 1993). Increasing municipality size may facilitate economies of scale in one area, but at the cost of another. Moreover, service production takes place at the site level within the municipalities and realizing scale economies would require merging the production units, such as schools and health care centers, along with the municipalities (Blom-Hansen et al. 2016).

Finally, as the size of the jurisdiction increases, the regional heterogeneity of the population increases as well, making it more difficult to tailor local services to match the preferences of the citizens (Alesina and Spolaore 1997; Ellingsen 1998). The preference heterogeneity may be particularly important in municipal mergers due to sorting in the way described in Tiebout (1956). This concern is also closely tied to local politics. The regional political power may shift considerably due to a merger as some regions may become politically marginalized in the post-merger decision-making body. This shift may also have detrimental effects on the functioning of local democracy and voter participation, which may further exacerbate the uneven distribution of political power and municipal services within a merger (Verba and Nie 1972; Dahl and Tufte 1973; Oliver 2000; Treisman 2007; Lassen and Serritzlew 2011; Saarimaa and Tukiainen 2016). Earlier literature also suggests that political considerations reflecting regional heterogeneity have played a major role in the merger

reforms in different countries (Bhatti and Hansen 2011; Saarimaa and Tukiainen 2014; Hyytinen et al. 2014 and Bruns et al. 2015).

We contribute to the small, but rapidly growing literature on the effects of municipal mergers (e.g. Reingewertz 2012; Allers and Geertsema 2016; Blom-Hansen et al. 2014 and Blom-Hansen et al. 2016) using data from a merger reform in Finland. The novel feature of our analysis is that we can use pre-merger municipality level data to analyze within-merger heterogeneity in the merger effects with respect to geographic political representation. We have access to detailed geocoded micro data (250 m x 250 m grids covering whole of Finland) on the location and number of jobs in public administration, schooling and health and social care. These data enable us to follow these outcomes at the pre-merger municipality level even when the official municipal statistics on municipal expenditures are produced only at the post-merger municipality level after merging. To our knowledge, our study is the first to use data directly related to municipal service production at a more disaggregated level than the post-merger municipalities.[1]

This level of data aggregation also allows us to dig deeper into the political determinants of merger effects. In particular, we have data on the exact residential location of municipal councilors, which allows us to study how the merger effects depend on the political representation of pre-merger municipalities in the new post-merger municipal council. While the relationship between representation in legislatures and the geographic distribution of public funds has received substantial attention in prior literature (e.g. Ansolabehere et al. 2002; Knight 2008; Berry et al. 2010; Albouy 2013; Hodler and Raschky 2014 and Fiva and Halse 2016), the issue has eluded the research on municipal mergers. This is an important gap in the literature given that mergers typically have large impacts on the representation of different geographic voter groups (Saarimaa and Tukiainen 2016).

The analysis of municipal merger effects comes with a number of methodological challenges. The Finnish mergers were *voluntarily* decided by municipality councils at the local level. This of course makes causal inference challenging due to possible selection bias. However, the voluntary nature of the mergers is a blessing in disguise because it allows us to construct a sensible control group. Often in the case of *forced* mergers the central government imposes a minimum population threshold that all municipalities need to fulfill through

[1] Egger et al. (2017) use night-light data to analyze changes in the overall economic activity within German mergers.

merging.[2] While it is in some sense true that forcing *all* municipalities below a given threshold to merge rules out selection bias, it does so by effectively washing away the entire relevant control group. In this case, the treatment group (small municipalities that were forced to merge) is very different from the control group (large municipalities that did not have to merge) by construction.

To address the endogeneity issue, we combine nearest neighbor matching with difference-in-differences (DID) methods. While these are well-established methods for causal inference, they need to be adjusted for the merger analysis because there are at least two partners in each merger (in our case up to ten partners). The causal effect we are after is the difference in the development of the outcome of interest when a group of municipalities merge and the counterfactual when they do not. This means that the relevant comparison should be made between a group of municipalities that merged (treatment group) and a group with the same number of municipalities with similar characteristics that did not merge (control group).

We construct this control group by first simulating all possible mergers involving up to ten municipalities (ten being the largest actual merger) that could have taken place according to the pre-merger municipality borders. We constrain these hypothetical mergers so that (i) we only allow mergers to take place between adjacent municipalities, which guarantees that the hypothetical new municipality is geographically contiguous, (ii) we allow mergers to take place only within county borders because none of the actual mergers crossed these borders, and (iii) we omit those hypothetical mergers that included at least one municipality that actually underwent a merger. We then build the control group from this universe of hypothetical mergers using nonparametric nearest neighbor matching based on merger level characteristic measured before the mergers took place. We use this control group also in the pre-merger municipality level analysis where we decompose the mergers in the control group to the municipality level.

Our findings can be summarized as follows. After merging, total municipal expenditures evolve in the same way on average in merged municipalities as in the control group of hypothetical mergers that did not take place. These zero findings are consistent with

[2] For example, in the Danish 2007 reform analyzed by Blom-Hansen et al. (2014) and Blom-Hansen et al. (2016) basically all municipalities with a population below 20,000 merged (compliance rate was 98 percent). Similar population thresholds set by the central government were used during the Swedish (Hinnerich 2009) and the German (Blesse and Baskaran 2016) merger reforms among others.

prior evidence concerning voluntary mergers (Moisio and Uusitalo 2013; Allers and Geertsema 2016; Baskaran and Blesse 2016). However, these merger level findings hide substantial heterogeneity within the mergers. In particular, while nothing happened to the number of jobs in schooling, the number of local public jobs in administration and in the health and social care sector decrease substantially in the politically marginalized (pre-merger level) municipalities relative to those municipalities that were more strongly represented in the post-merger municipal council. In fact, the jobs in the latter two sectors increased somewhat in these politically stronger municipalities. We also detect divergence in house prices between the politically marginalized municipalities and the municipalities with more representation, which suggests that the quality of the service-tax bundle deteriorated in the smaller merger partners. However, we lack reliable data on the development house prices in some of the smallest merged municipalities due to low number of transactions. This means that these results are less reliable than the results concerning the number of local public sector jobs and should be interpreted cautiously.

We subject the results to a number of validity checks, including placebo tests. Most importantly, we are able to show long common pre-treatment trends for the control and the merger groups and also within the merger group based on political representation for our main outcomes of interest. This offers strong, although indirect, support for the common trends assumption, which is crucial for a causal interpretation of the DID results. We also show that in the small municipalities that did not merge the number of local public jobs do not exhibit similar changes as in the small municipalities that merged and that these groups had common pre-treatment trends in these outcomes. This suggests that the heterogeneous effects within the mergers can be attributed to mergers and to differences in local political representation.

Compared to prior literature, our results paint a much more nuanced picture of what happens to municipal services when municipal boundaries are redrawn and which voter groups are affected the most. The results suggest that local political representation and the details of the local electoral system should have a larger role in the planning and evaluation of major merger reforms. We elaborate on the implications of our findings in more detail in the conclusions.

2 Institutional background

2.1 Finnish municipalities

Municipal tasks: Finland has a two-tier system of government consisting of the central government and municipalities as the local level.[3] Municipalities have extensive tasks and fiscal autonomy. In addition to local public goods and services, municipalities are responsible for providing most of social and health care services along with primary and secondary schooling. This makes municipalities of considerable importance to the whole economy. The GDP share of municipality spending is roughly 18 percent and they employ around 20 percent of the total workforce.

Revenue sources: The most important revenue sources are local taxes and operating revenues, such as fees. The most important tax instrument is the local income tax. The tax rate is flat and the municipalities can set the level freely. The property tax is of much less importance and municipalities can set the property tax rates only within limits set by the central government. The corporate income tax is a state level tax, but municipalities receive a share of the tax revenue based on profits and employment of firms within their borders. In 2012, the average share of the income tax of total revenue was 46 percent, while the shares of the property and corporate taxes were only 3 percent, respectively.

Regional tax base and cost disparities are offset by a central government grant system. The system is based on estimates of average costs and tax bases so that municipalities have very limited possibilities to influence the amount of grants that they receive. The grant system covers about 20 percent of total municipal revenues, but this share varies considerably from one municipality to another.

Municipal politics: Finland has a proportional representation (PR) system with eight parties that dominate national and municipal politics. Municipal councils are the main seat of power in the municipal decision making. The council term lasts for four years starting from January after the elections. Only permanent residents of a municipality can vote or run for a council seat. During our analysis period, the council size was a step function of population and varied between 13 and 85.[4] The merged municipalities were allowed to have a larger council size than the law dictated for the first post-merger election term. Each municipality

[3] Technically, there is a middle-tier consisting of counties. However, counties do not have major tasks and do not have elected officials.
[4] In 2017, the law concerning council size was slightly changed.

has only one electoral district (i.e. constituency) and no geographic quotas are in place, even after a merger.

The municipal elections use an open-list method. Voting for a single individual candidate is mandatory. Party vote is calculated as the sum over its candidates' votes. Council seats are allocated to parties based on the party vote shares in accordance with competitive indices set by the d'Hondt method. Personal votes determine the position of the candidates within the party list. The elections in October 2008 already used the new post-merger municipal division, although the mergers came to effect at the start of 2009.

Parties do not typically form stable ruling coalitions in municipal councils. Instead, majority coalitions are often formed issue by issue. Moreover, party discipline is less strict at the local level than in the national parliament (see e.g. Hyytinen et al. 2014). In this environment, regional political representation can play a role in municipal decision making either directly through coalition formation across party lines or by influencing within party positions (see Baron and Ferejohn 1989 and Laver and Shepsle 1990). In the former case, the councilors from the same pre-merger municipality would vote in the council as a coalition that is independent of the formal parties and the councilors' own party affiliation. In the latter case, the regional political representation within the parties may affect the policy position of the parties. In both situations, having more councilors from their home (pre-merger) municipality should be beneficial to voters.

2.2 The merger reform

In 2005, the central government initiated a plan that aimed at reforming the municipal revenue structure and more importantly making the production of statutory municipal services more efficient. The reasons behind the reform included aging related expected increases in municipal spending and disparities in municipal revenue bases due to urbanization. A provisional law enacted in 2007 states that municipalities should have strong enough revenue and labor force bases to cope with the production of statutory municipal services. Municipal mergers were the main tool for reaching these goals. The municipalities were allowed to decide voluntarily whether and with whom to merge.

The central government encouraged mergers using four policy instruments. First, central government grants were guaranteed not to decrease for the first five years after merging. Second, municipalities were not allowed to lay off their permanent employees

during the first five post-merger years. The central government did not, however, enforce additional restrictions on the merging municipalities concerning how they handle their finances before the mergers take place (Saarimaa and Tukiainen 2015). Third, the central government granted merger subsidies to merged municipalities. The subsidy amount depended on the populations of the pre-merger municipalities, the population of the resulting new municipality and the number of participating municipalities in the merger. The mergers in 2008 and 2009 received larger subsidies than the subsequent ones, possibly explaining why most mergers took place in 2009. The subsidies were paid in annual installments over a three year period after the mergers had taken place. On average over the merged municipalities, the merger subsidy was about 330 Euros per capita, but in some mergers the per capita subsidy was as high as 1100 Euros.

The merger process itself is as follows. The process usually starts with unofficial discussions which may lead to an initial feasibility study that is conducted by an external consultant. Based on the consultant's report, municipal boards make a proposal of the merger to the municipal councils. This proposal is then voted on by the councils. If the proposed merger gains a majority in *all* the participating councils, the merger goes through. If not, it is cancelled and all the municipalities continue as they were or a subset of the municipalities in the original plan merges. The process from the consultancy report to the final merger decision can take up to two years.

In 2006, there were 431 municipalities in total. Following the central government plan, 14 mergers took place in 2007, 1 in 2008 and 32 in 2009. This reduced the number of municipalities to 348 (see Fig. 1). The number of municipalities involved in a given merger ranged from 2 to 10 municipalities.

We concentrate on the mergers that took place in 2009, which are highlighted in the map in Fig. 1. We exclude three of the 32 mergers that took place in 2009 from our analysis because they were part of another merger just before or just after 2009. We focus on the 2009 mergers, because for them the 2008 elections take place before any potential merger effects materialize. For the 2007 mergers, an explanatory variable based on the 2008 elections could potentially suffer from a bad control issue (see e.g., Angrist and Pischke 2009). Moreover, focusing only on the 2009 mergers allows for a transparent graphical presentation of our results. Finally, the central government merger incentives scheme is different for 2009 mergers than the previous and subsequent ones.

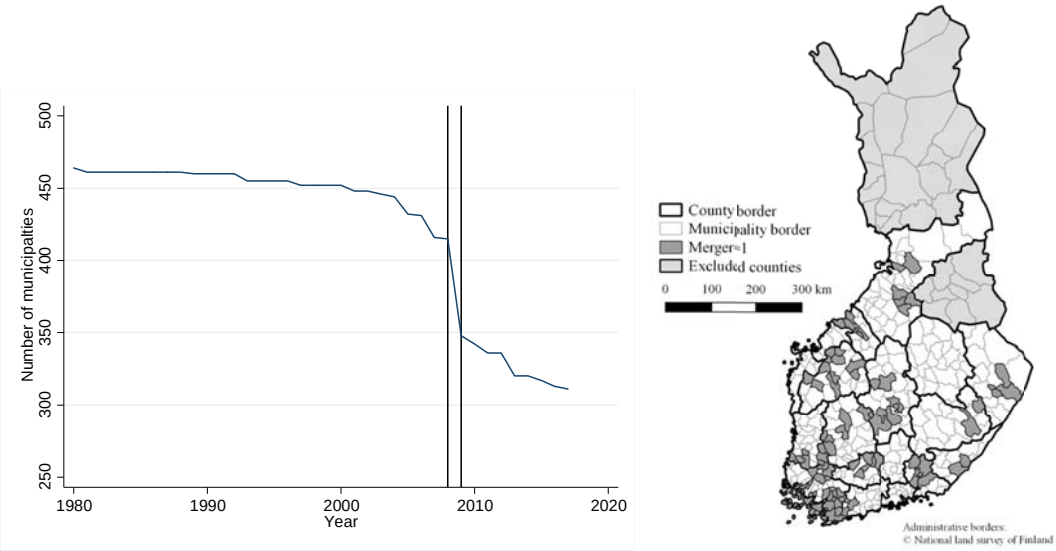

Fig. 1. Number of municipalities in Finland 1980–2017 and the 2009 mergers.

Saarimaa and Tukiainen (2014) describe the determinants of these mergers and find evidence suggesting that fiscal pressure, voter preferences and local democracy considerations influence the merger decisions. Hyytinen et al. (2014) find that also councilors' re-election concerns play a role. The voluntary nature of the mergers and the predictability of the merger decisions raise the issue of non-random selection that may bias the evaluation of the merger effects. Next, we describe our identification strategy.

3 Research Design

3.1 Creating the control group

Ideally, we would compare the outcomes of interest in the merged municipalities to the outcomes in these same municipalities had they not merged. Of course, we never observe both outcomes for the same municipalities and we need to impute a credible counterfactual that serves as the baseline when estimating the causal effect. The Finnish mergers were voluntary decided by municipality councils, and thus, the municipalities that merged may be different from the municipalities that did not merge in ways that are unobservable to us. This of course provides challenges for causal inference due to possible selection bias.

To address this endogeneity issue, we combine a nearest neighbor matching algorithm with DID methods. While these are well-established methods for causal inference, they need to be adjusted for merger analysis. Mergers include multiple partners (in our case ranging from two to ten), which means that the relevant unit of observation is a merger, i.e. a group of

municipalities that either merged (treatment group) or a group that did not merge (control group). The key issue for our analysis is constructing a valid control group.

We construct this control group by first simulating all possible mergers involving up to ten municipalities that could have taken place according to the pre-merger municipality borders.[5] We constrain these hypothetical mergers in the following ways: First, we only allow mergers to take place between adjacent municipalities, i.e. the hypothetical new municipality has to be geographically contiguous. Second, we allow mergers to take place only within county borders because none of the actual mergers crossed these borders (see Fig. 1).[6] Finally, to make sure there is no contamination of the control group, we omit from this set of hypothetical mergers all those mergers that included at least one municipality that actually underwent a merger in the period 2005–2015.

After these sample restrictions, we are left with a total of 7,965 hypothetical mergers (see Table A1).[7] We then use a nearest neighbor matching algorithm based on merger level characteristic from the pre-merger period to find suitable controls for the actual mergers.[8] We use exact matching with respect to number of municipalities in the merger. For example, for a merger including, say, four municipalities we pick five control mergers among those hypothetical mergers that also include four municipalities.[9] Within these merger size groups, the matching is based on the following covariates: total population of the merger, median distance of the citizens to the business center of the largest municipality in the merger, indicator for whether all the partners belong to the same health care cooperation unit, and within-merger heterogeneity in per capita taxable income, expenditures and deficit. These variables should reflect the potential for economies of scale, and the differences between

[5] We use the FANMOD software by Wernicke (2006) to construct the potential mergers with 2–6 partners. For the larger mergers we resort to using the ISMAGS software produced by Houbraken et al. (2014).

[6] We also omit the counties of Kainuu and Lapland, which are highlighted in the map in Fig. 1. Kainuu was experimenting with a new county level administration of health and social care services during the merger wave. Lapland, on the other hand, is an outlier with geographically large and sparsely populated municipalities. There were no mergers in these counties in 2009.

[7] All of these mergers would not be possible simultaneously in reality because an individual municipality may be involved in more than one of them.

[8] We use the teffects nnmatch command in STATA where the matches are sampled with replacement. We use the command only to construct the control group and do not use it in the estimation as we rely on DID estimation, want to allow for heterogeneity in the treatment effect and also analyze the effects at the pre-merger municipality level. The other commonly used matching method is propensity score matching, but it cannot facilitate exact matches in a subset of covariates and has also received some critique recently in King and Nielsen (2016).

[9] The choice of the number of control units is arbitrary and driven by the somewhat limited number of treated units. The optimal number of controls involves a standard bias-variance trade-off: Fewer controls means that they are more similar to the actual mergers, but this may lead to imprecise estimates. We can test the similarity of the control and treatment groups both in terms of pre-treatment levels and pre-treatment trends.

citizen's preferences and the financial situation of the municipalities in a merger. Moreover, Saarimaa and Tukiainen (2014) found that most of these variables had predictive power in explaining these merger decisions.

We use this control group, instead of all non-merged municipalities, also in the pre-merger municipality level analysis, in which we simply decompose the hypothetical mergers back to the municipality level. This has the advantage of allowing us to conduct placebo tests using this group. Furthermore, matching on pre-determined variables is the only way to control for systematic differences in observables, because we cannot measure these variables at the pre-merger municipality level after merging. Matching on pre-determined variables also minimizes the risk of including bad controls (or alternative outcomes) into the right-hand-side of the regressions (e.g. Angrist and Pischke 2009).

3.2 Data

Merger level: Table 1 presents descriptive pre-merger statistics on the mergers and the control group based on nearest neighbor matching. The potential mergers in the control group are slightly larger in terms of total population than the actual mergers. Otherwise the matching algorithm produces a very comparable control group in terms of pre-merger characteristics. The magnitude of the mean differences is small and none of them are statistically significant.

Table 1. Descriptive statistics for merger level data in 2008.

	Merger = 0		Merger = 1		
	Mean	SD	Mean	SD	*p*-value
Number of observations	145		29		
Number of municipalities	3.17	1.75	3.17	1.77	
Merger population	43,996	35,885	36,043	35,378	0.276
Median distance (km)	18.9	9.2	17.4	9.9	0.431
Expenditures (€ per capita)	5,175	491	5,334	545	0.121
Operating margin (€ per capita)	-4,206	289	-4,261	391	0.381
Tax rate (%)	19.2	0.6	19.2	0.8	0.865
Taxable income (€ per capita)	12,681	1,877	13,151	1,756	0.215
Central government grants (€ per capita)	1,620	533	1,471	586	0.178
Cooperation (0/1)	0.41	0.49	0.45	0.51	0.733
Merger subsidy (€ per capita)	279.4	167.3	329.1	223.6	0.170
Log house prices (€/m^2)	7.11	0.24	7.16	0.24	0.292

Note: The data come from Statistics Finland. The last column reports the *p*-value from a *t*-test on the equality of means. For house prices, the number of observations is 132 and 27, respectively.

Pre-merger municipality level: In addition to merger level effects, we are interested in within merger heterogeneity and whether the effects depend on the political representation in the post-merger councils. This analysis is made possible by high quality GIS data that allows us to aggregate spatial micro data to match the pre-merger municipality borders. These data come from three sources.

First, we have obtained the exact address of all the municipal councilors (and candidates in municipal elections) from the Population Register Center. These data allow us to calculate the seat shares at the pre-merger municipality level in the post-merger municipal councils, which we use as a treatment variable.

Second, we use the Community Structure Database (YKR) produced by the Finnish Environment Institute (SYKE) and Statistics Finland. This geocoded database covers the whole of Finland in 250 m x 250 m grids, which include information on the number of jobs in different sectors located in the grids. The data are available for the years 2000, 2003, 2005, 2007, 2009, 2010, 2012 and 2014. They allow us to analyze the development of these jobs through time regardless of the redrawing of municipal borders. Unfortunately, we cannot categorize the jobs into municipal and other jobs, and we need to rely on the categorization available in the data. We use the following four job categories: public administration and defense, health and social care, schooling, and employees from all other sectors. The first

three categories are our main job outcomes while the latter can be used in placebo tests because mergers should not have a sizable direct effect on these jobs, although an indirect effect is possible. Schooling and health and social care are the most important expenditure and employment categories as they make up some 70 percent of all municipal expenditures.

The problem with these categories is that some of these jobs may, in fact, be in the private sector (especially in health care) or in central government (obviously defense, but also some schooling because of universities). These jobs should not, however, confound our analysis as long as there are no changes in these jobs that coincide with the timing of the mergers. In practice this means that jobs in the military, private sector health care firms and in universities need to develop smoothly through the merger reform. If this is the case, the DID strategy produces a causal effect on municipal jobs, i.e. any changes in the number of these jobs can quite safely be interpreted as changes in municipal jobs.[10]

Finally, we use house transaction data at the zip-code level obtained from Statistics Finland. As zip-codes remain unchanged throughout the merger process, we can follow average house prices per square meter at the pre-merger municipality level. House prices are often used as an indirect measure of the quality of the service-tax bundle.

Table 2 presents descriptive statistics for these variables using the matched data at the pre-merger municipality level. The municipalities are divided into four groups for illustrative purposes. First is the control of group municipalities that did not merge. Second, since our interest lies on the effects of political representation, we have divided the merged municipalities into three equal-sized groups based on the council seat share they obtained in the first post-merger elections in 2008.[11] As Saarimaa and Tukiainen (2016) describe in detail, in the Finnish open-list system, candidates from smaller merger partners have to compete with candidates from the larger partners within the party lists. This means that very small merged

[10] In order to be on the safe side, we have also interviewed management from the two leading private sector heath care providers in Finland: Terveystalo (Päivi Metsäniemi, Chief physician) and Attendo Finland (Lauri Korkeaoja, Director of Communications and PR). They communicated to us that the location decisions by their firms have been independent of the municipal mergers and mostly independent of public sector providers' decisions more generally. Some of their activities may follow municipal activities because the municipal sector buys radiology and diagnostic services from the private sector, but here the location decision only takes place conditional on the public sector response. Furthermore, private sector operators sometimes find the space abandoned by the public sector convenient, thus possibly leading to a small amount of under-estimation of the effects on our part.

[11] The second post-merger elections were held in 2012. The correlation between pre-merger municipal level seat shares between the first and second post-merger elections is very high at 0.98, so we use the first post-merger election results throughout our analysis. Moreover, the 2012 elections could be influenced by the mergers, and thus, the 2012 election results perhaps better serve as an alternative outcome rather than a treatment.

municipalities have difficulties in getting any local representatives into post-merger councils. These groups are used in the graphical analyses; whereas the continuous seat share variable is used in the subsequent DID regressions.

The Weak group in Table 2 includes the municipalities with only a weak representation in the new council with an average seat share of only 6.4 percent. In the Medium group the average seat share is 20.8, while in the Strong group this share is 62.9 percent on average. In fact, 75 percent of the municipalities in the Strong group had a majority of the council seats. We cannot divide the control group into these subgroups, because they obviously have not had joint post-merger elections. However, we conduct placebo analysis using this group, where we use municipal population shares as proxies for seat shares.

Table 2. Descriptive statistics for pre-merger municipality level data in 2007.

	No merger		Weak		Medium		Strong	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Number of observations	460		31		31		30	
Seat share in post-merger council	1.000	0.000	0.064	0.031	0.224	0.079	0.669	0.163
Population	10,750	15,372	2,424	1,946	7,467	8,417	24,419	28,561
Population share in merger	0.257	0.211	0.054	0.032	0.209	0.077	0.695	0.187
Jobs per capita:								
Administration and defense	0.013	0.016	0.008	0.005	0.016	0.016	0.018	0.011
Schooling	0.023	0.010	0.014	0.008	0.021	0.013	0.029	0.012
Health and social care	0.050	0.018	0.043	0.022	0.047	0.020	0.067	0.022
Other	0.232	0.074	0.168	0.043	0.219	0.076	0.274	0.076

Notes: The Weak, Medium and Strong representation groups are constructed based on the pre-merger municipality level seat shares in the first post-merger municipal elections.

3.3 Difference-in-differences estimation

In addition to using a matching algorithm in constructing the control group, our identification strategy is based on the difference-in-differences method, where a control group of hypothetical mergers (or non-merging municipalities) is compared to actual mergers (or merged municipalities) before and after a treatment has taken place. In presenting our results, we will rely heavily on graphical evidence, but we will also report regression results to assess the statistical significance of the results.

When using the merger level data, we estimate the following type of DID models:

$$y_{it} = \alpha + \theta_t + \beta \cdot merger_i + \sum_{j=2009}^{2014} \delta_j merger_i \cdot year_j + u_{it}, \quad (1)$$

where y is one of our outcomes of interest in merger i at time t. θ_t are year dummies. We are interested in the coefficients of the post-merger interaction terms *merger·year*.

The model using pre-merger municipal level data and the seat shares takes the form:

$$y_{it} = \alpha + \theta_t + \beta \cdot merger_i + \gamma \cdot seatshare_i + \sum_{j=2009}^{2014} \delta_j merger_i \cdot year_j + \sum_{j=2009}^{2014} \mu_j merger_i \cdot seatshare_i \cdot year_j + u_{it}, \quad (2)$$

where the coefficients μ now captures any heterogeneous effects with respect to political representation. Note that the council seat share measure is defined only for the municipalities that actually merged and is set to zero to non-merged municipalities. Thus, we do not include the term $seatshare_i \cdot merger_i$ or the terms $\sum_{j=2009}^{2014} seatshare_i \cdot year_j$.

The key concern is that selection issues may relate both to the decision to merge and to the extent of political representation within mergers. However, selection based on pre-treatment differences in municipality characteristics does not bias the DID estimates as long as the common trends assumption holds. This assumption means that the outcomes would follow the same time trend in the control and treatment groups in the absence of treatment. We can indirectly test this assumption by analyzing pre-treatment trends using several comparisons: between mergers and non-mergers at the merger level, within mergers with respect to political representation at the pre-merger municipality level and between merged and non-merged of similar size at the pre-merger municipality level.

In the municipality-level analysis, we use two-way clustered (municipality and merger) standard errors to account for within merger and municipality level dependencies across time and within the control group (see Bertrand et al. 2004 and Cameron et al. 2011). The within control group dependency arises naturally in our setting, because of the nearest neighbor matching was done with replacement. In the merger-level analysis, clustering is at the merger level.

4 Empirical results

4.1 Merger level

To facilitate comparison with the existing literature and to understand the overall average effects of the merger reform, we first report the results of the merger level analysis. We begin by presenting graphical evidence. This also allows us to assess (indirectly) the common trends assumption, which is crucial for a causal interpretation of the results.

Fig. 2 presents the development of the means of per capita expenditures, operating margins[12], tax rates and log house prices per square meter from 2000 to 2014. The comparison is between the actual mergers and the control group of hypothetical mergers based on nearest neighbor matching. The blue vertical lines highlight the post-merger period and the red vertical lines highlight when the 5-year layoff protection for municipal employees ended after the mergers, as we also want to understand whether removing this constraint affected the municipalities' opportunities to cut expenditures.

Based on visual inspection, the two groups clearly follow a common pre-treatment trend in all outcomes. Formal tests presented in Table A2 in the supporting information, support this claim, especially after 2003. Moreover, the levels are almost identical due to our matching procedure. Did the mergers have an effect on per capita expenditures? According to Fig. 2 the answer seems to be no. There is no immediate change in expenditures after merging or the subsequent six years. If anything, expenditures increase slightly faster in the merger group.[13] There does not seem to be any effect on operating margin either. The regression results in Table 3 confirm that the differences are not statistically significant.

[12] We use operating margin as our alternative measure of municipal expenditures because some municipalities produce and sell services to other municipalities, which adds noise to the expenditure measure. These services show up on the municipality's expenditure side, but they also receive operating revenue from the sale of these services that needs to be netted-out. Operating margin measures the difference between operating revenues (such as fees) and operating costs. So in effect, it measures the expenditures that the municipality finances through taxes and central government grants.

[13] The small increase in expenditures in 2008, also detected in Table A2 in the supporting information, may be due to common pool exploitation reported in Saarimaa and Tukiainen (2015) who studied the anticipatory effects of the same merger reform.

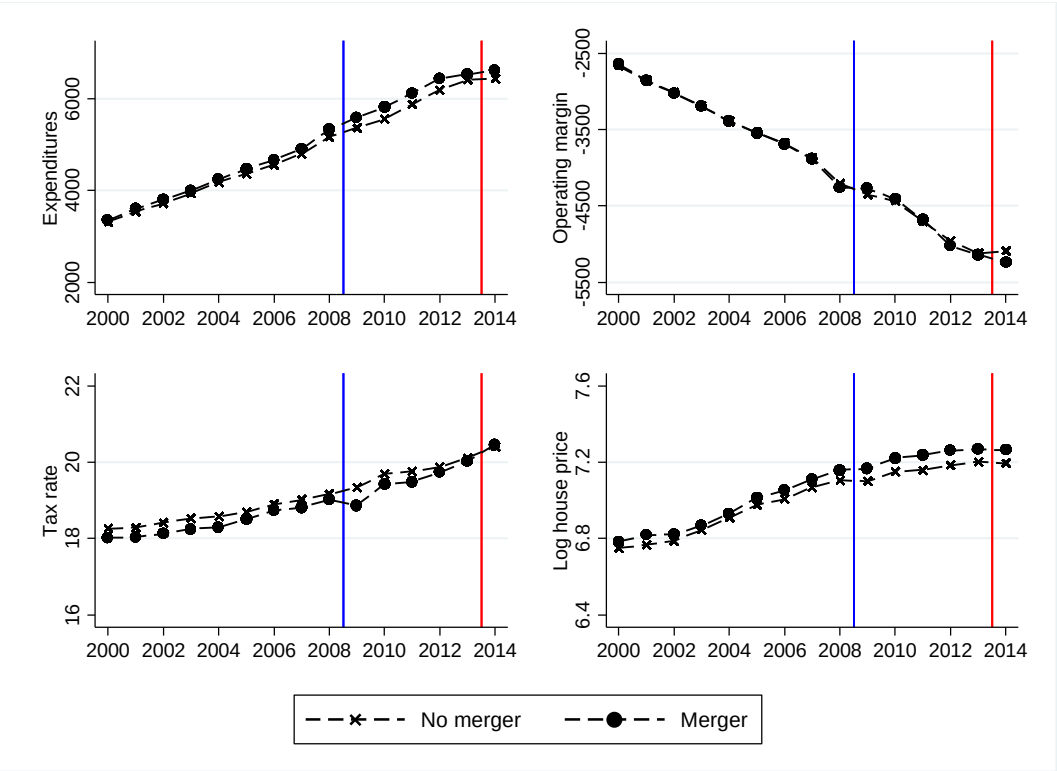

Fig. 2. Graphical DID results with merger level data.

Note: The non-merged control group is constructed using nearest neighbor matching algorithm

Interestingly, there is a small but temporary drop in tax rates in the merger group. One interpretation is that this is due to tax rate harmonization within a merger. However, we do not wish to draw too strong conclusions regarding the tax rates as the effects are not statistically significant according to Table 3.

Based on the fiscal outcomes, it seems that the main goal of the policy-maker was not achieved.[14] However, the fact that we do not find effects on expenditures does not necessarily mean that mergers had no effect on citizens' welfare. It could be, for example, that merged municipalities are able to produce higher quality services with the same expenditures than the municipalities that did not merge. Unfortunately, we do not have good data on service quality, and instead, we need to use indirect measures of the quality of the service-tax bundle. An often used measure for this purpose is house prices (Reingewertz 2012; Allers and Geertsema 2016). The idea is based on a revealed preference argument so that changes in quality of the service-tax bundle should be reflected in the demand for housing in the municipality and in

[14] We also analyzed between merger heterogeneity with respect to the number of municipalities in a merger, pre-merger cooperation among the merging municipalities and geographic compactness of the merger. Again, we found no effects with respect to these merger subgroups. These results are available from the authors by request.

house prices in the short run. Based on Fig. 2, house prices diverge slightly between the two groups, but the results in Table 3 indicate that the difference is not statistically significant. Moreover, some divergence seems to begin already prior to merging.

Table 3. Regression DID results with merger level data.

	Expenditures	Operating margin	Tax rate	House price
	[1]	[2]	[3]	[4]
Constant	3313.1**	-2664.1**	18.31**	6.749**
	[31.3]	[18.7]	[0.053]	[0.017]
Merger	86.2	-6.9	-0.086	0.038
	[86.8]	[62.4]	[0.116]	[0.043]
Merger*2009	132.7	87.9**	-0.267	0.028
	[99.4]	[32.1]	[0.221]	[0.016]
Merger*2010	174.8	30.2	-0.115	0.033
	[117.5]	[36.2]	[0.199]	[0.019]
Merger*2011	152.5	27.9	-0.175	0.039
	[125.4]	[41.1]	[0.199]	[0.023]
Merger*2012	162.1	-58.1	-0.169	0.039
	[138.5]	[47.7]	[0.209]	[0.027]
Merger*2013	38.1	-6.4	-0.27	0.027
	[142.5]	[58.6]	[0.206]	[0.025]
Merger*2014	89.2	-147.2	-0.037	0.031
	[142.3]	[90.5]	[0.257]	[0.025]
R^2	0.74	0.84	0.59	0.34
N	2610	2610	2490	2385

Notes: The results are from OLS models. The non-merged control group is constructed using nearest neighbor matching algorithm. All the models include year dummies. Standard errors are clustered at the merger level and reported in brackets. ** and * indicate statistical significance at 1 and 5 percent level, respectively.

4.2 Pre-merger municipality level

The merger level analysis abstracts away from a number of interesting questions with respect to within-merger heterogeneity. For example, the services may be relocated to the largest municipality in the merger, but the overall service level, and thus, the expenditure level may remain the same. This may hurt the citizens in the smaller partner, but we would not be able to detect these relocations from the merger level data.

In this section, we use pre-merger level municipalities in the analysis and ask whether the effects of mergers vary across municipalities and whether they depend on political representation. In order to assess the results visually, we divide the merged municipalities into

subgroups based on the (pre-merger) municipality level representation in the post-merger council as described in Table 2. The results are portrayed in Fig. 3, while Table 4 presents the corresponding regression results where the post-merger seat shares are used as a continuous treatment variable in a DID model presented in Eq. (2). Because of large differences in the number of total jobs, all the jobs measured in per capita terms.[15]

Fig. 3 also includes placebo tests using the municipalities in the control group. Since we do not have merger level elections for these municipalities, we divide the control group into subgroups based on their population share in their hypothetical merger. We divide the control group into three equal sized groups based on the population share and label these subgroups the same way as the merger subgroups (Weak, Medium and Strong) so that the Strong group again includes the municipalities with the largest population share in their hypothetical merger.

According to Fig. 3, administrative jobs (top-left) clearly decrease in the municipalities with Weak and Medium representation in the council, whereas they slightly increase in the municipalities with Strong representation. This suggests that municipal administration is concentrated to the largest municipality in the merger. For administration, pre-treatment common trends are not particularly clean, but the formal pre-treatment tests do not indicate a problem (see Table A3 in supporting information) and the largest changes clearly coincide with the mergers and these changes seem to be permanent. Furthermore, there are no changes in administrative jobs in the control group or in the subgroups that would coincide with the merger reform (top-right in Fig. 3).

A similar pattern is evident in the health and social care sector jobs (middle-left in Fig. 3). These jobs clearly decrease in the Medium and especially the Weak representation group, while there is no visible change in the Strong group. The initial reduction is substantial and the divergence between the groups permanent. The common trends assumption seems quite plausible for this outcome based on the pre-treatment trends and formal tests (see Table A3 in supporting information). Moreover, these jobs develop smoothly throughout the analysis period in each of the population share subgroups in the control group (middle-right in Fig. 3).

There seem to be no permanent effects in the schooling sector (bottom-left in Fig. 3). After a small initial decline, the number of jobs quickly returns to the pre-merger levels and the groups do not diverge permanently. Formal pre-treatment tests do not indicate any

[15] The results are not driven by changes in population levels in the municipalities are they develop smoothly in all the municipality groups (Fig. A1 in the supporting information).

problems (see Table A3 in supporting information) and these jobs develop smoothly also in the control group (bottom-right in Fig. 3).

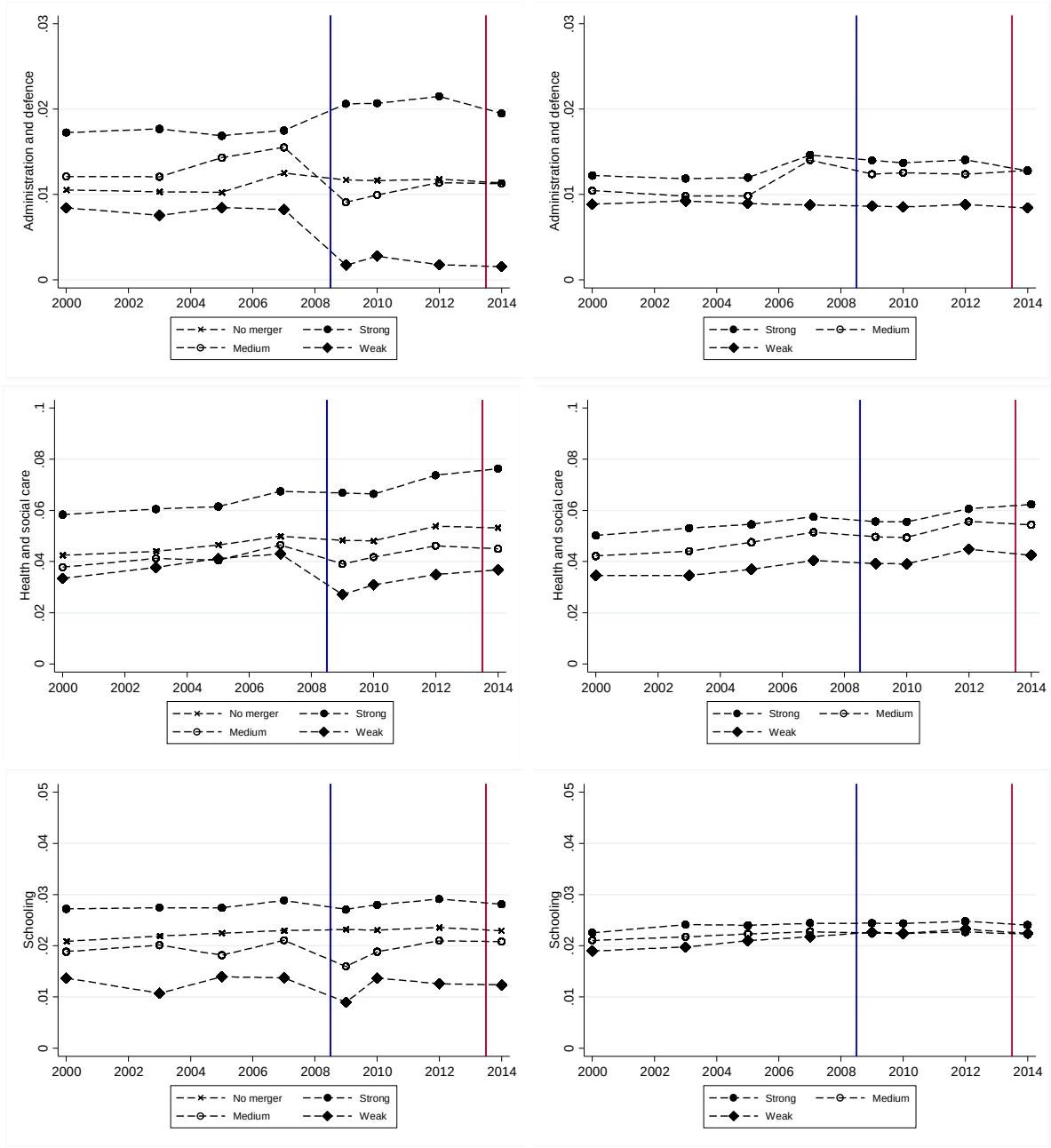

Fig. 3. Graphical DID results with pre-merger municipality level data.

Notes: The left-hand side figures illustrate the true treatment effects based on municipal seat shares in the post-merger councils. The right-hand side figures illustrate the placebo treatments for the non-merged control group based on municipal population shares. The non-merged control group is constructed using nearest neighbor matching algorithm.

The regression results presented in Table 4 are in line with the graphical findings.[16] The effects on administration and health and social care are substantial, persistent and also statistically highly significant. For example, based on the regression results, in the first post-merger year 2009 the health and social care jobs decrease by almost a third on average for the municipalities in the Weak representation group (seat share equal to 6.4 percent on average) compared to 2007. The interpretation of the regression coefficients for *Seat share* is somewhat tricky because the treatment effect may be a combination of overall reductions in the number of jobs and a reshuffling of jobs within the mergers from politically marginalized municipalities to the larger ones. Thus, Fig. 3 and Table 4, give a correct picture of the availability of services at municipality level, but do not say anything about the total number of jobs.

In order to check, which the correct interpretation is, we calculated the number of total jobs in the merged municipalities before and after the mergers. The total number of health and social care jobs in the merged municipalities was 74,479 in 2007 and 74,329 in 2009. This suggests that the results are mostly due to reshuffling of these jobs from politically marginalized municipalities to the larger ones within the mergers. This is also in line with the zero effects on total expenditures. A plausible explanation is that some small local health care centers are closed and the employees are moved to health care centers in the municipality with more representation.

[16] Estimating these models using only the merged municipalities and omitting the merger-dummy and its interactions with the year-dummies produces the same the point estimates for the *Seat share* and year interaction terms.

Table 4. Regression DID results with pre-merger municipality level data.

	Administration	Schooling	Health and social care
	[1]	[2]	[3]
Constant	-0.001	-0.001	0.005
	[0.003]	[0.004]	[0.007]
Merger	0.010**	0.013**	0.027**
	[0.002]	[0.003]	[0.006]
Seat share	0.011**	0.022**	0.037**
	[0.003]	[0.004]	[0.007]
Merger*2009	-0.008**	-0.006**	-0.014**
	[0.001]	[0.001]	[0.003]
Merger*2010	-0.007**	-0.001	-0.010**
	[0.001]	[0.002]	[0.003]
Merger*2012	-0.007**	-0.001	-0.011**
	[0.001]	[0.002]	[0.003]
Merger*2014	-0.007**	-0.001	-0.010**
	[0.002]	[0.001]	[0.003]
Seat share*2009	0.016**	0.006*	0.029**
	[0.003]	[0.003]	[0.007]
Seat share*2010	0.014**	0	0.021**
	[0.004]	[0.004]	[0.006]
Seat share*2012	0.016**	0.003	0.025**
	[0.003]	[0.003]	[0.007]
Seat share*2014	0.015**	0.002	0.026**
	[0.003]	[0.003]	[0.007]
R^2	0.03	0.07	0.1
N	4400	4400	4400

Notes: The results are from OLS models. The non-merged control group is constructed using nearest neighbor matching algorithm All the models include year dummies. Standard errors are clustered at the merger and municipality level and reported in brackets. ** and * indicate statistical significance at 1 and 5 percent level, respectively.

4.3 Discussion and validity checks

Effects on welfare. The findings in Fig. 3 and Table 4 beg the question of whether the mergers were detrimental to the welfare of the residents in the politically marginalized municipalities that experienced reductions in health and social care services. To answer this question, we would ultimately need to know what happened to the overall quality of the bundle of municipal services and taxes in these municipalities.

Unfortunately, we do not have data on overall service quality so we again turn to house prices as a proxy measure. The problem in using house price data at the pre-merger

municipality level is that the number of housing transactions per year is small in the smallest municipalities. Having only a few housing transactions in a given year would not give us a reliable measure of the quality of life in the municipality. In order to get reliable results, we have constrained the sample so that a municipality needs to have at least ten transactions per year during the whole 2000–2014 period to be included in the analysis. This means that for the visual inspection of the development of house prices we have to combine the Medium and Weak representation groups. The combined Medium and Weak group contains 20 municipalities in total (16 Medium and 4 Weak). While the results are more reliable, this has the clear disadvantage that we effectively drop many of the municipalities that had the largest reductions in health and social care jobs.

Fig. 4 presents the development of house prices (log price per square meter) in the representation subgroups and the population share subgroups of the control group. Table A4 in the supporting information reports the corresponding regression results. From the left panel of Fig. 4, it is evident that house prices follow a rather clean pre-treatment common trend, which is also confirmed by formal pre-treatment tests (see Table A5 in supporting information). After merging house prices diverge between the Strong and the combined group of Medium and Weak representation municipalities. The combined Medium and Weak group diverges also from the control group from 2012 onwards. The differences are statistically significant for 2013 and 2014 as can be seen from Table A4 in the supporting information. At face value, this result means that the mergers were, at least to some extent, harmful to the residents in the politically marginalized municipalities.

The population share subgroups exhibit common pre-treatment trends (see Table A5 in supporting information) and develop quite similarly also after the merger reform. The groups diverge slightly in the last year, but the DID estimates is not statistically significant (see Table A4 in supporting information). However, this finding together with the small number of transactions from the Weak representation group implies that we should treat these results and their interpretation with respect welfare with some caution.

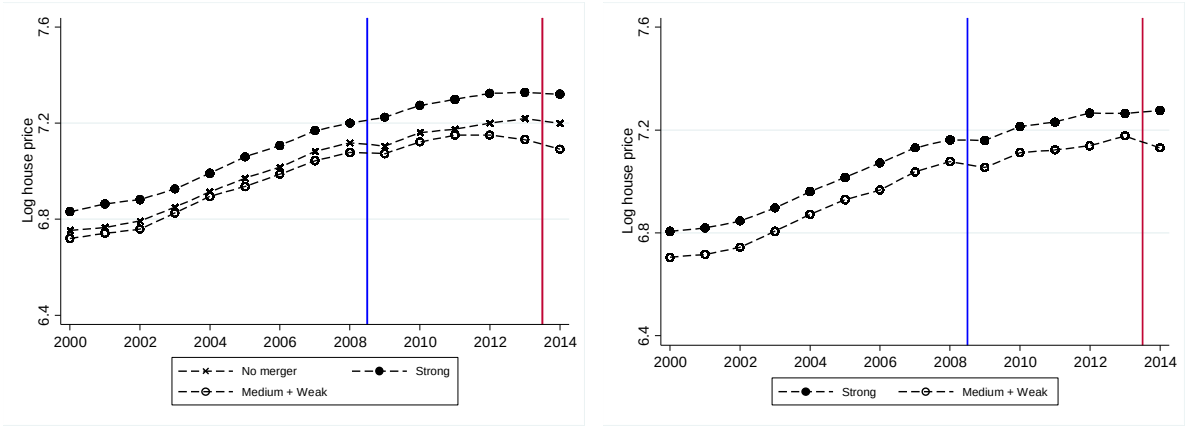

Fig. 4. House prices at pre-merger municipality level.

Note: The left-hand side figure illustrates the true treatment effects based on municipal seat shares in the post-merger councils. The right-hand side figure illustrates the placebo treatments for the non-merged control group based on municipal population shares. The non-merged control group is based on nearest neighbor matching algorithm. Due to small number of observations in the Weak representation group, Medium and Weak groups are combined in the figure.

Validity checks. We have conducted two additional validity checks. The first check is related to the house price results. One might worry that the small merged municipalities are in a different business cycle compared to their larger partners, and this might have impacts also on local public jobs employment and house prices. We address this issue by analyzing the number of other jobs in the municipalities, defined as all jobs minus the job categories analyzed in Fig. 3 and Table 4. These jobs, although they may include some municipal jobs, can be seen as a placebo outcome in the sense that merging should not have a direct effect on them. According to Fig. A2 in supporting information, nothing happened to these jobs in any of the representation subgroups or in the control group and its subgroups. This result adds credibility to our results as it suggests that the different-sized merged municipalities are in the same business cycle, at least when it comes to the labor market outcomes.

The second validity check addresses the interpretation of the results. The results so far suggest that representation in the post-merger council is an important driver of what happens to local services after the merger. However, it is not the only explanation that is consistent with these findings. One alternative explanation is that it simply makes sense to concentrate some services to larger municipalities and shutdown facilities in smaller places. Under this interpretation, the effects would still be due to merging, but they would not be related to political representation *per se*.

We test this alternative by estimating a model where we include the pre-merger population *level* of the municipality (the 2007 population) as an additional treatment variable in the DID regression. If the results are not driven by representation, but instead by municipal size, the population treatment should capture the effects and we should observe a zero effect for the seat share treatment.

The results from these models are presented in Table A6 in the supporting information. The pre-merger population level of the municipality is correlated (0.58) with the post-merger council seat share, but nonetheless we are able to identify both of these effects fairly precisely. There is identifying variation in seat shares over and above population levels because seat shares are more related to the relative size of the municipality in the merger rather than absolute size of the municipality. The results suggest that the post-merger council seat share is driving the results instead of population, although population has its own effect as well on administrative jobs.

However, even with these additional results, we cannot claim to have a bullet proof identification strategy with respect to political representation. One possibility would be to follow Hyytinen et al. (2017) and use close elections for identification. However, we do not have enough close races in the merged municipalities to exploit them in this paper.

5 Conclusions

We have analyzed the effects of municipal mergers using novel geocoded data on municipal services and local politicians place of residence. Our results suggest that the mergers had practically no effects on total expenditures. This result is in line with previous research that shows that voluntary mergers are unlikely to result in cost savings. However, these aggregate effects hide interesting patterns within the mergers indicating that the benefits and costs of merging are distributed unevenly within the mergers. The municipalities who were politically marginalized in the post-merger council experienced a substantial reduction in local public jobs in administration and, more importantly, in health and social care sectors in their area relative to the municipalities with stronger representation. Furthermore, analysis of house price development provides suggestive evidence that this political marginalization was harmful to the residents of these small municipalities.

Our results speak to the literatures that have examined the effects of redrawing jurisdictional boundaries and the effects of representation, and also inform the policy debate

on the effects of merging local jurisdictions. Compared to prior literature on the effects of municipal mergers, our results give a much more nuanced picture of what happens to municipal services and how these effects are tied to the political representation at the local level. The increased regional inequality that we document cannot be detected by simply comparing merged and non-merged municipalities meaning that the prior literature misses an important aspect of merger reforms. As better spatial data become more broadly available, analyzing the within merger effects in other countries with the methodological refinements introduced in this paper seems like a fruitful avenue for further research.

It seems plausible that our results generalize at least to countries with a similar political system and public services at the local level. Furthermore, considering the extent of the literature from many different countries and political systems concerning the relationship between representation in legislatures and the geographic distribution of public funds, we think that the effects documented in this paper are not confined to Finland.

At the same time, the details of the political system may affect how unequal the geographic representation becomes after a merger reform, and thus, the subsequent effects on service availability within the merged municipalities. The key features of election systems, such as ballot type, election formula, and district number and magnitude, are all likely to affect the resulting representation in subtle ways as they shape the incentives and actions of all political actors (Duverger 1954 and Taagepera and Shugart 1989).

In addition to pointing out new directions in how to evaluate merger reforms, our results suggest that political efficacy and representation issues should feature more prominently in the planning stages of merger reforms. These reforms are often unpopular among the electorate and our results together with those by Lassen and Serritzlew (2011) indicate that concerns for deteriorating local democracy and representation have merit. Our study shows that if major merger reforms are planned without putting local political representation at the center stage of the analysis, there may be unanticipated and undesired outcomes on regional equality.

In addition to paying attention to how the prevalent electoral system influences the effects of mergers, policy-makers should consider whether there is a need to adjust the electoral system when municipal borders are redrawn. The lessons from the extensive literature on redistricting (e.g. Gelman and King 1994 and Coate and Knight 2007) should provide a useful starting point also for the planning of merger reforms and future research.

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Supporting Information.

Table A1. Number of hypothetical, matched and actual mergers by merger size.

Number of municipalities	Hypothetical mergers	Matched controls	Actual mergers
2	277	70	14
3	432	35	7
4	687	20	4
5	1,090	5	1
6	1,643	10	2
10	3,836	5	1
Total	7,965	145	29

Table A2. Tests for pre-treatment common trends for expenditures, operating margin, tax rate and house prices with merger level data.

	Expenditures	Operating margin	Tax rate	House price
	[1]	[2]	[3]	[4]
Constant	3322**	-2669.32**	18.32**	6.750**
	[30.18]	[18.05]	[0.055]	[0.016]
Merger	30.36	24.62	0.897**	0.356**
	[73.62]	[55.68]	[0.048]	[0.011]
Merger * 2001	32.83*	-26.58	-0.012	0.017
	[15.11]	[14.66]	[0.029]	[0.012]
Merger * 2002	56.93*	-32.61	-0.053	0.002
	[22.47]	[17.78]	[0.049]	[0.014]
Merger * 2003	27.2	-24.52	0.026	-0.01
	[26.44]	[19.69]	[0.060]	[0.014]
Merger * 2004	28.81	-16.74	0.005	-0.012
	[34.77]	[22.64]	[0.070]	[0.018]
Merger * 2005	74.71	-30.41	0.11	0.004
	[42.00]	[27.54]	[0.090]	[0.023]
Merger * 2006	75.5	-34.26	0.188*	0.013
	[47.56]	[31.95]	[0.089]	[0.022]
Merger * 2007	78.2	-38.75	0.116	0.01
	[51.69]	[32.24]	[0.091]	[0.025]
Merger * 2008	128.20*	-79.62*	0.121	0.019
	[59.13]	[35.01]	[0.111]	[0.026]
R^2	0.68	0.77	0.26	0.27
N	1566	1566	1566	1431

Notes: The results are from OLS models and correspond to the results in Table 3 in the main text. The data are from 2000–2008. The non-merged control group is constructed using nearest neighbor matching algorithm. All the models include year dummies. Standard errors are clustered at the merger level and reported in brackets. ** and * indicate statistical significance at 1 and 5 percent level, respectively.

Table A3. Tests for pre-treatment common trends for number of jobs per capita with pre-merger municipality level data.

	Administration	Schooling	Health and social care
	[1]	[2]	[3]
Constant	-0.000	0.001	0.005
	[0.003]	[0.004]	[0.007]
Merger	0.009**	0.013**	0.026**
	[0.002]	[0.003]	[0.006]
Seat share	0.011**	0.020**	0.037**
	[0.002]	[0.004]	[0.007]
Merger*2003	-0.001	-0.003	0.002
	[0.001]	[0.002]	[0.002]
Merger*2005	0.001	-0.002	0.001
	[0.001]	[0.001]	[0.002]
Merger*2007	-0.001	-0.001	0.001
	[0.002]	[0.001]	[0.002]
Seat share*2003	0.002	0.004	-0.001
	[0.001]	[0.003]	[0.003]
Seat share*2005	-0.000	0.001	-0.002
	[0.002]	[0.002]	[0.003]
Seat share*2007	0.001	0.002	0.003
	[0.003]	[0.002]	[0.004]
R^2	0.03	0.08	0.08
N	2208	2208	2208

Notes: The results are from OLS models and correspond to the results in Table 4 in the main text. The data are from 2000, 2003, 2005 and 2007. The non-merged control group is constructed using nearest neighbor matching algorithm All the models include year dummies. Standard errors are clustered at the merger and municipality level and reported in brackets. ** and * indicate statistical significance at 1 and 5 percent level, respectively.

Table A4. Regression DID results for house prices with pre-merger municipality level data.

	House prices [1]		House prices [2]
Constant	6.500**	Constant	6.683**
	[0.089]		[0.041]
Merger	0.166*		
	[0.068]		
Seat share	0.253**	Population share	0.210*
	[0.084]		[0.085]
Merger * 2009	-0.012		
	[0.020]		
Merger * 2010	-0.031		
	[0.026]		
Merger * 2011	-0.002		
	[0.032]		
Merger * 2012	-0.03		
	[0.040]		
Merger * 2013	-0.082		
	[0.050]		
Merger * 2014	-0.112*		
	[0.053]		
Seat share*2009	0.067*	Population share*2009	0.023
	[0.032]		[0.038]
Seat share*2010	0.089*	Population share*2010	0.022
	[0.037]		[0.032]
Seat share*2011	0.057	Population share*2011	0.039
	[0.046]		[0.031]
Seat share*2012	0.093	Population share*2012	0.055
	[0.056]		[0.037]
Seat share*2013	0.150*	Population share*2013	-0.023
	[0.063]		[0.055]
Seat share*2014	0.206**	Population share*2014	0.108
	[0.066]		[0.065]
R^2	0.34		0.35
N	4365		3750

Notes: The results are from OLS models and correspond to Fig. 4 in the main text. The non-merged control group is based on nearest neighbor matching algorithm All the models include year dummies. Standard errors are clustered at the merger and municipality level and reported in brackets. ** and * indicate statistical significance at 1 and 5 percent level, respectively.

Table A5. Tests for pre-treatment common trends for house prices with pre-merger municipality level data.

	House prices [1]		House prices [2]
Constant	6.526**	Constant	6.679**
	[0.077]		[0.039]
Merger	0.148**		
	[0.056]		
Seat share	0.228**	Population share	0.221**
	[0.071]		[0.072]
Merger * 2001	0.012		
	[0.015]		
Merger * 2002	-0.013		
	[0.011]		
Merger * 2003	0.002		
	[0.016]		
Merger * 2004	0.006		
	[0.020]		
Merger * 2005	-0.017		
	[0.022]		
Merger * 2006	-0.017		
	[0.026]		
Merger * 2007	-0.018		
	[0.031]		
Merger * 2008	-0.019		
	[0.031]		
Seat share * 2001	0.008	Population share * 2001	-0.001
	[0.024]		[0.016]
Seat share * 2002	0.039*	Population share * 2002	0.002
	[0.018]		[0.021]
Seat share * 2003	0.001	Population share * 2003	-0.017
	[0.026]		[0.036]
Seat share * 2004	0	Population share * 2004	-0.019
	[0.032]		[0.039]
Seat share * 2005	0.047	Population share * 2005	-0.027
	[0.035]		[0.043]
Seat share * 2006	0.052	Population share * 2006	0.005
	[0.040]		[0.052]
Seat share * 2007	0.043	Population share * 2007	-0.012
	[0.046]		[0.056]
Seat share * 2008	0.038	Population share * 2008	-0.025
	[0.048]		[0.058]
R^2	0.27		0.29
N	2619		2250

Notes: The results are from OLS models and correspond to Fig. 4 in the main text and Table A4. The data are from 2000–2008. The non-merged control group is based on nearest neighbor matching algorithm All the models include year dummies. Standard errors are clustered at the merger and municipality level and reported in brackets. ** and * indicate statistical significance at 1 and 5 percent level, respectively.

Table A6. Alternative regression results for number of jobs with pre-merger municipality level data.

	Administration	Schooling	Health and social care
	[1]	[2]	[3]
Constant	0.006	0.011**	0.021**
	[0.003]	[0.004]	[0.007]
Merger	0.004	0.002	0.012
	[0.003]	[0.003]	[0.006]
Seat share	0.005	0.010**	0.021**
	[0.003]	[0.004]	[0.007]
Population	0.002*	0.003**	0.004**
	[0.001]	[0.001]	[0.001]
Merger*2009	-0.009**	-0.007**	-0.016**
	[0.001]	[0.001]	[0.003]
Merger*2010	-0.008**	-0.001	-0.011**
	[0.002]	[0.002]	[0.003]
Merger*2012	-0.008**	-0.002	-0.013**
	[0.001]	[0.001]	[0.003]
Merger*2014	-0.008**	-0.002	-0.011**
	[0.002]	[0.001]	[0.003]
Seat share*2009	0.008*	0.005	0.024**
	[0.003]	[0.003]	[0.008]
Seat share*2010	0.009**	0	0.014*
	[0.004]	[0.003]	[0.006]
Seat share*2012	0.011**	0.003	0.021**
	[0.003]	[0.003]	[0.007]
Seat share*2014	0.009*	0.003	0.022**
	[0.004]	[0.002]	[0.008]
Population*2009	0.003*	0.002*	0.004
	[0.001]	[0.001]	[0.002]
Population*2010	0.003*	0.001	0.004*
	[0.001]	[0.001]	[0.002]
Population*2012	0.003*	0.001	0.003
	[0.001]	[0.001]	[0.002]
Population*2014	0.003*	0.001	0.003
	[0.001]	[0.001]	[0.002]
R^2	0.05	0.1	0.13
N	4400	4400	4400

Notes: The results are from OLS models. The non-merged control group is based on nearest neighbor matching algorithm All the models include year dummies. Population refers to municipal population in 2007. Standard errors are clustered at the merger and municipality level and reported in brackets. ** and * indicate statistical significance at 1 and 5 percent level, respectively.

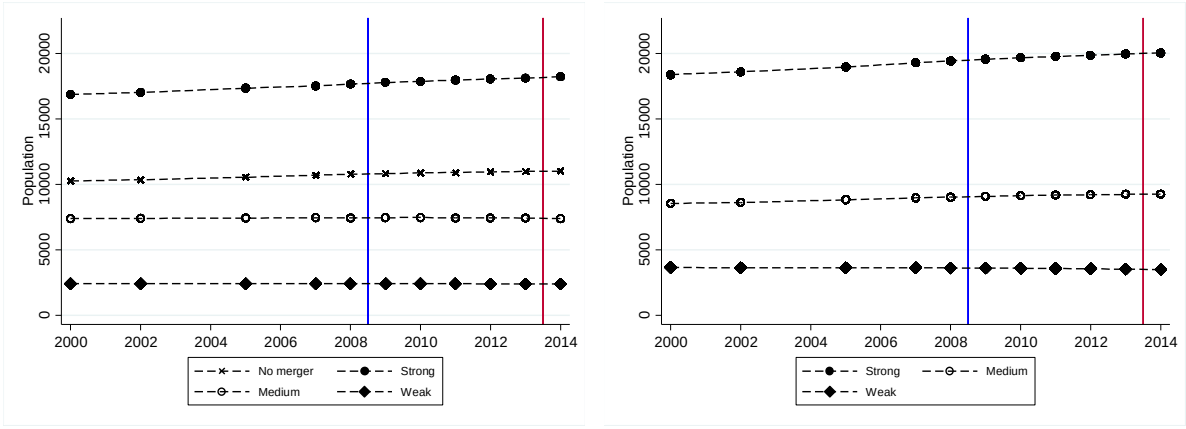

Fig. A1. Population with pre-merger municipality level data.

Notes: The left-hand side figures illustrate the true treatment effects based on municipal seat shares in the post-merger councils. The right-hand side figures illustrate the placebo treatments for the non-merged control group based on municipal population shares. The non-merged control group is based on nearest neighbor matching algorithm.

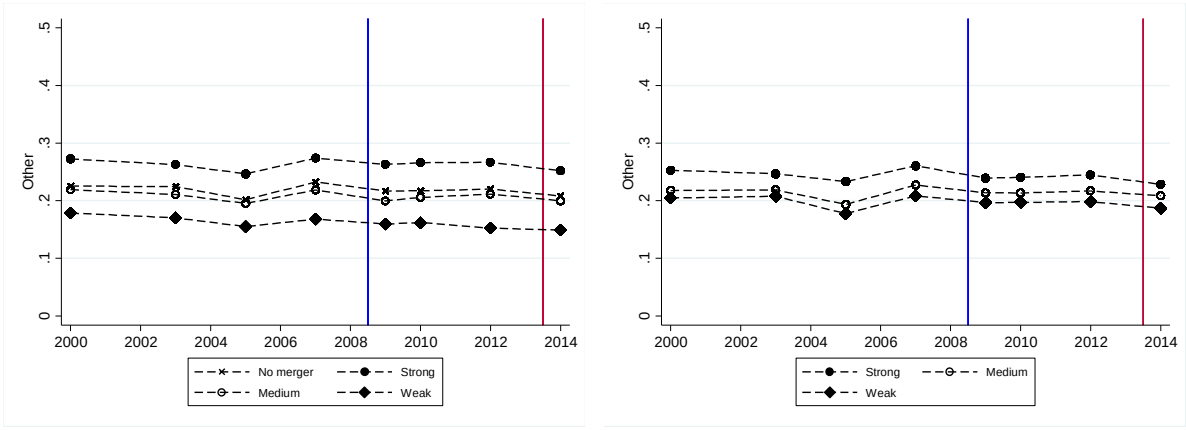

Fig. A2. Number of jobs in all other sectors with pre-merger municipality level data.

Notes: The left-hand side figures illustrate the true treatment effects based on municipal seat shares in the post-merger councils. The right-hand side figures illustrate the placebo treatments for the non-merged control group based on municipal population shares. The non-merged control group is based on nearest neighbor matching algorithm.