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# Social Democratic Representation and Welfare Spending: A Quantitative Case Study\*

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## Abstract

The welfare state literature argues that Social Democratic party representation is of key importance for welfare state outcomes. However, few papers are able to separate the influence of parties from voter preferences, which implies that the partisan effects will be overstated. I study a natural experiment to identify a partisan effect. In 1995, the Labour Party (Ap) in the Norwegian municipality of Flå filed their candidate list too late and could not participate in the local election. Ap was the largest party in Flå in the entire post-World War period, but have not regained this position. I use the synthetic control method to study the effects on welfare spending priorities. I find small and insignificant partisan effects.

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## Introduction

Do political parties matter for policy outcomes? The modern literature on this classic topic goes back at least to Hibbs (1977), who argued that class differences in preferences for unemployment and inflation policies are reflected in the behaviour of left and right parties in government. The view that partisanship matters has been particularly influential in the welfare state literature, where the role of Social Democratic parties in welfare state development has been emphasized (Korpi and Palme 2003; Esping-Andersen 1990). Recently, the literature has taken a new direction to the study of partisan effects at the local level (Fiva, Folke, and Sørensen forthcoming).

The partisanship literature is often unclear in its definition of partisan effects. Most studies follow in the spirit of Hibbs (1977) in that they point to class differences in economic preferences and parties' electoral constituencies as the basis for partisan effects on public policy. This understanding of partisan effects risks conflating voter effects and partisan effects (see Lee, Moretti, and Butler 2004, and the extended discussion in the online appendix). Following the political economy literature (Besley and Coate 1997), partisan effects refer to effects of who governs, controlling for voter preferences. To separate voter and partisan effects is an empirical challenge. In essence, the challenge is to estimate the effect of partisanship while controlling for voter preferences. Unfortunately, we do not have good measures of voter preferences across different policy outcomes. Thus, one needs exogenous variation in partisanship to estimate a credible partisan effect (Lee, Moretti, and Butler 2004).

I leverage a natural experiment to get exogenous variation in partisanship. In 1995, the Labour party (Ap) in the Norwegian municipality Flå failed to file their candidate list in time to participate in the local election, implying that they had no representation in the following election period. I study the effects of this shock on four spending outcomes using the synthetic control method (Abadie, Diamond, and Hainmueller 2010). This method is particularly suited for quantitative case studies and has been labeled “the most important innovation in the policy evaluation literature in the last 15 years” (Athey and Imbens 2017, 9). I follow a number of recent recommendations of how to apply the

synthetic control method to avoid potential pitfalls. The paper should therefore be of interest for researchers who want to apply the synthetic control method.

The overall conclusion is that the welfare spending consequences of the decline of Ap are small and insignificant. In the conclusion I relate the findings to the previous literature on partisan effects.

## **The Decline of Flå Ap**

Norwegian municipalities are governed by municipal councils elected in a proportional electoral system with a single electoral district. The municipal council elects an executive municipal board with proportional representation of the elected parties. One member of the executive board is elected as the major. The mayor chairs the council meetings and has representational obligations, but is granted limited legal and formal power by law. Local elections are important, as municipalities are responsible for local infrastructure and the provision of the key welfare services, including childcare, primary education, elderlycare, and social assistance.

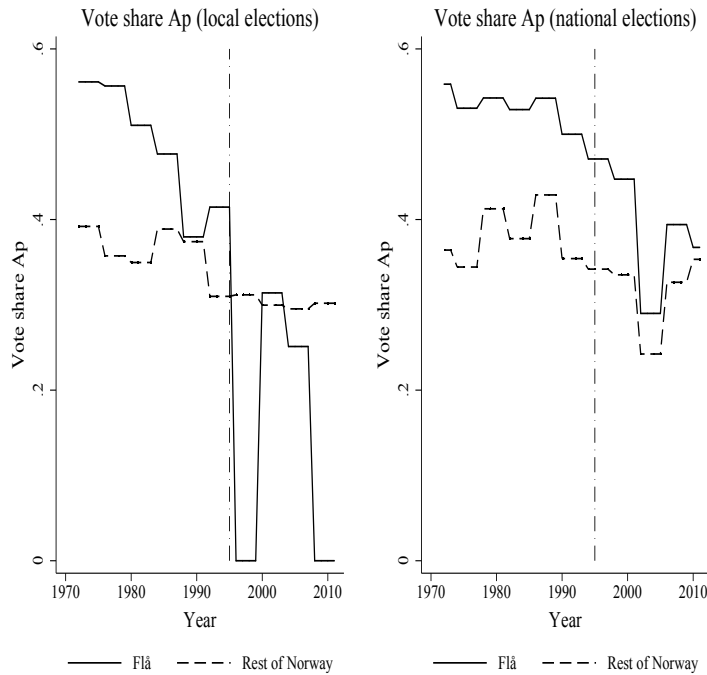
Flå is located in Buskerud county, 2-3 hours drive north-west of Oslo. It is a rural municipality with 1081 inhabitants (2017). Ap dominated local politics in Flå in the post World War period: They had more than 50 percent vote share in all elections until 1983 (see Figure 1), had the major until 1988, and had the highest vote share in all elections prior to 1995. It was therefore a major event when Ap were not allowed to run for election in 1995. The election law demanded that parties had to register their list of candidates by June 1, 1995 in order to participate in the September election. Unfortunately, the party leader mistakenly believed that the deadline was June 25, so he failed to deliver the list on time. I argue that this personal mistake could have happened in any other small municipality where the party organization is run by voluntary work.<sup>1</sup>

Since Ap did not run, they had no representation in the 1995-1999 election period. The majority of Ap sympathizers stayed at home in 1995, as turnout fell from 79 percent

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<sup>1</sup>One possibility is that the mistake is a signal of a local party organization in disarray. I have not come across any evidence indicating that this was the case. See the online appendix B for further discussion.

Figure 1: Ap's vote share in Flå and in the rest of Norway 1972-2011.



in 1991 to 43 percent in 1995. The votes were split between Bygdelista (B) and Borgerlig Fellesliste (BF). B, Ap's main competitor, is a local list with no official ties to any of the national parties. BF is a center-right list of candidates from the Conservative Party, the Christian Democratic Party, and the Center Party. No parties to the left of Ap had any representation, thus the absence of Ap implied a strong rightward shift of the municipal council.<sup>2</sup> At the time when Ap was disqualified, B and BF had filed their candidate lists and decided their party programs, thus they could not credibly change their policies in response to Ap's exit from the election.

In the 1999 election Ap received 31 percent of the vote share, which (except for 1995) was the worst election result in the post-war period. B manifested its position as the major party. Ap's vote share fell to 25 percent in 2003, while in 2007 they failed to get enough candidates to run its own list and filed a joint list with the Center Party and the Christian People's Party. Thus, the 1995 error had long-run repercussions. Figure 1 displays the vote shares for Ap in Flå and the rest of Norway in local and national

<sup>2</sup>See the online appendix B for an approach to quantify the size of the shift. I find that the left-right shift in Flå from 1991 to 1995 is the most extreme shift compared to the shifts in the control municipalities. The shift is also large in comparison with the shifts from 1987 to 1991.

elections, 1972-2011. As evident, Ap's vote share in local elections was stable in the rest of Norway from 1995 and onwards. Importantly, we see that nothing dramatic happened to Ap's vote share in Flå in the national elections in 1993 and 1997, which is consistent with stable voter preferences in Flå around 1995. Thus, we have a shock in representation which is unrelated to voter preferences.

A key premise for the existence of partisan effects is ideological polarization. It is not obvious that ideological polarization exists in small municipalities, since citizens' legislated rights to welfare services have been strengthened over the last decades (Østerud and Selle 2006). Moreover, the importance of individual characteristics of local politicians has been emphasized in the literature (Munkerud 2007; Hyytinen et al. forthcoming). Still, Fiva et al. (2017) present results from surveys of local council members 1999-2007 which show left-right-divides on tax and spending preferences. In particular, left parties prioritize spending on children, while right parties prioritize the elderly. Although these results are averages across municipalities and might not be perfectly accurate for Flå, Fiva et al. (2017: 15) find that politicians' spending preferences vary "only moderately" across time and space. Moreover, survey data indicate that voters in small municipalities believe that local politics matter (see online appendix B).

## **Empirical Strategy**

The empirical challenge is to estimate the counterfactual development of welfare spending in Flå. Two strategies dominate in the literature; the difference-in-difference (DD) approach and the synthetic control (SC) method. In this note I apply the SC method.

The main benefits of SC compared to DD are a data-driven selection of comparison cases and a less restrictive assumption regarding time-varying confounders. While the DD-approach gives equal weight to each unit in the control group, the SC method allows the units to have different weights in order to construct a synthetic control group that matches the pre-treatment outcomes of the treated unit. The idea is that similar outcomes in the pre-treatment period make it more plausible that the post-treatment outcomes of the synthetic control is a good counterfactual to the development of the treated unit.

The SC modeling of the outcomes can be expressed by the following equation (Abadie, Diamond, and Hainmueller 2010):

$$Y_{it} = \alpha_{it}FLA_{it} + Y_{it}^C = \alpha_{it}FLA_{it} + \delta_t + \boldsymbol{\theta}_t\mathbf{Z}_i + \boldsymbol{\lambda}_t\boldsymbol{\mu}_i + \epsilon_{it}$$

where  $Y_{it}^C$  is the counterfactual outcome and  $\alpha_{it}$  is the treatment effect of Flå Ap's decline in period  $t$ . The counterfactual outcome is constructed from time fixed effects  $\delta_t$ , a set of variables  $\mathbf{Z}_i$  which are not affected by the treatment, with  $\boldsymbol{\theta}_t$  as a set of unknown associated parameters. Finally,  $\boldsymbol{\lambda}_t$  is a set of time-varying unobserved variables with associated unknown and unit-varying factor loadings  $\boldsymbol{\mu}_i$ . By constructing a set of weights across the control units, the SC method matches the pre-treatment development of the treated unit with a weighted combination of the control units. Following Abadie et al. (2010: 495), if the weighted control fits the pre-treatment development of the treated unit, then it is plausible that it will approximately fit the unobserved confounders as well.

A number of issues arises in empirical applications of the SC method. The first regards the restriction of potential contributors to the synthetic control. Abadie et al. (2010, 2015) recommend that the donor pool of potential control units is restricted to units with similar pre-period characteristics as the treated unit. This is a useful restriction because the linear combination of the control units to match Flå might involve a lot of interpolation, and hence bias, if donor pool units are very different (Abadie et al. 2010: 495). Since the administrative error is unlikely to have happened in a large municipality, I restrict the donor pool to the 164 municipalities who, like Flå, are classified as "least central" in the 1994 version of Statistics Norway's index of centrality.<sup>3</sup>

The second issue regards what variables to use as inputs. There are two decisions to be made; i) how many pre-treatment outcomes to include and ii) what covariates to include. The first is the most important issue since the pre-treatment outcomes have the strongest predictive power (Doudchenko and Imbens 2016). Abadie et al. (2010) do not offer much guidance on this issue, and the lack of guidance can result in considerable

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<sup>3</sup>See <https://www.ssb.no/klass/#!/klassifikasjoner/128/versjon/469>. I restrict the sample to municipalities with unchanged boundaries over the analysis period.



researcher degrees of freedom to pick the specification that yields the preferred conclusions (Ferman, Pinto, and Possebom 2017). To reduce this worry I estimate a set of different specifications and use decision rules to pick specifications for further analyses (see Ferman, Pinto, and Possebom 2017; Dube and Zipperer 2015).

Specifically, I estimate ten different SC specifications, following Ferman et al. (2017).<sup>4</sup> The specifications are listed in the note to Table 2. For each of the ten specifications I first run the model with Flå as the treated unit and then with each of the control units in the donor pool as the (placebo) treated unit. To assess how well the synthetic control fit the trend in Flå, I calculate Ferman et al’s (2017) normalized mean squared error index  $\tilde{R}^2$ :

$$\tilde{R}^2 = 1 - \frac{\sum_{t=1}^{T_0} (Y_{FLA,t} - \hat{Y}_{FLA,t}^N)^2}{\sum_{t=1}^{T_0} (Y_{FLA,t} - \bar{Y}_{FLA})^2} \quad (1)$$

where  $T_0$  are the pre-treatment years,  $Y_{FLA,t}$  are the outcomes in Flå,  $\hat{Y}_{FLA,t}^N$  the outcomes for the synthetic control, and  $\bar{Y}_{FLA}$  is the average for Flå over the pre-treatment period.  $\tilde{R}^2 = 1$  implies perfect fit. Next I derive a p-value for the treatment effect using the permutation test in Abadie et al. (2010). The ranking of Flå’s ratio in the distribution of all ratios is used to derive the p-value of the average treatment effect.

Finally, to choose among the ten treatment estimates and associated p-values, I exclude models with  $\tilde{R}^2$  below .8 (Ferman et al. 2017) since the SC should only be used when the pre-fit is good (Abadie et al. 2010). Next I calculate the mean post-period MSPE for the placebo estimates, and treat the specification with the smallest MSPE as the “best” specification (Dube and Zipperer 2015). This rule follows a cross-validation logic where the pre-period estimates are the “training” sample and the post-period estimates the “validation” sample. Thus, I choose the specification with the best out-of-sample prediction properties. As an alternative, I average over the models with good pre-fit (Imbens and Rubin 2015, Ferman et al. 2017).

The outcomes are the share of total spending on child care, education, elderly care, and health care. Together these areas constitute the majority of local government spending. The data are from Fiva, Halse, and Natvik (2015).

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<sup>4</sup>I use the fully nested optimization procedure.

Table 1: Results from 10 different synthetic control specifications. Short run effects.

Model	Childcare			Education			Elderlycare			Health care		
	Pre $\tilde{R}^2$	Post MSPE	p	Pre $\tilde{R}^2$	Post MSPE	p	Pre $\tilde{R}^2$	Post MSPE	p	Pre $\tilde{R}^2$	Post MSPE	p
1	.91	1.90	.57	.93	15.24	.16	.88	24.10	.82	.94	6.50	.50
2	.91	1.90	.57	.93	15.24	.16	.88	24.11	.82	.94	6.51	.50
3	.85	1.90	.55	.93	15.45	.02	.71	24.69	.91	<b>.80</b>	<b>6.02</b>	<b>.73</b>
4	.88	1.93	.37	.93	15.26	.01	.63	24.89	.93	.52	6.38	.77
5	.65	1.88	.71	.77	15.21	.32	.68	24.27	.73	.84	6.29	.58
6	.62	1.87	.71	.08	15.42	.93	.50	23.78	.85	.81	6.90	.46
7	.74	2.07	.37	.93	15.95	.04	.80	24.81	.80	.93	7.74	.30
8	.67	2.28	.57	.92	15.76	.01	.81	30.01	.76	.93	8.54	.21
9	<b>.88</b>	<b>1.87</b>	<b>.49</b>	<b>.93</b>	<b>15.02</b>	<b>.09</b>	<b>.87</b>	<b>24.02</b>	<b>.83</b>	.93	6.56	.45
10	.86	2.12	.48	.93	15.56	.09	.87	26.48	.81	.93	6.93	.42

*Note:* The ten models are: 1) All pre-treatment outcomes + log of population size, share of the population above 65 years of age, share of population in school age, share of population below school age, 2) All pre-treatment outcomes, 3) Pre-treatment outcomes for even years + the covariates, 4) Pre-treatment outcomes for even years, 5) Pre-treatment outcomes for odd years + the covariates, 6) Pre-treatment outcomes for odd years, 7) The first half of the pre-treatment outcomes + the covariates, 8) The first half of the pre-treatment outcomes, 9) The first three fourths of the pre-treatment outcomes + the covariates, 10) The first three fourths of the pre-treatment outcomes.

## Empirical results

Table 2 presents the results from the ten specifications. The post-treatment period is the four budget years following the 1995 election. We see that the pre-treatment fit varies across the specifications. The models with poor fit tend to be the even/odd outcome specifications, which is because these models often fail to capture a number of spikes in the spending levels caused by large investments in particular years. The model does not fit well when these investment years are not among the pre-treatment outcomes.

The post-treatment MSPE also varies across models. In line with arguments against maximizing pre-treatment fit by using all pre-treatment outcomes (Kaul et al. 2017), Models 1 and 2 never produce the smallest post-treatment MSPE. The difference in post-treatment MSPE across models with good pre-treatment fit is not always large, however, implying that model averaging across specifications with good pre-treatment fit is a necessary robustness check.

The results reveal that there is not much room for specification mining to find sig-

nificant treatment effects for child care, elderly care and health care spending. The Flå treatment effect has to be ranked number eight or better among the 164 estimates for the effect to be significant at the 5 percent level ( $8/164 = .05$ ). This is never the case for these three outcomes. For education we see that models with equally good pre-treatment fit produce different conclusions regarding the significance of the estimates, which gives the researcher the freedom to pick a model specification with a low p-value. This result illustrates the importance of presenting results from different specifications when using the SC method.

The specifications with the lowest post-MSPE (conditional on good pre-treatment fit according to  $\tilde{R}^2$ ) are highlighted in bold text. Figure 2 shows the trends in the four outcomes in Flå and the synthetic control in these selected specifications. The stippled line distinguishes between the pre- and the post-period. In line with the  $\tilde{R}^2$  results, the synthetic control fits the trend in Flå quite well for all outcomes, which make the trends for the synthetic controls plausible counterfactuals in the treatment period.

The average outcomes for the 1996-2000 period for the two groups are presented in Table 2. Panel A presents the results when picking the specifications with the lowest post-MSPE, while Panel B presents the results when averaging over models with good pre-treatment fit. The average for Flå is about .5 percentage points lower for child care and elderly care spending, about 1 percentage points higher for health care spending, and about 2.5-3 percentage points lower for education spending. The education spending estimate is significant at the ten percent level.

Figure 2: Trends in the outcomes. Flå versus synthetic control.

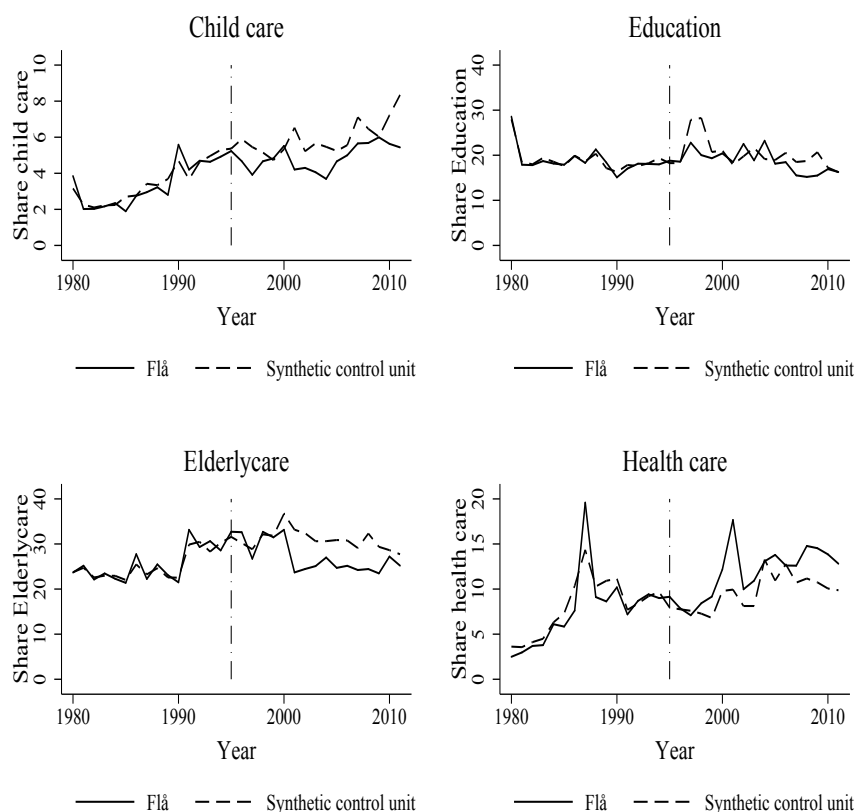
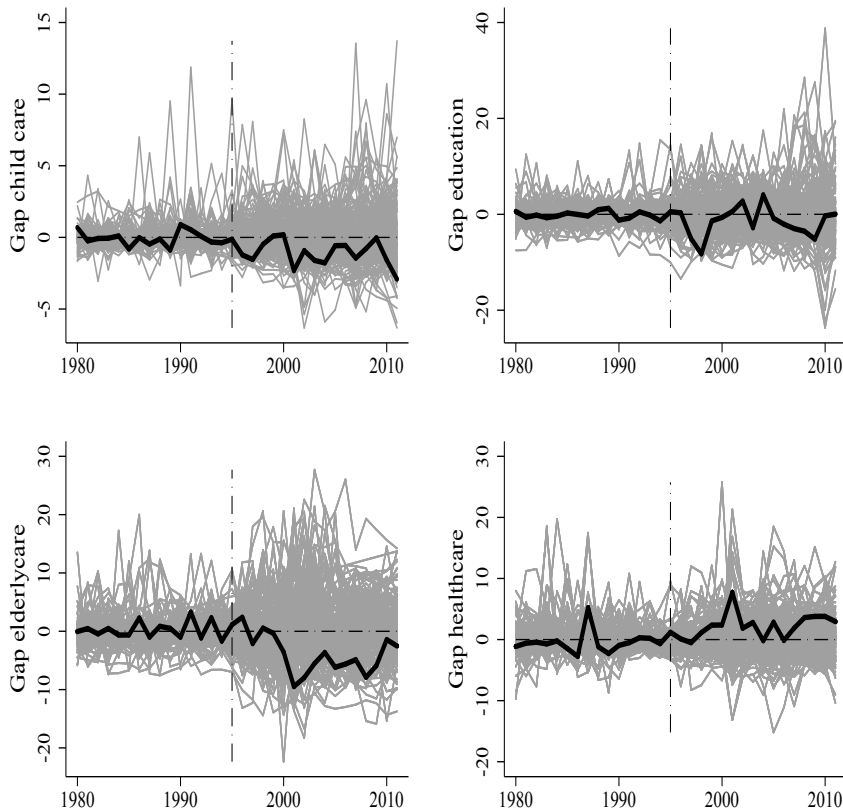


Table 2: Average spending in the 1996-2000 period.

	Flå	Synthetic control	Difference	p-value
A: Lowest Post-MSPE				
Child care	4.72	5.23	-0.51	.49
Education	20.23	23.22	-2.99	.09
Elderlycare	31.34	31.94	-0.60	.83
Health care	8.95	7.85	1.10	.73
B: Model averaging				
Child care	4.72	5.46	-0.74	.53
Education	20.23	22.83	-2.60	.11
Elderlycare	31.34	31.91	-0.57	.85
Health care	8.95	8.14	0.81	.45

The long-run estimates are presented in Online Appendix B. As Figure 2 visualizes, the short run effect on education diminishes over time as it is caused by a two-year up-thick in education spending in the synthetic control group. An inspection of the spending

Figure 3: Differences in the outcomes. Flå versus synthetic control in black, placebos in grey.



patterns in the municipalities which constitute the synthetic control—see Online Appendix B for the unit weights—reveal that one of the control municipalities made large investments in education in the late 1990s. The impact of this investment diminishes over time so that the long term effect is small and insignificant. The other treatment effects are larger in size in the long term, however, none of them are statistically significant.

Figure 3 clearly illustrates why the treatment effects are not statistically significant. For all outcomes, a relatively large number of placebos produce treatment effects of the same size. Thus, the trajectories of welfare spending in Flå do not stand out as being particularly unusual, i.e. we cannot rule out that the difference between Flå and the synthetic control would have been the same without the decline of Flå Ap.

## **Concluding remarks**

The analysis provides no clear evidence of partisan effects on welfare spending. This conclusion apparently contrasts with a number of recent well-identified studies in similar institutional contexts (Pettersson-Lidbom 2008; Folke 2014; Fiva, Folke, and Sørensen forthcoming). I rely on the sudden decline in Ap representation to identify partisan effects, which is more dramatic than the variation from close election that the previous literature relies on. The stronger treatment intensity should, in contrast to what I find, imply stronger partisan effects. That said, Fiva et al. (2017) report that the partisan effects are stronger for far-left and far-right parties, so my results can be read as supportive of their finding of heterogeneity on partisanship. Moreover, in my case the external validity is limited to small municipalities. Although parties appear to be polarized also in small municipalities (Fiva et al. 2017), small partisan effects are in line with qualitative evaluations of the room for partisanship in small municipalities (Østerud and Selle 2006). Given polarization, small partisan effects can be viewed as a democratic challenge since voters cannot elect policy changes. Future research might want to examine this issue further by studying partisan effects on outcomes where state regulation is weaker, such as e.g. local taxation and user fees, zoning, and local transport.

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## Online appendix to “Social Democratic Representation and Welfare Spending: A Quantitative Case Study” (Finseraas 2018)

### Appendix A: Extended Theoretical Discussion of Partisanship and Welfare Spending

It is useful to consider the two ways voters can influence public policies to understand what is meant by partisan effects (see Lee, Moretti, and Butler 2004). The classic view in political economy is that the competition for votes pushes the platforms of political parties towards the center. The prime example is Downs’ (1957) median voter model, where competition for votes forces the two parties to converge on the same policy platform. Although the full convergence result is unrealistic and empirically false, the model is useful because it illustrates how voters have the power to *affect* policies. Later models in the Downsian tradition allow parties to run on different platforms, but voters still affect policies by pushing party platforms toward the political center, away from the preferred policies of the party ideologues.<sup>5</sup>

One strong assumption in the classic view is that politicians’ policy promises are credible, i.e. that they will implement exactly the platform they propose to the voters in the election. However, voters cannot sanction politicians between elections if they deviate from their promises, which makes it tempting for parties in office to follow their actual policy preferences instead of their electoral promises. Besley and Coate (1997) argue that the lack of convergence in policy platforms that we witness in the real world is a consequence of the lack of credibility in party promises. Without the possibility of between-election sanctioning, the parties do not converge on similar platforms, but instead run on their true policy preferences. Voters are still powerful, however, since they determine the outcome of the election. Moreover, free entry to run for office ensures that the system is democratic. However, while voters *affect* policies in the Downsian view, they *elect* policies in this alternative view.

It is clear that these two perspectives have different views on partisan effects. In the Downsian tradition there is no or limited room for partisanship to matter. Since platforms have more or less converged, who governs will not matter much for policy outcomes. If implemented policies differ across political entities, this will be because voter preferences differ across the entities. In the alternative view, however, implemented policies might differ strongly across political entities, even if voter preferences are identical across polities. One party will inevitably be elected for office and this party will implement its preferred policy, irrespective of the position of the median voter.

The latter scenario represents what is meant by a partisan effect. It refers to an effect of who governs, controlling for voter preferences. It is challenging to empirically identify the representation effect, because we do not have perfect measures of voter preferences. Lee et al. (2004) put forward a regression discontinuity framework which studies close elections in order to identify the partisan effect. By comparing policy outcomes in polities where the Democratic candidate barely defeated the Republican candidate, one compares the effect of party representation in contexts where voter preferences do not differ much. They find large partisan effects. A couple of papers have extended this framework to multiparty-systems (Pettersson-Lidbom 2008; Folke 2014; Fiva, Folke, and Sørensen forthcoming). Fiva et al. (forthcoming) is of particular interest since they study policy outcomes in

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<sup>5</sup>See Persson and Tabellini (2000) for an overview of these models. See Barth, Finseraas, and Moene (2015) for an recent application on welfare politics.

Norwegian municipalities. They find partisan effects on two types of welfare spending. Rightist representation tend to increase elderly care spending and decrease child care spending. They find insignificant effects on education and health care spending.

My paper complements Fiva et al. (forthcoming) by estimating the partisan effect on another margin, namely the collapse of the dominating, Social Democratic party. This margin is potentially very important. An influential literature in the comparative welfare state research argues that Social Democratic parties are instrumental for the degree of public responsibility for income redistribution and social insurance (Korpi 1983; Huber, Ragin, and Stephens 1993; Huber and Stephens 2001; Korpi and Palme 2003; Allan and Scruggs 2004; Korpi 2006). In essence, this literature argues that there are three components to Social Democratic representation which will ultimately have consequences for the welfare state: Organizational, institutional and ideological. The organizational component refers to the importance of organizing wage earners in the political sphere and ensuring that class politics have a high salience in political competition. The institutional component concerns Social Democratic parties' interest in developing and maintaining collective wage bargaining and corporatist institutions. Finally, the ideological component refers to the recruitment of politicians with a commitment to full employment and public responsibility for social rights. In power resources theory, Social Democratic parties are inherently partisan, representing working-class voters' interests, and, together with unions, solving collective action problems for less-advantaged voter groups. This view of parties is far from the Downsian tradition, and closer to the view that voters elect policies.

## **Appendix B: Additional Empirical Material**

### **I: On the party organization**

One possibility is that the failure to submit the list is a signal of a local party organization in disarray. I have not come across any evidence indicating that this was the case. In Norwegian local elections, voters can make changes on the party list, such as changing the ranking of the candidates and thereby affect what candidates get elected from the party list. A high share of changes of the party list might be interpreted as an indicator of local voter unhappiness with the party establishment. The number of changes made to the list is public information. In the 1991 election (the election prior to 1995), almost 50 percent of the Ap-voters in Flå made changes to the Ap list. Although this sounds like a large number, about 60 percent of the votes in Flå were changed. In the rest of Norway, about 38 percent of Ap voters changed the Ap-list, but this number is close to the total share of changed votes (41 percent). Thus, these numbers do not suggest that voters in Flå were particularly unhappy with the local list compared to Ap-voters elsewhere.

### **II: On the rightward shift of the Flå municipal council**

It is hard to quantify the size of the rightward shift of the council, but one approach is to assume i) that the average party positions reported in Fiva et al. (forthcoming) are representative of the parties in Flå, ii) that party positions are fairly stable over time—again with reference to Fiva et al. (forthcoming)—and iii) that the left-right position of BF is the average over the positions of Sp, KrF and H. Using these assumptions, we can calculate the left-right position of the council by adding together the left-right-positions and use the seat shares as weights. Doing so, I find that the council shifted from 4.75 in 1991 to 5.58 in 1995, a shift of .83 on the 0-10 left-to-right-scale. In comparison, the shift from the 1987 to 1991 election was .19 units (from 4.94 to 4.75). If I use the same assumptions on the other municipalities in my sample, I find that no municipality experienced a similarly large shift on the left-right scale. The average shift across the other municipalities is .05 (SD=.23) from 1991 to 1995 and .09 (.20) from 1987 to 1991. With the caveats associated with the assumptions I make in mind, I argue that the 1995 shock to the council was unusual and politically significant.

### **III: On political efficacy in small municipalities**

The survey *Kommuneundersøkelsen* from 1993 has three questions on the importance of local politics which shed some light on voters' political efficacy in small municipalities. Table A1 shows the share of respondents who disagree or disagree strongly with three statements on political inefficacy. The sample is broken down by small and large municipalities, where small refers to the “least central” municipalities according to the 1994 version of Statistics Norway's index of centrality,<sup>6</sup> while large municipalities are the rest. In small municipalities, 55 percent of the respondents disagree that “it does not matter what parties have power in the local council” and 59 percent disagree that “it does not matter for me personally who runs the local council”. The former share is higher in large municipalities, but the latter is smaller in large municipalities. In any case, a majority of the respondents in small municipalities believe that the local council and local politics matter. The belief that “the local public administration runs the municipality, not the

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<sup>6</sup>See <https://www.ssb.no/klass/#!/klassifikasjoner/128/versjon/469>

politicians” is however fairly widespread as well, as about one third disagree, but this share is the same in small and large municipalities. Thus local politics is perceived to matter, but within a regime where the local administration is considered a competing, powerful force.

Table A1: Share in small and large municipalities disagreeing with statements.

	Small municipalities	Large municipalities
It does not matter what parties have power in the local council	55	64
It does not matter for me personally who runs the local council	59	56
The local public administration runs the municipality, not the politicians	34	33

*Note:* Own calculations from Kommuneundersøkelsen 1993.

#### IV: Long-run estimates

Table A2: Results from 10 different synthetic control specifications. Long run post-MSPE

Model	Childcare			Education			Elderlycare			Health care		
	Pre $\tilde{R}^2$	Post MSPE	p	Pre $\tilde{R}^2$	Post MSPE	p	Pre $\tilde{R}^2$	Post MSPE	p	Pre $\tilde{R}^2$	Post MSPE	p
1	.91	3.45	.43	.93	21.40	.31	.88	29.27	.49	.94	10.58	.55
2	.91	3.45	.43	.93	21.40	.31	.88	29.26	.49	.94	10.58	.55
3	<b>.85</b>	<b>3.11</b>	<b>.56</b>	.93	20.68	.14	.71	29.21	.76	<b>.80</b>	<b>10.18</b>	<b>.64</b>
4	.88	3.53	.21	.93	21.39	.08	.63	29.90	.66	.52	10.23	.77
5	.65	3.16	.80	.77	20.79	.48	.68	28.94	.52	.84	10.51	.60
6	.62	3.43	.70	.08	21.25	.41	.50	29.08	.70	.81	10.44	.49
7	.74	3.30	.55	.93	21.36	.18	.80	29.60	.47	.93	10.98	.37
8	.67	3.70	.50	.92	22.50	.12	.81	34.28	.27	.93	11.23	.23
9	.88	3.21	.43	<b>.93</b>	<b>20.55</b>	<b>.26</b>	<b>.87</b>	<b>29.11</b>	<b>.46</b>	.93	10.41	.49
10	.86	3.46	.32	.93	22.20	.27	.87	32.76	.37	.93	10.67	.48

*Note:* The ten models are: 1) All pre-treatment outcomes + log of population size, share of the population above 65 years of age, share of population in school age, share of population below school age, 2) All pre-treatment outcomes , 3) Pre-treatment outcomes for even years + the covariates 4) Pre-treatment outcomes for even years, 5) Pre-treatment outcomes for odd years + the covariates, 6) Pre-treatment outcomes for odd years, 7) The first half of the pre-treatment outcomes + the covariates , 8) The first half of the pre-treatment outcomes, 9) The first three fourths of the pre-treatment outcomes + the covariates, 10) The first three fourths of the pre-treatment outcomes.

Table A3: Average spending in the 1996-2010 period.

	Synthetic			
	Flå	control	Difference	p-value
A: Lowest Post-MSPE				
Child care	4.87	5.96	-1.09	.56
Education	18.77	20.34	-1.57	.26
Elderlycare	26.96	30.96	-4.00	.46
Health care	11.96	9.69	2.27	.64
B: Model averaging				
Child care	4.87	6.66	-1.79	.41
Education	18.77	20.56	-1.79	.24
Elderlycare	26.96	30.87	-3.91	.49
Health care	11.96	9.43	2.53	.54

## V: Unit weights

Table A4: Municipalities with positive synthetic control weights.

Childcare (short run)	Childcare (long run)	Education	Elderlycare	Health care
Dovre .37	Meråker .34	Valle .51	Gulen .27	Rendalen .53
Meråker .31	Bykle .22	Nissedal .31	Røyrvik .23	Tydal .25
Veste Slidre .09	Hasvik .09	Modalen .07	Etnedal .11	Kvitsøy .14
Sør-Aurdal .08	Leka .08	Rindal .06	Kvitsøy .10	Bykle .05
Bykle .06	Modalen .07	Åseral .03	Stor-Elvdal .08	Utsira .02
Hasvik .03	Sør-Aurdal .07	Træna .02	Rendalen .07	Nesseby .001
Rendalen .03	Solund .06		Flatanger .07	
Målselv .03	Veste Slidre .05		Vega .03	
Midtsund .01	Båtsfjord .02		Røst .03	

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