

The impact of boundary reforms on social capital: evidence from an individual-level panel study

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Abstract

Citizens' social capital is considered to be foundational for their political engagement. A widespread view is that individuals in small communities have higher levels of social capital and that changes to the boundaries of political communities negatively impact citizens' integration in their community, and, by extension, their place-specific social capital. However, we lack robust causal and individual-level evidence on the accuracy of this view. In this article, I combine data on 461 municipal merger processes in Switzerland with data from the Swiss Household Panel (1999-2022) to assess whether boundary reforms negatively impact citizens' social capital. Using staggered difference-in-differences analysis on a sample of 23,578 respondents (not) subject to local government amalgamation reforms, I analyze whether respondents report fewer contacts and exchanges with their neighbors, less voluntary engagement and associational involvement, and lower levels of social trust after a municipal merger. In addition, I assess potential effect heterogeneity depending on the "size shock" respondents experience as well as their local ties (residence duration, homeownership, and kids in household) – factors commonly considered to increase integration in local communities. The results show no adverse effects of boundary reforms on individuals' social capital, neither in the short- or in the long-run nor for different subgroups of respondents. This suggests that fears of a "decline of community" after boundary reforms are overstated.

Keywords: Social capital, Municipal merger, Panel analysis, Difference-in-differences, Political community, Public opinion

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1 Introduction

Social capital is considered to be one of the pillars of modern democratic societies. Social capital, defined as the “connections among individuals – social networks and the norms of reciprocity and trustworthiness that arise from them” (Putnam, 2000, 19), renders individuals more oriented towards the public good, enables them to critically scrutinize governmental action (Putnam, 2000, 338) and is considered one of the key resources that enable citizens to participate in politics (Brady et al., 1995).

While social capital hinges on many individual-level factors, scholars also have a keen interest in how (local) context characteristics – socio-economic, institutional, and structural – impact individuals’ social capital (see e.g., Freitag, 2004; Kay and Johnston, 2007; Stolle et al., 2008). A structural factor that figures prominently among these contextual characteristics is the size of a community. Ferdinand Tönnies has prominently coined the distinction between *Gemeinschaft* and *Gesellschaft*. The former is an organic community where members contribute to its wellbeing, whereas the latter is a mechanic community which members “use” instrumentally for the realization of their personal interests (Tönnies, 1991). Geographically, Tönnies saw the interpersonal relationships he called *Gemeinschaft* embodied in small, rural communities, whereas *Gesellschaft* was what characterized (large) cities. This idea of small, common-good oriented, as opposed to large, atomistic, communities was taken up in the “decline of community” model, which states that citizens’ connections are lower in large communities, because life in large communities is more impersonal (Verba and Nie, 1972, 229-247). Yet, Verba and Nie (1972) also formulated a counter-hypothesis, namely the “mobilization” model, which states that larger communities are more diverse, which in turn leads to a proliferation of opportunities to get involved in different subgroups and, thus, contributes to the generation of social capital (Denters et al., 2014, 78).

Existing research on the impact of community size on social capital has pointed in direction of the “decline of community” model. In a general review and analysis of the impact of polity size on social cohesion, Gerring and Veenendaal (2020) find that polity size negatively impacts social cohesion, in particular the connectedness between individuals. In their seminal analysis of size and its impact on local democracy Denters et al. (2014) find that local government size negatively affects citizens’ integration in their neighborhood and their membership in associations. And Oliver (2000, 369) finds that lower levels of political participation in American local politics partly arises from the fact that residents in larger political communities are less likely to be *asked* to get involved by fellow residents. In sum, these results suggest that social capital is lower in larger polities.

However, this evidence is based on a comparison of polities of different sizes and of individuals living therein. This means that it is difficult to attribute the observed differ-

ences in social capital to polity size directly, since particular individuals might sort into certain kinds of polities and hence the differences in social capital would stem from a polity’s composition and not from its structural size. One way to test the causal impact of polity size is to look at sudden changes in jurisdictional boundaries, which by extension also alter the size of the political community – like in the case of local government amalgamation. Yet, existing studies of amalgamation reform effects have not assessed their impact on individuals’ social capital (for general reviews regarding amalgamation reform effects, see [Blesse and Rösel, 2017](#); [Tavares, 2018](#); [Galizzi et al., 2023](#)).

This paper combines geo-coded individual-level panel data from the Swiss Household Panel (SHP) ([SHP Group, 2024](#)) with a unique database on Swiss municipal merger projects to assess the impact of boundary reforms, and changes in polity size, on individuals’ social capital. Staggered difference-in-differences analysis with a sample of 23,578 respondents and with outcome variables measuring different aspects of social capital – neighborhood integration, associational and voluntary engagement, as well as social trust and local attachment – do not yield any statistically significant effects of boundary reforms on social capital. Further analyses regarding possible effect heterogeneity – individuals that experience stronger polity size changes or those with strong local ties might be particularly affected – do not show any negative impacts of boundary reforms either.

These results, the first employing a within-subject panel design to study boundary reform effects, suggest that polity size does not exert a direct causal effect on individuals’ social capital – in contrast to what previous studies have suggested (see e.g., [Denters et al., 2014](#), ch. 4).

2 Boundary reforms, polity size, and social capital

Does individuals’ social capital hinge on the type of community they live in? More precisely, is there a causal effect of community size on citizens’ social capital? Existing research demonstrates a clear relationship between (local) polity size and social capital. In their seminal book on the effects of polity size on different social and political outcomes, [Gerring and Veenendaal \(2020, ch. 3\)](#) review important studies that examine citizens’ connectedness in their (local) community – i.e., the extent of their interpersonal networks as well as their attitudinal predisposition towards it. These studies all find that citizens in larger communities are/feel less connected to their political community. In their book on the impact of size on local democracy in four European countries (Denmark, Norway, The Netherlands, and Switzerland), [Denters et al. \(2014, ch. 4\)](#) essentially confirm this general impact of size on citizens’ social capital. They study four different dimensions of citizens’ social capital (or “social embeddedness” as they call it): neighborhood integration, membership/engagement in associations, social trust, and local attachment.

Citizens in all four countries report fewer contacts and exchange with their neighbors when they live in larger communities and tend to be members of fewer associations. With respect to social trust and local attachment, [Denters et al. \(2014\)](#) suggest that these two dimensions hinge on citizens' neighborhood integration and associational membership – and hence the causal effect of size might be indirect as well. They do find this to be the case but report mixed results regarding a direct effect of local government size on local attachment and social trust.

In their analysis of the relationship between size and the quality of local democracy in Switzerland, [Ladner and Bühlmann \(2007, 95\)](#) also analyze the impact of size on individuals' embeddedness and connectedness in their local communities and find that citizens in smaller municipalities are more strongly connected and exchange more with their fellow citizens. [Bühlmann \(2012\)](#) distinguishes three dimensions of municipal identity, local attachment, informal integration (neighbor contact) and formal integration (associational engagement) and finds, in line with [Denters et al. \(2014\)](#), that size is negatively related to informal and formal integration, but, counterintuitively, citizens in larger municipalities identify more strongly with their community than those in smaller ones.

These studies are all based on cross-sectional analyses, comparing individuals in differently sized communities with one another. This means that they can only rule out compositional effects to a limited extent: respondents might choose to live in differently sized municipalities depending on their preferences regarding social connections in their local community as well as depending on their general predisposition or the importance they ascribe to local communities.

It is, thus, challenging to identify a causal effect of polity size on citizens' social capital. One way to address this issue is to study sudden changes in polity size that result from boundary reforms. Existing research has exploited local government consolidation reforms as quasi-experimental settings in which some local communities merge and hence become bigger while others remain intact. Using difference-in-differences (DiD) designs, such studies have mainly focused on the economic effects of such reforms (see [Gendźwiłł et al., 2020](#), for a review) as well as on certain aspects of local democracy, such as electoral turnout (see [Rodrigues and Tavares, 2020](#), 4-5 for a review). Scholars generally find that turnout decreases after local amalgamation reforms. On the one hand, this finding is explained by the change in polity size that voters experience. In line with the “decline of community” explanation, it is argued that voters are more distant from their representatives in larger polities and they feel like they can have little impact on local policy-making, so they abstain ([Allers et al., 2021](#); [Heinisch et al., 2018](#); [Lapointe et al., 2018](#)). On the other hand, however, scholars also point to the effects of boundary reforms themselves: these reforms are considered a “shock” to local communities and voters that they first need to digest and they need time to adapt to the new structures. This explains negative

impacts on voter turnout, but mainly on the short term (Koch and Rochat, 2017; Rodrigues and Tavares, 2020). To study the impact of polity size on citizens' social capital, one, thus, has to disentangle a possible size from a reform effect.

Only few studies have analyzed the individual-level effects of boundary reforms on citizens' attitudes over time. Most of them examine the impact of the Danish structural reform of 2007, a large-scale local government reform where the national government required municipalities with less than 20,000 inhabitants to merge with other municipalities (Callanan et al., 2024, 2048). They have focused on citizens' perceptions of (local) politics and democracy by examining the impact on citizens' feeling of internal political efficacy (Lassen and Serritzlew, 2011a,b; Lapointe et al., 2018), their political trust (Hansen, 2013; Solvang et al., 2023), and their satisfaction with democracy (Hansen, 2015; Baskaran and Blesse, 2019) as well as on their political interest (Christensen et al., 2022). They find that, also at the individual level, boundary reforms have a negative impact on citizens' political and democratic perceptions and that these effects are more pronounced for respondents living in communities that experienced a bigger change in polity size (Lassen and Serritzlew, 2011a; Hansen, 2013, 2015). Yet, Christensen et al. (2022) find most of these effects to be short-lived, pointing to transitional reform, rather than permanent size effects. A limitation of these studies is that their data are repeated cross-sections of individuals and not individual-level panel data.¹ This limits the potential for causal inference at the individual level, since these studies cannot rely on within-subject analyses.

To the best of my knowledge, there are almost no studies that measure the individual-level effects of such boundary reforms on social capital or some of its subdimensions. One important exception is the study by Hansen and Kjaer (2020), who analyze the effect of the Danish structural reform on individuals' attachment to their local community and find it to decrease permanently among citizens living in communities that experienced more pronounced changes in size. Another study of a single Australian local government merger by Alexander (2013) focuses on citizens' social trust towards members of their old compared with members of the new community and finds that the latter is significantly lower than the former. However, this study neither compares citizens' that experienced a boundary reform to citizens that did not, nor does it compare citizens' perceptions of social trust before and after a reform. Nevertheless, it provides interesting insights into possible post-reform dynamics in affected local communities.

Based on this existing research, we can expect that boundary reforms negatively impact citizens' social capital – either through permanent size or through transitional reform effects. The first hypothesis therefore reads as follows.

H₁: Boundary reforms have a negative impact on citizens' social capital.

¹With the exception of the studies by Hansen (2013, 2015), who uses data on the same respondents at one point in time before and after the Danish structural reform.

2.1 Possible sources of heterogeneity: polity size and local ties

The effects boundary reforms have on citizens' social capital might differ depending on the context they live in as well as on their personal characteristics. Above, we have seen that several existing studies, both at the aggregate- as well as the individual level, have found that the detrimental boundary reform effects on democracy are more pronounced when local communities experience larger changes in size. The same might be the case for boundary reforms' impact on citizens' social capital. For example, if citizens live in a relatively large local government that "annexes" a small one, there are likely only minor changes to their lived experience in the local community as a result of the reform. However, those living in the "annexed" part find themselves in a completely new setting after a reform to which they need to adjust. It might also put their non-political local associational and communal structures into question, because the same structures also exist in the larger part and there might be pressures to consolidate these as well. I, therefore, hypothesize that:

H₂: The impact of boundary reforms on citizens' social capital is stronger for those experiencing a larger increase in community size.

Apart from this contextual variation regarding the impact of boundary reforms, there might also be individual-level variation. In particular, effects might be stronger among individuals that have more steady ties to their local community. For individuals that are more strongly rooted in their local community, a boundary reform might represent a bigger shock than for individuals that only have an ephemeral connection to a particular place.

I explore three factors or dimensions of local ties that have previously been found to affect different dimensions of social capital. A first one is the time an individual lives in a community, i.e., her residence duration. Existing research has shown that citizens with longer residence tenure are more attached to their local community (Lewicka, 2011) and engage more therein (Sampson, 1988; Magre et al., 2014). Citizens' with longer residence tenure might, thus, react more strongly to boundary reforms.

Second, homeowners are also considered to be more strongly rooted in their municipality than tenants, and hence might be more affected by boundary reforms. For most of them, the lion's share of their economic capital is bound in their home and hence at their place of residence. Scholars argue that this turns homeowners into more active members of local communities: they have a strong interest that their municipality is doing well and to make sure that local policies are in their interest, because their homevalue (partly) depends on the development of their municipality (Fischel, 2001; McCabe, 2013). By consequence, homeowners are more integrated in local networks. A boundary reform can represent a shock to these networks, because the make-up of the political community

changes. Homeowners might, thus, be more strongly affected by boundary reforms than tenants.

Third, parents with minor children are also considered to be more strongly rooted in their municipality. The main reason for this is that their kids normally go to school in their municipality and hence they have their friends and social circle there. Parents, thus, get more into contact with other local dwellers via their children than residents without kids (Ladner and Bühlmann, 2007, 69). Moreover, parents also have an interest that there is an attractive offer of leisure activities for their kids close to where they live. In smaller communities, such an offer is usually provided through voluntary work in sports or cultural associations and parents actively contribute to maintain this offer. As a result, they also have stronger local networks than residents without children and might be more strongly affected by boundary reforms. In sum, individuals with stronger local ties – due to long residence tenure, homeownership, or being a parent – likely are more integrated in their local communities and hence might react more strongly to local boundary reforms than residents with weak local ties. Therefore, a third hypothesis reads as follows:

H₃: The impact of boundary reforms on citizens' social capital is stronger among those with stronger local ties.

3 Research design

3.1 Institutional context: Local government and boundary reforms in Switzerland

This study focuses on local governments in Switzerland. Municipalities are key actors in the multilevel structure of the Swiss federal state (Linder and Mueller, 2021, 74-79). First, they are central for the implementation of higher-tier policies – be it in the areas of social assistance, education, or elderly care. Local governments thus also account for one third of total public spending in Switzerland. Second, they have substantive discretion in policy-making (Ladner et al., 2019). For instance, Swiss municipalities set their own income tax rates and decide on naturalization (Hainmueller and Hangartner, 2019). Finally, and most importantly for this study, Swiss municipalities are important focal points for citizens' place attachment and local identities as well as for collective will-formation and participation processes, through elections, popular votes, as well as the widespread reliance on lay politicians in Swiss local government (Ladner, 2005). In a representative survey of Swiss citizens in the early 2000s, about 50% of respondents indicated that the municipality, as opposed to the canton (regional tier), or the national level, was the most important level of government in their daily lives (Ladner and Bühlmann, 2007, 52). While this share would probably be lower nowadays due to internationalization processes

and increased mobility, it still reveals the high importance municipalities have for their inhabitants.

(Swiss) municipalities are also focal points for associational life and voluntary engagement. Many associations or organizations are locally anchored, like sports clubs or churches, and as a result, many citizens' engagement is localized in their local community as well. What further contributes to the importance of local communities is that Swiss municipalities are very small in international comparison (Ladner et al., 2019, 58). Their mean size amounts to 4,200 inhabitants and half of Swiss municipalities have less than 1,700 inhabitants. Many Swiss municipalities, thus, host small political communities in which many citizens likely know each other personally.

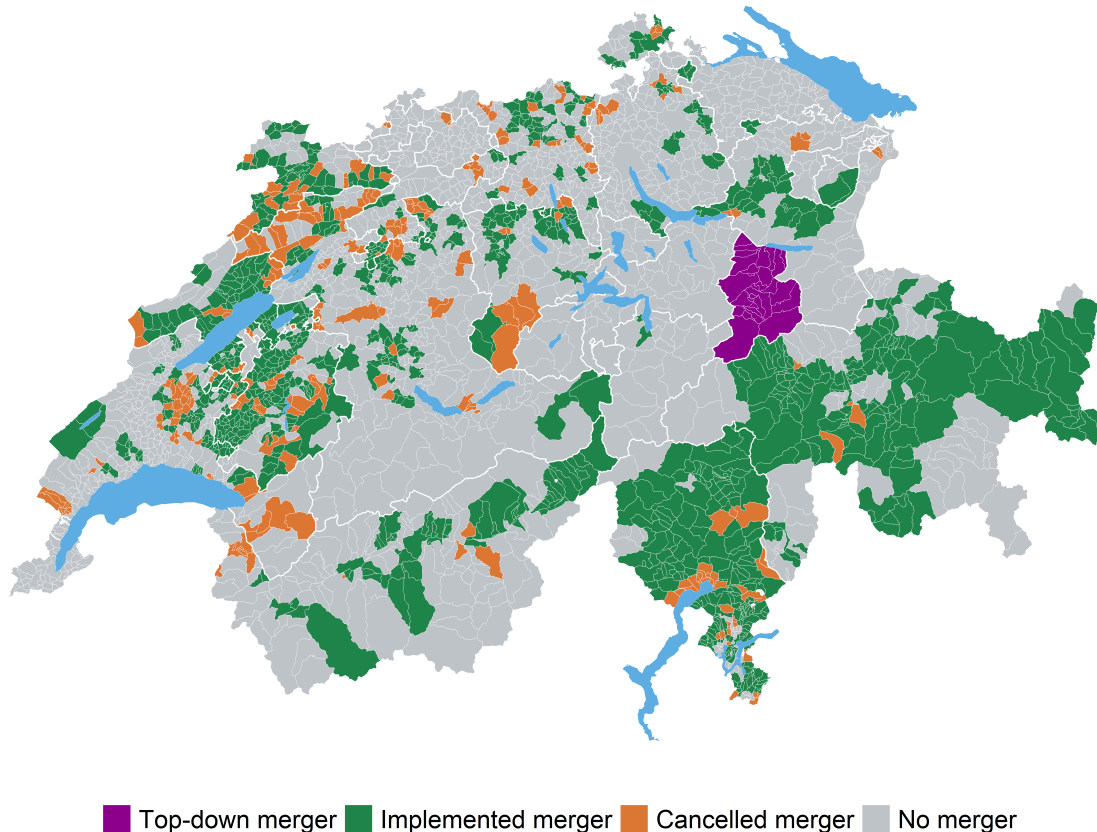
This fragmented and small local government structure has consequences for local governance, however. Many small municipalities have difficulties to cope with their tasks – due to a lack of resources and professionalization – and to find qualified personnel. Local amalgamation reforms are considered a possible solution to deal with these challenges (Swianiewicz et al., 2022, 101). Over the last decades, Switzerland has experienced a significant decline in the number of municipalities due to amalgamation reforms. In 1999, there were still 2,903 municipalities. By 2022, this number had dropped to 2,148, which is a reduction of more than 25%. This is an impressive consolidation process, all the more so if one considers that municipal mergers in Switzerland are voluntary and bottom-up processes – unlike in many other countries, where local government reforms have been implemented top-down (Strebel, 2018; Klausen, 2024).

Swiss cantons, the level of government responsible for municipalities and local government structures, limit their role to the provision of financial incentives – lump-sum payments per inhabitant in case of a merger – to encourage municipalities to merge. Such incentives are an almost necessary condition for Swiss municipal mergers to occur (Kaiser, 2014). In a typical Swiss amalgamation reform process, local elites, elected representatives or officials, react to these cantonal incentives and initiate or participate in municipal merger projects and negotiate merger contracts with the other municipalities in the merger coalition. This represents the first stage of a merger process (Strebel, 2018). In a second step, citizens are the decisive actors. For a merger project to be implemented, a majority of voters in each participating municipality needs to approve the merger contract in a popular vote – either at the ballot box or in a municipal assembly (el-Wakil and Strebel, 2022). If a merger project fails to pass this threshold, it is terminated. This means that voters in each municipality can de facto veto a merger project's implementation.²

²As always, there are exceptions to this rule. In the cantons of Jura and Ticino, some merger projects were implemented even though the voters of one or several municipalities rejected it at the ballots: those municipalities that accepted the merger still merged, without a renewed vote. Moreover, the cantonal government of Ticino has also forced municipalities that rejected a merger to merge nevertheless, thus overruling the outcome of the local popular vote. Yet, this concerns only 9 municipalities.

Figure 1 shows all municipalities that voted on a merger contract between 1999 and 2022. In this period, more than half of all Swiss municipalities that existed in 1999 (1,600 out of 2,903) voted on 461 different merger projects. These merger projects are distributed across the whole of Switzerland and concern a substantive part of the Swiss territory. Of those municipalities that voted on a merger contract, 15% rejected it, and hence 28% of all merger projects that were voted on were not implemented (Strebel, 2025).

Figure 1: Map: Swiss municipalities that voted on a municipal merger contract, 1999-2022



Note. Thin white lines=municipal boundaries of the year 2000. Thick white lines=cantonal boundaries.

One Swiss canton, Glarus, followed a different, top-down, reform approach. Here, the *Landsgemeinde*, the canton’s citizen assembly that gathers outdoors on the main square of the canton’s capital once a year to make legislative decisions, decided to consolidate the 25 existing municipalities into just 3 in 2006 (municipalities in violet in Figure 1). The reform was implemented in 2011 (Hofmann and Rother, 2019; Frey et al., 2023). While it was a participatory process with citizens taking the decision, individual municipalities and their inhabitants did not have a say in it, let alone were able to veto it. Since the *Landsgemeinde* is a cantonal, and not a municipal, institution, this reform can be considered top-down. For the purposes of this study, I exclude respondents from the canton of Glarus given that they experienced a very different merger process than respondents in the rest of

Switzerland (see also below).

3.2 Data: Swiss municipal merger processes and the Swiss Household Panel

To examine the impact of boundary reforms on citizens' social capital, I combine the data on Swiss municipal merger projects with geo-coded individual-level panel data from the Swiss Household Panel (SHP). For each municipality involved in a merger project, I have information on all the involved municipalities, the year and the outcome of the local popular vote, as well as the year the merger was implemented – if it passed the popular vote stage. This data, thus, allows to differentiate municipalities based on their merger process and outcome.

This dataset is combined with the Swiss Household Panel (SHP) data (SHP Group, 2024). The SHP is a panel survey that is fielded on a yearly basis since 1999. A total of 15,981 households were recruited in four samples, the original sample of 1999, and refreshment samples in 2004, 2013, and 2020 (Tillmann et al., 2022, 405). For the period 1999-2022, the SHP includes a total of 41,436 household members. The SHP, together with the German Socio-Economic Panel and the British Household Panel, figures among the longest running panel studies in the world and provides high-quality data to measure social and political change.

A crucial feature of the SHP for my purpose is that it geo-codes respondents' place of residence. Upon signing a data protection agreement, researchers can obtain access to the official municipality codes for each household-year that is present in the SHP. Respondents' household IDs and municipality codes are matched to the municipal merger database based on the pre-merger municipality code. Knowing in which pre-merger municipality a respondent lives is important, because it allows to examine heterogeneous treatment effects depending on the pre-merger characteristics of their place of residence, in particular the polity size change a respondent experienced as a result of a boundary reform.

Given the panel structure of the data and the study context with changing politico-administrative boundaries and municipalities that disappear/are newly created, certain respondents need to be removed from the sample for a cleaner identification of boundary reform effects (see Table A.1 in the Appendix). In particular, to conduct a within-subject analysis, there needs to be clear pre- and post-treatment information for treated respondents and multiple time points for all respondents. Therefore, respondents are excluded from the sample that i.) are only recorded pre- or post-treatment or only in a single panel wave, ii.) experienced multiple mergers in the study period, iii.) are potentially treated but have a survey participation gap in the relevant years of merger implementation, iv.)

live in municipalities that rejected a merger, or v.) experienced a top-down merger (living in the canton of Glarus, see Section 3.1). A detailed discussion regarding these different types of respondents and their exclusion can be found in Appendix A.

After removal of all these respondents, we are left with a sample of 23,578 respondents (=210,886 respondent-years). 1,566 respondents belong to the “treatment” group, i.e., lived in a municipality that implemented a municipal merger project. The vast majority (21,596 respondents) live in municipalities that never voted on a merger project between 1999 and 2022. Finally, a third group of 419 respondents lives in municipalities that participated in a merger project that was not implemented due to a lack of voter support in the popular vote in another municipality of the merger coalition (see below).

To measure individuals’ social capital, I follow Denters et al. (2014, ch. 4) and distinguish four subdimensions: neighborhood integration, associational involvement and voluntary engagement, social trust, and local attachment. Table 1 gives an overview of the different indicators for these subdimensions, the exact question wording, and the panel waves in which the question was included.

Three indicators measure neighborhood integration. Respondents were asked about the number of neighbors with whom they have a close relationship as well as whether they think these neighbors could provide them with support in practical (e.g., in small craft projects), or emotional matters. Another three indicators capture respondents’ voluntary and associational engagement. They are asked whether or not they do voluntary work, whether or not they participate in clubs or other groups, and whether they are a passive or active member of a list of different associations. For this third question, I focus on respondents involvement in local, parents, or women association, which is one of the possible answer categories.³ To capture social trust, the SHP includes the standard question regarding respondents generalized trust, like it is fielded in international surveys such as the World Values Survey or the European Social Survey. Finally, respondents were also asked about their place attachment to different territorial scales (from municipal to European). However, these items were only fielded in three panel waves and hence the number of respondents is substantively smaller than for the other questions and the analysis can only give indicative results.

3.3 Identification and estimation strategy

To assess how boundary reforms impact respondents’ social capital, I rely on a staggered difference-in-differences (DiD) design (Roth et al., 2023). DiD is widely used to study the causal effects of boundary reforms (Blesse and Rösel, 2017; Gendźwiłł et al., 2020; Rodrigues and Tavares, 2020). The key identifying assumption of a DiD is that treated and

³Other associations listed were: sports, cultural, environment, union, political party, religious, charity, and tenants.

Table 1: Dependent variables: question wording

Indicator	Question wording	Recoding	Waves
Neighbor contact	“With how many of your neighbours are you on good terms and enjoy a close relationship?” [0 = no neighbors, 1 = 0, 2 = 1, 3 = 2, 4 = 3 to 5, 5 = 6 to 10, 6 = >10]	Binary: 0 = 0/1, 1 = 2-6	1-12, 15, 18, 21, 24
Neighbor: practical support	“If necessary, in your opinion, to what extent can these neighbours provide you with practical help, this means concrete help or useful advice, if 0 means ‘not at all’ and 10 ‘a great deal’?” [0-10]	–	1-12, 15, 18, 21, 24
Neighbor: emotional support	“To what extent can these neighbours be available in case of need and show understanding, by talking with you for example, 0 means ‘not at all’ and 10 ‘a great deal’?” [0-10]	–	1-12, 15, 18, 21, 24
Voluntary work	“Do you have honorary or voluntary activities within an association, an organisation or an institution?” [1 = yes, 2 = no]	Binary: 0 = 2, 1 = 1	1-24
Participation in clubs	“Do you take part in clubs’ or other groups’ activities, religious groups included?” [1 = yes, 2 = no]	Binary: 0 = 2, 1 = 1	1-24
Association membership	“I will now read out a list of associations and organisations. Could you tell me for each of them whether you are an active member, a passive member or not a member?” - local, parents, women association [1 = active, 2 = passive, 3 = not a member]	Binary: 0 = 3, 1 = 1/2	1-11, 13, 16, 19, 22
Social trust	“Would you say that most people can be trusted or that you can’t be too careful in dealing with people, if 0 means ‘Can’t be too careful’ and 10 means ‘Most people can be trusted’?” [0-10]	–	4-24
Municipal identity	“Tell me to what extent is belonging to the following locations important for your identity, if 0 means ‘not important at all’ and 10 ‘very important’?” - belonging to the commune of residence [0-10]	–	16, 20, 23

control groups would have followed the same trajectory in the absence of treatment, here the municipal merger (Wing et al., 2024). To check the plausibility of this assumption, it is common to examine whether the two groups show parallel trends prior to the treatment, which is the case here (see Section 4).

Since Swiss municipal mergers occur in an incremental fashion, and hence municipalities enter treatment at different points in time, I use a staggered DiD design to avoid “forbidden comparisons” between prior and later treated units and to have correctly weighted estimates that take into account possible heterogeneity across cohorts of treated units (see e.g., Goodman-Bacon, 2021, for a discussion of this problem). Here, I rely on the double-robust DiD estimator introduced by Callaway and Sant’Anna (2021) and calculate both aggregated average treatment effects on the treated (ATTs) as well as event study estimates using the DiD package in R.⁴

To allow for causal inference, treatment assignment, being subject or not to a boundary reform, ideally is exogenous. In analyses of top-down reforms, such as the Danish structural reform of 2007, this is the case: (most) municipalities could not influence whether they merge or not, this was determined by the population threshold of 20,000 inhabitants. By contrast, in the case at hand, municipalities self-select into treatment. They decide whether or not to participate in a merger process and the majority of voters in each municipality ultimately decides whether a merger is implemented or not. There might thus be confounding factors that both affect the dependent variable as well as treatment uptake.⁵

There are two ways in which this issue is attenuated in the present setting. First, treatment assignment happens at the municipal and not at the individual level. Individual respondents can only influence to a limited extent whether they will be exposed to a reform or not: they can only have a negligible impact on the outcome of the merger process through their vote. In this sense, it is municipalities but not necessarily respondents that self-select into treatment.

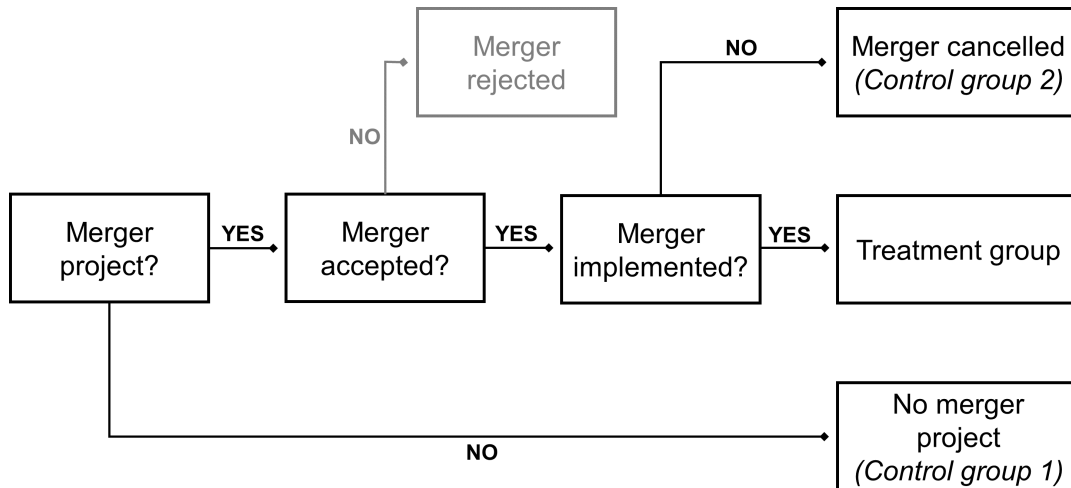
Second, the setting allows to circumvent the self-selection problem because I can leverage an alternative control group than those respondents living in municipalities that never voted on a merger project. Figure 2 gives an overview of the typical Swiss municipal merger process. After the first stage of the merger process – project initiation or participation – voters in each involved municipality decide whether or not they accept the negotiated merger contract and hence merger implementation in a second stage. Depending on the popular vote outcome in their own and in their partner municipalities,

⁴To avoid estimations on small cohorts I exclude those with less than 100 treated observations.

⁵Yet, analyses of top-down jurisdictional reforms often suffer from another issue: the absence of a viable control group (Harjunen et al., 2021, 74). Since top-down reforms often come with a population threshold, they essentially remove the possibility of having a reasonable control group in a DiD setting, since all municipalities below the population threshold are treated. Using municipalities above that population threshold as a control group might thus itself lead to biased estimates.

respondents can end up in three different groups. If all municipalities in a merger coalition accepted a merger, it is implemented and hence respondents in these municipalities end up in the treatment group. If a majority of voters in the municipality they live in voted against the merger contract, they pertain to the group of municipalities that rejected a merger. Finally, if their municipality voted in favor of the merger contract, but *another* municipality in the merger coalition voted against it, respondents live in a municipality that had its merger cancelled. Despite having voted in favor of it, these municipalities will not implement their merger project and they are *exogenously deselected* from merger implementation. In addition to the “naive” control group of respondents in municipalities that never voted on a merger project (control group 1), I, thus, use respondents living in municipalities that had their merger cancelled as an alternative control group (control group 2) to partially address the self-selection issue.

Figure 2: The typical Swiss municipal merger process: treatment and control groups



4 Results

Do boundary reforms have a nefarious effect on citizens’ social capital? Table 2 shows the aggregated average treatment effects on the treated (ATTs) for all indicators of the outcome variable and for both control groups.

The estimated effect of municipal boundary consolidation on the different dimensions of respondents’ social capital is positive, but statistically insignificant, across (almost) all indicators and subdimensions of social capital. This is, thus, clear evidence against hypothesis H_1 : boundary reforms do not impact citizens’ social capital negatively. In the analysis with control group 2, i.e., comparing treated respondents to respondents living in municipalities that had their merger cancelled, there is even evidence for a positive effect

Table 2: Aggregated ATTs

<i>Control group 1: no vote</i>										
	Neighbor contact (1)	Practical support (2)	Emotional support (3)	Voluntary work (4)	Participation in groups (5)	Local association member (6)	Social trust (7)	Local identity ^a (8)	Local identity ^b (9)	
Aggregated	0.083	0.108	0.018	0.06	0.026	0.064	0.173	-0.091	-0.012	
ATT	(0.065)	(0.35)	(0.313)	(0.07)	(0.067)	(0.061)	(0.306)	(0.282)	(0.273)	
N	89963	60700	60071	94987	122956	89322	136347	5635	5589	
N individuals	19044	15280	15243	20959	22094	21308	20846	2183	2163	
Treated	1255	1056	1055	1307	1341	1275	1354	79	59	

<i>Control group 2: cancelled merger</i>										
	Neighbor contact (1)	Practical support (2)	Emotional support (3)	Voluntary work (4)	Participation in groups (5)	Local association member (6)	Social trust (7)	Local identity ^a (8)	Local identity ^b (9)	
Aggregated	0.108*	0.259	0.199	0.093*	0.044	0.077*	0.271	0.47	-0.124	
ATT	(0.044)	(0.233)	(0.233)	(0.046)	(0.039)	(0.032)	(0.182)	(0.615)	(0.449)	
N	10304	7129	7092	11301	14206	9687	16732	304	258	
N individuals	1604	1353	1353	1662	1713	1638	1728	117	97	
Treated	1255	1056	1055	1307	1341	1275	1354	79	59	

Note. Staggered DiD estimated with did package in R (Callaway and Sant'Anna, 2021), except for models 8 and 9 which are standard two-way fixed effect DiD models estimated with feols. Models include individual- and year-fixed effects; Municipality-clustered standard errors in parentheses; * = $p < 0.05$, ** = $p < 0.01$, *** = $p < 0.001$. ^atreated between 2015 and 2018, ^btreated between 2019 and 2021.

of municipal mergers on certain dimensions of social capital. In particular, respondents that experienced a merger are more likely to report having close contacts with at least one neighbor, of doing voluntary work and of being a (passive or active) member of a local, parents, or women association. Given that the estimations are based on linear probability models, the coefficients can be interpreted as percentage point changes in the probability of these outcomes. Neighbor contact becomes 10, voluntary work 9 and being a local/parents/women association member 8 percentage points more likely.

A note of caution is required regarding the analyses for local identity. Given that information on this item is only available for three panel waves and hence the number of treated individuals per cohort becomes too small for staggered DiD analysis, I ran two separate two-way fixed effects DiD regressions: a first one for respondents that were treated between the first and the second wave the item was included and a second one for those treated between the second and the third wave. As one can see from Table 2, the number of treated respondents is quite small and hence results of these analyses should be interpreted with caution.

Figures 3-5 show the corresponding event study estimates for the different indicators and for both control groups. These Figures allow for three important observations.

First, they give us hints about whether the parallel trends assumption might possibly be violated. Given that the pre-treatment event study estimates are all statistically insignificant and fluctuate around zero across all periods, control groups, and indicators, we can be rather reassured that the parallel trends assumption holds in the present case. Yet, given the lack of sufficient panel waves for the local identity item, it is not possible to conduct an event study and the parallel trends assumption can, thus, not be evaluated for this indicator.

Second, the Figures allow for an examination of possible temporal dynamics. In particular, one might argue that the aggregated ATTs hide short-term negative effects that are followed by a longer-term recovery and hence the aggregated ATTs in sum are statistically indistinguishable from zero. This is not the case, however. For no indicator is there a significant negative short-term impact of a boundary reform. Again, in most cases, the post-treatment estimates fluctuate around zero with no clear time-trends visible. However, for some indicators, a positive over-time tendency is visible, particularly when comparing treated respondents to respondents from control group 2. This is the case for the probability of conducting voluntary work and for social trust (Figures 4a and 5). Here, the difference between treated and control respondents seems to grow over time – even if the individual event study estimates are far from statistically significant.

Third, the event study estimates also provide a cautionary note against overinterpreting the relevance of the significant aggregated ATTs from Table 2. For none of the three indicators – neighbor contact, voluntary work, and associational membership – are there

any statistically significant individual event study estimates. They merely are all positive and once aggregated, confidence bands become sufficiently small for estimates to be significant at the 95% level.

In sum, regarding the main effects of boundary reforms on citizens' social capital, there is, thus, no evidence for the “decline of community” hypothesis and for a detrimental effect of municipal mergers on individuals' social embeddedness.

Yet, maybe these general analyses just hide underlying effect heterogeneity in different subsamples of respondents? To rule out this possibility and to test hypotheses H_2 and H_3 , I rerun the same analyses as in Table 2 among particular subsamples of respondents (see Tables in Supplementary Materials B). Hypothesis H_2 stated that respondents exposed to a more substantial size change might react more strongly to a boundary reform. To test this, I analyze the subgroup of respondents that live in a pre-merger municipality that makes up less than 50% of the total merger coalition population at the time of the reform. These are respondents that likely experienced a fundamental change of local government and local community compared to what they knew before. This group might, thus, show nefarious effects in their social capital due to the scale of the reform impact. However, Table B.1 shows that this is not the case. Again, most of the aggregated ATTs are positive but statistically insignificant, which is clear evidence against H_2 . The idea that (substantive) changes in community size have a negative impact on individuals' social capital is, thus, not confirmed by this analysis, which speaks against the “decline of community” model.

Respondents might not only react differently to boundary reforms depending on contextual heterogeneity, but also depending on individual-level heterogeneity. Hypothesis H_3 stated that individuals with strong local ties – due to long residence tenure, homeownership, or having minor kids in their household – react more negatively to boundary reforms, because the latter might put the community they are closely tied to in question. Tables B.2-B.4 show the results of subgroup analyses for respondents with more than 5 years of residence tenure, those who own a home, and those who have minor kids – always measured at the time of the reform, since these variables are time-varying. The results remain the same as in the main analysis and the analysis of contextual heterogeneity: most of the estimated aggregated ATTs are positive and most of them are statistically insignificant – again with the exception of the ATTs for neighbor contact, voluntary work, and associational membership when comparing treated respondents to those living in municipalities with cancelled mergers. There is, thus, no evidence for hypothesis H_3 either.

Also after these subsample analyses intended to uncover possible heterogeneous treatment effects, the main conclusion remains the same: boundary reforms, here municipal mergers, do not negatively impact citizens' social capital – challenging the arguments of the “decline of community” proponents.

5 Conclusion

Existing research has argued that the size of a community has a negative effect on the social capital of its members, because larger communities make it more difficult for their members to establish strong connections among one another and with the community as a whole (Verba and Nie, 1972; Denters et al., 2014). Using a within-subject design and leveraging boundary reforms to identify possible size effects on individuals' social capital, the present study does not find evidence for this "decline of community" argument. Boundary reforms do not have a negative impact on respondents' neighborhood integration, their voluntary and associational engagement, their social trust, or their local attachment. If anything, the results point in the opposite direction, namely that citizens affected by a boundary reform develop stronger social capital in its aftermath – even if these effects are subject to substantial uncertainty. Overall, this suggests that polity size does not exert a direct causal effect on individuals' social capital. The effects of size found in existing studies are likely either due to indirect effects of community size on other community-level characteristics that influence social capital, such as the diversity and heterogeneity of a community, or they are due to compositional effects resulting from residential sorting. The present results thus call for a modification and nuancing of the "decline of community" hypothesis.

How generalizable are these results and what are their scope conditions? There are two contrasting observations that can be made. On the one hand, Swiss municipalities can be seen as a most likely case to observe a direct size effect on individuals' social capital. Swiss local communities are small-scale in international comparison and those subject to boundary reforms particularly so. Moreover, these small local communities are closely knit and play an important role in many citizens daily lives. If no effects are found here, they are even more unlikely to be found elsewhere. On the other hand, the nature of the reform individuals were exposed to makes it less likely to observe an impact on social capital. Swiss local boundary reforms are characterized by a strong participatory component, citizens make the final decision on merger implementation, and this might act as a shield against adverse consequences of boundary reforms on social capital. The collective experience of participating in a transformation process as fundamental as an amalgamation reform might actually bring local communities closer together and mobilize individuals to get involved more. Irrespective of which of these two arguments regarding the generalizability and scope conditions of the Swiss context has the upper hand, the present case allows to rule out a direct adverse causal effect of polity size on individuals' social capital: if such an effect would be present, it should be observed independently of the reform process and context.

The present study has several limitations. First, regarding the measurement of the

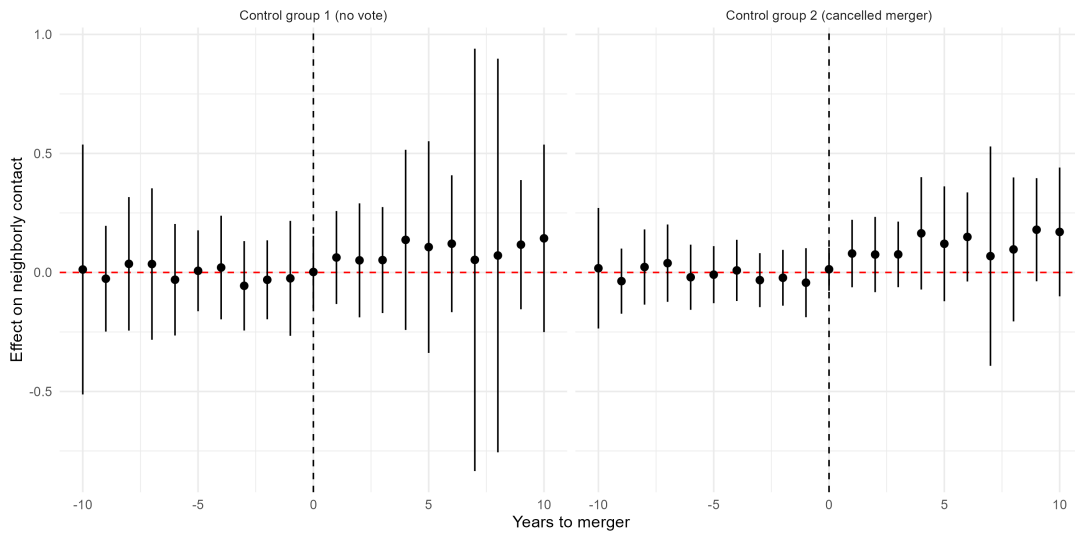
different dimensions of social capital, it is not (always) clear to what extent these indicators are capturing a local activity or dimension. For instance, membership in a sports club is likely in one's own municipality (e.g., the local football club), but it might also be in a neighboring municipality or in the next city if it's a more "fringe" sport (e.g., water polo). The same is true for interaction with neighbors: some respondents might have a very liberal understanding of what is a neighbor, while others might only consider persons living in the house next door or in the same apartment block. Ultimately, this is a trade-off of using large-scale panel studies: while they offer unique advantages in terms of causal identification and within-subject analysis, a drawback is that question wording can rarely be influenced and at times is rather general.

A second caveat concerns the causal ordering and hierarchy of the different dimensions of social capital. As argued by [Denters et al. \(2014\)](#), neighborhood integration and voluntary/associational engagement might act as mediators between community size and social trust or local attachment. In the context of the present study, it is difficult to assess the causal ordering of these factors. Yet, this is also not the aim here. Rather, the goal is to study the direct impact of boundary reforms and polity size on social capital, which precludes mediation through other factors.

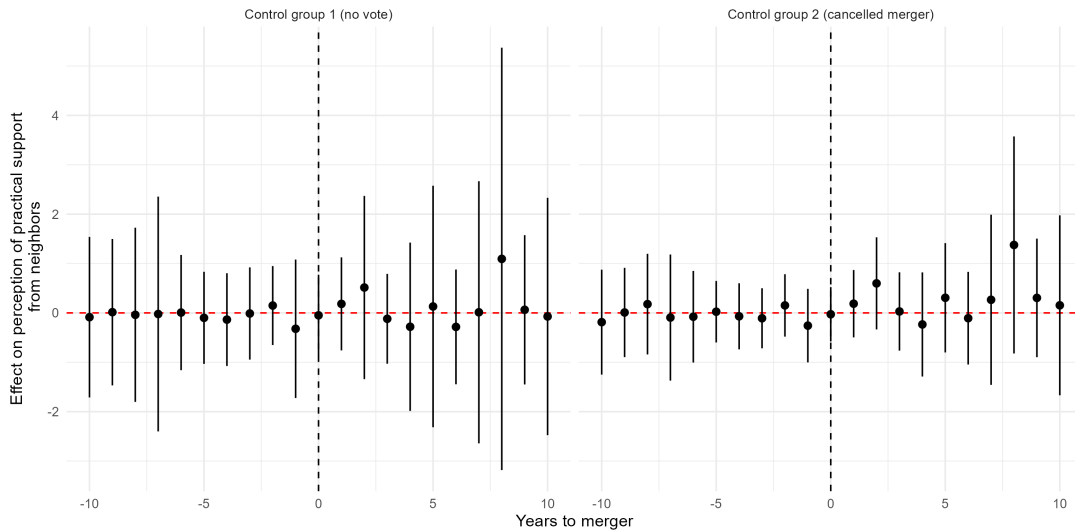
A final limitation of this study is that it might be subject to atomistic fallacy. By only measuring social capital at the individual level, the study might miss aggregate-level impacts on a community's social cohesion, e.g., the disappearance of local associations or member losses that result from a boundary reform. Unfortunately, no systematic data on Swiss municipalities' associational landscape is available – only data on a selected sample of municipalities ([Freitag and Ackermann, 2016](#)). Collecting and analyzing such data is far beyond the scope of this study but represents a valuable endeavor for future research.

Figure 3: Event study estimates: neighborhood integration

(a) Neighbor contact (=1)



(b) Neighbor practical support (0-10)



(c) Neighbor emotional support (0-10)

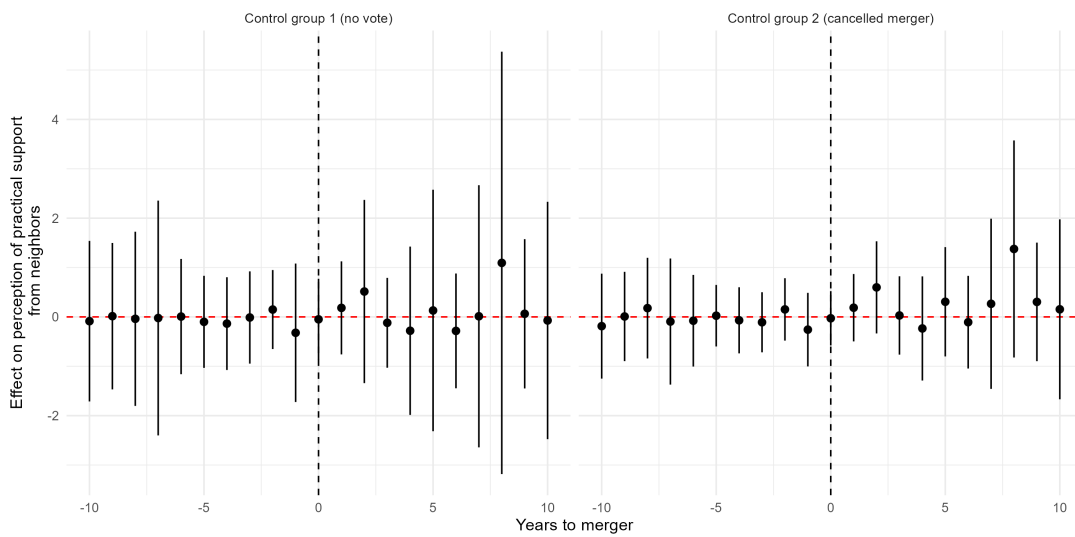
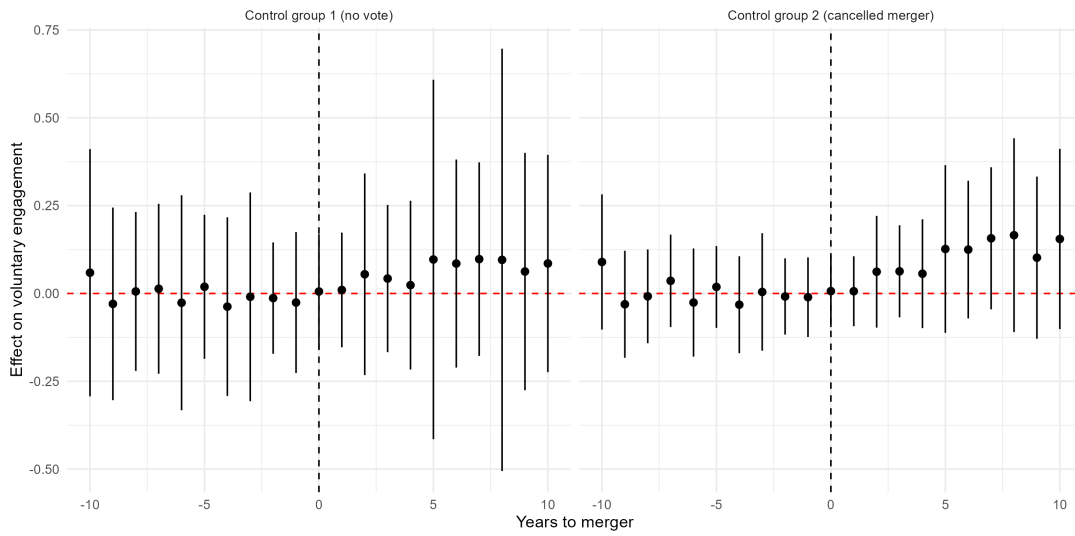
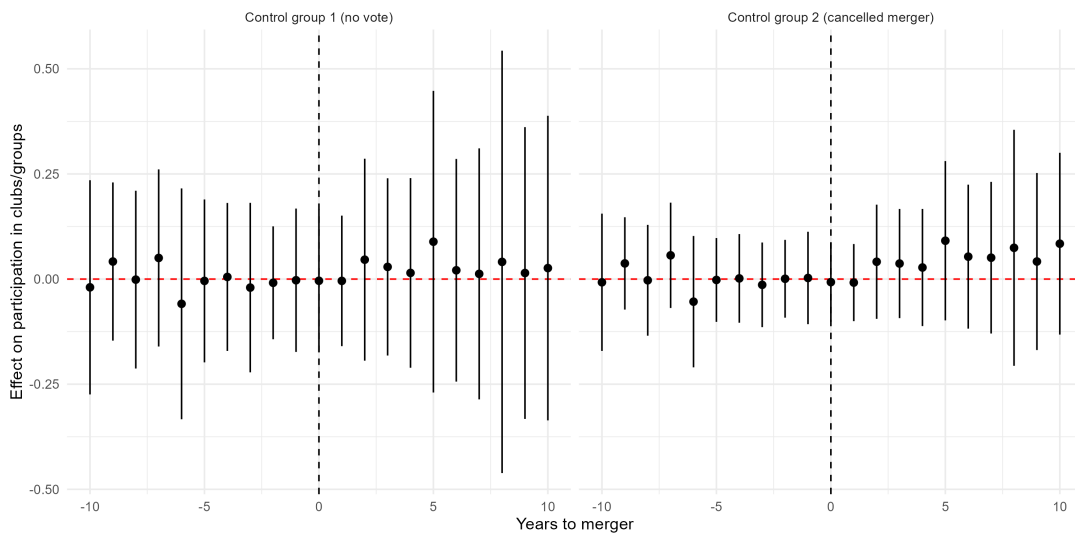


Figure 4: Event study estimates: voluntary and associational engagement

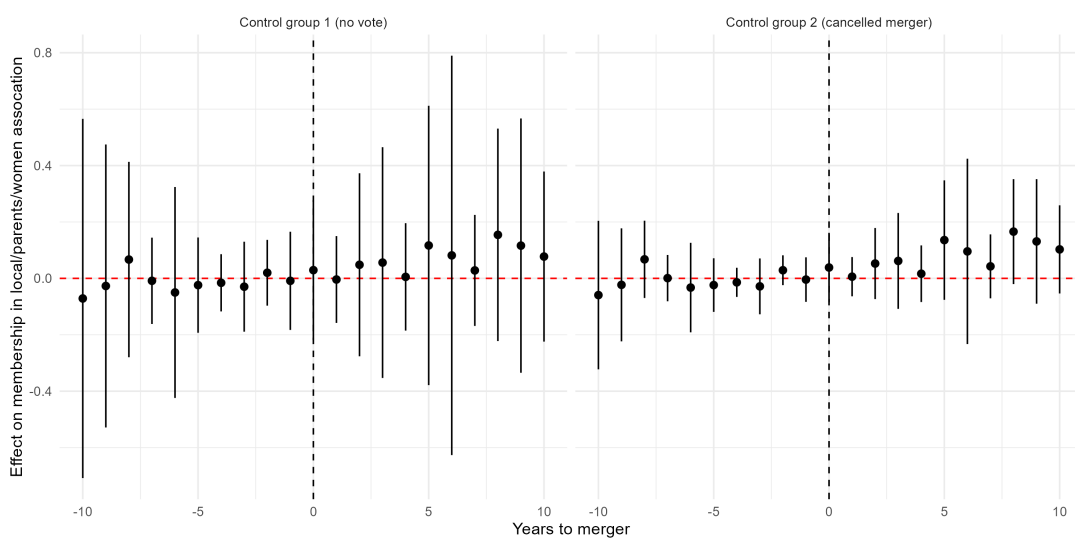
(a) Voluntary work (=1)



(b) Participation in clubs/groups (=1)

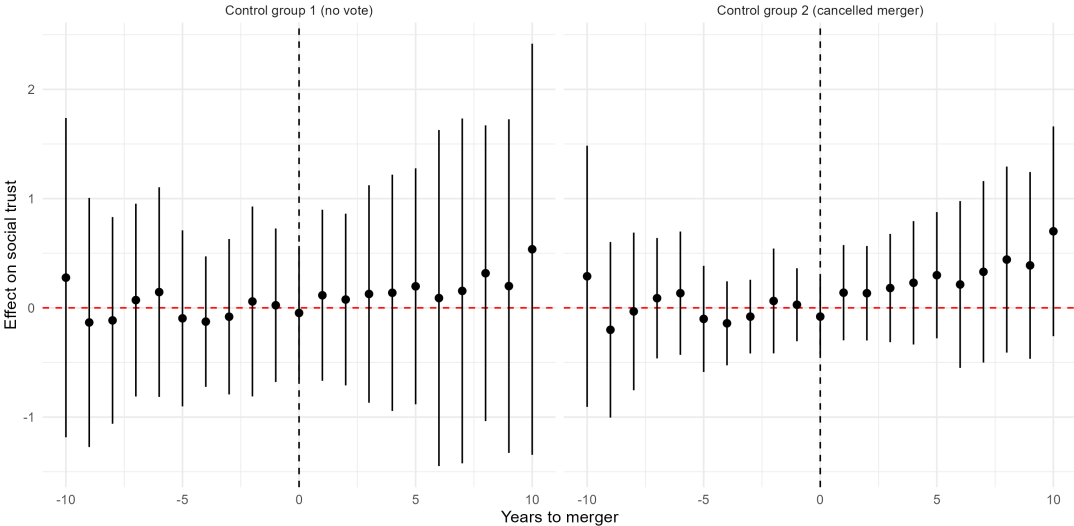


(c) Member in local/parents/women association (=1)



Note. Error bars represent 95% confidence intervals.

Figure 5: Event study estimates: social trust



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The impact of boundary reforms on social cohesion: evidence from an individual-level panel study

Supplementary Information

A	Data cleaning and preparation	1
B	Contextual and individual-level heterogeneity	3

A Data cleaning and preparation

In the present study setting, creating a clean geo-coded panel dataset with clear assignment to treatment and the two control groups (see Figure 2) poses a number of challenges that require consequential decisions, i.e., the removal of respondents from the sample (see Table A.1). Most of these challenges relate to the fact, that only 4% of all respondents (N=1,532) participated in all 24 panel waves between 1999 and 2022. Given that I conduct a within-subject analysis, I need information on treated respondents both pre- and post-treatment. This leads to the exclusion of a first group of respondents that live in a merged municipality but entered the panel only *after* the merger was implemented. We, thus, do not have information on the dependent variable for them for the pre-treatment period. This concerns 4,399 out of 41,436 respondents.

Table A.1: Overview of respondents in-/excluded in sample

Respondent type	N	In sample?
All SHP respondents	41,436	–
Pre-/post-treatment/single wave only	12,094	No
Multiple mergers	431	No
Participation gaps	602	No
Mover	4,170	No
Municipality rejected merger	492	No
Municipality in Glarus	69	No
Study sample	23,578	Yes
Control group 1: no merger project	21,596	Yes
Control group 2: cancelled merger	419	Yes
Treatment group: implemented merger	1,566	Yes

Second, there are respondents that experienced more than one municipal merger project. This can be the case, because the municipality they lived in participated in a merger project several times – for example because a first merger project the municipality voted on was cancelled due to a lack of popular support and then a second attempt succeeded. Another possibility is that the respondent moved from one municipality to another municipality that was involved in a merger project. Since it is difficult to decide in such situations, which years after the first merger are to be treated as post-merger years of the first merger and which years as pre-merger years for the second one, I equally exclude these respondents from the sample (N=431).

Third, there are respondents with panel participation gaps. I might, for instance, have a respondent that participated from 1999-2001, and then again from 2005-2009. The Swiss municipal merger database includes information on merger projects from the year they were voted on until the year prior to their implementation. This means that if in the

example just mentioned, a merger project was voted on in 2003 and implemented in 2004, I would miss this information and wrongly assign the respondent to the control group. For this reason, I exclude all respondents that have lived in a municipality that was involved in a merger project and whose first and last year in the panel lie before/after the years of the merger, but where the matching procedure does not result in a match, because the respondent did not participate in the survey in the relevant years (N=602).

Fourth, I also exclude the panel years in which the respondents that belong to the treated group – i.e., those living in a municipality involved in a merger while the merger process took place – were not living in the respective municipality that was affected by a merger. For example, if a respondent lived in a municipality from 1999 to 2006, and the municipality merged in 2003, it experienced the merger there. But if the household then moves to another municipality in 2007 and remains in the panel until 2009, I exclude the years 2007-2009 from the data. I do so, because I cannot be sure whether potential changes in citizens' perceptions in the post-treatment period are due to the merger experience or to the move. This concerns 6,891 out of 262,284 respondent-years.

Fifth, I also cannot include those respondents in our analysis that were either only present in the panel in one year – and hence I do not have a time-series – or where I only have observed pre-treatment years. This concerns 7,101 respondents that belong to the control group, i.e., live in municipalities that did not vote on a merger project, and 594 treated respondents, i.e., those that experienced a merger process while living in a particular municipality but for whom I do not have post-treatment information on the dependent variable.

Sixth, and finally, I exclude two types of respondents from the sample for the main analysis based on their municipal merger process experience. On the one hand, there are 492 respondents living in municipalities where a merger got rejected in a popular vote (“merger rejected” in Figure 2). These respondents have experienced a merger process, but a majority of their fellow citizens did not approve of it and hence they cannot be included as a viable control group for respondents living in municipalities that approved and implemented a merger. On the other hand, I exclude 69 respondents from the canton of Glarus because it is a case of top-down and not a bottom-up merger (see also subsection 3.1). Here, municipalities themselves did not have a possibility to refuse to merge, since the authoritative decision was made by the cantonal citizens assembly and hence a higher level. Respondents from Glarus, thus, experienced a top-down and not a bottom-up merger like the rest of the respondents in the treated group.

B Contextual and individual-level heterogeneity

Table B.1: Respondents in municipalities with < 50% of coalition population: aggregated ATTs

Control group 1: no vote

	Neighbor contact (1)	Practical support (2)	Emotional support (3)	Voluntary work (4)	Participation in groups (5)	Local association member (6)	Social trust (7)	Local identity ^a (8)	Local identity ^b (9)
Aggregated ATT	0.11 (0.2)	-0.038 (0.732)	-0.12 (0.91)	0.124 (0.153)	0.012 (0.14)	0.017 (0.184)	0.06 (0.942)	-0.254 (0.433)	-0.672 (0.393)
N	84265	56895	56280	88876	115307	83971	127286	5530	5467
N individuals	18164	14543	14507	20044	21162	20398	19898	2144	2118
Treated	375	319	319	392	409	365	406	40	14

Control group 2: cancelled merger

	Neighbor contact (1)	Practical support (2)	Emotional support (3)	Voluntary work (4)	Participation in groups (5)	Local association member (6)	Social trust (7)	Local identity ^a (8)	Local identity ^b (9)
Aggregated ATT	0.146* (0.072)	0.088 (0.306)	0.065 (0.404)	0.147* (0.063)	0.016 (0.052)	0.041 (0.059)	0.122 (0.306)	0.298 (0.703)	-0.783 (0.543)
N	4606	3324	3301	5190	6557	4336	7671	199	136
N individuals	724	616	617	747	781	728	780	78	52
Treated	375	319	319	392	409	365	406	40	14

Note. Staggered DiD estimated with did package in R (Callaway and Sant'Anna, 2021), except for models 8 and 9 which are standard two-way fixed effect DiD models estimated with feols. Models include individual- and year-fixed effects; Municipality-clustered standard errors in parentheses; * = p < 0.05, ** = p < 0.01, *** = p < 0.001. ^atreated between 2015 and 2018, ^btreated between 2019 and 2021.

Table B.2: Respondents with ≥ 5 years of residence tenure: aggregated ATTs

Control group 1: no vote

	Neighbor contact (1)	Practical support (2)	Emotional support (3)	Voluntary work (4)	Participation in groups (5)	Local association member (6)	Social trust (7)	Local identity ^a (8)	Local identity ^b (9)
Aggregated ATT	0.066 (0.071)	0.056 (0.276)	-0.027 (0.299)	0.059 (0.073)	0.017 (0.071)	0.065 (0.066)	0.157 (0.357)	-0.127 (0.311)	-0.116 (0.258)
N	89249	60411	59781	94077	121947	88882	135038	5603	5579
N individuals	18863	15179	15142	20782	21914	21160	20673	2171	2158
Treated	1074	955	954	1130	1161	1127	1181	67	54

Control group 2: cancelled merger

	Neighbor contact (1)	Practical support (2)	Emotional support (3)	Voluntary work (4)	Participation in groups (5)	Local association member (6)	Social trust (7)	Local identity ^a (8)	Local identity ^b (9)
Aggregated ATT	0.092 (0.048)	0.213 (0.224)	0.162 (0.225)	0.091* (0.044)	0.034 (0.041)	0.079* (0.033)	0.251 (0.174)	0.435 (0.629)	-0.22 (0.445)
N	9590	6840	6802	10391	13197	9247	15423	272	248
N individuals	1423	1252	1252	1485	1533	1490	1555	105	92
Treated	1074	955	954	1130	1161	1127	1181	67	54

Note. Staggered DiD estimated with did package in R (Callaway and Sant'Anna, 2021), except for models 8 and 9 which are standard two-way fixed effect DiD models estimated with feols. Models include individual- and year-fixed effects; Municipality-clustered standard errors in parentheses; * = $p < 0.05$, ** = $p < 0.01$, *** = $p < 0.001$. ^atreated between 2015 and 2018, ^btreated between 2019 and 2021.

Table B.3: Homeowners: aggregated ATTs*Control group 1: no vote*

	Neighbor contact (1)	Practical support (2)	Emotional support (3)	Voluntary work (4)	Participation in groups (5)	Local association member (6)	Social trust (7)	Local identity ^a (8)	Local identity ^b (9)
Aggregated ATT	0.052 (0.1)	0.157 (0.356)	0.013 (0.336)	0.11 (0.093)	0.018 (0.091)	0.059 (0.097)	0.16 (0.454)	0.033 (0.366)	-0.079 (0.292)
N	86753	58743	58118	91763	118655	86446	131297	5557	5527
N individuals	18485	14838	14801	20404	21516	20764	20261	2153	2140
Treated	696	614	613	752	763	731	769	49	36

Control group 2: cancelled merger

	Neighbor contact (1)	Practical support (2)	Emotional support (3)	Voluntary work (4)	Participation in groups (5)	Local association member (6)	Social trust (7)	Local identity ^a (8)	Local identity ^b (9)
Aggregated ATT	0.078 (0.048)	0.348 (0.235)	0.239 (0.227)	0.141* (0.043)	0.028 (0.039)	0.072* (0.036)	0.237 (0.182)	0.595 (0.66)	-0.204 (0.458)
N	7094	5172	5139	8077	9905	6811	11682	226	196
N individuals	1045	911	911	1107	1135	1094	1143	87	74
Treated	696	614	613	752	763	731	769	49	36

Note. DiD estimated with did package in R (Callaway and Sant'Anna, 2021), except for models 8 and 9 which are standard two-way fixed effect DiD models estimated with feols. Models include individual- and year-fixed effects; Municipality-clustered standard errors in parentheses; * = p < 0.05, ** = p < 0.01, *** = p < 0.001. ^atreated between 2015 and 2018, ^btreated between 2019 and 2021.

Table B.4: Minor children in household: aggregated ATTs

Control group 1: no vote

	Neighbor contact (1)	Practical support (2)	Emotional support (3)	Voluntary work (4)	Participation in groups (5)	Local association member (6)	Social trust (7)	Local identity ^a (8)	Local identity ^b (9)
Aggregated ATT	0.02 (0.198)	-0.101 (0.666)	-0.083 (0.578)	0.114 (0.159)	0.06 (0.14)	-0.003 (0.217)	0.039 (0.563)	0.348 (0.561)	-0.328 (0.411)
N	83732	56341	55728	88280	114525	83464	126412	5503	5479
N individuals	18083	14397	14361	19963	21080	20332	19821	2133	2123
Treated	294	173	173	311	327	299	329	29	19

Control group 2: cancelled merger

	Neighbor contact (1)	Practical support (2)	Emotional support (3)	Voluntary work (4)	Participation in groups (5)	Local association member (6)	Social trust (7)	Local identity ^a (8)	Local identity ^b (9)
Aggregated ATT	0.049 (0.072)	0.033 (0.308)	0.083 (0.285)	0.147* (0.063)	0.071 (0.053)	0.01 (0.074)	0.109 (0.229)	0.909 (0.796)	-0.464 (0.54)
N	4073	2770	2749	4594	5775	3829	6797	172	148
N individuals	643	470	471	666	699	662	703	67	57
Treated	294	173	173	311	327	299	329	29	19

Note. Staggered DiD estimated with did package in R (Callaway and Sant'Anna, 2021), except for models 8 and 9 which are standard two-way fixed effect DiD models estimated with feols. Models include individual- and year-fixed effects; Municipality-clustered standard errors in parentheses; * = $p < 0.05$, ** = $p < 0.01$, *** = $p < 0.001$. ^atreated between 2015 and 2018, ^btreated between 2019 and 2021.