

**DO MUNICIPAL MERGERS RESULT IN
SCALE ECONOMIES? EVIDENCE FROM A
GERMAN FEDERAL STATE**

Sebastian Blesse, Thushyanthan Baskaran

GEORG-AUGUST-UNIVERSITÄT GÖTTINGEN

Do municipal mergers result in scale economies?

Evidence from a German federal state

Sebastian Blesse*[†] Thushyanthan Baskaran^{††}

[†]Deutsche Bundesbank and Goethe University Frankfurt, sebastian.blesse@bundesbank.de

^{††}University of Göttingen, thushyanthan.baskaran@uni-goettingen.de

Abstract

The extant empirical evidence on the fiscal consequences of municipal mergers is ambiguous. We therefore revisit this question by making use of a merger reform in the German federal state of Brandenburg in 2003. In addition to identifying the causal effects of mergers on municipal expenditures by relying on a natural experiment, one novel contribution of our study is to explore the fiscal consequences of both compulsory and voluntary municipal mergers within the same institutional setting. Using a difference-in-difference design with municipality-level panel data (aggregated to post-reform territorial boundaries), we find substantial and immediate reductions in total, administrative and current expenditures per capita after compulsory mergers. Voluntary mergers, on the other hand, have smaller and less robust effects.

Keywords: Municipal mergers, economies of scale, voluntary and compulsory mergers

JEL codes: H11, H72, H77, R53

*Corresponding author: Research Centre, Deutsche Bundesbank, P.O box 100602, Frankfurt am Main, Germany, Tel: (+49)6995667087, Fax: (+49)6995664062. This paper represents the authors' personal opinions and does not necessarily reflect the views of the Deutsche Bundesbank or its staff.

1 Introduction

In the last few decades, many industrialized countries have embarked on municipal mergers (Fox and Gurley, 2006). These merger reforms were mainly initiated because policy makers believed that larger municipalities are more capable to exploit economies of scale in public service provision.¹ Whether mergers indeed entail economies of scale and lower costs, however, is unclear. Some extant studies suggest insignificant effects or even diseconomies of scale after mergers (Moisio and Uusitalo, 2013; Lüchinger and Stutzer, 2002), while others show substantial cost reductions (Blom-Hansen et al., 2011; Welling-Hansen et al., 2012; Reingewertz, 2012; Fritz, 2011).

Existing studies vary mainly along two dimensions. First, they vary in the country (i. e. the institutional setting) that is being studied. Second, studies vary according to the type of the merger process, i. e. whether municipalities had some say in whether and with whom to merge (voluntary mergers) or whether mergers were designed and enforced by the central government (compulsory or forced mergers). It is, therefore, not obvious whether the results in the existing literature differ because the fiscal effects of mergers vary between countries or only between types of mergers.

In this paper, we study the fiscal consequences of municipal mergers once more. Our setting covers municipalities in the German State of Brandenburg over the period 1998-2005. More specifically, Brandenburg merged 1320 municipalities into 262 larger units during the 2000-2003 period in the context of a major merger reform, while leaving 159 municipalities unaffected. This opens up a natural experiment with which we can identify the causal effect of mergers on a number of municipal expenditure items using difference-in-difference (DD) regressions.

The main advantage of our setting over the extant quasi-experimental literature is that the municipal mergers in Brandenburg were staggered. That is, the state government of Brandenburg reached in mid-2000 the political decision to reduce the number of municipalities. In an initial period, mergers were voluntary in the sense that municipalities that were in principle eligible for mergers were allowed to choose when and with whom to merge. In 2003, however, the state government passed a law that enforced mergers of those eligible municipalities that had not yet merged, thereby bringing the reform to an end. We have, therefore, a setting with both compulsory and, to some degree, voluntary mergers within the same institutional context. The previous literature, on the other hand, relies either on big bang type of reforms where municipalities were merged according to

¹From a normative perspective, there are a number of additional advantages to larger municipalities, such as the possibility to provide a wider range of public services and goods (Oates, 1972), the ability to internalize externalities (Eichenberger and Frey, 2006), and an improved resilience to macroeconomic shocks due to resource pooling (Hinnerich, 2009).

the central government's design at the same date (see for example Reingewertz (2012) who analyzes the Israeli merger reform of 2003) or on entirely voluntary mergers (see for example Lüchinger and Stutzer (2002) for Switzerland and Moisio and Uusitalo (2013) for Finland).

Our first result is that mergers result in economies of scale for total expenditures. Already in 2004, one year after the conclusion of the merger reform in Brandenburg, expenditures are lower in merged municipalities than in the pre-reform period. The negative effect persists to the second year. Second, we analyze different expenditure items. Here, we find that current and administrative expenditures decline after the conclusion of the reform, but not staff expenditures. Third, we establish that the cost savings are mostly due to enforced mergers. While voluntary mergers, too, have a negative effect on expenditures in the baseline regressions, these effects are noticeably smaller than for compulsory mergers, suggesting municipalities that merge voluntarily experience smaller cost savings. In addition, the results for voluntary mergers are also less robust than for compulsory ones. Consequently, one reason why the previous literature on scale economies due to municipal mergers is ambiguous seems to be that different types of merger processes were analyzed.

We also explore whether the declining expenditures in amalgamated municipalities are the result of lower public service provision rather than a consequence of cost savings. That is, we analyze whether municipal service provision changes as a result of mergers. We do not find evidence for service cuts. Therefore, economies of scales on the local level seem to be present.

To our knowledge, Fritz's (2011) study of the merger reform in the West German state of Baden-Württemberg in 1975 is the only one exploring municipal fiscal outcomes of merger reforms in Germany to date. Our paper, in contrast, is the first attempt to causally evaluate an Eastern German merger reform. Moreover, it provides the first comparison of scale effects between voluntary and forced mergers in the literature.

The remainder of this paper is structured as follows. Institutional details are discussed in section 2, in particular the merger reform and its influence on municipal structure in Brandenburg. Section 3 gives an overview of the existing evidence on the fiscal effects of municipal mergers. In Section 4, we discuss some methodological issues. Subsequently, we provide a brief theoretical discussion of the link between mergers and scale economies in Section 5. Section 6 introduces our empirical approach. Section 7 gives a concise overview of the data. The results are discussed in Section 8. We conclude in Section 9.

2 Institutional background

2.1 Local governance structure in Germany

Germany has a complex federal structure of governance, but consists mainly of three governmental tiers: in addition to the national government, there were 16 federal states and 11 292 municipalities at the end of 2011 (Statistisches Bundesamt, 2013). The units of analysis here are the municipalities in the East German federal state of Brandenburg. Article 28 II of the German constitution (*Grundgesetz*) guarantees all municipalities the freedom and the right to run their own affairs. No further details are codified, and hence municipalities fulfill services like sewage disposal or fire safety to different degrees, but are obliged to assume several core tasks according to instructions from the state government, such as civil register's office services. Generally, German municipalities have, besides significant responsibilities at the expenditure side of the budget, also considerable revenue autonomy, including measures like user charges, several taxes like the trade tax (*Gewerbesteuer*) or property taxes (*Grundsteuer*) as well as remunerations for benefits and services (Zimmermann, 1999). In summary, municipalities in Germany are important and multipurpose economic actors².

2.2 Municipal merger reform in Brandenburg

2.2.1 Reasons, aims and process of the reform

In order to achieve a more efficient local government structure, many West-German states implemented fundamental merger reforms in the 1960s and 1970s (Kauder, 2011). After the German reunification, East German states like Brandenburg followed the western role models.

In Brandenburg, the Ministry of Interior is in charge of local governments. The ministry's policy was always supportive to mergers, both on the municipality and district level. Despite the installation of municipal associations in 1992 and the reduction of rural districts in 1993 from 38 to 14, state officials continued to diagnose a chronic lack of administrative capacity and efficiency (Landtag Brandenburg, 2012). Particularly, 58.2% of the municipalities in Brandenburg had less than 500 residents, and especially these very small rural communes have been perceived to fulfill their public service obligations neither sufficiently nor efficiently (Ministerium des Innern Brandenburg, 2011).

²For a more comprehensive review of the German federal system, see Zimmermann (1999).

Therefore, municipal mergers were seen as an instrument to achieve cost savings.³ Mergers were expected, in particular, to achieve higher efficiency in terms of fiscal and administrative capacities of the municipalities (Landtag Brandenburg, 2000)⁴. Immediately after the state elections at the end of 1999, the new state government agreed on a comprehensive merger reform. Subsequently, the state parliament instructed the government to suggest a concrete concept by mid-2000. Once agreed upon, the reform had high priority and was implemented quickly. Particularly, the government proposed a concept called “guidelines for the development of rural structures” (*Leitlinien der Landesregierung für die Entwicklung der Gemeindestruktur im Land Brandenburg*) in July 2000, which was passed in September 2000 by the state parliament. It outlined how mergers had to be organized. The timeline of the reform is sketched in Figure 1.

First, a voluntary merger phase was initiated, which lasted until the 21th March 2002. To support voluntary mergers, changes in municipal laws by the so called “municipal reform law” (*Gemeindereformgesetz*) complemented the guidelines in March 2001 (Ministerium des Innern Brandenburg, 2001). The latter provided a financial incentive scheme for municipal mergers within the voluntary time period with a maximum premium of 2.5 million Euros from the state’s government. The incentive scheme was mainly based on the selection criterion of a municipal minimum size of 500 residents, i. e. municipalities with less than 500 inhabitants were eligible for mergers.⁵ Municipalities were allowed to choose with whom to merge as long as the new municipality conformed to a number of requirements set by the state government (such as minimum population size). All assets, debts and administrative workers of amalgamating pre-reform municipalities were to be taken over by the respective post-reform municipality.

On March 2003, the state government decided on six “laws of reorganization” (*Neugliederungsgesetze*) to finally force all eligible municipalities to merge that had not been merged yet or had not applied for merging until the end of the voluntary period (Landtag Brandenburg, 2003). The remaining mergers became effective by 26th October 2003 in line with the selection criteria and the municipal merger reform was finally put to an end.

The reform and its implementation were extremely controversial. Massive objections against the legal enforcement of the final mergers occurred at the end of reform, resulting in 255 constitutional complaints at the state’s constitutional court (Verfassungsgericht

³An unsuccessful initiative in support of voluntary mergers was launched in the beginning of 1998 (Landtag Brandenburg, 1998). The few resulting voluntary mergers by this act and their effects are addressed in the robustness checks of Section 8. Particularly, we obtain similar results regardless of whether the prior mergers are considered in the analysis or not.

⁴Another aim of the reform was a higher local democratic participation since the number of candidates contesting local elections was often insufficient (Ministerium des Innern Brandenburg, 2001).

⁵Other selection criteria were the administrative status, i.e. whether a municipality belongs to a municipal association (see below) as well as the municipal location (Landtag Brandenburg, 2000). However, population size remains the most important selection criterion.

Brandenburg , 2006). The main concern of the complaints were based on the municipal right for local autonomy. However, except for two formal mistakes, no revisions to the reform were made until the end of the lawsuits in 2006 (Verfassungsgericht Brandenburg (VGB), 2006; LDS, 2005)⁶ .

2.2.2 Descriptive statistics on municipal mergers

The merger reform reduced the number of municipalities substantially, from 1479 down to 421 (Table 1). Mergers occurred also before the reform, but the reform increased the numbers of completed mergers drastically (Figure 2). Specifically, 98 of the completed mergers occurred 2003 on a compulsory basis as a result of the reorganizational laws. The number of municipalities remained stable after the end of the reform⁷. During the reform, 1320 municipalities merged to 262 larger units, with the merged units encompassing from 2 up to 22 pre-reform municipalities. 159 municipalities, on the other hand, were left unaffected (LDS, 2005). Particularly, the share of small jurisdictions with less than 500 inhabitants declined from 58.2% in 1999 to 1.4% of all municipalities in 2004. Average municipal population size rose from 1759 in 1999 to 6099 inhabitants at the end of 2004. This is still a relatively small number compared to the German municipal average of 7114 inhabitants at the end of 2011 (Statistisches Bundesamt, 2013) or compared to other countries (Fox and Gurley, 2006, p. 3). Brandenburg’s municipalities kept a large variance in their population size ranging from 411 to 145,707 residents (Landesbetrieb für Datenverarbeitung und Statistik, 2005).

3 Evidence on fiscal effects of municipal mergers

3.1 Descriptive evidence

Several papers investigate economies of scale at the local level, but find at best mixed evidence (Byrnes and Dollery, 2002; Fox and Gurley, 2006). Methodological problems of the respective studies are a plausible reason for the inconclusive results. Fox and Gurley (2006) review the empirical literature on the fiscal effects of jurisdictional size on subnational governments, concluding that the fiscal benefits of municipal mergers are hard to measure and depend on a variety of factors. For instance, empirical evidence uses often only municipal cross-sections and can thus not distinguish between the true impact of jurisdictional size on fiscal outcomes and the influence of other possible time-constant

⁶According to Verfassungsgericht Brandenburg (2006) most trials started in mid-2003.

⁷Only two further mergers took place after the end of the reform (Amt für Statistik Berlin-Brandenburg , 2013).

factors related to the fiscal stance of municipalities. Therefore, cross-sectional studies face a problem of endogeneity which bias the estimate of economies of scale (Besley and Case, 2000).⁸

3.2 Quasi-experimental evidence

Only recently, municipal merger reforms have been used in the empirical literature to credibly estimate the effect of a municipality's size on fiscal outcomes. In the following, we discuss the quasi-experimental literature on municipal mergers⁹. Two different strands of the respective literature can be distinguished, studying effects on either pre- or post-merger budgets.

3.2.1 Evidence on pre-merger budgets

Policy makers are typically interested in economies of scale that unfold in the aftermath of mergers. But opportunistic pre-merger behavior may lower the intended post-merger economic benefits and therefore deserves to be mentioned, even if we are interested in the post-reform effects of mergers in this paper. Opportunistic behavior on pre-merger budgets occurs as the anticipation of getting amalgamated gives municipalities an incentive to free-ride on the debt or the tax base of the expected post-reform municipality. This behavior entails a common pool problem (Jordahl and Liang, 2010).

The respective evidence focuses primarily on Scandinavian countries such as Denmark (Blom-Hansen, 2010; Welling-Hansen, 2012) or Sweden (Jordahl and Liang, 2010; Hinnerich, 2009). The results support the existence of common pool problems, especially in the final year before the merger. However, the findings differ regarding whether the size of the common pool is relevant, i.e. the number of participating municipalities. Only Hinnerich (2009) and Welling-Hansen (2012) find evidence that the size of the common pool matters.

Opportunistic behavior which lowers post-reform merger outcomes has been found for expenditures (Welling-Hansen, 2012), debt and assets (Jordahl and Liang, 2010; Hinnerich, 2009) or budget overruns (Blom-Hansen, 2010). All of these studies use a DD estimator on panels of pre-reform municipalities. However, common pools are only one cause for higher pre-merger spending as transition costs during mergers can occur, for e.g. by preparations for merging offices or streamlining different management systems.

⁸For Germany, Blume and Blume (2007) offer a cross-sectional analysis of the fiscal consequences of mergers in West-Germany.

⁹Table A.1 of the appendix gives an overview of the respective studies.

3.2.2 Evidence on post-merger budgets

The second strand of the literature tries to answer whether mergers entail economies of scale. Nevertheless, the results in this literature are also ambiguous. Moisio and Uusitalo (2013) as well as Lüchinger and Stutzer (2002) analyze voluntary municipal mergers in Finland and Switzerland, respectively. Both studies find little evidence for economies of scale due to municipal mergers. Moisio and Uusitalo (2013) find that total expenditure growth per capita is even in the long run, i.e. up to ten years after merging, much higher for amalgamated municipalities than for non-amalgamated ones. Lüchinger and Stutzer (2002) analyze merger-induced expenditure effects in obligatory core administration tasks. Using a DD estimator, the authors also find no economies of scale, although the studied municipalities are with 500 inhabitants on average very small.

The results, however, seem to be different for mergers which have been enforced by a higher tier of government like in Sweden, Denmark or Israel. Specifically, Blom-Hansen et al. (2011) and Welling-Hansen et al. (2012) find improved fiscal outcomes for a Danish merger reform in 2007 while using again a DD estimator. Blom-Hansen et al. (2011) analyze administration costs, which are derived by deflating administration expenditures by an index of compulsory municipal tasks and find substantial economies of scale effects as a result of mergers. Welling-Hansen et al. (2012) use measures such as budgeted and realized operational results. The results indicate that fiscal management has improved for budgeted and realized operational results after four years, although there have been negative effects just after the merger. Finally, Reingewertz (2012) uses the Israeli merger reform of 2003 to implement a DD strategy. The results indicate an 8% decrease in total expenditures after the reform for amalgamated municipalities compared to non-amalgamated municipalities.

Fritz (2011) is to our knowledge the only quasi-experimental study on post-merger fiscal outcomes for the German federal states. This author explores the municipal merger reform between 1967–1975 in the state of Baden-Württemberg. He finds that mergers increase municipal debt and total expenditures, but also identifies economies of scale for administrative staff expenditures.

Overall, the evidence is rather mixed. These mixed results can be due to different time periods or countries of analysis. The type of mergers, i.e. whether mergers occurred voluntarily or on a compulsory basis, might matter too. The advantage of our setting is that it can give a partial answer whether the type of merger – i.e. voluntary or compulsory – is a relevant determinant of whether scale economies emerge.

4 Empirical method

4.1 Evaluation problem

Our aim is to draw causal conclusions about the impact of municipal mergers on the costs of providing local public goods and services. Ideally one would compare the counterfactuals of being merged and not being merged for the same observational unit at a given time to single out the true budgetary effect of mergers. Unfortunately, such counterfactual analysis is impossible in the real world. Causal identification would still be possible if participants in municipal mergers were randomly distributed, i.e. if no statistical difference between the amalgamated and non-amalgamated existed. However, amalgamated municipalities are not randomly chosen. Therefore, one needs to control for variables that drive the selection into treatment.

4.2 Difference-in-Difference approach

To account for selection issues, we employ the DD estimator. The DD estimator describes the difference between the within-group differences over time (Khandker et al., 2010). Particularly, the average gains over time of merged municipalities compared to non-merged units are estimated given the treatment, i. e. the average treatment effect on the treated (ATT) (Angrist and Pischke, 2009).

Hence, the net reform impact is defined as follows:

$$DD = (\bar{y}_{Amalg=1,t=1} - \bar{y}_{Amalg=0,t=1}) - (\bar{y}_{Amalg=1,t=0} - \bar{y}_{Amalg=0,t=0}) \quad (1)$$

which is the difference of average outcomes of amalgamated and non-amalgamated after treatment minus the respective difference in average outcomes across groups before treatment.

One crucial identifying assumption of the DD estimator is the existence of parallel trends, i. e. that groups would follow the same time trend in the absence of treatment (Angrist and Pischke, 2009). Hence, merged and non-merged municipalities are supposed to have similar expenditure trends in the pre-reform period. The assumption seems reasonable as all municipalities are part of the same federal state and is further verified in the plots of Figure 3.

A related DD assumption is that the selection into treatment is quasi-random, or more formally that the error term is uncorrelated with the treatment status. Endogenous policy choice, which jointly determines mergers and expenditure outcomes by omitted factors, would thus raise a selection bias of unknown direction (Besley and Case, 2000).

In general, it is reasonable to treat the selection into treatment as quasi-random once we account for municipal and year fixed effects. The most important selection criteria employed by the state government for applying the merger treatment were location and municipal population size (Ministerium des Innern Brandenburg, 2011). Location is time-constant. Population size does also only change slowly. Cross-section fixed effects should thus adequately control for municipal size. However, we can further account for selection into treatment by including a set of time-varying control variables, such as actual population size.

Voluntary mergers raise self-selection issues on top of the selection issues of enforced mergers. Municipalities that endogenously select with whom and when to merge might differ both in observable and unobservable variables. However, voluntary mergers have to be seen against the background of potential later enforcement if municipalities do not merge, which gives them only a higher degree of voluntariness (Kauder, 2011). In other words, voluntariness implies in our setting that municipalities could choose with whom to merge but not whether to merge. Consequently, municipal fixed effects together with a set of time-varying control variables should be sufficient to account for (self-)selection into treatment.

5 Economies of scale in local public goods provision

5.1 Definition of economies of scale

Economies of scale can be defined in terms of the input-output relationship in local public goods provision. Particularly, positive economies of scale are achieved if increasing all inputs results in a disproportional growth in service provision. This holds vice versa for a rise in service outcomes. However, the necessity that all inputs are variable is problematic as in reality some factors of production, like existing employment contracts of administrative staff or capital assets in service provision are fixed, at least in the short run (Fox and Gurley, 2006). In what follows, we prefer a less strict definition in which economies of scale only rely on a relation between average costs of production and output (Zimmermann, 1999). Accordingly, economies of scale mean that larger municipalities may produce a given output or set of outputs with lower average unit costs than smaller jurisdictions.

5.2 Reasons for economies of scale

Declining average costs of production for a given service outcome are mainly due to a higher specialization of larger localities which boosts up their productivity of local

public goods provision. Thus, municipalities being merged to a larger entity might reap synergies. For instance, they enhance their bargaining power vis-à-vis externals (for e.g. private suppliers of inputs) and reduce their respective purchase prices (Dollery and Fleming, 2006). Moreover, duplicates of identical institutions can be merged together, which reduces fixed expenditures in per capita terms while keeping the same service levels (Pitlik and Wirth, 2012). Often particularly small municipalities are not able to provide obligatory services on their own because of missing professionalism and organizational size. Therefore, higher demands for administrative services implied by merged units may reveal synergies and make use of a more specialized personnel staff. Intuitively, lower costs also result over time by an increasing productivity due to higher experience and learning effects the more the municipality produces. On the one hand, larger municipalities have a higher capacity via being more productive in both service quality and quantity given the same expenditures. On the other hand, municipalities experiencing economies of scale could also reduce expenditures for a given service level and quality.

5.3 Reasons for diseconomies of scale

Even though mergers are often initiated to reap economies of scale, they may in fact entail diseconomies. Unit costs of production might rise after some municipal size due to enhanced managerial incapability's in terms of higher coordination and communication costs (Coase, 1937). The administrative body entails higher transaction costs the bigger it gets (Tullock, 1969). New and more service demands have to be taken care of by the amalgamated municipality which calls for a bigger, more diversified and specialized administration. This new complex is more difficult to manage as both the information and control costs of the local decision makers rise. Also the replacement of formerly voluntary and part-time workers with professionals enhances service quality and specialization, but necessitates higher staff expenditures. Moreover, specialization might enhance operational costs due to the purchase of more up-to-date technical equipment in an enlarged administration.

Municipal size may also increase informational asymmetries between actors on the local level. On the one hand, bureaucrats may maximize their budgets (Niskanen, 1968) or slack (Wyckoff, 1990) above an efficient level as politicians and voters are not fully able to control the activities of the administration. On the other hand, politicians may free-ride on the informational advantage over their voters by acting in line with selfish goals, contrasting the aim of maximizing social welfare (Oates, 2005). The larger the entity, the larger are the monopoly powers of both administration and politicians and the lower is the ability of the voters to control the policies and services provided. Moreover, higher jurisdictional size lower the chances of residents to compare local politics to similar

communes in terms of yard-stick competition (Besley and Case, 1995). Therefore, control and sanctioning costs of non-benevolent behavior may rise given larger jurisdictions.

Amalgamating political units reduces not only political accountability by means of enhanced monopoly power but also reduces inter-municipal competition due to a lower number of competing municipalities. Citizens might be less able to reveal their preferences via voting by feet, i.e. migrating to the locality with the utility-maximizing mix of services and taxes (Tiebout, 1956) as distances are bigger in amalgamated units and migration costs are higher both in terms of monetary units and time costs. Inhabitants suffer from frustration in large entities as it takes longer and is also more costly to access public services. Moreover, services are more likely to be removed from people's preferences in enlarged municipalities due to lost closeness (Oates, 1972).

Budgeting mechanisms may increase expenditures as well. Pooling pre-merger unit budgets to a single post-merger budget can cause so called fiscal illusions, making revenue sources more complex (Dollery and Worthington, 1996). Thus, opportunistic behavior of politicians may raise expenditures. Rising expenditures and debt levels also occur because of politicians' anticipation of mergers which establishes a common pool of post-merger resources on which pre-merger units can free-ride (Hinnerich, 2009).

Other costs of large entities are agglomeration or crowding costs like a higher criminality assuming that the local service offered is at least partially rivalry. As this is true for almost all local public goods, one can expect diseconomies to arise after some threshold of size for either overall budgets or most public goods (Reiter and Weichenrieder, 1997).

However, interpreting higher expenditures per capita with respect to scale effects might be ambiguous too. Because of economies of scale, enlarged communes offer a relatively high service quality which may lead to higher respective expenditures per capita than in smaller units given a relatively price elastic demand Büttner and Holm-Hadulla (2013). Therefore, higher per capita spending does not necessarily imply diseconomies of scale but can also suggest a higher demand for public services.

5.4 Empirical implications

Whereas it is theoretically ambiguous if economies of scale emerge because of municipal mergers, one can say something about the expected fiscal consequences. It is plausible that any fiscal gains of mergers accumulate over time since at first costs of transition might occur. Whereas pure financial costs might arise just in the short-term, for e.g. by purchasing new office facilities for the merged local councils, re-organizational costs might last longer. Especially the layoff of administrative staff as an important source of cost savings may occur only in the long run due to continuing employment contracts. Hence,

economies of scale can be expected to rise over time and, at least for some expenditure items, to unfold only after some time after the municipal mergers.

Economies of scale depend on expenditures and both output quality and quantity. Whereas expenditures are observable, one has to be cautious when measuring service outputs. Due to their non-rival character one cannot put prices on public goods, no quality level can be observed and even the output quantity cannot be measured comprehensively, as municipal tasks are manifold and often carried out by not only the local but also other governmental tiers (Büttner et al., 2009). Thus, we cannot measure the actual production costs but we will approximate changes in service provision due to mergers by indirect measures like births and inter-municipal migrations which account for the socio-economic attractiveness of a municipalities. Particularly, incoming migration captures Tiebout’s idea of people’s sorting into a relatively preferred locality (Lüchinger and Stutzer, 2002). Moreover, we control for recreational area and streets as further outputs.

6 Empirical specification

6.1 Benchmark models

Our benchmark results establish the aggregated treatment effects of all amalgamated municipalities after the end of the official reform, i. e. starting from 2004 (given that the reform was completed in October 2003). After estimating these baseline models, heterogeneity of treatment effects across post-reform years and types of mergers are considered to gain further insight about the fiscal consequences of the merger reform.

Given our multiple period panel, we use a regression framework to implement the DD estimator. More specifically, we estimate the following model:

$$y_{it} = \beta_0 + \beta_1 Amalg_i + \beta_2 Post_t + \beta_3 (Amalg * Post)_{it} + \epsilon_{it} \quad (2)$$

where y_{it} defines one of the expenditure items of interest for municipality i and year t ¹⁰. The binary variable $Amalg$ equals 1 for all periods if the respective municipality has been amalgamated within the reform period. Otherwise it is 0. The dummy $Post$ is set to 1 for all years from 2004 onwards and to 0 for all the years before. Accordingly, $Amalg * Post$ is the interaction of the dummies for merger and post-reform periods, i.e. being 1 if both $Amalg$ and $Post$ equal 1. As we are interested in post-reform budgetary outcomes for amalgamated municipalities, the latter interaction denotes the treatment effect to be

¹⁰Furthermore, outcomes are also measures of service production and quality in Table 10.

estimated¹¹. ϵ_{it} is a normally distributed error term. We refer later to regression (2) without fixed effects as the ordinary least squares (OLS) model.

As mentioned previously, one may include further exogenous controls X_{it} in the regressions to ensure consistent estimates. Therefore, equation (3) modeled as

$$y_{it} = \beta_3(Amalg * Post)_{it} + \delta X_{it} + \alpha_i + \phi_t + \epsilon_{it} \quad (3)$$

contains time-varying observables to account for group-specific trends. Moreover, we include municipality fixed effects (α_i) and year fixed effects (ϕ_t). The variables *Amalg* and *Post* are displaced because of perfect linearity with the respective fixed effects. Note that we will refer to the linear regression with fixed effects in equation (3) in the later analysis as the fixed effects (FE) model.

To account for heteroscedasticity and autocorrelation, we always report results based on robust and clustered standard errors (Bertrand et al., 2004). Standard errors are clustered at the municipal level.

6.2 Effect heterogeneity

Beside the aforementioned OLS and FE regressions, we will provide some additional specifications to gain further insights on the nature of the treatment effects. Firstly, we estimate year-specific treatment effects, which allows for heterogeneous between-year merger effects. Particularly, year dummies for the post-reform years 2004 and 2005 are included in the preferred FE models instead of the aggregated *Post* dummy. Second, we distinguish between voluntary and enforced merger effects in an FE regression framework by replacing the aggregate of all amalgamating municipalities *Amalg* with the respective group dummies *Voluntary* and *Forced* (Table 2). Again, we use the year-specific treatment effects specification to show the evolution of treatment effects across the two types of mergers.

Finally, we study whether the reform caused changes in service provision to establish that the expenditure reductions are due to economies of scale rather than due to changing service levels. Births and incoming migration as well as traffic and recreational area per capita on the local level are used as alternative outcome variables.

6.3 Robustness checks

We implement several robustness checks in order to explore whether the benchmark results of the regression framework are altered and hence the respective DD assumptions

¹¹Note that the treatment effect β_3 from the regressions (2) or (3) is equivalent to the DD estimate of equation (1).

are violated. Firstly, we introduce placebo tests by setting the reform date in the pre-reform years. Assuming similar budgetary trends to exist, we would expect no or only substantially smaller treatment effects (i. e. we might still observe treatment effects as the placebo post-treatment period encompasses the true post-treatment period, but these effects should be smaller). By using different historical control groups, we also account, in the spirit of Hinnerich (2009), for the fact that some municipal mergers in Brandenburg occurred even before the merger reform on a voluntary basis (for example, we report regressions where the control group consists of only municipalities that never merged). Furthermore, we provide several misspecification tests by changing the outcomes into logarithms or their levels (rather than per capita) as well as by including different time trends. Finally, we follow Reingewertz (2012) and implement another evaluation method, i. e. nearest neighbor matching. The matching estimator provides an alternative way to control for selection on observables.

7 Data

7.1 Units of observation

We use a panel of post-reform municipalities of the federal state Brandenburg for the years 1998–2005. The final analysis encompasses 258 post-reform municipalities to have been part of municipal mergers (treatment group) and 153 municipalities to be unaffected from boundary changes (control group). This sample is slightly smaller than the full universe of 421 post-reform municipalities (262 merged and 159 non-merged) since we drop ten municipalities for various reasons (see below). Overall, the municipal panel provides $411 \times 8 = 3288$ observations.

Defining the treatment to begin in the year 2004 leaves us with exactly six pre- and two post-reform years, thereby allowing us to study whether there are immediate spending effects of the voluntary and forced mergers. The panel is constructed by aggregating treated pre-reform municipalities to their post-reform size at the end of 2005¹². Therefore, budgetary outcomes for amalgamated units before the treatment are the sum of all respective pre-reform municipalities¹³.

¹²Aggregation was done by the *Statistisches Landesamt Berlin-Brandenburg* (SBB). Hence, pre-reform observational units are not available to us.

¹³Some old municipalities split up and amalgamated with different pre-reform units (Landesbetrieb für Datenverarbeitung und Statistik, 2005). It would be appropriate to split up the pre-reform communes according to their later post-reform municipality share (Blom-Hansen et al., 2011), but the SBB distributed pre-reform units to the largest incorporating unit. This may bias the results, but they are robust to dropping the splitting post-reform municipalities (not shown).

As indicated above, Brandenburg had 421 municipalities after the merger reform, including the district-free cities Frankfurt/Oder, Potsdam, Brandenburg an der Havel and Cottbus. The latter carry out both municipal and district-related tasks, but have no disaggregated budgets. As one cannot compare district-free with district-affiliated municipalities, the former are excluded from the sample. We also eliminate five municipalities which were under boundary changes after the official reform end in October 2003¹⁴. Moreover, the municipality Hohensaaten is excluded as no budgetary data at the level of the 2005 boundaries is available for this municipality. This leaves the final number of 411 municipalities for the analysis below.

7.2 Treatment status

Mergers are an ongoing process, but we require a clear cut definition of treatment for our analysis. We therefore define treated municipalities as the ones which completed¹⁵ their mergers within the reform period¹⁶. Control municipalities do not amalgamate within the reform but could have nonetheless merged before the reform.

We assume that the post-reform period starts in 2004 as this is the first fiscal year without mergers. Note that voluntary mergers within the reform do also occur before the end of the reform in October 2003, i. e. in the years 2000 to 2002 (Figure 2). However, this does not harm our estimation strategy below, but lets us interpret budgetary gains of merged municipalities as lower limits of the true reform impact. Given that voluntary mergers preceded compulsory ones, we will tend to underestimate economies of scale if we let the treatment start in 2004. However, we also explore further below models where we allow economies of scale effects to emerge before 2004.

There are also other reasons for the interpretation of the treatment effect as a lower bound. Firstly, only two post-reform years are analyzed but one would assume economies of scale to evolve over time. Moreover, numerous constitutional complaints were ongoing until 2006. The affected municipalities are likely to spend less effort in restructuring the local administration system toward a more efficient structure. This might be true due to their uncertainty of success at the state's constitutional court. Hence, lower economies of scale might be expected for these municipalities.

¹⁴These are the municipalities Königs-Wusterhausen, Heiligengrabe, Wittstock/Dose, Neuhausen/Spree and Spremberg (LDS, 2005).

¹⁵Note that we do not distinguish whether boundary changes occurred either in form of mergers between equals or in terms of incorporations of several smaller units by a larger locality.

¹⁶Some municipal cost effects could be realized before the merger is completed. But it is plausible that administrative restructuring begins only when the merger is finalized. Further below, we show explicitly that no significant cost reductions emerge before the mergers are completed.

Finally, some municipalities could have already exploited some potential economies of scale well before the reform by being part of municipal associations. The associations are an implicit government level acting on behalf of affiliated member municipalities. Whereas the number of association members dropped by 81.4% to 272 units, the number of municipalities independent from associations rose to 148 in 2004 during the reform (Landesbetrieb für Datenverarbeitung und Statistik, 2005). The mergers affected the propensity of municipalities to join associations. Some municipalities affiliated to municipal associations transitioned to the independent status, but no independent units became affiliated members. Such administrative change can be interpreted as an intermediate outcome of treatment and thus should not be explicitly controlled for.

7.3 Variables

Table 2 describes the outcome and explanatory variables. For our expenditure outcomes as well as for several exogenous control variables we make use of administrative data from the SBB. All budget measures are based on yearly realized municipal accounts¹⁷ but due to data limitations only input-oriented budgets are accessible, i. e. revenues and expenditures are not differentiated according to tasks but only according to generic budget numbers. Variables are measured by per capita values to estimate the respective average cost effects per unit of output. Moreover, monetary units are deflated by the consumer price index on the district level and are expressed in constant Euros with the base year 2005.

In line with our aim to detect expenditure changes as a result of mergers the dependent variables chosen are total and staff spending in per capita terms as well as administrative and current expenditures per capita. Whereas staff expenditures might be sluggish as employment contracts of pre-reform municipalities were taken over by the respective post-reform units, administrative and current expenditures are open to substantial and immediate scale effects by mergers. Specifically, we expect declining expenditures for amalgamated municipalities in the present context as the municipalities in question are relatively small and might thus experience substantial economies of scale, notwithstanding the arguments for potential diseconomies of scale due to mergers as discussed in Section 5.

Several time-varying controls are included in the analysis covering both economic and political variables to avoid omitted variable bias. Particularly, we include revenues (grants and total revenues per capita), demographics (share of above 65 year olds), population size and density as well as variables of the local economy like unemployment rate and lo-

¹⁷Note that the territorial state of the so called *Jahresrechnungsstatistiken* is the end of the year 2012.

cal GDP. To account for any non-linear relationship between expenditures per capita and population size, the squared value of the population size is introduced as a control variable. Considering politics, we control for the political ideology variable *Left*. Moreover, a dummy for municipal location (*capital region*) is included as a time-constant control variable. Specifically, it defines the municipal proximity to the federal capital Berlin to account for regional spillover effects. This time-constant factor is only of importance for the regressions without fixed effects included in the later analysis. However, the fixed effects model controls for time-invariant unobserved heterogeneity.

Table 3 provides summary statistics for dependent and control variables in 1999, which is the last year before the first mergers began. Regarding the municipal expenditure levels arise several interesting features. Whereas staff spending per capita is not significantly different between amalgamated and non-amalgamated municipalities, other municipal expenditures are unbalanced across groups. Total, administrative and current expenditures per capita are significantly higher in the pre-reform period for merged units than for non-merged ones.

8 Results

8.1 Graphical evidence

First, we illustrate the treatment effect graphically over the period 1998–2005. Figure 3 shows how mean total and disaggregated municipal expenditures per capita evolve over time, comparing the respective spending levels for amalgamated and non-amalgamated municipalities. Clearly, expenditures of amalgamated municipalities drop more sharply than of non-amalgamated ones in 2004, i. e. in the first post-reform year.

Mean total expenditures for merged units have been consistently higher than for their non-merged counterparts, but this gap has lowered somewhat in the course of the reform. However, also non-merged units experienced decreasing expenditures in the post-treatment period, which suggests the presence of common time trends. Similarly, the respective pre-reform trends for merged and non-merged municipalities in average expenditures are similar. Therefore, the parallel trend assumption seems to be fulfilled. The same holds for staff, administration and current expenditures per capita.

Note that reductions of mean expenditures seem to have taken place already in 2003, the year of the official end of the reform. As the later analysis will show, some treatment effects via the earlier voluntary mergers prior to the compulsory ones are presumably responsible for this decline. Nevertheless, the illustrations presented here are only de-

scriptive with respect to potential treatment effects, because exogenous controls, year effects and municipality fixed effects are not included.

8.2 Regression based results

8.2.1 Benchmark models

Treatment effects for total expenditures per capita

Table 4 shows the results of various regression models using the outcome of total expenditures per capita. Mergers significantly reduce the aggregate expenditures per capita. Hence, economies of scale appear to exist. The treatment effect is -59.74 Euros per capita and almost significant in column 1. Controlling for municipal location, population and revenue measures in column 2 increases the significance of the estimate considerably. The inclusion of socioeconomic and political controls in column 3, however, drives the treatment effect up to more than 71 Euro per capita. Supplementary municipality fixed effects (column 4) or year fixed effects (column 5) do not change the magnitude of the results significantly, although additional municipality fixed effects seem to increase the significance level of the results slightly. Column 6 shows the preferred model, including both municipality and year fixed effects as in equation 3 above. The respective treatment effect amounts to -72.98 Euros per capita and is significant at the one percent level.

Other than the treatment effect of interest, i.e. the interaction of amalgamated municipalities and post-reform years, also the corresponding dummies *Amalg* and *Post* are significant and may explain the outcome variable. As *Amalg* measures the pre-reform difference in the outcome between amalgamated and non-amalgamated municipalities, the large positive and highly significant coefficients indicates that amalgamated municipalities had higher expenditures on average. The dummy for the post-treatment period *Post* shows that general time trends are also of importance. Moreover, the negative signs of both the treatment effect and the time dummy indicate an overall decline of total expenditures in the post-reform years as shown in subfigure (a) of Figure 3.

Treatment effects for expenditure items

Similar to column 3 of Table 4, we now consider OLS models on the disaggregated expenditure items staff, administrative and current expenditures per capita using a full set of controls (Table 5). Except for staff expenditures per capita, which show a slightly positive, but insignificant treatment effect, all expenditure measures reveal substantial and significant negative treatment effects. Whereas administrative expenditures decline by 13.15 Euros per capita, both total and current expenditures come down by about 73.06 Euros per capita.

Considering the control variables, the dummy *capital region* for municipalities being located in the metropolitan area of Berlin shows lower expenditures in total, administrative and current measures. Therefore, it seems plausible that municipalities neighboring Berlin experience a positive regional spillover and lower their supply of local public goods while free-riding on Berlin’s services. Population size and its squared value are significant but of a different sign. This supports the U-shaped relationship of population and expenditure per capita reported in the literature as the squared population sign is close to zero. Some other control variables seem also to be relevant, but it is unclear whether they can be interpreted causally. Still, it is reassuring that the models with time-varying control variables support the baseline results.

By including municipality and year fixed effects, we study the fixed effects model from Table 4 column 6 with a full set of covariates (Table 6)¹⁸. Municipal fixed effects controls for unobserved heterogeneity between municipalities. The inclusion of year-effects accounts for time-specific shocks which are common to both amalgamated and non-amalgamated units. The FE results indicate similar fiscal effects of mergers as the OLS ones. In fact, some significance is gained in the estimated treatment effects for total, administrative and current expenditures. The variables *Amalg*, *Capital region* and *Post* are dropped because of perfect linearity in the respective fixed effects.

8.2.2 Effect heterogeneity

Heterogeneous treatment effects across years

Using the preferred FE models, we now allow for year-specific treatment effects for each post-reform year to observe the progress of the reform effects (Table 7). We do this by including an interaction of the group dummy with a dummy for every year after the merger, namely *Amalg*2004* and *Amalg*2005* (denoted by treatment effect 2004, 2005) instead of the aggregate treatment effect *Amalg*Post*. One would assume that potential scale economies need time to evolve and hence to observe a rising amount of expenditure reductions. However, this is only in place for total expenditures where the treatment effect grows from -56.24 in 2004 to -89.71 Euros per capita in 2005. The effects on both administrative and current expenditures per capita are similar for each of the post-reform years. Generally, we note that the treatment effects unfold quite quickly as substantial effects are already visible one year after the conclusion of the reform. Staff expenditures per capita stay insignificant after the reform.

¹⁸Later on in the text we will refer to these models in Table 6 as the benchmark models.

Heterogeneous treatment effects across types of mergers

We also distinguish between the reform effects of both voluntary and forced mergers (Table 8). Therefore, interactions of the respective merger type dummies with the years 2003 to 2005 are introduced, given that voluntary mergers happen already before 2004.

We observe negative and significant treatment effects for voluntary mergers already in 2003 for total, administrative, and current expenditures. As expected, corresponding treatment effects for compulsory mergers only emerge in 2004. Overall, however, treatment effects unfold rapidly for both compulsory and voluntary mergers

The magnitude of the treatment effects is smaller for voluntary than for compulsory mergers. For voluntary mergers, the treatment effects also seem to be declining over-time while they are rising for compulsory mergers. This pattern suggests that some organizational costs emerge in the initial year for compulsory mergers. The opposite pattern for voluntary mergers suggests that either the ability or the incentives to exploit economies of scale declines over time for municipalities that have merged voluntarily.

One reason why treatment effects seem lower for voluntary mergers might be that they are a bad selection of merging municipalities as municipalities might merge voluntarily because they are fiscally weaker and needier than those who are being merged later on. The declining scale economies for voluntarily merged municipalities might emerge because these municipalities have already exploited their potential economies of scale by the second year and thus be again on a rising cost path. Unfortunately, we cannot explore this issue in more detail given the limited time dimension available in our dataset. But we conclude that, in any case, the reform impact is different for voluntary and compulsory mergers, with the latter leading to substantial reductions in expenditures and the former having a noticeably smaller effect.

An additional difference between compulsory and voluntary mergers is that for the former, the treatment effects are most pronounced for current spending whose decline can almost entirely explain the cuts in total expenditures. For voluntary mergers, the decline in current expenditures is a relatively less important explanation for the decline in total expenditures.

Do service levels change because of mergers?

Another concern is that expenditure cuts as a result of mergers are not due to scale economies but due to declines in service output or quality. Unfortunately, we cannot measure output comprehensively while the quality of local public goods cannot be measured at all (Büttner et al., 2009). Lüchinger and Stutzer (2002) argue that the output or the factors of production are unlikely to change within the context of mergers as only the jurisdictional size increases in the short timeframe of the analysis. Whereas this argu-

ment might be valid, we will test here on the top of that whether mergers affect indirect outcomes of local public service provision such as live births or immigration per thousand inhabitants on the local level (Table 9)¹⁹. If local public goods provision declined as a result of mergers, one would expect the birth rates and municipal immigration to shrink as a result of a lower attractiveness of the municipality. We do not see any significant reform-induced changes in these service measures. Moreover, we test whether the outcome variable $\log(\text{use area})$, i.e. the logarithmized sum of municipal traffic and recreational area per capita has changed as a result of the municipal mergers²⁰. The estimated treatment effect is insignificant. Hence, we find no indication that expenditure reductions have been to the disfavor of service provision. Therefore, mergers indeed seem to entail economies of scale.

8.3 Robustness checks

8.3.1 Placebo treatments

A possible threat to the estimates might be the presence of pre-existing trends. Given these trends, we might obtain negative coefficient estimates for the treatment not because of the treatment itself, but due to some correlated pre-existing trend in expenditure reduction in terms of a continuous consolidation process. Concerning total expenditures per capita, Figure 3 for example shows declining expenditure levels before the treatment in 2004 and even prior to the introduction of the merger guidelines. Therefore, the benchmark models are tested for the validity of the parallel trend assumption by introducing a placebo treatment in the year 2000 (Table 10)²¹. Assuming no pre-existing trends in the outcomes, we expect significantly smaller treatment effects. Accordingly, we find no significant treatment effect for total expenditures per capita. Moreover, reductions in staff expenditures remain low and insignificant. The treatment effects for administrative and current expenditures are significant, but noticeably lower than at the true treatment year. In all likelihood, these estimates are only significant because the true post-treatment period overlaps to some extent with the placebo post-treatment period.²²

¹⁹For definitions and summary statistics of the respective variables see the tables A.2 and A.3 of the appendix.

²⁰We use the sum of these items as recreational area is usually small, provides low variation between units or over time and is also measured in hectare like traffic area (Bönisch et al., 2011).

²¹Mergers are rare here as in the year 2000 was only one completed merger with the municipality Teichland.

²²We obtain similar results by setting a placebo treatment in the pre-reform year 1999.

8.3.2 Different sub-control groups

There have been mergers prior to the merger reform, which are represented here to be part of the control group additional to those municipalities which have never been merged. One might therefore argue that control group members that have been some time under mergers before the reform might have selected themselves into non-treatment²³. Hence, we distinguish between control group members, which have not been merged since the year 1875 (henceforth, never merged) and those which have been merged at some time after that year²⁴. We choose 1875 as the cutoff because official records start in this year. Similarly, we distinguish between control group members that have experienced boundary changes after the conclusion of the reform and those that remained geographically intact.

Table 11 shows the restricted regressions comparing the treated with the aforementioned subgroups of control members. However, the respective expenditure items remain widely in line with the benchmark results in both magnitude and significance. The few mergers in the course of the decade before the merger reform as well as minor boundary changes of control units in the pre-reform sample period do not alter the results.

8.3.3 Misspecification tests

Another concern of the DD estimators is their dependence on the functional form. Therefore, robustness checks for misspecification are presented in the following (Table 12 and 13). However, log-linear models, i.e. using the natural logarithm of the outcomes instead of their levels, does not lead to substantially different results than the preferred FE models (Table 12).

Additionally we use in Table 12 the expenditure levels themselves (rather than in per capita terms). We find similar results to the benchmark regressions for both the estimated sign and statistical significance of the treatment effect.

Although common macro trends are already accounted for by the inclusion of year fixed effects in the preferred FE models, one can test whether different functional forms of time trends might alter the results (Table 13). We find that, regardless whether a linear, squared or square rooted trend is used, the sign and significance level of the treatment effects for total expenditures per capita are similar to the ones in the benchmark models. However, the magnitude of the treatment effect is higher if we include time trends instead of using yearly fixed effects. Whereas the latter account for common macro shocks, they do that unlike time trend variables in a non-parametric way. However, not allowing for any time effects also increases the estimated treatment effects.

²³As mentioned previously, see Hinnerich (2009) for a similar reasoning for the Swedish merger reform.

²⁴Data on timing of mergers are retrieved from LDS (2005) .

8.3.4 Nearest neighbor matching

Finally, we implement a matching design, which is an alternative selection on observables approach to linear regression. Panel (1) of Table 14 consequently uses only the outcome variables in levels (rather than per capita) as the selection criterion on which the municipalities are matched. We find that the results for administrative and current expenditures per capita change, i.e. their aforementioned treatment effects are smaller than in the regression design and insignificant. The results for total expenditures, however, are similar. In Panel (2), we match on the control variables from the benchmark analysis. In Panel (3), we match on both the respective expenditures in levels and the control variables. In Panel (2) and (3), current expenditures gain magnitude and turn out to be significant again. Hence, matching only based on outcome variables in levels seems to result in an omitted variables bias, at least for current expenditures. Additional time-varying control variables ensure similar baseline and matching results.

To explore the robustness of our results further, we divide the sample of municipalities into two subsamples, which compare either only voluntary mergers or compulsorily merged municipalities with all non-amalgamating municipalities. Then, nearest neighbor matching is repeated for both subsamples (Tables 15 and 16). The high treatment effects for the forced mergers in the FE models are confirmed at least for total and current expenditures, whereas their magnitudes for administrative expenditures drop somewhat.

The results for voluntary mergers, however, change significantly compared to the baseline results. The estimates for the merger treatment are much smaller and typically insignificant. Overall, the evidence for scale economies in voluntary mergers found in the baseline regressions is not robust to a reanalysis in the matching framework.

9 Concluding remarks

This paper explores the impact of municipal mergers in the German federal state of Brandenburg on municipal expenditure levels. DD estimations with a panel of (post-reform) municipalities in Brandenburg for the years of 1998–2005 confirm recent quasi-experimental findings of economies of scale outcomes as a result of compulsory municipal mergers such as Reingewertz (2012).

The preferred FE models show that amalgamated municipalities substantially cut expenditures compared to non-amalgamated ones if mergers are compulsory. The only exception is the item of staff expenditures per capita, which have not been reduced as a result of the reform. Given largely unaffected service levels by the reform, one can infer that economies of scale are present for total, administrative and current expenditures. Moreover, the spending cuts unfold rapidly, that is already in the first post-merger year.

On the other hand, we also find that economies of scale are smaller for voluntary than for compulsory mergers. The evidence for scale effects due to voluntary mergers is also not robust to different estimation methods. Hence, one reason why the extant literature on mergers is inconclusive appears to be that existing studies explore different types of mergers.

One concern with the results is the short post-treatment period. The analyzed years might be insufficient to both capture treatment effects of mergers on the local level comprehensively as well as to evaluate long-term effects. This lack of a longer post-treatment period might explain the insignificance of the mergers for staff expenditures. Due to the long-term nature of employment contracts, it might take some time before any treatment effects can be observed for this expenditure item. On the other hand, it is remarkable that substantial scale economies emerge already within one year after compulsory mergers for total, current, and administrative expenditures.

Future research on fiscal outcomes of merger reforms should nevertheless attempt to use longer post-treatment periods and a broader panel data set, preferably of pre-reform observational units. More focus should also be given to why compulsory mergers seem to be more effective than voluntary ones. While we have offered an explanation further above – i. e. that municipalities which merge voluntarily in our setting might be more prone to fiscal problems – a more detailed analysis of this issue would be welcome.

With respect to external validity, our results presumably carry over to other East-German States given the similarities between them and Brandenburg. Despite socio-economic differences between East and West Germany, our results should also be relevant for the western states given that local budgeting and merger processes are quite similar. However, note that we obtain different results than the only existing ex-post evaluation of a West-German merger reform by Fritz (2011). International comparisons are more difficult as the organization of the subnational government tier varies significantly even between industrialized countries. However, the fact that our results for the expenditure effects of compulsory mergers are still in line with findings from Denmark or Israel indicates that the results presented here can be transferred to other developed countries.

Given our findings, it might seem tempting for policy makers to make further use of the instrument of mergers to harvest scale effects and reduce costs. On the other hand, one should not neglect the non-monetary costs of mergers, such as less local democracy. Whether mergers are overall beneficial should, therefore, be decided on a case-by-case basis, taking into account all the benefits and costs.

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Size	Municipality by size				Change	
	1999		2004		Number	in %
<500	861	58.2	6	1.4	-855	-99.3
500-2000	423	28.6	214	50.8	- 209	-49.4
2000-5000	95	6.4	73	17.4	- 22	-23.2
5000-10000	47	3.2	55	13.1	8	17.0
10000-20000	28	1.9	46	10.9	18	64.3
>20000	25	1.7	27	6.4	2	8.0
Sum	1.479	100.0	421	100.0	- 1.058	-71.5

Source: Amt für Statistik Berlin-Brandenburg (SBB, 2013). Numbers use year end dates and include district-free cities.

Table 1: CHANGE OF PRE- AND POST-REFORM MUNICIPALITY SIZE

Variable	Measurement	Source
Dependent variables		
Staff expenditures pc	Staff expenditures divided by population (minus special financial operations)	Own calculations based on SBB
Total expenditures pc	Total expenditures are the sum of current and capital expenditure account divided by population (minus special financial operations)	Own calculations based on SBB
Administrative expenditures pc	Administrative expenditures measure materially administrative and operating expenses divided by population (minus special financial operations)	Own calculations based on SBB
Current expenditures pc	Current expenditures divided by population minus special financial operations and payments of the German social code 2 (SGB II)	Own calculations based on SBB
Institutional variables		
Amalg	Dummy=1 if post-reform municipality was under boundary changes between 20 th September 2000 – 26 th October 2003	Own calculations based on LDS (2006)
Voluntary	Dummy=1 if amalgamation is not enforced by six reorganization laws	Own calculations based on LDS (2006)
Forced	Dummy=1 if amalgamation is enforced by six reorganization laws	Own calculations based on LDS (2006)
Post	Dummy=1 if year \geq 2004	Own calculations based on LDS (2006)
AmalgPost	Dummy=1 if Amalg=1 and Post=1	Own calculations based on LDS (2006)
Exogenous controls		
Population	Number of inhabitants of a municipality	SBB
Population density	Number of inhabitants divided by the total municipal area	Own calculations based on SBB
Total revenues pc	Total revenues per capita divided by population (minus special financial operations and payments of the German social code 2)	Own calculations based on SBB
Old pthc	Share of population above 65 years per 1000 inhabitants	SBB
Capital region	Dummy=1 if affiliation to capital region of Berlin	SBB
Unemployment	Number of unemployed people divided by the employment force. Only district level available	Bundesagentur für Arbeit
Local GDP pc	Gross domestic product per capita. Only district level available	SBB
General grants pc	Unconditional grants from the federal state divided by population	Own calculations
Left	Number of socialist seats, i.e. SPD & PDS/Die Linke, as a percentage of the overall number of seats of local councils	Own calculations

Note: All variables are based on the territorial state of 31st December 2005 or are adjusted to that territorial state where such data have not been available. Budgetary variables are cleared by special financial operations as well as social transfers according to social code 2 (SGB II) if given. The latter is because of i) new municipal responsibilities by the introduction of the SGB II in 2005, ii) no discretion of municipalities in that matter and iii) the fact that social transfer payments are due to exogenous factors rather than changes in local politics. All economic variables are set to their real values by deflating the respective nominal values by the consumer price index with the base year 2005. Moreover, (pthc) pc figures refer to per (thousand) capita values. Exact account numbers are available from the author upon request.

Table 2: DEFINITION AND SOURCE OF VARIABLES

	Overall	Amalgamated	Non-amalgamated
Units	411	258	153
Dependent variables			
Total expenditures pc	1121.60 (381.33)	1172.05 (386.30)	1036.51 (358.20)
Staff expenditures pc	178.16 (107.26)	182.99 (111.39)	170.00 (8.06)
Administrative expenditures pc	172.46 (87.93)	185.00 (90.91)	151.32 (78.51)
Current expenditures pc	780.99 (206.05)	817.57 (219.90)	719.30 (163.27)
Exogenous controls			
Capital region	0.15 (0.36)	0.15 (0.36)	0.14 (0.35)
Population	5159.32 (7293.77)	5125.63 (6510.21)	5216.12 (8475.36)
Population density	110.32 (210.01)	60.64 (71.83)	194.08 (314.63)
General grants pc	304.85 (55.69)	303.46 (56.62)	307.20 (54.19)
Total revenues pc	1177.61 (390.95)	1213.14 (391.90)	1117.69 (383.17)
Old pthc	226.87 (32.69)	227.73 (31.64)	225.41 (34.45)
Unemployment	17.67 (2.80)	17.60 (2.84)	17.80 (2.74)
Local GDP pc	14235.38 (1359.40)	14223.36 (1406.16)	14238.79 (1281.19)
Left	23.75 (22.58)	18.89 (17.68)	31.94 (27.19)

Note: Standard deviations are in parentheses. The first column represents the mean of the dependent variables in the overall sample, whereas columns 2 and 3 depict the mean of these figures for amalgamated and non-amalgamated municipalities, respectively. Moreover, (pthc) pc figures refer to per (thousand) capita values.

Table 3: SUMMARY STATISTICS FOR BENCHMARK ANALYSIS, 1999

Dependent variable	Total expenditures pc					
	(1)	(2)	(3)	(4)	(5)	(6)
Amalg	117.38*** (30.40)	42.51* (23.02)	54.97** (23.93)		54.26** (23.89)	
Post	-54.08*** (20.19)	-64.27*** (15.41)	-68.60*** (19.41)	-37.57* (20.79)		
AmalgPost	-59.74 (37.19)	-60.04** (25.80)	-71.76** (29.49)	-72.91*** (25.07)	-71.63** (29.35)	-72.98*** (24.92)
Capital region		-123.63*** (42.40)	-99.22** (38.72)		-94.89** (39.13)	
Population		0.02** (0.01)	0.02** (0.01)	-0.05** (0.02)	0.02** (0.01)	-0.05* (0.02)
Population density		-0.11** (0.06)	-0.15** (0.07)	-0.16 (0.18)	-0.15** (0.07)	-0.14 (0.18)
Squared population		-0.00** 0.00	-0.00** 0.00	0.00** 0.00	-0.00** 0.00	0.00* 0.00
General grants pc		-0.97*** (0.27)	-1.06*** (0.28)	0.01 (0.27)	-1.08*** (0.30)	-0.09 (0.27)
Total revenues pc		0.37* (0.22)	0.36* (0.22)	0.23 (0.16)	0.36* (0.22)	0.23 (0.16)
Old pthc			0.68* (0.38)	-0.38 (0.44)	0.79** (0.39)	-0.25 (0.51)
Unemployment			0.88 (2.54)	8.99 (7.35)	1.38 (2.69)	-2.15 (9.91)
Local GDP pc			0 0.00	-0.01** 0.00	0 0.00	-0.01* (0.01)
Left			1.19** (0.58)	-0.06 (0.45)	1.17** (0.58)	-0.03 (0.45)
Year FE	-	-	-	-	+	+
Individual FE	-	-	-	+	-	+
N	3288	3268	3268	3268	3268	3268
R ²	0.02	0.4	0.41	0.19	0.42	0.21

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. All models include a constant, which coefficient is not reported. Robust standard errors clustered at the municipal level. Moreover, (pthc) pc figures refer to per (thousand) capita values.

Table 4: TREATMENT EFFECTS ON TOTAL EXPENDITURES PER CAPITA, OLS AND FE MODELS

Dependent variable	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc
Amalg	54.97** (23.93)	-6.66 (8.30)	20.14*** (7.67)	54.33*** (15.29)
Post	-68.60*** (19.41)	-6.94 (4.60)	-10.78** (4.79)	-2.70 (8.96)
AmalgPost	-71.76** (29.49)	1.68 (4.40)	-13.15** (6.27)	-73.06*** (13.27)
Capital region	-99.22** (38.72)	4.22 (13.47)	-14.58 (13.15)	-75.37*** (26.83)
Population	0.02** (0.01)	0.02*** (0.00)	0.01*** (0.00)	0.02*** (0.00)
Population density	-0.15** (0.07)	-0.00 (0.03)	-0.00 (0.02)	-0.09** (0.04)
Squared population	-0.00** (0.00)	-0.00*** (0.00)	-0.00*** (0.00)	-0.00*** (0.00)
General grants pc	-1.06*** (0.28)	-0.06 (0.06)	-0.09* (0.06)	-0.38** (0.17)
Total revenues pc	0.36* (0.22)	0.03 (0.02)	0.05* (0.03)	0.14* (0.08)
Old pthc	0.68* (0.38)	0.02 (0.10)	0.09 (0.10)	0.31 (0.19)
Unemployment	0.88 (2.54)	-0.47 (1.17)	2.98** (1.17)	3.86* (1.98)
Local GDP pc	-0.00 (0.00)	0.00 (0.00)	0.01*** (0.00)	0.01*** (0.00)
Left	1.19** (0.58)	0.07 (0.15)	0.10 (0.16)	0.24 (0.28)
Year FE	-	-	-	-
Individual FE	-	-	-	-
N	3268	3268	3268	3268
R ²	0.41	0.48	0.27	0.4

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. Robust standard errors clustered at the municipal level. All models include a constant, which coefficient is not reported. Moreover, (pthc) pc figures refer to per (thousand) capita values.

Table 5: AMALGAMATION IMPACT ON EXPENDITURE ITEMS, OLS MODELS WITH FULL SET OF CONTROLS

Dependent variable	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc
AmalgPost	-72.98*** (24.92)	-1.11 (3.61)	-17.29*** (5.56)	-79.15*** (11.71)
Population	-0.05** (0.02)	0.01* (0.01)	0.01 (0.01)	0.00 (0.02)
Population density	-0.14 (0.18)	0.09 (0.07)	-0.03 (0.08)	0.04 (0.15)
Squared population	0.00* (0.00)	-0.00*** (0.00)	-0.00 (0.00)	0.00 (0.00)
General grants pc	-0.09 (0.27)	-0.00 (0.02)	0.04* (0.03)	0.08 (0.14)
Total revenues pc	0.23 (0.16)	0.01 (0.00)	0.01 (0.01)	0.04 (0.03)
Old pthc	-0.25 (0.51)	0.16 (0.11)	-0.09 (0.13)	-0.11 (0.31)
Unemployment	-2.15 (9.91)	-6.02*** (1.66)	5.62** (2.5)	15.57*** (3.33)
Local GDP pc	-0.01** (0.01)	-0.00 (0.00)	0.00 (0.00)	-0.01** (0.00)
Left	-0.03 (0.45)	0.08 (0.07)	0.10 (0.11)	0.41 (0.25)
Year FE	+	+	+	+
Individual FE	+	+	+	+
N	3268	3268	3268	3268
R^2	0.21	0.07	0.05	0.16

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. Robust standard errors clustered at the municipal level. All models include a constant, which coefficient is not reported. Moreover, (pthc) pc figures refer to per (thousand) capita values.

Table 6: AMALGAMATION IMPACT ON EXPENDITURE ITEMS, FE MODELS WITH FULL SET OF CONTROLS

Dependent variable	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc
Amalg2004	-56.24** (28.21)	-0.16 (3.41)	-19.45*** (5.68)	-78.01*** (11.86)
Amalg2005	-89.71*** (27.43)	-2.05 (4.06)	-15.12*** (5.85)	-80.29*** (13.01)
Year FE	+	+	+	+
Individual FE	+	+	+	+
N	3268	3268	3268	3268
R^2	0.21	0.07	0.05	0.16

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. Robust standard errors clustered at the municipal level. Moreover, (pthc) pc figures refer to per (thousand) capita values. Although not shown, full control set of benchmark regression is still in use. All models include a constant, which coefficient is not reported.

Table 7: AMALGAMATION IMPACT ON EXPENDITURE ITEMS, FE MODELS WITH YEARLY TREATMENT EFFECTS

Dependent variable	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc
Voluntary2003	-75.88** (32.70)	-4.42 (3.70)	-10.00* (5.57)	-26.12** (12.29)
Voluntary2004	-14.50 (31.79)	-2.68 (4.38)	-13.25** (6.50)	-44.95*** (13.37)
Voluntary2005	-54.13** (26.85)	-3.46 (5.04)	-5.58 (6.51)	-37.34*** (13.87)
Forced2003	-18.23 (35.64)	2.22 (3.98)	5.99 (6.87)	18.10 (11.48)
Forced2004	-153.82*** (39.21)	3.30 (4.46)	-32.18*** (8.75)	-139.35*** (18.46)
Forced2005	-175.42*** (41.58)	-0.65 (5.34)	-33.53*** (9.03)	-158.60*** (20.63)
Year FE	+	+	+	+
Individual FE	+	+	+	+
N	3268	3268	3268	3268
R^2	0.22	0.07	0.06	0.20

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. Robust standard errors clustered at the municipal level. Moreover, (pthc) pc figures refer to per (thousand) capita values. Although not shown, full control set of benchmark regression is still in use. All models include a constant, which coefficient is not reported.

Table 8: FE MODELS WITH YEARLY TREATMENT EFFECTS FOR VOLUNTARY AND FORCED MERGERS

Dependent variable	Births pthc	Immigration pthc	Use area pc
AmalgPost	-0.27 (0.19)	-1.25 (2.46)	5.83 (15.63)
Population	0.00 (0.00)	-0.01* (0.01)	-0.11*** (0.01)
Population density	0.00 (0.00)	-0.05 (0.05)	-0.11 (0.09)
Squared population	-0.00** (0.00)	0.00* (0.00)	0.00*** (0.00)
General grants pc	-0.00 (0.00)	-0.08*** (0.02)	0.07 (0.06)
Total revenues pc	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)
Old pthc	-0.01*** (0.00)	-0.21* (0.12)	1.12*** (0.36)
Unemployment	-0.01 (0.07)	0.86 (0.91)	-0.03 (3.11)
Local GDP pc	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Left	0.00 (0.00)	-0.04 (0.04)	-0.56** (0.25)
Year FE	+	+	+
Individual FE	+	+	+
N	3263	3268	3256
R^2	0.01	0.14	0.43

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. Robust standard errors clustered at the municipal level. All models include a constant, which coefficient is not reported. Moreover, (pthc) pc figures refer to per (thousand) capita values.

Table 9: AMALGAMATION IMPACT ON LOCAL PUBLIC GOODS PROVISION, FE MODELS

Dependent variable	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc
AmalgPost	-24.38 (21.26)	-3.53 (3.92)	-9.35* (5.31)	-26.63* (10.04)
Population	-0.05* (0.02)	0.01** (0.01)	0.01 (0.01)	0.00 (0.02)
Population density	-0.15 (0.18)	0.09 (0.07)	-0.04 (0.08)	0.03 (0.18)
Squared population	0.00* (0.00)	-0.00*** (0.00)	-0.00 (0.00)	0.00 (0.00)
General grants pc	-0.10 (0.27)	0.00 (0.02)	0.04 (0.03)	0.07 (0.14)
Total revenues pc	0.23 (0.16)	0.01 (0.00)	0.01 (0.01)	0.04 (0.03)
Old pthc	-0.21 (0.51)	0.16 (0.11)	-0.08 (0.13)	-0.07 (0.31)
Unemployment	-1.64 (9.94)	-6.03*** (1.66)	5.72** (2.51)	6.84 (5.22)
Local GDP pc	-0.01* (0.01)	-0.00 (0.00)	0.00 (0.00)	-0.01** (0.00)
Left	-0.29 (0.47)	0.07 (0.07)	0.04 (0.11)	0.13 (0.26)
Year FE	+	+	+	+
Individual FE	+	+	+	+
N	3268	3268	3268	3268
	0.2	0.07	0.05	0.14

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. Robust standard errors clustered at the municipal level. All models include a constant, which coefficient is not reported. Moreover, (pthc) pc figures refer to per (thousand) capita values.

Table 10: CONTROLLING FOR PARALLEL TRENDS IN SPENDING, FE MODELS FOR PLACEBO TREATMENT 2000

Controls in use				
Panel (1)	Never merged			
Dependent variable	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc
AmalgPost	-73.96** (32.09)	-1.74 (4.52)	-20.28*** (6.64)	-84.49*** (13.89)
N	2701	2701	2701	2701
Panel (2)	Merged since 1875			
AmalgPost	-81.09*** (28.79)	0.71 (4.68)	-15.80** (6.97)	-76.80*** (14.14)
N	2617	2617	2617	2617
Panel (3)	Geographically intact units in 1998–2005			
AmalgPost	-69.72*** (25.30)	-1.58 (3.83)	-15.89*** (5.80)	-80.05*** (11.99)
N	2993	2993	2993	2993
Panel (4)	Geographically intact units in 1998–2005, but never subject to mergers			
AmalgPost	-75.09** (33.20)	-2.16 (4.89)	-20.37*** (7.17)	-92.33*** (13.73)
N	2426	2426	2426	2426

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. Robust standard errors clustered at the municipal level. Moreover, (pthc) pc figures refer to per (thousand) capita values. Although not shown, full control set of benchmark regression is still in use. All models include a constant, which coefficient is not reported.

Table 11: CONTROLLING FOR SELECTION BIAS, FE MODELS ON VARIOUS SUB-CONTROL GROUPS

Dependent variable	Panel (1)				Panel (2)			
	Log(expenditures pc)				Expenditure outcomes (in thousand euros, not per capita)			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
AmalgPost	-0.05*** (0.02)	-0.03 (0.03)	-0.05** (0.03)	-0.08*** (0.01)	-980.58*** (194.25)	-10.62 (24.84)	-204.61*** (59.01)	-765.99*** (124.56)
Year FE	+	+	+	+	+	+	+	+
Individual FE	+	+	+	+	+	+	+	+
N	3268	3268	3268	3268	3268	3268	3268	3268
R^2	0.20	0.08	0.05	0.18	0.24	0.34	0.09	0.26

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. Robust standard errors clustered at the municipal level. Although not shown, full control set of benchmark regression is still in use. All models include a constant, which coefficient is not reported. Note that Panel (1) uses the natural logarithm of outcomes. Outcomes in both panels are (1) total, (2) staff, (3) administrative and (4) current expenditures.

Table 12: CONTROLLING FOR MISSPECIFICATION ON EXPENDITURE ITEMS, FE MODELS

Dependent variable: Total expenditures per capita					
	Benchmark FE	Without trend	Linear trend	Quadratic trend	Square root trend
AmalgPost	-72.98*** (24.92)	-102.63*** (19.28)	-91.98*** (21.68)	-91.96*** (21.69)	-91.98*** (21.68)
Year FE	-	-	+	+	+
Individual FE	+	+	+	+	+
N	3268	3268	3268	3268	3268
R ²	0.19	0.19	0.19	0.19	0.19

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. Robust standard errors clustered at the municipal level. Moreover, (pthc) pc figures refer to per (thousand) capita values. Although not shown, the full control set of benchmark regression is in use. All models include a constant, which is not reported.

Table 13: CONTROLLING FOR SEVERAL FUNCTIONAL FORMS OF TIME TRENDS, FE MODELS

Panel (1)		Control: outcome			
	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc	
AmalgPost	-56.15* (32.63)	0.28 (2.90)	-4.06 (4.53)	8.2 (27.92)	

Panel (2)		Control: control variables from benchmark analysis			
	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc	
AmalgPost	-64.48*** (15.48)	-4.05 (5.37)	-0.41 (4.97)	-26.62*** (8.40)	

Panel (3)		Control: both outcome and controls from benchmark analysis			
	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc	
AmalgPost	-59.37*** (13.99)	-8.02 (4.96)	-1.03 (4.49)	-25.17*** (8.19)	

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. Heteroscedasticity robust standard errors are in parentheses. Moreover, (pthc) pc figures refer to per (thousand) capita values. Panel (1) uses only the respective outcome in levels as the matching variable. Panel (2) uses the complete set of controls of the benchmark analysis as matching variables. Finally, Panel (3) uses both variables of Panel (1) and (2) as variables to match on. All panels use only one-to-one matches. Nearest neighbor matching is performed using the user-written Stata command *nnmatch* by Abadie et al. (2004).

Table 14: CONTROLLING FOR ESTIMATION METHOD: NEAREST NEIGHBOR MATCHING, ALL MERGERS

Panel (1)		Control: outcome		
	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc
ForcedPost	-119.90*** (31.04)	-8.54* (5.11)	-9.76 (7.09)	-53.02*** (17.76)

Panel (2)		Control: control variables from benchmark analysis		
	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc
ForcedPost	-99.87*** (20.11)	12.65* (7.31)	3.31 (6.25)	-46.07*** (11.91)

Panel (3)		Control: both outcome and controls from benchmark analysis		
	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc
ForcedPost	-95.15*** (20.11)	5.95 (7.06)	1.77 (5.98)	-54.93*** (11.93)

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. Heteroscedasticity robust standard errors are in parentheses. Moreover, (pthc) pc figures refer to per (thousand) capita values. Panel (1) uses only the respective outcome in levels as the matching variable. Panel (2) uses the complete set of controls of the benchmark analysis as matching variables. Finally, Panel (3) uses both variables of Panel (1) and (2) as variables to match on. All panels use only one-to-one matches. Nearest neighbor matching is performed using the user-written Stata command *nnmatch* by Abadie et al. (2004).

Table 15: CONTROLLING FOR ESTIMATION METHOD OF FORCED MERGERS: NEAREST NEIGHBOR MATCHING, COMPULSORY MERGERS

Panel (1)		Control: outcome			
	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc	
VoluntaryPost	20.15 (44.75)	-1.6 (3.46)	7.58 (5.54)	63.96 (39.24)	

Panel (2)		Control: control variables from benchmark analysis			
	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc	
VoluntaryPost	-27.31 (18.94)	-13.16* (6.85)	0.48 (6.62)	-7.6 (10.60)	

Panel (3)		Control: both outcome and controls from benchmark analysis			
	Total expenditures pc	Staff expenditures pc	Administrative expenditures pc	Current expenditures pc	
VoluntaryPost	-28.77 (18.13)	-15.18** (6.33)	3.86 (3.37)	-3.4 (10.35)	

Note: *** Significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors are in parentheses. Heteroscedasticity robust standard errors are in parentheses. Moreover, (pthc) pc figures refer to per (thousand) capita values. Panel (1) uses only the respective outcome in levels as the matching variable. Panel (2) uses the complete set of controls of the benchmark analysis as matching variables. Finally, Panel (3) uses both variables of Panel (1) and (2) as variables to match on. All panels use only one-to-one matches. Nearest neighbor matching is performed using the user-written Stata command *nnmatch* by Abadie et al. (2004).

Table 16: CONTROLLING FOR ESTIMATION METHOD OF VOLUNTARY MERGERS: NEAREST NEIGHBOR MATCHING, VOLUNTARY MERGERS

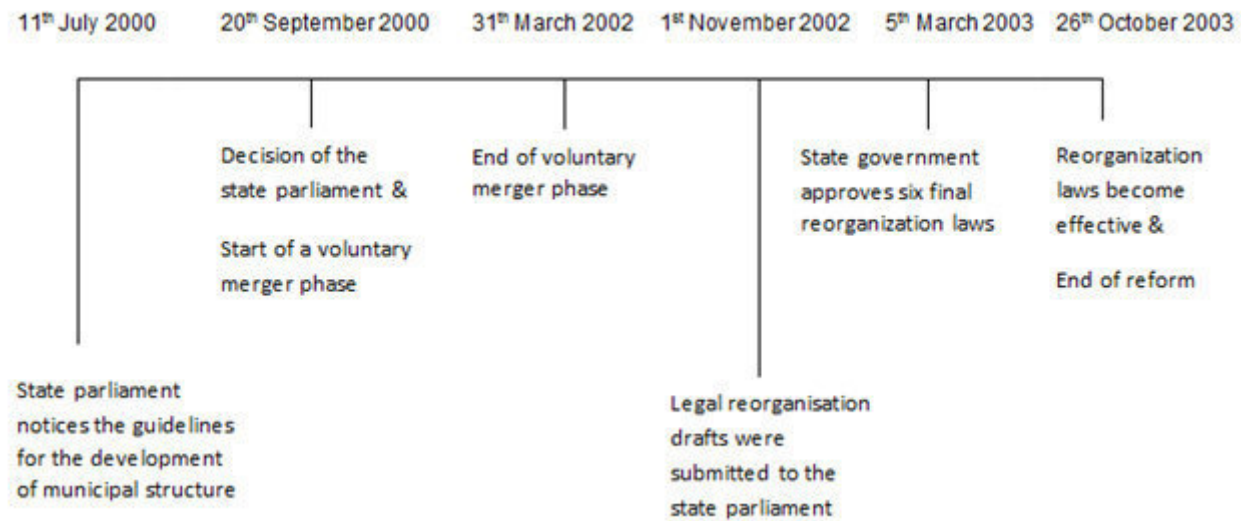


Figure 1: TIMELINE OF THE REFORM.

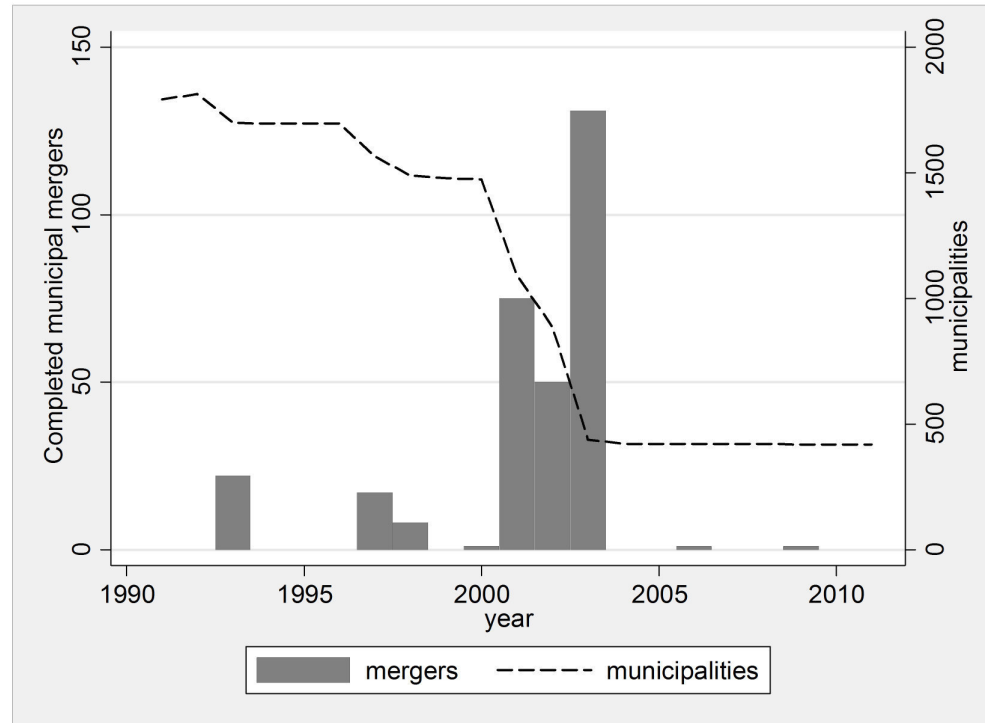
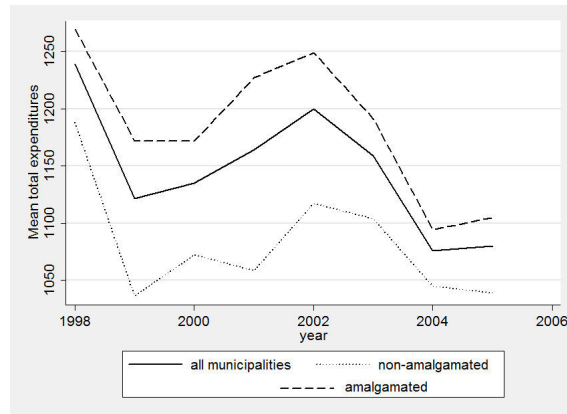
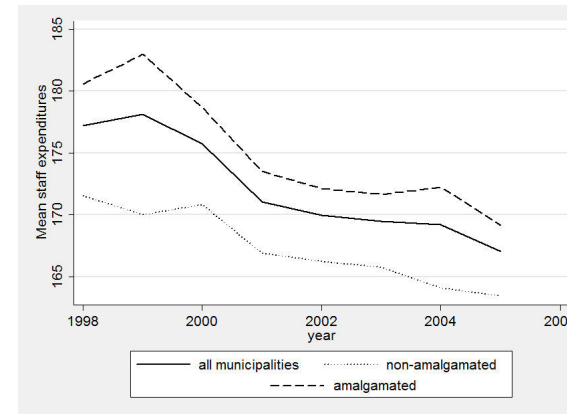


Figure 2: NUMBER OF MUNICIPALITIES AND COMPLETED MERGERS OVER TIME.

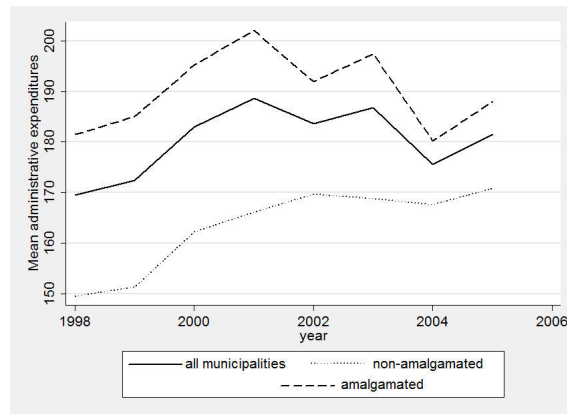
SOURCE: SBB (2013) AND SDL (2005). ALL NUMBERS USE END OF YEAR DATES AND INCLUDE DISTRICT-FREE CITIES.



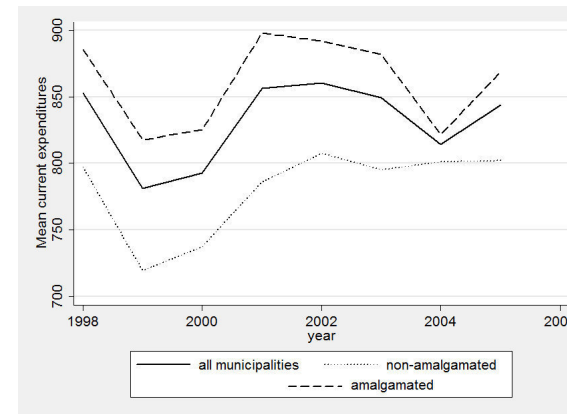
(a) TOTAL EXPENDITURES



(b) STAFF EXPENDITURES



(c) ADMINISTRATIVE EXPENDITURES



(d) CURRENT EXPENDITURES

Figure 3: MEAN EXPENDITURES PER CAPITA BY GROUP STATUS, 1998-2005 IN CONSTANT 2005 EUROS.

THE FIGURE SHOWS THE MEAN OF EXPENDITURES ITEMS (PER CAPITA, IN CONSTANT 2005 EUROS) IN ALL MUNICIPALITIES (SOLID LINE), AMALGAMATED MUNICIPALITIES (DASHED LINE) AND NON-AMALGAMATED MUNICIPALITIES (DOTTED LINE) FOR THE PERIOD 1998-2005.

Appendix

Study	Country	Period	Relevant outcomes	Method	Results
Hinnerich (2010)	Sweden	1962–1974	Short and long-term as well as overall debt per capita	DD	Significant free riding effect of about 17% on average
Welling-Hansen (2012)	Denmark	1996–2006	Final account and budget overruns of current or capital spending per capita	DD	Positive and significant finding of existence and size of common pool in final pre-merger year
Blom-Hansen (2010)	Denmark	2003–2006	Budget overruns in capital and current expenditures per capita	DD	Positive and significant finding of existence in the final year before merger and mainly for capital account spending but only slightly for operational costs like school or road expenditures
Jordahl and Liang (2010)	Sweden	1944–1952	Debt per capita	DD	Significant debt increase of 52% of new debt issued
Moisio and Uusitalo (2013)	Finland	1970–1981	Change in spending items per capita	Nearest neighbor matching	Diseconomies of scale of 6% after 10 years
Fritz (2011)	Germany	1964–1988	Total and staff expenditures as well as debt per capita	DD	(Dis)economies of scale in (total) administrative staff expenditures; debt levels rise
Welling-Hansen et al. (2012)	Denmark	2003–2011	Budgeted and realized operating result and liquid assets or long-term debt per capita	DD	Negative effects in first post-reform years; after 4 years significant scale effects for operating results
Lüchinger and Stutzer (2002)	Switzerland	1989–1998	Total, current and 4 year-averaged investment expenditures per capita	DD	Significant diseconomies of scale in current expenditures; insignificant economies of scale in total and investment expenditures
Reingewertz (2012)	Israel	1999–2007	Total expenditures per capita plus several spending items	DD	Economies of scale of 9%
Blom-Hansen et al. (2011)	Denmark	2005–2011	Administrative costs per capita	DD	Economies of scale of 8%

Table A.1: OVERVIEW OF QUASI-EXPERIMENTAL STUDIES ON FISCAL EFFECTS OF MERGERS

Variable	Measurement	Source
Births pthc	Number of live births per 1000 inhabitants	Own calculations based on SBB
Immigration pthc	Number of immigrants per 1000 inhabitants	Own calculations based on SBB
Use area pc	Logarithm of sum of recreational and infrastructure area in hectare per capita. Logarithmized value is multiplied by 1000. Only collected at the municipal level in 1997 and 2004.	Own calculations based on SBB

Note: All variables are based on the territorial state of 31th December 2005 or are adjusted to that territorial state where such data have not been available. Moreover, (pthc) pc figures refer to per (thousand) capita values. Exact account numbers are available from the author upon request.

Table A.2: DEFINITION AND SOURCE FOR VARIABLES OF SERVICE PROVISION

	Overall	merged	Non-merged
Units	411	258	153
Births pthc	6.76 (2.77)	6.85 (2.37)	6.61 (3.34)
Immigration pthc	67.83 (59.36)	67.30 (56.12)	68.73 (64.63)
Use area pc	-2884.00 (769.87)	-2.694.90 (653.93)	-3203.74 (844.21)

Note: Standard deviations are in parentheses. The first column represents the mean of the dependent variables in the overall sample, whereas columns 2 and 3 depict the mean of these figures for merged and non-merged municipalities, respectively. Moreover, (pthc) pc figures refer to per (thousand) capita values. The values for use area pc are logarithmized.

Table A.3: SUMMARY STATISTICS FOR SERVICE OUTCOMES, 1999