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Voter turnout and municipal amalgamations —evidence from Denmark

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ABSTRACT

The study utilizes evidence from the Danish 2007 municipal reform to inquire into the relationship between municipal amalgamations and voter turnout, that is, the classical discussion on size and democracy. The Danish municipal reform is particularly suited for investigating the relationship as a large number of units were merged due to reasons thought to be unrelated with the democratic performance while others were left unchanged. This allows us to investigate the relationship in a quasi-experimental setup. The study finds some evidence of a short-term positive effect on turnout and only limited evidence of a medium- to long-term negative effect when comparing amalgamated and non-amalgamated municipalities. However, stronger indications of negative effects on turnout are found when considering the intensity of the amalgamations.

KEYWORDS Voter turnout; amalgamation; participation; quasi-experiment; jurisdiction size

Introduction

Do municipal amalgamations influence voter turnout? Municipal mergers are a frequently occurring phenomenon in recent decades in many countries, and they are on the political agenda in several others (e.g., Koch and Rochat 2017, 215; Steiner and Kaiser 2017, 232). The possible advantages and disadvantages of municipal amalgamations are intimately related to the fact that mergers entail an increase in the polity size. Amalgamations are often motivated by considerations of economic efficiency due to possible economies of scale as the size of the entities increases (e.g., Boyne 1995; Blom-Hansen, Houlberg, and Serritzlew 2014). However, while administrators often hope to harvest economies of scale in the amalgamations, a common concern is that democracy might suffer, among other things because larger entities are associated with weakening ties between citizens

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and their representatives and fewer representatives per citizen (Dahl and Tufte 1973; Lassen and Serritzlew 2011).

While the debates about the ideal polity size can be traced back to antiquity (Aristotle 1981; Dahl and Tufte 1973), our empirical knowledge about the relationship between amalgamations and democratic outcomes is still limited by methodological challenges (e.g., Denters et al. 2014). One challenge is that often amalgamations occur in a few and/or selected entities at the time, and therefore, it can be difficult to distinguish between the impact of the amalgamations, selection, and unrelated trends. Perhaps, therefore, much of the evidence on the relationship between jurisdictional size and democracy is based on cross-sectional designs.

In this study, we examine the effect of a comprehensive municipal amalgamation on voter turnout, namely the Danish municipal amalgamation in which 239 municipalities were merged into 66 while 32 other municipalities were left unchanged (see also Bhatti and Hansen 2010). The Danish municipal reform constitutes an interesting case for studying consequences of amalgamations since a large number of units merged simultaneously for reasons thought to be exogenous to their democratic performance, while others remained unchanged (e.g., Lassen and Serritzlew 2011; Hansen 2012, 2015). This allows us to provide good causal estimates of amalgamation effects. Voter turnout is a particularly interesting outcome since it is often seen as the most prominent indicator of democratic health and since it can be measured objectively.

The structure of the remainder of the article is as follows. After this introduction, we discuss the theoretical mechanisms related to the possible relationship between amalgamations and voter turnout. Thereafter, we discuss the empirical context, the Danish municipal reform, followed by the design of the study. The results section is divided into three sub-sections. First, we depict some relevant descriptive relationships related to the population of the municipalities and turnout. Second, we investigate the relationship between the amalgamation and turnout and estimate the main models of change over time. Third, we conduct a series of robustness tests. In the last section of the study, we discuss the results and their implications.

Theory and existing studies

The study of amalgamation effects relates closely to the study of polity size. The central mechanism by which amalgamation is thought to have effects on democratic outcomes is through the increased size when several entities become one. The possible dilemma between democratic legitimacy and democratic effectiveness when deciding the optimal polity size is a classic discussion in political science (e.g., Aristotle 1981; Dahl and Tufte 1973; Larsen 2002, 319; Kjær and Mouritzen 2003; Denters et al. 2014, 3–7).

On the output side, it is often argued that larger municipalities lead to a higher level of self-sufficiency and more efficient production due to economies of scale driven by lower fixed costs and increasing possibilities of specialization (Hirsch 1959; Blom-Hansen, Houlberg, and Serritzlew 2014, 791; Blom-Hansen et al. 2016, 813–816). Thus, while there are also functional arguments in favor of limitations to the size of local entities – such as a lack of information and coordination or inability to satisfy differences in preferences (e.g., Tiebout 1956) – amalgamations are often justified with reference to output side arguments (e.g., Structural Commission 2004, 24–27; Steiner and Kaiser 2017, 233).

An often-presented concern related to amalgamations is that they may come with costs at the input side of democracy and, among other things, decrease political participation, including voter turnout. One of the most frequently cited mechanisms is that larger entities may entail lower information levels and lower subjective feeling of being able to comprehend local politics because it is more difficult for voters to have intimate knowledge of local service production when size and complexity increase (Dahl and Tufte 1973; Lassen and Serritzlew 2011). Another mechanism may be that the feeling of local community and affective attachment to the municipality decreases as the size of the entity becomes larger (Verba and Nie 1972; Mouritzen 1997, 269; Denters et al. 2014, 225). When municipal sizes increase, there are also typically more citizens per politician (Hansen and Hjelmar 2015, 465), and citizens are, therefore, less likely to know the local politicians, which, in turn, may affect both knowledge of and identification with the local political system.

While most of the theoretical literature emphasizes the advantages of smaller polities, there could also be theoretical arguments in favor of expecting higher turnout in larger entities. The possible better performance of larger municipalities in terms of output and its better functional capabilities may feed back into citizen engagement (Newton 1982, 202; Hansen and Hjelmar 2015, 467). Additionally, it has been argued that larger polities entail more diversity, which allows for mobilization of smaller groups and thereby ensures a more vibrant civic culture and political system (Denters et al. 2014, 20; Verba and Nie 1972; Dahl and Tufte 1973). In sum, the opposing theoretical expectations call for an empirical investigation.

The existing empirical literature mainly finds a negative or no relationship between local entity size and voter turnout. In a meta-analysis of aggregate studies on voter turnout at the national and sub-national level, Cancela and Geys (2016) show that 49 percent of the national level studies and 69 percent of the sub-national level studies find a mainly negative significant impact of population size on turnout. In a meta-analysis of the relationship between municipal population size and political participation in general, Houwelingen (2017) finds 60 negative and 10 positive relationships. Among

studies with significance tests, 22 are significantly negative, 16 are not significant while one is significant positive. Existing studies from a Danish context find a negative relationship between size and turnout or mixed results (e.g., Mouritzen 1997; Frandsen 2003; Juul-Madsen and Skou 2006; Bhatti and Hansen 2010).

It should be noted that while size-related effects are central to the study of municipal amalgamations, there could additionally be short-term disruption effects due to the reform process itself, which may be positive or negative. When municipalities are merged, citizens need to become accustomed to new politicians, a changed (or new) municipality, and possibly, as a consequence, new issues. This disruption may have a negative effect on citizens' ability to understand the political issues, their knowledge of the local politicians, their emotional attachment to the municipality, and thereby limit political participation in the short run. Changing polling stations and districts may also have a disruptive effect (McNulty, Dowling, and Ariotti 2009; Brady and McNulty 2011; Bhatti 2012; Hansen 2016a). Positive short-term effects are also possible. For instance, citizens after a reform may mobilize to protect their local community in the new municipality, that is, turnout may increase because it is important to voters that politicians from their old entity are selected as representatives in the new municipality. It could also be that the reform itself increases the focus on local politics in the media and thereby the tendency to participate (see, e.g., Bækgaard et al. 2014 on the relationship between local media coverage and participation).

In conclusion, the existing theoretical and empirical literature provides the most support to the expectation that voter turnout is negatively related to amalgamations due to the increase in municipal size though there are both theoretical and empirical counterarguments. Furthermore, one could expect additional short-term effects since citizens need to adjust to the new political context, and empirical analyses could benefit from studying both short-term and medium- to long-term effects (e.g., Hansen and Hjelmar 2015).

The Danish municipal reform

Denmark has a high degree of government decentralization and the municipalities play a key role in the delivery of public services in the universal welfare state. More than 30 percent of the GDP is spent at the municipal or regional level, and municipalities deliver core welfare services such as childcare, schools, care for the elderly, libraries, local roads, and some health care services. Municipal elections are held every fourth year simultaneous with regional elections, and historically turnout has fluctuated around 70 percent. Voter registration is automatic, and adult Danish citizens, EU citizens, citizens from Nordic countries with permanent residence in the country are

eligible. Citizens from other countries are eligible upon three years of permanent residence in the country.

In January 2007, Denmark underwent a comprehensive reform of local government in which the number of municipalities was reduced. Additionally, the municipalities received several new tasks, primarily from the old counties. The reform was especially motivated by considerations of economics of scale and self-sufficiency in the delivery of public services. The reform was largely unexpected, and the decision process was relatively short. Debate about a possible reform began in the summer of 2002, and the decision to amalgamate was taken in 2004 after the so-called Structural Commission delivered its analysis to the government. The reform took effect in 2007, but already, in 2005, elections were held for the new municipal councils as the year 2006 was used as a transferring period (Blom-Hansen, Elklit, and Serritzlew 2006; Christiansen and Klitgaard 2008; Bhatti and Hansen 2011). The new municipalities were required to have a minimum size of 20,000 inhabitants, although a few exceptions were granted mainly to island municipalities. Of the 271 municipalities, 239 were merged into 66 entities while 32 others were left unchanged.[1] The average size of the municipalities increased from approximately 20,100 to 55,600 inhabitants. There is general agreement in the existing literature that the decision regarding whether to merge and who to merge with was driven by structural factors and not by the democratic or economic performance of the individual entities (e.g., Bhatti and Hansen 2011; Lassen and Serritzlew 2011; Blom-Hansen et al. 2016; Hansen 2016b, 82).

The Danish context has previously been used for studying amalgamation effects and polity size effects on measures of democratic functioning. While voter turnout has only been scarcely investigated (however, see Bhatti and Hansen 2010; Hansen and Hjelmar 2015), previous studies of the 2007 municipal reform indicate that it has come with democratic costs at the citizens level (for a review, see Hansen 2016b). Lassen and Serritzlew (2011) find that citizens' internal political efficacy declined in amalgamated municipalities relative to non-amalgamated municipalities when measured before and one year after reform, strongly suggesting a negative reform effect. Hansen (2012) similarly finds that trust declined relatively in the merged municipalities when measured before and two years after the reform, especially in those municipalities that experienced a large population increase in the reform process. In another study using the same survey years, Hansen (2015) shows that merged municipalities experienced a relative decline in satisfaction with democracy and the perception of how problems were handled. Nielsen and Vestergaard (2014) finds that external efficacy was lower in larger and amalgamated municipalities in 2009. Hansen and Hjelmar (2015) analyze longer-term effects and find very modest effects of the reform on subjective indicators of local democracy.

While the Danish municipal reform has been studied extensively, almost all studies look at short-term effects and are based on subjective attitudinal measures. Thus, besides being interesting by itself, turnout is relevant to study to examine whether the patterns identified in the existing literature are the same when studying objective and behavioral outcomes that may be less sensitive to institutional changes and survey methodology. Finally, it is worth highlighting that we have no sampling issues as we are working with the official turnout from all municipalities.

Design and measures

The Danish municipal reform is an interesting case for studying the effect of municipal amalgamations and, by extension, the effect of municipal size on democracy. In some countries, municipal amalgamations have occurred gradually and possibly in response to individual municipalities' performance, and it can, therefore, be difficult to estimate the effects of the amalgamations in a stringent causal manner. Also, most of the existing literature on the relationship between municipal size and turnout, in general, is based on designs utilizing cross-sectional variation in municipal size (e.g., Mouritzen 1997; Ladner 2002; Denters et al. 2014; see also Koch and Rochat 2017, 217). While such designs are useful in providing some insights into the relationship, they are vulnerable to compositional effects due to the non-random settlement patterns (Lassen and Serritzlew 2011; Denters et al. 2014). Thus, the lower turnout of large municipalities may be caused by less resourceful individuals tending to settle in the larger cities or because voters who value electoral participation the most also happen to value living in smaller jurisdictions.

The Danish case is particularly suited for studying the relationship between amalgamations and turnout because a large number of municipalities were amalgamated simultaneously while others were left unchanged. This allows us to apply a difference-in-difference logic and avoid bias from time-invariant differences in municipal types. Furthermore, the decision regarding whether to amalgamate or not in Denmark was mainly driven by structural considerations and not by the municipalities' democratic performance or lack thereof. To emphasize, we do not claim that it was random who amalgamated (see also Hansen and Hjelmar 2015; Hansen 2016b) – it is clear that those municipalities that amalgamated where, on average, smaller and had a lower population density than those that continued. Nevertheless, we believe that the Danish case presents us with fewer endogeneity issues than present in most of the existing literature.

The effect of the amalgamation is estimated using first-difference models where the dependent variable is the change in turnout over time, and the main independent variable is whether a municipality underwent an amalgamation. The unit of analysis is the post-reform municipalities. Thus, in

pre-reform years, measures were aggregated to post-reform units in order to have a stable unit of analysis over time.[2] The dependent variable of the study is the change in voter turnout across time at the municipal level. We utilize the official turnout rate given by the number of votes cast divided by the number of eligible voters multiplied by 100. The main independent variable is a dummy variable for whether a municipality was amalgamated or not. In order words, we test whether turnout in amalgamated and non-amalgamated municipalities changes differently over time.

We use the 1997 municipality election as the base year in the main models. This election is chosen rather than the election directly prior to the reform, 2001, where the municipal elections were simultaneous with a national election and therefore constituted very atypical elections (this has happened only this one time). As reform discussions surfaced around 2002, the 1997 elections are clearly pre-treatment. The post-treatment election years are 2005, 2009, 2013, and 2017. The 2005 election is included even though the reform was decided, but not implemented, as politicians were on election for the post-reform municipal boards.

An advantage of our long period of investigation is that it is possible to track both short-term (2005 and 2009) and medium- to long-term effects of the amalgamations (2013 and 2017). However, this advantage, of course, comes with a cost: In the time span between 1997 and 2017, it is possible that there could be socio-demographical changes that differ between the reformed and non-reformed municipalities. We try to consider this by including variables for changes in socio-demographical composition in the models across all years though it is a potential threat to the causal estimates. More specifically, we take into account changes in municipal composition with respect to educational level, number of voters, the share of immigrants, and age composition. We also include in the first-difference models the pre-reform level of the variables to allow for different trends over time based on initial municipal characteristics. Finally, we also include the municipal area as a control and a dummy for small island municipalities. We emphasize that the key threat to the causal estimates provided is that the amalgamated municipalities, conditional on the controls, would have developed differently than the non-amalgamated in the absence of the amalgamation. Ultimately, there is no way we can establish that this is not the case with certainty, but we add placebo models in the robustness test as an indication of the credibility of the claim.

Results

Descriptive relationship between size and turnout

Before turning to the results, we note that, descriptively, a clear negative relationship existed between municipal size (operationalized as natural

logarithm to the number of eligible voters) and voter turnout both before and after the reform. Figure 1 illustrates the relationship for the two elections right before the reform (1993 and 1997) and the elections after (2005–2017). The 2001 election is omitted due to the simultaneous national election which made that election atypical.

The negative bivariate relationship is relatively strong in all years. In most of the elections, the unstandardized coefficient is about −3, corresponding to a decrease in turnout of about 2 percentage points when going from a municipal size of 10,000 eligible individuals to 20,000 individuals.[3] In the 2009 elections, the relationship is even slightly stronger.

This clear negative relationship strengthens the expectation that an amalgamation which increases the average municipal size would lead to a decrease in turnout and, more generally, that there is a negative connection between size and turnout. However, while the descriptive relationship is intriguing, it may reflect compositional differences between small and large municipalities, and, therefore, a direct investigation of the amalgamation effects is in order, following the design discussed above.

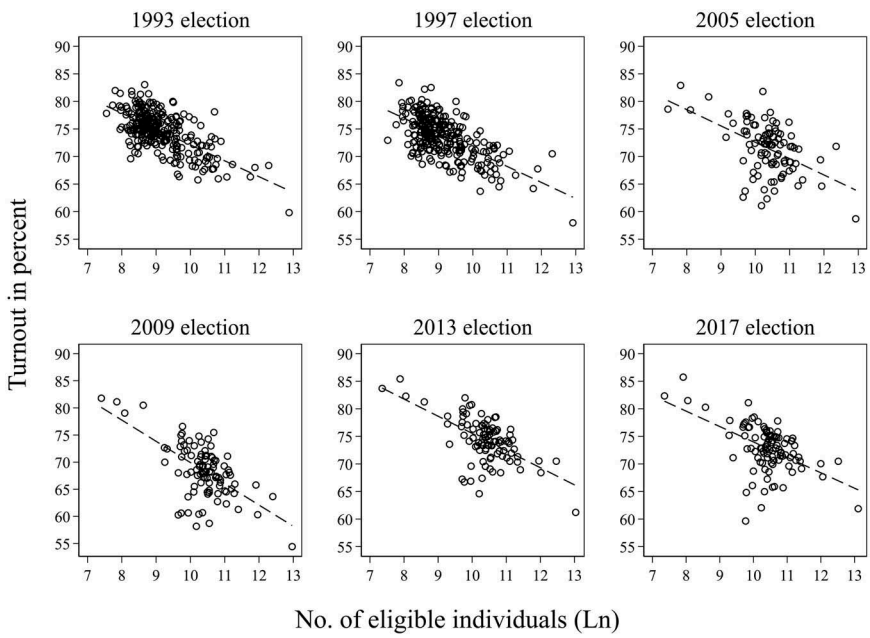

Figure 1. Bivariate relationship between No. of eligible voters (ln) and voter turnout at the municipal level.

Note: N = 275 for 1993 and 1997, and N = 98 for 2005–2017.

Amalgamation effects

We begin the analysis by providing a descriptive overview of the development in the (unweighted) average turnout across municipalities in the non-amalgamated and amalgamated group, respectively. The first election for the new municipalities was in 2005.

Figure 2 does not provide clear evidence in support of the expectation of a negative amalgamation effect. Before the reform in 1997, the difference in average turnout between the two groups of municipalities was about 2 percentage points and relatively stable over time (with a slight tendency of a narrower gap over time). The difference is roughly the same for 2009, 2013, and 2017. One interesting pattern is that amalgamated municipalities have an approximately 3.5 percent higher turnout than non-amalgamated municipalities in 2005, which could indicate a short-term mobilizing effect of the reform because, for instance, voters in the first election are eager to ensure that their old municipality obtains sufficient representation in the new municipal board. In 2009, the difference between non-amalgamated and amalgamated municipalities is again about 2 percent.

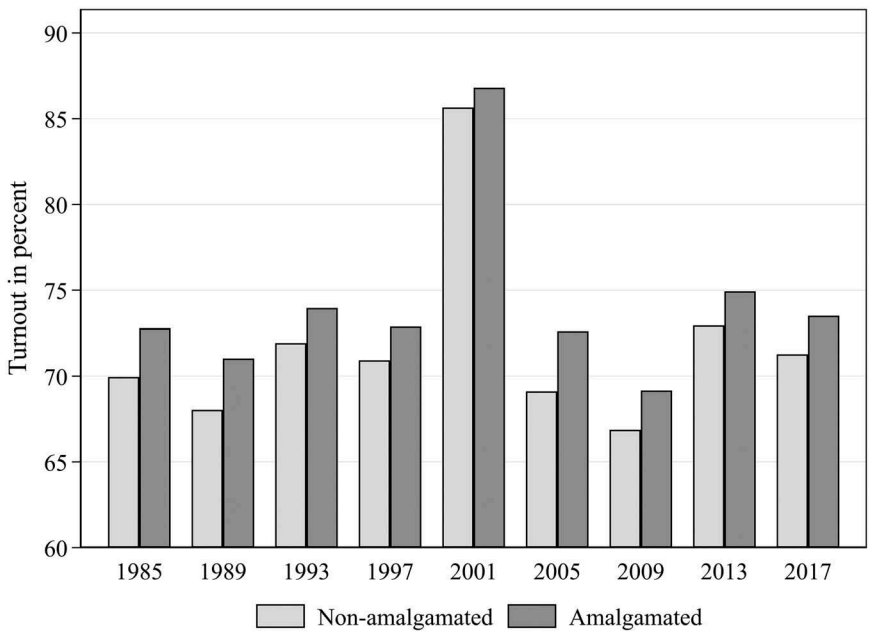

Figure 2. Turnout in non-amalgamated and amalgamated municipalities over time (unit of analysis = new municipalities).

Note: N = 98–31 non-amalgamated municipalities and 67 amalgamated, constituting the entire population of Danish municipalities. Please note that 2001 is atypical due to the simultaneous national election.

In Table 1, we examine the differences between amalgamated and non-amalgamated municipalities over time in first-difference models. We use 1997 as the baseline and examine the difference with the reform period (2005), shortly after the reform (2009), and the medium- to long-term (2013 and 2017). For each year, we estimate two models – one without controls and one with controls for pre-reform levels[4] and differences in the socio-demographical composition over time.

Models (1) and (2) indicate a positive short-term effect of the amalgamations. Turnout in amalgamated municipalities increased about 1½ percentage points compared to non-amalgamated municipalities from 1997 to the first elections for the new municipalities in 2005. When controls are added, the estimated effect is 1.8 percentage points. The effect fits theoretically with the possibility that citizens rally around their local candidates in the short term to ensure that their area gets sufficient representation in the newly amalgamated municipality.

Models (3)-(8) show no statistically significant evidence of medium- to long-term effects of the municipal amalgamations on voter turnout. This is interesting given that most of the existing literature finds negative effects though in settings that are less conducive for solid causal inference than the present case. However, it should be noted the point estimates for 2013 and 2017 are negative and that, given the size of the confidence intervals, we cannot preclude the possibility of effects in the order of about 2 percentage points.

Robustness tests and extensions

We run a range of alternative models to inquire about the robustness of the results. First, as a placebo test, we repeat the first-difference models using specifications identical to the main models for the years prior to the baseline – that is, 1985–1997, 1987–1997, and 1993–1997 – as a test of the plausibility of the assumption that the amalgamated and non-amalgamated municipalities would also develop equally in the absence of the reform (parallel trends). The results depicted in Table A1 in the appendix and in general show negative but insignificant coefficients (except for model 12, which just clears the 0.05-threshold). Thus, if anything, there was a slightly negative trend among amalgamated municipalities, which would favor finding a significant negative reform effect on turnout. While the point estimates for 1985–1997 is only in the order of 0.8–0.9 percentage points and, in general, comforting from the standpoint of the validity of the design, it should be emphasized that the fact that they are not zero leaves some room for concern over the parallel-trends assumption.

We also tried alternative ways of estimating the causal effect. One concern about the models in Table 1 could be that, even though we control for

Table 1. First-difference models of turnout in percent with the 1997 election as a baseline.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ97–05	Δ97–05	Δ97–09	Δ97–09	Δ97–13	Δ97–13	Δ97–17	Δ97–17
Amalgamation	1.54**	1.80**	0.33	0.56	0.029	−0.71	0.29	−0.21
	(0.46)	(0.66)	(0.67)	(0.92)	(0.54)	(0.79)	(0.70)	(0.78)
Δ Immigrants pp.		−0.65		−0.51		−0.49**		−0.55***
		(0.34)		(0.26)		(0.17)		(0.12)
Δ Young and older pp.		−0.21		−0.31		−0.32*		−0.27
		(0.15)		(0.19)		(0.15)		(0.15)
Δ School education pp.		−0.29		−0.28		−0.35		−0.52**
		(0.27)		(0.28)		(0.20)		(0.17)
Δ Eligibles percent		−0.20**		−0.12		−0.017		−0.00041
		(0.064)		(0.067)		(0.044)		(0.035)
Immigrants percent (1997)		0.016		−0.18		−0.060		−0.057
		(0.14)		(0.18)		(0.15)		(0.14)
Young and older percent (1997)		0.22*		0.42*		0.14		0.21
		(0.11)		(0.17)		(0.14)		(0.14)
School education percent (1997)		−0.12*		−0.14		−0.085		−0.23**
		(0.053)		(0.082)		(0.076)		(0.081)
Ln(no. eligible) (1997)		−0.46		−1.45		0.21		0.11
		(0.62)		(0.89)		(0.77)		(0.75)
Ln(areal) 1997		−0.21		−0.68		−0.93*		−0.99*
		(0.38)		(0.52)		(0.45)		(0.44)
Small island municipality		1.06		0.79		0.16		0.47
		(1.32)		(1.83)		(1.60)		(1.57)
Constant	−1.81***	2.56	−4.05***	10.2	2.02***	4.26	0.35	7.27
	(0.38)	(5.21)	(0.55)	(7.23)	(0.45)	(6.28)	(0.63)	(6.12)
N	98	98	98	98	98	98	98	98
F-test	11.43	6.094	0.246	5.942	0.00289	4.472	0.165	8.971
R^2	0.106	0.438	0.00256	0.432	0.000030	0.364	0.00218	0.534
RMSE	2.102	1.761	3.086	2.461	2.493	2.101	2.870	2.072

Unstandardized OLS coefficients. Standard errors in parentheses (robust standard errors applied in model 7). *p < 0.05, ** p < 0.01, *** p < 0.001. 'pp.' = percentage points.

inhabitant size (ln of number of eligible voters) at the outset, the treated and non-treated municipalities are not comparable because the smaller municipalities were under the most profound pressure to amalgamate. To take this potential critique into account, we repeat the multivariate analyses in Table 1 but use coarsened exact matching on 1997 municipality size before the regression (Blackwell et al. 2009; Iacus, King, and Porro 2011; Iacus. King, and Porro 2012) followed by first-difference models.[5] The idea here is only to compare municipalities within the same strata of municipality size; in this case, we use inhabitant sizes of 5,000, 10,000, 20,000, 30,000, 40,000, 70,000, 100,000, 200,000 as cut-offs to define the strata. Strata without at least one treatment and one control unit are eliminated from the analysis. In the subsequent first-difference models, we use weights to take into account the difference in the relative number of treatment and control individuals in each stratum (Blackwell et al. 2009). The results are depicted in Table A2 in the appendix. The results are consistent with Table 1 and even increase the positive short-term effect estimate somewhat to 2.6 percentage points instead of 1.8 percentage points. None of the other models are statistically significant.

Third, we estimate the effect using difference-in-difference models instead of first-difference models. The dependent variable in these models is the level of turnout in the years under investigation, and the amalgamation effect is estimated using interactions between year dummies and an amalgamation dummy. We estimate three models – one without controls, one with controls, and one with controls and their interactions with years to allow for more flexibility in the effect of the controls, that is, that their effect can change over time. The second of the models, the one with controls and no interactions, that is, the standard difference-in-difference model, is considered unrealistic as it assumes that the effect of the controls is constant over the 32 years (see for instance Bhatti et al. 2014 which indicate differences in the socio-demographical differences over time). Therefore, one should put more weight on the first and especially the third model. The results are depicted in Table A3 in the appendix. Unsurprisingly, the results of the model without controls (model 19) is practically identical to the bivariate models in Table 1. The model with controls (model 20), but where we assume the controls to have the same effect over the entire 32-year period investigated, indicate negative amalgamation effects of about 1.7 in 2013 and 2017. However, it should be noted that the same models also find significant effects of almost the same magnitude in the placebo period prior to 1997, which gives additional ground for some skepticism. When, in the final model (model 21) and perhaps more realistically, we allow the control variables to have different effects over time, no significant effects are found though the point estimates for 2009–2017 are still negative. In this model, there

are no significant effects in the placebo period which increase the credibility.

Fourth, we examined the robustness to the year of beginning by substituting the election year 1997 with 1993. The results are depicted in Table A4 and are consistent with the main results, the main difference being that the positive estimate for 2005 is significant at only the $p < 0.10$ level in the multivariate model (in the bivariate model the point estimates and standard errors are very similar to the main analysis).

Fifth, we estimated the causal effect using the synthetic control method (e.g., Abadie and Gardeazabal 2003; Abadie, Diamond and Hainmueller 2010; Abadie, Diamond, and Hainmueller 2015). The advantage of this method over straightforward difference-in-difference models is that it uses a weighted combination of control units to provide a better comparison with the treatment unit(s). For each of the 67 amalgamated municipalities, we estimate the reform effect using a weighted combination of non-amalgamated municipalities from a donor pool of the entire group of non-amalgamated municipalities. The results from these 67 estimations are then averaged. The results (Table A5) resemble the main models, though with a slight tendency of larger negative point estimates (−0.5 to −0.9 percentage points in 2009–2017).

Finally, as an extension of the analysis, we consider the possibility that even though there is no significant treatment effect when we compare amalgamated and non-amalgamated municipalities, relevant differences may exist when we consider the different intensity or complexity of the amalgamations among the amalgamated municipalities. Not all amalgamations were equal. Some were minor in the sense that they included only a few municipalities and that a large municipality just added a small municipality, perhaps a satellite city. Others were more complex and included more municipalities and municipalities of similar size. In the more complex amalgamations, turnout might be more affected than in other amalgamations since the amalgamation might be felt more intensely by the average citizen and more citizens experienced a substantial increase in size. To capture this difference in complexity, we use the Herfindahl index based on the share of voters from the old municipalities in the new municipality, which informs us about the fragmentation of the new municipality in terms of municipalities (for a similar logic, see Blom-Hansen, Monkerud, and Sørensen 2006).[6] A Herfindahl index of 1 indicates that the new municipality comprises only one old municipality. The closer to 0, the more spread the voters are with respect to old municipalities, indicating a more complex amalgamation. We also use an alternative – and simpler – measure of complexity, namely the number of municipalities in the amalgamation.

When the Herfindahl index in Table A6 is used instead of the amalgamation dummy, we find significant effects in the expected direction for

1997–2013 and 1997–2017 while 1997–2009 is insignificant but $p < 0.10$. In Table A7 we added the Herfindahl index squared and find curvilinear tendencies, indicating that the effect of complexity may be strongest for the most complex amalgamations (low values of the index).

Similar results are found when using three dummy variables for the number of old municipalities in the new one as an alternative measure (Table A8). The higher the number of old municipalities, the more turnout declines in the periods 1997–2009, 1997–2013, and 1997–2017 though the 5 + municipalities dummy is only significant in 1997–2013 (the dummies are also jointly significant in 1997–2005 and 1997–2009 while the 5+ municipalities dummy reaches $p < 0.10$ in 1997–2009 and 1997–2017). Like in Table A7 there is some tendency of effect to mainly set-in for the highest level of complexity (the 5 + dummy) though we are somewhat limited by the number of cases in each category. The results indicate that municipalities that experienced the most complex amalgamations experienced a decrease in turnout even though there is no robust overall difference between amalgamated and non-amalgamated municipalities. In conclusion, the robustness tests support the main results but add some nuances.

Discussion and conclusion

In this study, we have utilized the Danish municipal reform to inquire into the possible effects of municipal amalgamations on turnout. At the same time, the study is relevant for the broader debate about the relationship between size and democracy as an increase in municipality size is one of the most likely ways amalgamations could affect turnout.

Overall, we find some evidence in favor of a short-term positive effect, which could be consistent with a rally-around-the-flag short-term tendency of electoral engagement to secure the influence of the old municipalities in the new ones. The positive effect seems to be in the order of 1.5–-2 percentage points. We find no clear evidence of a medium- to long-term negative effect on turnout as one would expect from the existing literature though it should be noted that most of the coefficients are negative but insignificant. Looking at the uncertainty of the estimates, effects in the order of about 2 percentage points cannot be precluded. We find stronger indications of negative effects of amalgamation on turnout for those municipalities that experienced the most profound changes.

The limited or mixed evidence in favor of a negative amalgamation effect may seem intriguing, particularly given that existing studies of the Danish municipal reform mainly find more unambiguous negative effects on democracy (e.g., Lassen and Serritzlew 2011; Hansen 2012; Nielsen and Vestergaard 2014; Hansen 2015). However, the results are not necessarily inconsistent. Most importantly, most of the existing studies look at relatively short-term effects

until 2008 or 2009, and besides the 2005 election, which is pre-reform, our first point of measurement is in late 2009. Existing studies of more long-term effects from 2013 generally find more limited reform effects than the studies that look at short-term effects (Hansen and Hjelmar 2015). Another explanation is the fact that most existing studies are based on subjective measures of democracy (and surveys). It is possible that the decrease in efficacy, trust, satisfaction with democracy, and so on does not translate into behavior in terms of less voting, for instance, because the norms of voting are so strong that they are not easily eroded. Finally, it should be noted that the findings of this study do not preclude small negative effects. In future studies, it would be interesting to inquire into whether amalgamation effects on voting can be found among new voters who have not yet built up a strong habit of voting (Franklin 2004).

Methodologically, this study has contributed to the existing literature by investigating a unique case in which amalgamations were conducted at the same point in time for reasons likely to be unrelated to democratic performance and where both treated and control municipalities existed. This strengthens the causal estimates from the study and constitutes a contribution to the existing literature. Could causal inference be strengthened even further? The main downside of the study is the spacing between the elections studied and the fact that which municipalities were merged was not random. Due to the simultaneous national and municipal elections in 2001, the baseline period in the models was 1997, about nine years before the reform was implemented. In such a period, it is possible to imagine changes in turnout that were not equally distributed among non-amalgamated and amalgamated municipalities and which, therefore, could be confused with reform effects. Thus, future studies could improve causal inference further by investigating cases with similar advantages as the Danish, but with a shorter time span between the pre-reform and the post-reform points of measurement or, alternatively, a case in which it was fully random which units were merged.

Notes

1. In the analyses, we operate with 67 merged municipalities and 31 non-merged as we include the municipality of Bornholm as a merged one. This municipality was merged from five to one in 2003, and as we will return to, we utilize 1997 as the base year in the analyses.
2. A few old municipalities were divided into several new municipalities in the reform. In the analyses, these are counted as part of the new municipality in which the majority of their constituent became a part. We have tested an alternative operationalization in which we use a proportional weight based on the share of voters who were transferred to the different municipalities (based on the number of eligible voters in the 2001 elections) which yields almost identical results as the ones in Table 1.

3. Ln(20000)-ln(10000) = 0.69 meaning that a coefficient of −3 corresponds to a decrease of about 2 percentage points. $p < 0.05$.
4. Percent young and older is operationalized as the percent of age 18–29 and 80 + among the population of 18+ year-olds in the municipality. Percent school education is the percentage of 15–69-year-olds with a school education (less than high school). Immigrants percent is the percent of 18+ year-olds in the municipality with a non-Danish background. Small-island municipality is the municipalities Langeland, Ærø, Fanø, Samsø, and Læsø. Note for the 2017 election (held November, 2017) we use education and area data from late 2016/early 2017. For robustness analyses that include elections prior to 1993, late 1990 data on education are used for 1985 and 1989 as this variable does not go further back in the register used.
5. Note that as the unit of analysis is post-reform municipalities, so these are also the basis for the matching.
6. The index is calculated as the sum of the squared proportions of eligible voters from each old municipality in the new municipality.

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No potential conflict of interest was reported by the authors.

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Appendix

Table A1. First-difference models of turnout in percent.

	(9)	(10)	(11)	(12)	(13)	(14)
	Δ85–97	Δ85–97	Δ89–97	Δ89–97	Δ93–97	Δ93–97
Amalgamation	−0.88	−0.81	−1.03	−1.50*	−0.079	−0.57
	(0.46)	(0.77)	(0.54)	(0.75)	(0.35)	(0.58)
Δ Immigrants pp.		−0.091		0.29		0.48
		(0.29)		(0.31)		(0.36)
Δ Young and older pp.		−0.43*		−0.82**		−0.69*
		(0.21)		(0.25)		(0.28)
Δ School education pp.		0.22		0.27		0.47
		(0.33)		(0.32)		(0.39)
Δ Eligibles percent		0.082		0.13		0.12
		(0.051)		(0.091)		(0.13)
Immigrants percent (start year)		−0.028		−0.28		−0.020
		(0.20)		(0.20)		(0.15)
Young and older p. (start year)		0.11		−0.22		−0.029
		(0.15)		(0.13)		(0.086)
School education p. (start year)		−0.031		−0.027		−0.048
		(0.062)		(0.059)		(0.043)
Ln(no. eligible) (start year)		0.52		2.09**		0.14
		(0.74)		(0.69)		(0.51)
Ln(areal) (start year)		−0.12		−0.038		0.64*
		(0.47)		(0.43)		(0.32)
Small island municipality		1.59		3.78*		2.05
		(1.56)		(1.50)		(1.08)
Constant	0.97*	−5.59	2.89***	−12.0	−1.00***	−3.06
	(0.38)	(8.36)	(0.48)	(7.30)	(0.29)	(5.10)
N	98	98	98	98	98	98
F-test	3.634	2.397	3.679	4.006	0.0521	1.993
R^2	0.0365	0.235	0.0448	0.339	0.000543	0.203
RMSE	2.128	2.003	2.241	1.970	1.593	1.503

Unstandardized OLS coefficients. Standard errors in parentheses (robust standard errors in model 11). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. 'pp.' = percentage points.

Table A2. First-difference models of turnout in percent with the 1997 election as baseline using a matched sample.

	(15)	(16)	(17)	(18)
	Δ97–05	Δ97–09	Δ97–13	Δ97–17
Amalgamation	2.64***	0.080	−1.01	−0.21
	(0.69)	(1.02)	(0.85)	(0.85)
Δ Immigrants pp.	−0.26	−0.56	−0.47**	−0.52***
	(0.35)	(0.29)	(0.17)	(0.12)
Δ Young and older pp.	−0.20	−0.23	−0.37*	−0.40*
	(0.14)	(0.18)	(0.15)	(0.16)
Δ School education pp.	−0.24	−0.20	−0.53**	−0.55**
	(0.27)	(0.30)	(0.20)	(0.18)
Δ Eligibles percent	−0.18**	−0.075	0.0057	0.0095
	(0.058)	(0.065)	(0.041)	(0.033)
Immigrants percent (1997)	−0.090	−0.18	−0.017	0.012
	(0.13)	(0.18)	(0.15)	(0.15)
Young and older percent (1997)	0.19	0.53**	0.15	0.24
	(0.10)	(0.17)	(0.14)	(0.14)
School education percent (1997)	−0.12*	−0.19*	−0.17*	−0.26**
	(0.049)	(0.080)	(0.076)	(0.081)
Ln(no. eligible) (1997)	−0.77	−1.85	0.37	0.47
	(0.63)	(0.94)	(0.79)	(0.79)
Ln(areal) 1997	−0.27	−0.27	−0.74	−0.88
	(0.37)	(0.54)	(0.47)	(0.46)
Small island municipality	1.61	2.34	1.55	1.53
	(1.42)	(2.06)	(1.73)	(1.72)
Constant	6.17	12.7	2.84	2.33
	(5.76)	(8.24)	(7.01)	(6.94)
N	93	93	93	93
F-test	8.468	5.133	4.978	8.274
R^2	0.535	0.411	0.403	0.529
RMSE	1.578	2.318	1.930	1.931
Treatment observations matched	67	67	67	67
Treatment observations unmatched	0	0	0	0
Control observations matched	26	26	26	26
Control observations unmatched	5	5	5	5

Unstandardized OLS coefficients. Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. 'pp.' = percentage points. Matching on no. of eligible individuals in 1997 (cut-offs: 5,000, 10,000, 20,000, 30,000, 40,000, 70,000, 100,000, 200,000). CEM-weights applied.

Table A3. Difference-in-difference models with turnout in percent as the dependent variable.

	(19)	(20)	(21)
	Turnout		
	All municipalities (98) across all 9 election years		
Amalgamation	1.96*	0.58	0.0096
	(0.94)	(0.81)	(0.96)
Year 1985	−0.97*	0.19	−6.24
	(0.42)	(0.66)	(5.42)
Year 1989	−2.89***	−1.52*	−0.95
	(0.48)	(0.62)	(5.59)
Year 1993	1.00**	1.84***	0.66
	(0.33)	(0.45)	(3.69)
Year 2001	14.7***	14.2***	−3.40
	(0.57)	(0.60)	(4.86)
Year 2005	−1.81***	−2.65***	2.22
	(0.43)	(0.49)	(5.25)
Year 2009	−4.05***	−4.58***	10.7
	(0.62)	(0.64)	(7.12)
Year 2013	2.02***	1.67**	4.10
	(0.50)	(0.62)	(5.81)
Year 2017	0.35	0.38	3.74
	(0.64)	(0.80)	(5.81)
Year 1985*amalgamation	0.88	1.67**	0.67
	(0.49)	(0.54)	(0.80)
Year 1989*amalgamation	1.03	1.72**	1.15
	(0.54)	(0.58)	(0.90)
Year 1993*amalgamation	0.079	0.49	0.30
	(0.37)	(0.39)	(0.60)
Year 2001*amalgamation	−0.80	−1.30	0.38
	(0.62)	(0.67)	(0.85)
Year 2005*amalgamation	1.54**	0.62	1.62*
	(0.49)	(0.45)	(0.74)
Year 2009*amalgamation	0.33	−1.05	0.41
	(0.71)	(0.61)	(0.97)
Year 2013*amalgamation	0.029	−1.73***	−0.71
	(0.58)	(0.48)	(0.72)
Year 2017*amalgamation	0.29	−1.67**	−0.23
	(0.71)	(0.60)	(0.72)
Immigrants percent		−0.24***	−0.0063
		(0.051)	(0.11)
Young and older percent		−0.11	−0.20
		(0.091)	(0.12)
School education percent		−0.28***	−0.30***
		(0.035)	(0.051)
Ln(no. eligible)		−3.66***	−4.11***
		(0.43)	(0.64)
Ln(area)		1.54***	2.28***
		(0.27)	(0.37)
Small island municipality		−0.14	−0.94
		(0.78)	(1.32)
Interactions between control variables and year	No	No	Yes
Constant	70.9***	116.3***	118.8***
	(0.87)	(3.65)	(5.41)
N	882	882	882
F-test	309.3	388.3	.
R^2	0.631	0.860	0.884
RMSE	3.871	2.390	2.236

Unstandardized OLS coefficients. Standard errors clustered at the municipal level in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Year specific controls are used with a few exceptions: Late 1990 data on education are used for 1985 and 1989 as this variable does not go further back in the register used while data from late 2016/early 2017 is used for education and area in 2017.

Table A4. First-difference models of turnout in percent with the 1993 election as baseline.

	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)
	Δ93–05	Δ93–05	Δ93–09	Δ93–09	Δ93–13	Δ93–13	Δ93–17	Δ93–17
Amalgamation	1.47**	1.30	0.25	0.15	−0.050	−1.12	0.21	−0.74
	(0.55)	(0.73)	(0.63)	(0.86)	(0.57)	(0.74)	(0.69)	(0.78)
Δ Immigrants pp.		−0.090		−0.57*		−0.46**		−0.49***
		(0.31)		(0.23)		(0.15)		(0.11)
Δ Young and older pp.		−0.27		−0.19		−0.29*		−0.26
		(0.14)		(0.16)		(0.13)		(0.14)
Δ School education pp.		−0.096		−0.069		−0.15		−0.25
		(0.22)		(0.21)		(0.16)		(0.15)
Δ Eligibles percent		−0.11		−0.075		−0.0072		0.0019
		(0.057)		(0.053)		(0.036)		(0.031)
Immigrants percent (1993)		0.026		0.080		0.18		0.086
		(0.21)		(0.22)		(0.19)		(0.19)
Young and older percent (1993)		0.13		0.47**		0.15		0.23
		(0.13)		(0.17)		(0.14)		(0.14)
School education percent (1993)		−0.16*		−0.12		−0.062		−0.17*
		(0.061)		(0.076)		(0.072)		(0.081)
Ln(no. eligible) (1993)		−0.26		−1.49		0.28		0.38
		(0.67)		(0.82)		(0.72)		(0.74)
Ln(areal) 1993		0.57		0.022		−0.23		−0.39
		(0.41)		(0.48)		(0.42)		(0.44)
Small island municipality		3.78**		3.46*		2.60		3.30*
		(1.40)		(1.67)		(1.48)		(1.54)
Constant	−2.81***	−1.70	−5.05***	3.95	1.02*	−2.45	−0.65	−1.63
	(0.40)	(5.85)	(0.52)	(6.70)	(0.42)	(5.88)	(0.63)	(6.01)
N	98	98	98	98	98	98	98	98
F-test	7.223	4.990	0.160	6.360	0.00770	4.610	0.0887	8.127
R^2	0.0862	0.390	0.00167	0.449	0.0000990	0.371	0.00121	0.510
RMSE	2.241	1.935	2.917	2.291	2.356	1.974	2.792	2.066

Unstandardized OLS coefficients. Standard errors in parentheses (robust standard errors in model 22, 26 and 28). *p < 0.05, ** p < 0.01, *** p < 0.001. 'pp.' = percentage points.

Table A5. Synthetic control method. Average across 67 treatment units.

	Average synthetic control	Average treatment	Average difference in percentage points
1985 election	72.64	72.79	0.15
1989 election	70.64	71.03	0.38
1993 election	73.77	73.96	0.20
1997 election	72.98	72.88	−0.10
2005 election	71.84	72.62	0.78
2009 election	69.61	69.16	−0.45
2013 election	75.84	74.94	−0.91
2017 election	74.07	73.52	−0.56

Immigrants percent, young and older percent, school education percent, ln(no. eligible), ln(area), small island municipality averaged over the pre-treatment period is used as predictor variables. Turnout (1985) and (turnout (1997) are also included.

Table A6. First-difference models of turnout in percent with the 1997 election as baseline.

	(30)	(31)	(32)	(33)	(34)	(35)	(36)	(37)
	Δ97–05	Δ97–05	Δ97–09	Δ97–09	Δ97–13	Δ97–13	Δ97–17	Δ97–17
Herfindahl index	−1.99**	−1.98	0.70	2.54	0.90	4.43***	0.55	3.00*
	(0.72)	(1.12)	(1.04)	(1.52)	(0.84)	(1.24)	(0.97)	(1.28)
Δ Immigrants pp.		−0.65		−0.47		−0.47**		−0.54***
		(0.35)		(0.26)		(0.16)		(0.11)
Δ Young and older pp.		−0.21		−0.38*		−0.45**		−0.37*
		(0.16)		(0.19)		(0.15)		(0.15)
Δ School education pp.		−0.25		−0.37		−0.40*		−0.51**
		(0.28)		(0.28)		(0.18)		(0.16)
Δ Eligibles percent		−0.18**		−0.12		−0.024		−0.0045
		(0.065)		(0.066)		(0.041)		(0.034)
Immigrants percent (1997)		−0.0037		−0.17		−0.058		−0.073
		(0.14)		(0.17)		(0.14)		(0.14)
Young and older percent (1997)		0.23*		0.26		−0.054		0.063
		(0.12)		(0.18)		(0.14)		(0.14)
School education percent (1997)		−0.13*		−0.14		−0.071		−0.21**
		(0.055)		(0.081)		(0.072)		(0.079)
Ln(no. eligible) (1997)		−0.55		−0.98		0.88		0.63
		(0.65)		(0.90)		(0.75)		(0.76)
Ln(areal) 1997		−0.033		−0.20		−0.54		−0.66
		(0.38)		(0.51)		(0.41)		(0.42)
Small island municipality		1.05		0.79		0.29		0.67
		(1.35)		(1.81)		(1.50)		(1.53)
Constant	0.43	5.23	−4.24***	4.46	1.50**	−4.71	0.22	0.85
	(0.48)	(5.78)	(0.69)	(7.74)	(0.56)	(6.43)	(0.64)	(6.52)
N	98	98	98	98	98	98	98	98
F-test	7.576	5.443	0.452	6.330	1.173	6.177	0.322	10.03
R^2	0.0731	0.410	0.00469	0.447	0.0121	0.441	0.00334	0.562
RMSE	2.141	1.804	3.083	2.427	2.478	1.969	2.868	2.009

Unstandardized OLS coefficients. Standard errors in parentheses. *p < 0.05, ** p < 0.01, *** p < 0.001. 'pp.' = percentage points.

Table A7. First-difference models of turnout in percent with the 1997 election as baseline.

	(38)	(39)	(40)	(41)	(42)	(43)	(44)	(45)
	Δ97–05	Δ97–05	Δ97–09	Δ97–09	Δ97–13	Δ97–13	Δ97–17	Δ97–17
Herfindahl index	9.93*	8.90*	24.3***	23.0***	19.5***	21.0***	20.6**	17.1***
	(4.96)	(4.40)	(6.95)	(5.69)	(5.60)	(4.66)	(6.53)	(4.84)
Herfindahl index^2	−9.18*	−8.58*	−18.2***	−16.2***	−14.3**	−13.0***	−15.4**	−11.1**
	(3.78)	(3.36)	(5.30)	(4.35)	(4.27)	(3.56)	(4.98)	(3.70)
Δ Immigrants pp.		−0.61		−0.36		−0.39*		−0.49***
		(0.34)		(0.24)		(0.15)		(0.11)
Δ Young and older pp.		−0.22		−0.40*		−0.50***		−0.41**
		(0.15)		(0.17)		(0.14)		(0.15)
Δ School education pp.		−0.34		−0.50		−0.48**		−0.55***
		(0.27)		(0.27)		(0.17)		(0.15)
Δ Eligibles percent		−0.21**		−0.16*		−0.051		−0.023
		(0.064)		(0.062)		(0.039)		(0.033)
Immigrants percent (1997)		0.027		−0.14		−0.048		−0.065
		(0.14)		(0.16)		(0.13)		(0.13)
Young and older percent (1997)		0.19		0.16		−0.13		0.0079
		(0.11)		(0.17)		(0.14)		(0.14)
School education percent (1997)		−0.12*		−0.14		−0.080		−0.22**
		(0.053)		(0.075)		(0.067)		(0.076)
Ln(no. eligible) (1997)		−0.40		−0.69		1.18		0.84
		(0.63)		(0.85)		(0.70)		(0.73)
Ln(areal) 1997		−0.15		−0.40		−0.73		−0.80
		(0.37)		(0.47)		(0.39)		(0.40)
Small island municipality		0.94		0.62		0.14		0.58
		(1.31)		(1.69)		(1.40)		(1.46)
Constant	−2.59	1.62	−10.2***	−1.62	−3.21*	−9.73	−4.86**	−3.19
	(1.33)	(5.78)	(1.86)	(7.40)	(1.50)	(6.17)	(1.75)	(6.38)
N	98	98	98	98	98	98	98	98
F-test	6.924	5.854	6.126	7.824	6.289	7.600	4.978	10.81
R^2	0.127	0.452	0.114	0.525	0.117	0.518	0.0949	0.604
RMSE	2.089	1.749	2.924	2.264	2.355	1.840	2.748	1.921

Unstandardized OLS coefficients. Standard errors in parentheses. *p < 0.05, ** p < 0.01, *** p < 0.001. 'pp.' = percentage points.

Table A8. First-difference models of turnout in percent with the 1997 election as baseline.

	(46)	(47)	(48)	(49)	(50)	(51)	(52)	(53)
	Δ97–05	Δ97–05	Δ97–09	Δ97–09	Δ97–13	Δ97–13	Δ97–17	Δ97–17
2 municipalities	2.04**	1.79*	1.73	0.72	0.68	−0.59	0.53	−0.27
	(0.69)	(0.71)	(0.96)	(0.96)	(0.78)	(0.81)	(0.91)	(0.83)
3–4 municipalities	1.66**	1.71*	0.80	−0.14	0.56	−1.35	0.89	−0.52
	(0.51)	(0.77)	(0.71)	(1.03)	(0.57)	(0.86)	(0.74)	(0.89)
5+ municipalities	0.88	1.16	−1.91*	−2.30	−1.76*	−3.43**	−1.36	−1.91
	(0.65)	(0.96)	(0.90)	(1.29)	(0.73)	(1.08)	(0.91)	(1.13)
Δ Immigrants pp.		−0.59		−0.32		−0.36*		−0.49***
		(0.35)		(0.26)		(0.16)		(0.12)
Δ Young and older pp.		−0.21		−0.33		−0.39**		−0.31*
		(0.16)		(0.18)		(0.14)		(0.15)
Δ School education pp.		−0.31		−0.34		−0.35		−0.50**
		(0.27)		(0.27)		(0.19)		(0.16)
Δ Eligibles percent		−0.21**		−0.15*		−0.043		−0.017
		(0.066)		(0.067)		(0.044)		(0.036)
Immigrants percent (1997)		0.017		−0.20		−0.093		−0.071
		(0.14)		(0.17)		(0.14)		(0.14)
Young and older percent (1997)		0.19		0.24		−0.020		0.11
		(0.11)		(0.17)		(0.14)		(0.14)
School education percent (1997)		−0.12*		−0.13		−0.072		−0.22**
		(0.054)		(0.079)		(0.073)		(0.081)
Ln(no. eligible) (1997)		−0.31		−0.62		1.04		0.60
		(0.66)		(0.90)		(0.78)		(0.79)
Ln(areal) 1997		−0.11		−0.27		−0.56		−0.75
		(0.39)		(0.51)		(0.43)		(0.45)
Small island municipality		1.19		1.47		0.85		0.90
		(1.34)		(1.78)		(1.53)		(1.57)
Constant	−1.81***	1.16	−4.05***	3.05	2.02***	−2.79	0.35	3.21
	(0.38)	(5.61)	(0.52)	(7.44)	(0.43)	(6.40)	(0.64)	(6.53)

(*Continued*)

Table A8. (Continued).

	(46)	(47)	(48)	(49)	(50)	(51)	(52)	(53)
	Δ97–05	Δ97–05	Δ97–09	Δ97–09	Δ97–13	Δ97–13	Δ97–17	Δ97–17
N	98	98	98	98	98	98	98	98
F-test	4.648	5.164	4.554	6.195	3.973	5.169	3.182	8.158
R^2	0.129	0.444	0.127	0.489	0.113	0.444	0.0754	0.558
RMSE	2.097	1.773	2.918	2.360	2.374	1.987	2.792	2.042

Unstandardized OLS coefficients. Standard errors in parentheses (robust standard errors in model 52). *p < 0.05, ** p < 0.01, *** p < 0.001. 'pp.' = percentage points.