

Welfare Retrenchments and Government Support: Evidence from a Natural Experiment

Erik Gahner Larsen*

School of Politics and International Relations, University of Kent, Canterbury CT2 7NX, UK

*Corresponding author. Email: E.G.Larsen@kent.ac.uk

Submitted December 2016; revised November 2017; accepted November 2017

Abstract

A large body of literature has provided mixed results on the impact of welfare retrenchments on government support. This article examines whether the impact of welfare retrenchments can be explained by proximity, i.e. whether or not the retrenched policy is related to people's everyday lives. To overcome limitations in previous studies, the empirical approach utilizes a natural experiment with data from the European Social Survey collected concurrently with a salient retrenchment reform of the education grant system in Denmark. The results confirm that people proximate to a welfare policy react substantially stronger to retrenchment reforms than the general public. Robustness and placebo tests further show that the results are not caused by non-personal proximities or satisfaction levels not related to the reform and the government. In sum, the findings speak to a growing body of literature interested in the impact of government policies on mass public.

In contemporary societies, welfare policies draw a high degree of public support (Blekesaune and Quadagno, 2003). However, governments face demographic and economic challenges making retrenchment reforms of popular policies an often-used policy instrument. Thus, while cherished welfare policies are rooted in mass policy preferences (Brooks and Manza, 2006a, b), governments still pursue retrenchment reforms of welfare policies (Pierson, 1994; Allan and Scruggs, 2004; Hacker, 2004; Vis and van Kersbergen, 2007). Accordingly, welfare retrenchment reforms are not limited to non-salient policy domains with reduced attention from the press and the public, but also popular policies such as healthcare and education. This has led to scholarly debates about the extent to which governments are in fact adversely affected by pursuing such policies and whether they can actually benefit from

pursuing welfare retrenchments (Giger and Nelson, 2010; Elmelund-Præstekær and Emmenegger, 2012).

Recent studies interested in the public response to welfare retrenchments provide important insights to how and when the public reacts to government reforms, and a growing body of literature examines the electoral consequences of welfare retrenchments on government support (e.g. Armingeon and Giger, 2008; Giger, 2010, 2012; Giger and Nelson, 2010, 2013; Schumacher *et al.* 2013; Elmelund-Præstekær *et al.*, 2015; Lee *et al.*, 2017). Overall, the literature has provided mixed evidence on the impact of welfare reforms, and a key contribution is that people do not always punish governments for welfare retrenchments. Although there is some evidence that the public on average reacts to welfare retrenchments and punishes the government for such policy choices (Giger, 2010), the public does not react to welfare retrenchments

in an unconditional manner (Armingeon and Giger, 2008). Recent studies show that the conditional punishment can be explained by communication strategies such as how governments pursue reforms (Elmelund-Præstekær and Emmenegger, 2012) and individual-level characteristics such as whether voters are interested in social policies (Giger, 2012).

Thus, the impact of welfare retrenchments might not be as direct and strong as theoretically expected, and while the literature has documented some of the dynamics shaping how people react to government policies, we do not fully understand *when* welfare retrenchments matter for government support. The contribution of this article is to resolve and test an important part of this puzzle, namely, the relevance of how proximate welfare reforms are to people's everyday lives (as proposed by Soss and Schram, 2007). In short, most studies on welfare retrenchments examine how policies have implications for the general electorate with limited information on how proximate people are to the policy being retrenched. To overcome this limitation, we provide a fine-grained test and examine in a natural experiment whether people with a proximate relation to the policy being retrenched will react more strongly to welfare reforms relative to the public less proximate to the retrenchment.

This approach follows recent studies interested in the heterogeneous effects of welfare retrenchments (de Vries and Hobolt, 2013; Lindh, 2015; Munoz *et al.*, 2014), and in particular how differences in personal proximity shape public responses to welfare policies (Soss and Schram, 2007; Hedegaard, 2014; Lü, 2014). In this context, we expect that people react more strongly to retrenchments to policies they are proximate to, and the general public, not proximate to welfare reforms, are less responsive to welfare retrenchments. Consequently, when studies do find general punishments in the electorate, these are potentially shaped by a strong response by the people being proximate to the welfare policy.

To test this in a systematic manner, we focus on a welfare reform in the domain of education initiated by the Danish government in 2013. This reform was presented on 19 February 2013, and led to cuts on 2.2 billion DKK (295 million EUR) in the state education grant system. Coincidentally, the reform was presented while the European Social Survey (ESS) was doing fieldwork in Denmark, allowing us to create a counterfactual group for not only the public, i.e. people interviewed before and after the reform, but also for the people proximate to the retrenched policy.

The methodological approach overcomes three crucial obstacles in the literature: first, the issue of a reliable counterfactual, as we can exploit variation in the groups

being studied; secondly, the issue of reverse causation, as governments might be more likely to retrench policies targeted people less satisfied with the government in the first place; and thirdly, the issue of the time frame, as we can zoom in on a short period of time with a specific welfare reform, and not a wide period with multiple policies retrenching and expanding the welfare state. This also ensures that macro level confounders such as economic and social developments are less of a concern. In sum, this strategy provides unique estimates on the causal effects of welfare reforms on the public with implications for the literature interested in government policies and public opinion dynamics.

In addition, the novel design and unique data makes it possible to test the relevance of alternative theoretical explanations on the impact of welfare retrenchments on the public. In particular, by using measures previously linked to social policy preferences, we are able to examine whether alternative notions of proximity such as policy socialization, labour market risks and family solidarity condition the impact of the reform on government support. Last, placebo measures substantiate that the results are not caused by differences not directly related to the reform and the government.

Welfare Retrenchments and Personal Proximity

People attribute credit and blame to governments for their actions and such assessments have direct implications for the support of governments (Marsh and Tilley, 2010). However, people are multi-issue oriented, have limited capabilities for information processing (Zaller, 1992; Lodge and Taber, 2013), and do not possess complete knowledge on all public policies and their consequences (Mettler, 2011). Consequently, there is no reason to expect that people will have identical experiences with welfare policies and respond in a homogeneous manner to changes in policies.

Over the years, several studies have examined the electoral impact of welfare retrenchment reforms on government support (Armingeon and Giger, 2008; Giger, 2010, 2012, Giger and Nelson, 2010, 2013; Schumacher *et al.*, 2013; Elmelund-Præstekær *et al.*, 2015; Lee *et al.*, 2017). However, these studies have one crucial aspect in common, namely, that they study how the general public reacts to policies with no or limited attention to the proximity to such policies. Thus, the literature focuses mostly on the aggregate response, including people with no personal relation to the policy being retrenched, and as a result, it lacks a systematic test of whether the proximity to a policy matters.

Policies are for some people proximate and provide tangible personal relations to political institutions (Kumlin, 2004; Bruch *et al.*, 2010). More generally, welfare policies have feedback effects on mass publics (Mettler and Soss, 2004; Campbell, 2012; Lenman and McCabe, 2017), and how proximate a policy is to the public is expected to substantially condition its impact (Soss and Schram, 2007; Hedegaard, 2014). Accordingly, Soss and Schram (2007) theorize how a proximity dimension matter for whether or not welfare reforms will have any effect on the public. In this framework, proximity is defined as how closely the policy is directly related to people's everyday lives.

The concern raised by scholars interested in the personal proximity to reforms is that when policy effects are studied in a population as aggregate, average effects with limited attention to how proximate the policy is, we underestimate the actual impact and relevance of public policies (MacLean, 2011). Thus, the theoretical expectation is that a high degree of proximity, i.e. that people have individual experiences with a policy in their everyday lives, is crucial in our understanding of when and how a policy will matter for the public.

There are two reasons to expect that those proximate to a policy will react more strongly to retrenchment reforms. First, people proximate to a policy have personal interests in ensuring the preservation of the policy they benefit from. Pierson (1994) describes how policies create groups of recipients with a strong interest in the preservation of the policies, whereas the economic benefits for the tax-payers of retrenching a policy are diffuse and less direct. Secondly, people differ in the extent to which they are aware of policies, and the more proximate a person is to a policy, the more likely that he/she will respond to an unpopular policy. Lü (2014), for example, finds that education policy benefits shape attitudes towards government responsibility as well as trust in the government in China, but only for those who were aware of the reform (for more on the importance of reform awareness, see Hetling *et al.*, 2008). In sum, studying welfare retrenchments at the aggregate level with no information on the proximity to the retrenchments does not provide evidence on how voters with differential experiences with policies react to welfare retrenchments.

For this reason, recent studies have devoted closer attention to how proximity or policy awareness matters for the impact of welfare reforms. Bendz (2015) shows that attention to politics in the domain of healthcare, measured with proximity variables such as geography and perceived health status, shape the impact of policies. Munoz *et al.* (2014) find that public-sector workers in Spain reacted strongly to an austerity package and increased their level of political participation. de Vries and Hobolt (2013)

find, using a reform with social spending cuts in child care in the Netherlands in 1995, that those proximate to the policy were affected more strongly and punished the government accordingly. Giger (2012) examines 19 elections between 2001 and 2006 and finds that government popularity is lower in countries which pursued welfare retrenchment reforms, but the effect is limited to the citizens interested in social policy.

To summarize, in line with the policy feedback literature interested in the personal experiences with welfare policies, proximity is expected to condition the impact of welfare retrenchments on government support. For people who are proximate to a policy being retrenched, the government will be evaluated more negatively, whereas the public not proximate to a policy will not punish the government.

Education Retrenchments and Public Opinion Dynamics

To test the proximity hypothesis, we focus on how welfare retrenchments in the domain of education matter for government support. Before turning to the empirical strategy, it is crucial to describe the policy domain and the implications for the study of proximity and public opinion dynamics. While studies on welfare reforms usually have focused on social policies (Rhodes, 2015), education policies are closer to the median voter and thus more likely to call for a response in the electorate (Jensen, 2012). Thus, education is a salient and popular part of the welfare state, and is a theoretically justified case to expect a general government punishment for welfare retrenchments.

Contrary to social policies, e.g. labour market related programmes, which are less popular among the median voter, education policies are popular and salient policies with a high level of support independent of socio-economic status (Busemeyer *et al.*, 2009; Jensen, 2012). However, that being said, education policies consist of complex redistributive dynamics (Garritzmann, 2017), making the impact of the partisan composition of a government on public education spending less clear (Garritzmann and Seng, 2016).

Accordingly, we need to disentangle the distinct ways in which people can be proximate to welfare retrenchments in the domain of education. Based on the literature interested in how people form attitudes towards social policies, we can derive additional expectations regarding how education policies are linked to people's response to welfare retrenchments. In other words, in the domain of education, there are potential ways in which welfare retrenchments might matter for government support beyond the direct personal

proximity. Importantly, there are theoretical reasons to believe that personal proximity might not be the most crucial factor explaining whether people respond to welfare retrenchments.

First, people have previous experiences with education policies. Policies create specific norms that socialize people with implications for how they perceive themselves and the role of the government (Jæger, 2009). Garritzmann (2015), for example, finds that education policies have positive feedback effects, making education policies difficult to retrench. Thus, education policies themselves shape how people react to reforms. In this context, we will examine whether prior proximity, i.e. the extent to which people have past experiences with education policies, shapes the response.

Secondly, people's socio-economic positions can condition whether they are more likely to respond to welfare retrenchments. While retrenchments within education policies are less direct in their socio-economic effects, people with a greater dependency on the welfare state, e.g. those more likely to encounter labour market risks (Rehm, 2009), can potentially be more likely to punish the government for retrenching welfare policies.

Thirdly, while some people have direct proximate relations to a policy being retrenched, other people are proximate to the people being affected. Accordingly, there are different degrees of proximity to government policies (Hedegaard, 2014), and in particular proximity within the family might explain whether people respond to welfare retrenchments or not. Previous research finds that inter-generational solidarity within the family helps explaining differences in social policy preferences, and that family solidarity matters for older people's attitudes towards public childcare provisions (Goerres and Tepe, 2010).

Overall, the focus on additional expectations beyond personal proximity is relevant for two reasons. First, to examine the potential relevance of less direct and alternative types of proximity in understanding when people respond to welfare reforms in their overall assessment of the government. In other words, there are ways in which people might respond to welfare reforms beyond their own proximate relation. Secondly, to ensure that the relevance of personal proximity is not confounded by other types of proximity. Thus, by using different measures and conducting a series of additional tests, we can test whether it is in fact personal proximity with the policy which matter for the response.

Method and Data

To examine the theoretical expectations, we utilize a welfare reform initiated by the Danish government in

2013 of the state education grant system. This reform was presented on 19 February 2013, and led to cuts for 2.2 billion DKK in the state education grant system. Importantly, the reform was presented while the 6th round of the ESS was collecting data in Denmark. This provides a novel opportunity to examine how people evaluated the government before and after the reform for not only the general public, but in particular the people who were proximate to the education policy and thus the state education grant system. Crucially, the education grant system is not a means-tested service, making people undergoing education at age 18 and above eligible to the grant. Accordingly, in this context, proximity is defined as a person who is currently undergoing education.

The key features of the reform were lower benefits as well as stricter requirements. More specifically, the reform shortened the period of the state education grant, led to additional requirements of study progression to be eligible for the grant, requirements for the universities to improve student completion times, changed state education grants for people living with their parents and changed the regulation of the state education grant to a transfer payment. Hence, the reform of the state education grant system is a retrenchment reform with no features of welfare expansions. Importantly, the reform was not communicated in relation to expansion reforms, e.g. presented as part of a greater package with multiple different reforms. This would bias the estimates of the reform's effect, as other parts of the electorate could be exposed to welfare expansions (Lee *et al.*, 2017).

The context of the study is Denmark in 2013. Denmark is a universal welfare state with a multi-party system often led by minority coalition governments with one or more centre parties. The government in 2013 consisted of the Social Democrats, the Social Liberal Party, and the Socialist People's Party. The education reform was presented by the Minister of Education, Morten Østergaard, from the Social Liberal Party. The government was known for pursuing multiple unexpected and unpopular reforms related to different policy areas in the election period from 2011 to 2015, but in this specific study period the primary focus was on education and not other salient reforms that could confound the results (for a description of the political context of the reform and the public's education spending preferences in Denmark, see [Supplementary Material A](#)).

The reform received extensive coverage in the mass media. [Figure 1](#) shows the coverage of the reform in the press in form of articles mentioning the reform of the state education grant system as well as retrenchment in

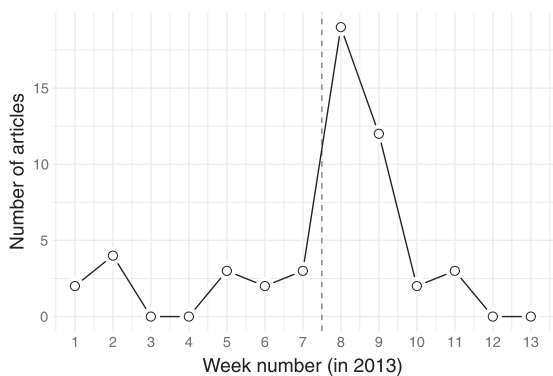


Figure 1. The coverage of the education reform, 2013

Note: The figure shows the discontinuity in the coverage of the media coverage of the reform as the government presented it. Obtained through the Danish media database Infomedia. All articles in the nationwide coverage (Arbejderen, Berlingske, BT, Børsen, Ekstra Bladet, Information, Jyllands-Posten, Kristeligt Dagblad and Politiken) mentioning ‘SU-reform*’ (state education grant reform) and ‘nedskæring*’ (retrenchment). For additional information on the media coverage of the reform in the study period and the individual articles, see [Supplementary Material B](#).

the nationwide coverage. The figure shows that there is an increase in the coverage of the reform after its presentation. This substantiates that people interviewed after 19 February to a greater extent will be exposed to the reform than people interviewed prior to 19 February.

Noteworthy, political reforms are not exogenous to the political process, and there was coverage related to reforms and retrenchments prior to 19 February. This can induce a bias in the estimated causal effect of the reform. However, three factors are relevant, making this less of a concern. First, it is implausible that the public never will be treated with some sort of political agenda, and hence the counterfactual of interest here is not necessarily a context without any talk about welfare reforms. Secondly, if people in the control group, i.e. people interviewed prior to 19 February, are exposed to welfare reforms, this will provide a more conservative estimate of the welfare reform under study. Thirdly, in a reading of the articles covering the issue prior to the presentation of the reform, we found no evidence indicating that people would know that the retrenchment reform would be presented on 19 February 2013 (see [Supplementary Material B](#)).

In the beginning of 2013, the ESS collected data for the 6th round in Denmark. While the ESS was intended to question citizens about a variety of non-political and political issues, it was by coincidence conducted, while the Danish government presented the reform of the state education grant system. This provides a novel sample in

which only some people are interviewed after the presentation of the education reform. The first subject was interviewed on 11 January, and the last subject was interviewed on 2 May (for the frequency of interviews in the study period, see [Supplementary Material C](#)).

The question of interest is whether or not people interviewed after the reform are less satisfied with the government, and in particular whether people undergoing education react more strongly. To test this, we need a parameter capturing the heterogeneous effect of the reform. Accordingly, R_i indicates whether or not a unit is exposed to the reform, where R_i , for $R_i \in \{0, 1\}$, shows exposure status for subject i . Whether or not a person is interviewed after the presentation of the reform is based on the day of interview, I_i . Thus, subjects interviewed prior to 19 February 2013 are not exposed to the reform, whereas subjects interviewed after 19 February 2013 are. Subjects interviewed on 19 February are excluded.

$$R_i = \begin{cases} 1, & I_i > \text{Feb 19} \\ 0, & I_i < \text{Feb 19} \end{cases}$$

The heterogeneous effect estimator is given by δ in:

$$y_{it} = \alpha + \beta E_i + \gamma R_{it} + \delta E_i R_{it} + \epsilon_{it}$$

In the equation, y is government popularity for unit i at time t . E is a binary variable indicating whether or not the person is undergoing education. α is the average government popularity for people not undergoing education prior to the reform. β is the difference in average government popularity between people undergoing education and those not undergoing education prior to the reform. γ is the difference in average government popularity before and after the reform conditional upon educational status.

Whether a person is undergoing education, as the indicator of whether or not the reform is proximate, is measured with a question about whether the respondent, within the past 7 days, has been undergoing education (for question wordings on all measures, see [Supplementary Material D](#)). Importantly, this measure excludes people undergoing education paid by an employer. Not all people undergoing education will be or feel proximate to the education policy and not all people undergoing education will know about the reform, but it provides a very strong measure on whether or not the person, on average, has a proximate relation to the policy domain being retrenched relative to the general public. Again, as noted, the state education grant system is a universal policy in so far that all students have access to and benefit from the policy.

Last, we assume that education status is time-invariant (indicated by the lack of a time indicator in education status).

This approach addresses three challenges in the existing literature. First, reforms rarely affect the whole electorate, but rather groups, making the reform a salient issue for this specific group (in this case indicated by E_i). As described above, we might underestimate the impact of reforms when they are targeted specific groups. Thus, this approach allows us to examine the causal heterogeneity in the effects of welfare retrenchments on government support.

Secondly, existing studies look for the most part at the effects of reforms between elections. Governments pursue several reforms over an election cycle, and these reforms are not exclusively policy retrenchments (Klitgaard *et al.*, 2015). In addition, governments face electoral incentives to pursue unpopular reforms in the beginning of an election period, underestimating the effect of individual reforms on the electorate when studying election results. Furthermore, other events taking place between elections, e.g. macroeconomic downturns, proves it difficult to estimate the causal effect of retrenchment reforms. In this study, we address the problem by limiting the time interval to the specific period before and after the presentation of a reform (given by R_{it}), taking institutional, political, economic, and cultural factors into account by design.

Thirdly, only a limited number of studies compare the effects of reforms to an explicit and realistic baseline (i.e. the outcome variable in the absence of a reform). To understand how a political reform affect citizens and especially different groups of citizens, we need to compare the effect of a reform to a comparable group that is unaffected or only to a minimal extent proximate to the reform. In the present study we use the subjects surveyed just before the presentation of a reform as the baseline government support. In addition, several outcome measures not directly related to the reform and government support, i.e. life satisfaction, democracy satisfaction and economy satisfaction, makes it possible to test whether the results reflect a general dissatisfaction with a variety of outcomes, and not a punishment of the government.

To further ensure that the groups interviewed before and after the reform are comparable and of equal size, the data are preprocessed with a 1:1 nearest neighbour matching with replacement (subject to a caliper constraint). While this technique takes observed differences into account (Ho *et al.*, 2007; Sekhon, 2009), it is important to note that it does not in and by itself substantiate a conditionally exogenous assignment to the retrenchment reform (Samii, 2016). However, the

design-features described above combined with the matching procedure provide a satisfactory set-up for studying how people react to welfare retrenchments (for information on the matching procedure, see [Supplementary Material E](#)). The specific variables chosen for the matching procedure and as covariates in the estimated models are gender, age, education level, subjective class, political interest, political news consumption and religiosity.

The outcome variable of interest is government popularity. We use a direct measure of the extent to which the respondent is satisfied with the national government. Noteworthy, the ESS does not have measures on vote intention. [Figure 2](#) shows the distribution of the outcome variable.

The outcome variable has a mean of 4.77 with a standard deviation of 2.23 (for summary statistics for all variables on the full and matched data, see [Supplementary Material F](#)). In sum, the distribution of the outcome shows that ordinary least squares (OLS) regression is suitable for our analysis with no floor or ceiling effects.

The next section will test whether people interviewed after the government presented the reform, on average, were less satisfied with the government, and in particular whether the people being proximate to the education policy reacted more strongly. In addition, we focus on

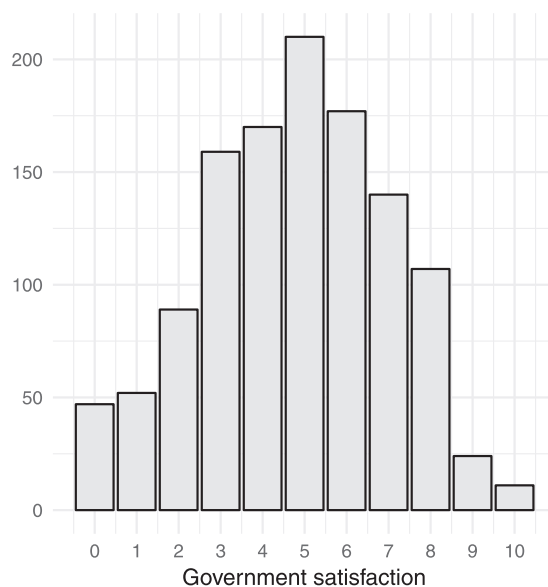


Figure 2. Distribution of government satisfaction
Note: Distribution of government satisfaction. Greater values indicate greater levels of satisfaction with the government. For question wording and descriptive statistics, see [Supplementary Material D](#) and [F](#).

alternative types of proximity, measures and models to examine the robustness and sensitivity of the results.

Results

Table 1 examines, in four models, how the public reacted to the education reform. In the first model, we estimate the average effect of the reform on the public's satisfaction with the government, i.e. all subjects included in the analysis. The effect is -0.30 with a standard error of 0.13, indicating an overall significant effect of the reform on people's evaluation of the government. More specifically, people interviewed after the presentation of the retrenchment reform were significantly less satisfied with the government. This lends support to the expectations that governments are adversely affected by pursuing retrenchment reforms. In the second model, we include the set of covariates also used in the matching procedure. The model shows that this has no implications for the results or the interpretation.

Next, we turn to the heterogeneous effects of the reform. To test whether the decrease in government support is shaped by proximity, the third model includes the interaction term between the reform and proximity. This model confirms that the decrease in support is statistically significant. The fourth model further includes the covariates and again confirm that this inclusion has no implication for the coefficient or statistical test. However, and importantly, the statistical significance should not be interpreted as substantial significance (Bernardi *et al.*, 2017). In substantial terms, people undergoing education became 0.73 less satisfied with

the government in the wake of the reform on the 11-point scale compared to the general public. In comparison, this effect is similar to the estimated difference in government satisfaction between people with a primary education and a tertiary education. While this shows that welfare retrenchments matter for people proximate to the retrenchments, it also shows that the effects on the average public support are not severe and devastating for the government.

To test whether the results are shaped by the choices made in the matching procedure, the results from the full and matched sample were compared, and the results were estimated with different calipers, different functional forms and alternative matching procedures. These results are substantively similar to the results presented above (see [Supplementary Material G](#) for the models).

In sum, the retrenchment reform made the public less satisfied with the government. However, this effect is driven by people who in their daily lives have experiences with the policy that was being retrenched. This lends support to the main expectation, namely, that proximity is an important condition for whether or not a retrenchment reform will elicit a public response.

Alternative Measures and Models

To ensure that the effects are not driven by the fact that people interviewed after the reform are more satisfied on aspects not directly related to the government and the reform, we estimated the same models with placebo satisfaction outcomes. In other words, we are interested in outcomes for which differences could account for the

Table 1. Welfare retrenchment and government satisfaction, OLS regression

	Average effect	Average effect, w. covariates	Conditional effect	Conditional effect, w. covariates
Reform	-0.30** (0.13)	-0.29** (0.12)	-0.18 (0.14)	-0.16 (0.14)
Education			0.11 (0.24)	0.39 (0.27)
Reform × Education			-0.71** (0.34)	-0.73** (0.33)
Male		0.11 (0.13)		0.10 (0.13)
Age		0.004 (0.004)		0.004 (0.005)
Education level		0.17*** (0.04)		0.17*** (0.04)
Subjective class		0.20*** (0.04)		0.20*** (0.04)
Pol. interest		-0.11 (0.09)		-0.10 (0.09)
Pol. news		-0.04 (0.06)		-0.04 (0.05)
Religiosity		0.10*** (0.02)		0.10*** (0.02)
Ideology		-0.17*** (0.03)		-0.17*** (0.03)
Constant	4.92*** (0.09)	3.59*** (0.39)	4.90*** (0.10)	3.47*** (0.43)
Observations	1,186	1,186	1,186	1,186
R ²	0.004	0.10	0.01	0.10

Note: Unstandardized regression coefficients with standard errors in parentheses. The dependent variable is government satisfaction (0–10), with greater values indicating greater levels of satisfaction with the government.

* $P < 0.1$, ** $P < 0.05$, *** $P < 0.01$.

results but should not be shaped by the education reform. Luckily, the ESS includes questions on satisfaction measures such as satisfaction with life, the economy, and democracy. Table 2 shows the results of these tests.

We find no evidence in any of the models that people interviewed after the presentation of the reform were less satisfied on measures not directly related to the reform and the government. Thus, it is not the case that people undergoing education were less or more satisfied on unrelated measures after the presentation of the reform. In other words, the results provide reassuring evidence that it was in fact the reform that changed people's level of satisfaction with the government, and not overall satisfaction differences in the period being studied.

Next, to examine the importance of the day of the interview, and especially potential announcement effects prior to the presentation of the reform, we pursued three additional strategies. First, the key results were estimated with a statistical control for the distance in days to the presentation of the reform. Secondly, the average effect of the reform was estimated with a sharp regression discontinuity design identification. Thirdly, the effects were estimated with all dates prior to the reform using only people interviewed before the presentation of the reform. The results from the three strategies provide additional evidence for the interpretations presented above, and in particular that the results are unlikely to be biased by announcements made prior to 19 February (for the results and further details, see [Supplementary Material H, I, and J](#)).

Alternative Types of Proximity

While we have studied the effects on the people with a direct proximity to education policies, alternative proximity measures might condition the impact of the reform, namely, that of previous proximity (a policy socialization effect), family proximity (a solidarity effect), and labour market proximity (an unemployment effect).

To measure previous proximity, we rely on variation in the level of education measured with the International Standard Classification of Education, where people with more education have a greater experience with education policies. To measure family proximity, we constructed a measure with information on whether a respondent's partner currently is undergoing education and whether there is a child in the household at the age most likely to receive the state education grant (from 18 to 25). Last, to measure differences in labour market proximity, we use a similar measure as for undergoing education, but for having been doing paid work within the past 7 days, i.e. a measure of whether or not the respondent is likely to be unemployed.

Table 3 presents models similar to the models presented above with the addition of the alternative types of proximity. The models show two key findings. First, none of the alternative proximity measures condition the impact of the reform on government popularity. Secondly, the direct proximity effect remains significant across all models taking the interaction between the alternative proximity measures and the reform into account.

Table 2. Welfare retrenchment and placebo satisfaction outcomes, OLS regression

	Life	Economy	Democracy
Reform	0.14 (0.09)	0.05 (0.13)	-0.02 (0.12)
Education	0.49*** (0.18)	0.11 (0.25)	0.10 (0.23)
Reform × Education	-0.12 (0.22)	-0.12 (0.30)	-0.30 (0.28)
Male	-0.16* (0.09)	0.16 (0.12)	0.12 (0.11)
Age	0.01*** (0.003)	0.004 (0.004)	-0.004 (0.004)
Education level	0.02 (0.03)	0.04 (0.04)	0.10*** (0.03)
Subjective class	0.24*** (0.03)	0.25*** (0.04)	0.15*** (0.04)
Pol. interest	-0.11* (0.06)	-0.05 (0.09)	0.25*** (0.08)
Pol. news	0.05 (0.04)	-0.03 (0.05)	0.12*** (0.05)
Religiosity	0.001 (0.02)	0.06** (0.02)	-0.01 (0.02)
Ideology	0.02 (0.02)	0.02 (0.03)	-0.03 (0.02)
Constant	6.51*** (0.29)	3.41*** (0.40)	5.31*** (0.37)
Observations	1,186	1,186	1,186
R ²	0.08	0.05	0.06

Note: Unstandardized regression coefficients with standard errors in parentheses. The dependent variable is government satisfaction (0–10), with greater values indicating greater levels of satisfaction with the government.

* $P < 0.1$, ** $P < 0.05$, *** $P < 0.01$.

Table 3. Welfare retrenchment and government satisfaction, different proximity measures, OLS regression

	Education level	Family	Paid work	Full model
Reform	-0.24 (0.32)	-0.18 (0.15)	-0.42* (0.23)	-0.39 (0.34)
Education	0.37 (0.27)	0.11 (0.27)	-0.08 (0.30)	0.12 (0.30)
Reform × Education	-0.71** (0.34)	-0.75** (0.34)	-0.58* (0.35)	-0.60* (0.36)
Education level	0.16*** (0.05)			0.19*** (0.05)
Reform × Education level	0.02 (0.07)			-0.003 (0.07)
Family		0.03 (0.22)		0.07 (0.22)
Reform × Family		0.07 (0.31)		0.001 (0.31)
Paid work			-0.32 (0.20)	-0.44** (0.21)
Reform × Paid work			0.39 (0.27)	0.34 (0.28)
Male	0.10 (0.13)	-0.01 (0.13)	-0.003 (0.13)	0.11 (0.13)
Age	0.004 (0.005)	0.002 (0.005)	-0.0002 (0.01)	0.001 (0.01)
Subjective class	0.20*** (0.04)	0.24*** (0.04)	0.25*** (0.04)	0.21*** (0.04)
Pol. interest	-0.10 (0.09)	0.04 (0.09)	0.04 (0.09)	-0.10 (0.09)
Pol. news	-0.04 (0.06)	-0.07 (0.06)	-0.07 (0.06)	-0.05 (0.06)
Religiosity	0.10*** (0.02)	0.09*** (0.02)	0.09*** (0.02)	0.10*** (0.02)
Ideology	-0.17*** (0.03)	-0.19*** (0.03)	-0.19*** (0.03)	-0.17*** (0.03)
Constant	3.52*** (0.46)	3.90*** (0.43)	4.19*** (0.46)	3.82*** (0.49)
Observations	1,186	1,186	1,186	1,186
R ²	0.10	0.09	0.09	0.11

Note: Unstandardized regression coefficients with standard errors in parentheses. The dependent variable is government satisfaction (0–10), with greater values indicating greater levels of satisfaction with the government.

* $P < 0.1$, ** $P < 0.05$, *** $P < 0.01$.

Overall, this substantiates that it is the personal proximity to the policy being retrenched and not alternative proximities to education policies that drives the response. In [Supplementary Material K](#), we further show that prospective family proximity, i.e. having kids below the age threshold of the state education grant, and other types of labour market proximities, did not condition the impact of the reform. Last, to ensure that the results are not explained by education status being a proxy for other factors, e.g. age differences, we estimated the main models after employing the matching procedure on education status (see [Supplementary Material M](#)).

Discussion and Concluding Remarks

Although vote-seeking governments might pursue retrenchments to gain votes ([Elmelund-Præstekær and Emmenegger, 2012](#)), the findings in the welfare retrenchment literature on how the public actually responds to welfare retrenchments are mixed. People punish governments for welfare retrenchments in some cases, but not always. The causal evidence presented here shows that one reason for this discrepancy can be attributed to the fact that not all citizens react in an identical manner to welfare retrenchments. Based on the policy feedback literature interested in welfare reforms

([Soss and Schram, 2007](#)), the expectation tested and documented is that the impact of retrenchment reforms is stronger for those proximate to the policy. In short, the public's proximity to a policy makes it hard for governments to retrench the policy without a strong and negative reaction from the public.

Since welfare policies are popular and often targeting specific groups with strong interests in their preservation, the electoral dynamics related to welfare retrenchments are distinct from welfare expansions ([Pierson, 1996](#)). Using a rare case of a major welfare reform presented during the collection of high-quality data, the findings presented here show that governments cannot, at least under some circumstances, retrench welfare policies without a response from those who are proximate to the policy being retrenched. Hence, while governments can pursue distinct blame avoidance strategies when retrenching welfare policies ([Pierson, 1994](#); [Lindbom 2007](#)), the results substantiate that government policies do not go unnoticed in the public. However, the evidence also suggests that not all voters react to government policies in an equal manner. This provides latitude for strategic governments and support the interpretation that policy makers can target retrenchments to specific parts of the electorate.

The empirical approach employed in this article utilized reform exposure in a quasi-random manner.

Although natural experiments providing as-if random exposure to welfare retrenchments are beneficial, as they can generate causal evidence when the possibilities for conducting randomized controlled trials are limited, there are specific limitations. First, the test presented here does not disentangle the different potential ways in which the personal proximity to a policy matters, but simply shows that those being proximate to a policy react more strongly to welfare reforms retrenching the policy. For example, if the reform being studied here affected recipients immediately, i.e. from one day to the other, one might expect that the response would be stronger (Garritzmann, 2015).

Secondly, just as the contribution to the literature is a causal test of the proximity argument, the present study has noteworthy constraints on the generalizability of the findings. Most importantly, the results are derived from a context of austerity, and previous research suggests that the public is less likely to punish the government for cutbacks in education policies under such conditions (Busemeyer and Garritzmann, 2017). Thirdly, the reform was presented in the Danish context of a multiparty system, where responsibility attribution is unclear. In the present case, only one party ended up voting against the reform. The lack of criticism from the opposition with regard to the retrenchment reform might have resulted in less punishment compared to a scenario where the opposition had provided a counterframe to the reform (Green-Pedersen, 2001; Elmelund-Præstekær et al., 2015).

To conclude, not all voters punish the government for welfare retrenchments. The evidence presented here substantiates when welfare retrenchments matter for government support, and in particular that governments are punished when the public is proximate to the policies being retrenched. Consequently, despite the popularity of welfare policies, people might not punish the government unconditionally for pursuing salient retrenchments. When studies find that retrenchments do not result in a direct and harsh punishment of the government, this can be partially explained by the fact that not all people have a proximate relation to the policy. To understand whether or not welfare retrenchments matter for the support of governments, we need to take the composition of the public into account and particularly the proximity to the policies being retrenched.

Supplementary Data

Supplementary data are available at ESR online.

Acknowledgements

For valuable feedback and insightful comments on earlier drafts of this article, I thank Zoltán Fazekas, Sune Welling Hansen, Paul Marx, Jane Gingrich, Christian Albrekt Larsen, Kristian Kongshøj, Michael Baggesen Klitgaard, Jørgen Goul Andersen, Romana Careja, Asmus Leth Olsen, four anonymous reviewers, and the journal editors. Previous versions of the article were presented at the Department of Political Science, University of Southern Denmark and at the Annual Meeting of the Danish Political Science Association, 2016.

References

- Allan, J. P. and Scruggs, L. (2004). Political partisanship and welfare state reform in advanced industrial societies. *American Journal of Political Science*, 48, 496–512.
- Armingeon, K. and Giger, N. (2008). Conditional punishment: a comparative analysis of the electoral consequences of welfare state retrenchment in OECD nations, 1980–2003. *West European Politics*, 31, 558–580.
- Bendz, A. (2015). Paying attention to politics: public responsiveness and welfare policy change. *Policy Studies Journal*, 43, 309–332.
- Bernardi, F., Chakhaia, L. and Leopold, L. (2017). ‘Sing Me a Song with Social Significance’: the (mis)use of statistical significance testing in european sociological research. *European Sociological Review*, 33, 1–15.
- Blekesaune, M. and Quadagno, J. (2003). Public attitudes toward welfare state policies: a comparative analysis of 24 nations. *European Sociological Review*, 19, 415–427.
- Brooks, C. and Manza, J. (2006a). Social policy responsiveness in developed democracies. *American Sociological Review*, 71, 474–494.
- Brooks, C. and Manza, J. (2006b). Why do welfare states persist? *Journal of Politics*, 68, 816–827.
- Bruch, S. K., Ferree, M. M. and Soss, J. (2010). From policy to polity: democracy, paternalism, and the incorporation of disadvantaged citizens. *American Sociological Review*, 75, 205–226.
- Busemeyer, M. R., Goerres, A. and Weschle, S. (2009). Attitudes towards redistributive spending in an era of demographic ageing: the rival pressures from age and income in 14 OECD countries. *Journal of European Social Policy*, 19, 195–212.
- Busemeyer, M. R. and Garritzmann, J. L. (2017). Public opinion on policy and budgetary trade-offs in European welfare states: evidence from a new comparative survey. *Journal of European Public Policy*, 24, 871–889.
- Campbell, A. L. (2012). Policy makes mass politics. *Annual Review of Political Science*, 15, 333–351.
- de Vries, C. E. and Hobolt, S. B. (2013). *Do Voters Blame Governments for Social Spending Cuts? Evidence from a Natural Experiment*. Working Paper Presented at the Department of Political Science, Odense, Denmark: University of Southern Denmark.
- Elmelund-Præstekær, C. and Emmenegger, P. (2012). Strategic re-framing as a vote winner: why vote-seeking governments pursue unpopular reforms. *Scandinavian Political Studies*, 36, 23–42.

- Elmelund-Præstekær, C., Klitgaard, M. B. and Schumacher, G. (2015). What wins public support? Communicating or obfuscating welfare state retrenchment. *European Political Science Review*, 7, 427–450.
- Garrizmann, J. L. (2015). Attitudes towards student support: how positive feedback-effects prevent change in the Four Worlds of Student Finance. *Journal of European Social Policy*, 25, 139–158.
- Garrizmann, J. L. (2017). The partisan politics of higher education. *PS: Political Science and Politics*, 50, 413–417.
- Garrizmann, J. L. and Seng, K. (2016). Party politics and education spending: challenging some common wisdom. *Journal of European Public Policy*, 32, 510–530.
- Giger, N. (2010). Do voters punish the government for welfare state retrenchment? A comparative study of electoral costs associated with social policy. *Comparative European Politics*, 8, 415–443.
- Giger, N. (2012). Is social policy retrenchment unpopular? How welfare reforms affect government popularity. *European Sociological Review*, 28, 691–700.
- Giger, N. and Nelson, M. (2010). The electoral consequences of welfare state retrenchment: blame avoidance or credit claiming in the era of permanent austerity? *European Journal of Political Research*, 50, 1–23.
- Giger, N. and Nelson, M. (2013). The welfare state or the economy? Preferences, constituencies, and strategies for retrenchment. *European Sociological Review*, 29, 1083–1094.
- Goerres, A. and Tepe, M. (2010). Age-based self-interest, intergenerational solidarity and the welfare state: a comparative analysis of older people's attitudes towards public childcare in 12 OECD countries. *European Journal of Political Research*, 49, 818–851.
- Green-Pedersen, C. (2001). Welfare-state retrenchment in Denmark and the Netherlands, 1982–1998: the role of party competition and party consensus. *Comparative Political Studies*, 34, 963–985.
- Hacker, J. S. (2004). Privatizing risk without privatizing the welfare state: the hidden politics of social policy retrenchment in the United States. *American Political Science Review*, 98, 243–260.
- Hedegaard, T. F. (2014). The policy design effect: proximity as a micro-level explanation of the effect of policy designs on social benefit attitudes. *Scandinavian Political Studies*, 37, 366–384.
- Hetling, A., McDermott, M. L. and Mapps, M. (2008). Symbolism versus policy learning: public opinion of the 1996 U.S. welfare reforms. *American Politics Research*, 36, 335–357.
- Ho, D. E. *et al.* (2007). Matching as nonparametric preprocessing for reducing model dependence in parametric causal inference. *Political Analysis*, 15, 199–236.
- Jensen, C. (2012). Labour market- versus life course-related social policies: understanding cross-programme differences. *Journal of European Public Policy*, 19, 275–291.
- Jæger, M. M. (2009). United but divided: welfare regimes and the level and variance in public support for redistribution. *European Sociological Review*, 25, 723–737.
- Klitgaard, M. B., Schumacher, G. and Soentken, M. (2015). The partisan politics of institutional welfare state reform. *Journal of European Public Policy*, 22, 948–966.
- Kumlin, S. (2004). *The Personal and the Political: How Personal Welfare State Experiences Affect Political Trust and Ideology*. New York: Palgrave Macmillan.
- Lee, S. *et al.* (2017). Risky business? Welfare state reforms and government support in Britain and Denmark. *British Journal of Political Science*; doi:10.1017/S0007123417000382.
- Lerman, A. E. and McCabe, K. T. (2017). Personal experience and public opinion: a theory and test of conditional policy feedback. *Journal of Politics*, 79, 624–641.
- Lindbom, A. (2007). Obfuscating retrenchment: Swedish welfare policy in the 1990s. *Journal of Public Policy*, 27, 129–150.
- Lindh, A. (2015). Public Opinion against markets? Attitudes towards market distribution of social services – a comparison of 17 countries. *Social Policy and Administration*, 49, 887–910.
- Lodge, M. and Taber, C. S. (2013). *The Rationalizing Voter*. Cambridge: Cambridge University Press.
- Lü, X. (2014). Social policy and regime legitimacy: the effects of education reform in China. *American Political Science Review*, 108, 423–437.
- MacLean, L. M. (2011). State retrenchment and the exercise of citizenship in Africa. *Comparative Political Studies*, 44, 1238–1266.
- Marsh, M. and Tilley, J. (2010). The attribution of credit and blame to governments and its impact on vote choice. *British Journal of Political Science*, 40, 115–134.
- Mettler, S. (2011). *The Submerged State: How Invisible Government Policies Undermine American Democracy*, Chicago: University of Chicago Press.
- Mettler, S. and Soss, J. (2004). The consequences of public policy for democratic citizenship: bridging policy studies and mass politics. *Perspectives on Politics*, 2, 55–73.
- Munoz, J., Anduiza, E. and Rico, G. (2014). Empowering cuts? Austerity policies and political involvement in Spain. In Kumlin, S. and Stadelmann-Steffen, I. (Eds.), *How Welfare States Shape the Democratic Public: Policy Feedback, Participation, Voting, and Attitudes*. Cheltenham: Edward Elgar Publishing, pp. 19–40.
- Pierson, P. (1994). *Dismantling the Welfare State? Reagan, Thatcher, and the Politics of Retrenchment*. Cambridge: Cambridge University Press.
- Pierson, P. (1996). The new politics of the welfare state. *World Politics*, 48, 143–179.
- Rehm, P. (2009). Risks and redistribution: an individual-level analysis. *Comparative Political Studies*, 42, 855–881.
- Rhodes, J. H. (2015). Learning citizenship? How state education reforms affect parents' political attitudes and behavior. *Political Behavior*, 37, 181–220.
- Samii, C. (2016). Causal empiricism in quantitative research. *Journal of Politics*, 78, 941–955.
- Schumacher, G., Vis, B. and van Kersbergen, K. (2013). Political parties, welfare image, electoral punishment and welfare state retrenchment. *Comparative European Politics*, 11, 1–21.
- Sekhon, J. S. (2009). Opiates for the masses: matching methods for causal inference. *Annual Review of Political Science*, 12, 487–508.

- Soss, J. and Schram, S. F. (2007). A public transformed? Welfare reform as policy feedback. *American Political Science Review*, 101, 111–127.
- Vis, B. and van Kersbergen, K. (2007). Why and how do political actors pursue risky reforms? *Journal of Theoretical Politics*, 19, 153–172.
- Zaller, J. (1992). *The Nature and Origins of Mass Opinion*. New York: Cambridge University Press.

Erik Gahner Larsen is a Lecturer in Quantitative Politics at the University of Kent. His current research focuses on the interconnections between public policies and public opinion. His most recent articles have been published in the journals *British Journal of Political Science* and *Journal of Elections, Public Opinion & Parties*.