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Welfare Retrenchments and Government Support: Evidence from a Natural Experiment

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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 pur-

suing 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 homogenous 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; Lerman 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 (Garrizmann, 2017), making the impact of the partisan composition of a government on public education spending less clear (Garrizmann 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 intergenerational 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 lead 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

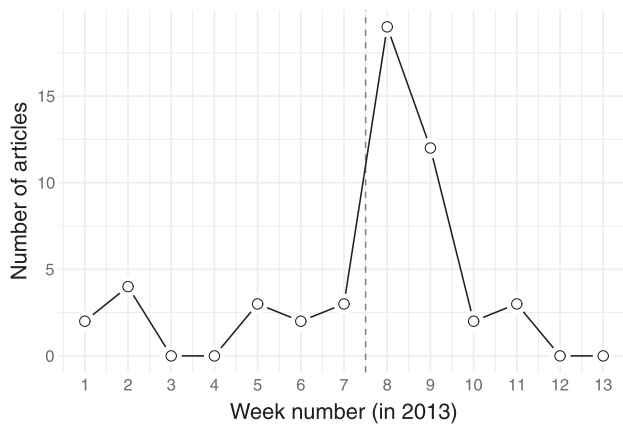


Figure 1. The coverage of the education reform, 2013

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

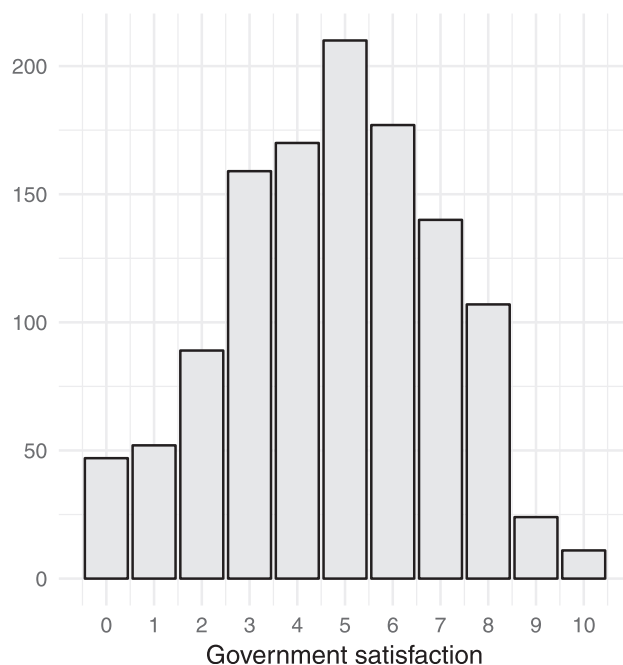


Figure 2. Distribution of government satisfaction
Note: Distribution of government satisfaction. Greater values indicate greater levels of satisfaction with the government. For question wording and descriptive statistics, see [Supplementary Material D](#) and [F](#).

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.

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Supplementary Material for “Welfare Retrenchments and Government Support”

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A. Case: Context of the state education grant reform

The state education grant reform was part of a broader welfare agenda of the government as described in the government coalition agreement from 2011 (The Government, 2011). The coalition agreement described that the government was a so-called reform government that would pursue multiple reforms, including education reforms (from the coalition agreement in Danish: “*Danmark har fået en reformregering. Vi vil gennemføre reformer af uddannelserne. Vi vil gennemføre en skatterreform. Vi vil gennemføre en vækstreform. Reformen, som fremtidssikrer Danmark.*”).

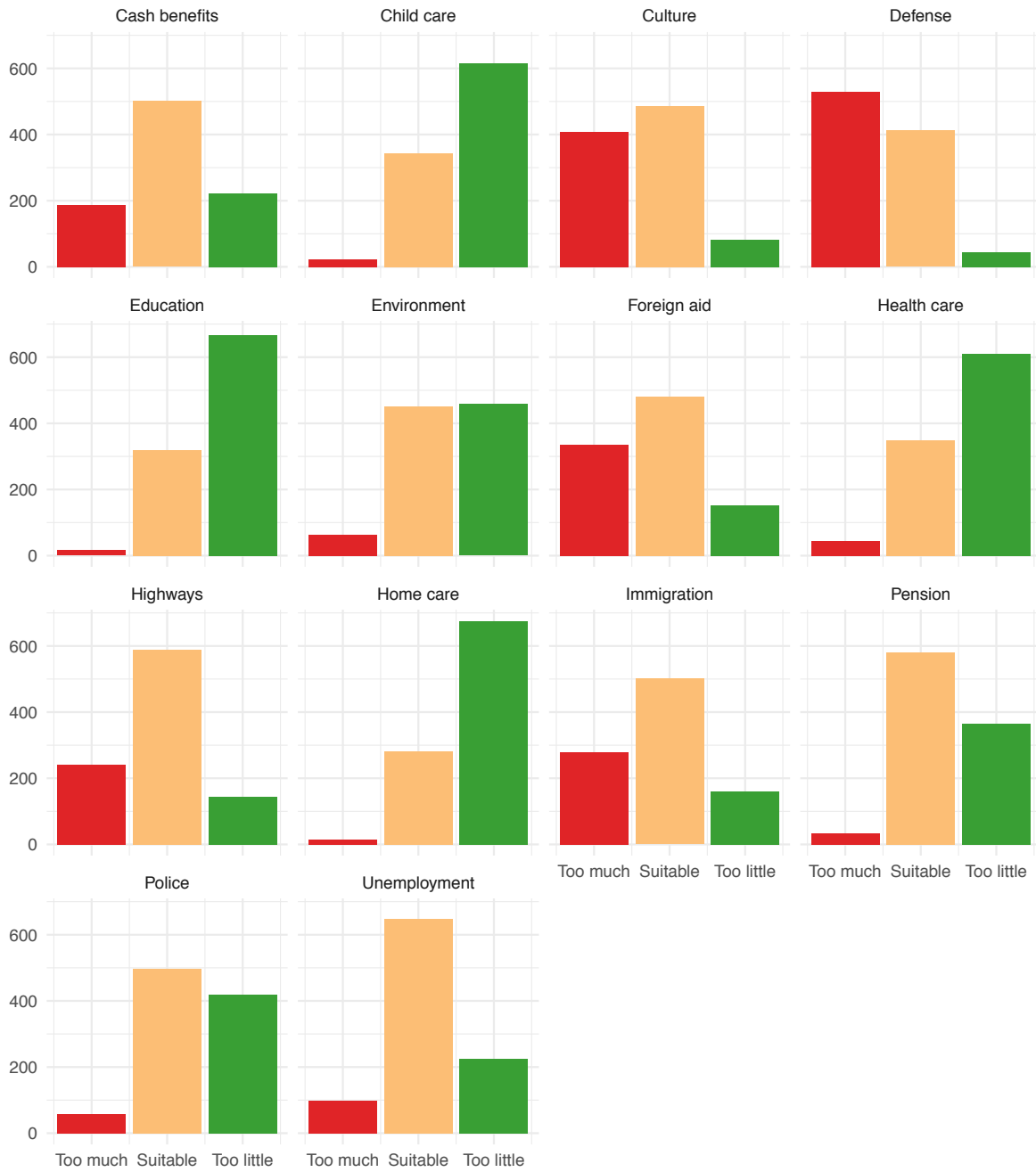
The government announced in 2011 that it would invest in education to ensure the quality of the education system and the welfare state, and it made it clear that it would not change the state education grant system, but keep it in its current form and reform other parts of the education system (from the coalition agreement in Danish: “*Derfor vil regeringen bevare SU-systemet i dets nuværende form, men undersøge mulige positive incitamenter indenfor SU-systemet, der kan få unge til både at begynde tidligere på deres uddannelse og gøre uddannelsen hurtigere færdig end i dag.*”).

The education reform was presented on February 19, 2013, as a press release from the Ministry of Higher Education and Science (2013). The government made it clear that the aim of the reform was to make it more attractive for students to complete their education more quickly and to improve public finances.

In order to examine the public’s preferences toward education, and in order to compare it to other policy domains, I use data from the Danish National Election Study 2011 (Stubager et al., 2013). This survey includes questions related to whether the government spend too much, a suitable amount or too little on the specific policy domain.

Figure A.1 show the spending preferences across the 14 policies. Overall, a majority of the public wants more spending on education, and only a very limited number of respondents say they want the government to spend less on education. This mimics the results for other policies such as child care, health care and home care.

Figure A.1: Public spending preferences, 2011, 14 policies

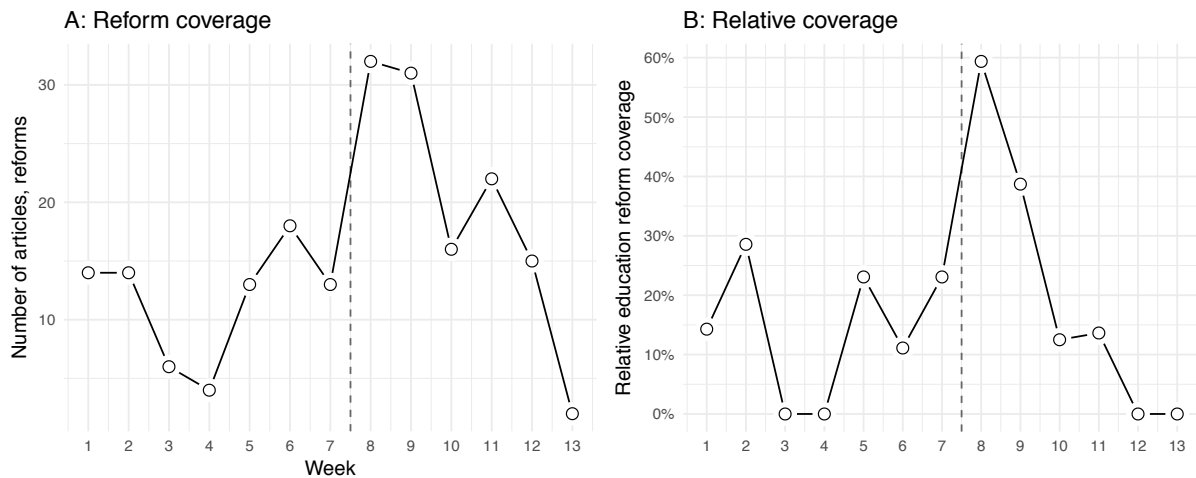


Note: The y-axis is showing the frequency of responses. *Source:* Danish National Election Study, 2011.

B. Case: Nationwide media coverage of the reform

There was an increase in the nationwide coverage of the state education grant system retrenchments after the presentation of the reform (cf. Figure 1 in the main text). Figure B.1 shows that there was an increase in the general retrenchment reform coverage (Panel A) and that the relative share of the mentioning of the state education grant reform in the overall reform coverage was greater after the presentation of the reform (Panel B).

Figure B.1: Nationwide media coverage of retrenchment reforms



Note: Number of articles mentioning reforms in the nationwide coverage (Panel A) and percentage of articles mentioning the state education grant reform of all articles mentioning reforms. Same collection procedure as for Figure 1 described in the main text.

The overall retrenchment reform coverage in the study period peaked in the week after the presentation of the reform. Furthermore, of all the articles mentioning retrenchment reforms after the presentation of the state education grant reform, 60% of the articles mentioned the state education grant reform. Thus, it is unlikely that other reforms dominated the coverage in the weeks after the presentation. However, as the figure also shows, there was coverage of reforms both before and after

the presentation of the reform, emphasizing that the counterfactual scenario to the state education grant reform is not a context with no talk about reforms at all, but a context with no specific retrenchment reform.

In order to examine the coverage of the state education grant reform in closer detail, all articles mentioning the state education grant reform in the retrenchment coverage was coded for specific information on the reform. More specifically, this analysis examined 1) the context of the article, i.e. why the state education grant system was mentioned, and 2) any specific information on reforms. For the specific information on a reform, the main focus was on statements about specific parts of the state education grant reform. Noteworthy, the coverage was only analyzed for the nationwide newspapers and might not be representative for the local coverage and the coverage in other types of media such as TV and radio.

Table B.1 presents an overview of all articles in the study period. The table provides an English translation of the title of the article with the unique ID in the parenthesis, media outlet, date of publication, the context and reform information.

Table B.1: Coverage of education reform retrenchments

Title (id)	Outlet	Date	Context	Information on reform
Week 1				
The state education grant is a target (e397d634)	Berlingske	January 5	Information on reform plans from government sources.	Considerations on how a education reform might look like.
Interview: It will bomb the social mobility back to the Stone Age if you cut directly in the SU (e39824d8)	Politiken	January 6	Prime Minister Helle Thorning-Schmidt's New Year Address.	The government is preparing an education reform.
Week 2				
Debate: Planned education retrenchments are short-sighted and expensive (e3991c29)	Politiken	January 9	Prime Minister Helle Thorning-Schmidt's New Year Address.	The government is preparing an education reform.
Debate: Thoughts on an state education grant reform	Information	January 11	Discussions about an education reform.	The government is preparing an education reform.

Debate: Why exactly the state education grant system? (e39aa14e)	Information	January 12	The need for welfare re-trenchments.	The government is preparing an education reform.
Debate: Direction towards an unfair state education grant reform (e39a8a2d)	Arbejderen	January 12	Discussions about an education reform.	The government is preparing an education reform.

Week 3

No articles

Week 4

No articles

Week 5

Retrenchment plans: Two billion requires radical changes (e3a1e572)	Politiken	January 31	Information on retrenchment plans of the state education grant system.	The government is preparing an education reform.
Debate: Reforms used to be something positive (e3a254cd)	Arbejderen	February 1	Years of welfare re-trenchments.	The government is preparing an education reform.
Rectors in showdown with SU-system (e3a2c1c0)	Berlingske	February 2	Ideas on how to reform the education grant system.	The government is preparing an education reform.

Week 6

Students protest against SU-proposals (e3a40bd4)	Jyllands-Posten	February 5	Protest against the proposals made by university rectors.	The government is preparing an education reform.
State education grant cuts pushes disadvantaged students out (e3a61cec)	Kristeligt Dagblad	February 9	Information on retrenchment plans of the state education grant system.	The government is preparing an education reform.

Week 7

Debate: A hope for Danish traditions (e3a83bf5)	Information	February 15	Information on retrenchment plans of the state education grant system.	The government is preparing an education reform.
The Ministry of Finance puts pressure on Østergaard before state education grant reform (e3a82ef7)	Politiken	February 15	Information on retrenchment plans of the state education grant system and in particular discussions between the Ministry of Finance and Ministry of Education.	The government is preparing an education reform.
Debate: It will be expensive to retrench the state education grant system (e3a8284c)	Politiken	February 15	Rumors about retrenchment plans.	The government is preparing an education reform.

Week 8

Minister changed direction in the debate on the state education grant reform (e3a96bf4)	Kristeligt Dagblad	February 19	Political debate about the reform.	The government is preparing an education reform.
Adolescents: Meaningless savings plan (e3a9ee99)	Politiken	February 20	The presentation of the state education grant reform.	Specific information on the state education grant reform.
Proposal for state education grant reform evokes relief (e3a9ee96)	Politiken	February 20	The presentation of the state education grant reform.	Specific information on the state education grant reform.

Debate: The necessary broken promise (e3a9f5f9)	Information	February 20	The presentation of the state education grant reform.	Specific information on the state education grant reform.
The government has presented its state education grant reform (e3a9f4a2)	Arbejderen	February 20	The presentation of the state education grant reform.	Specific information on the state education grant reform.
Debate: State education grant reform – please reply (e3a9fa50)	Information	February 20	The presentation of the state education grant reform.	Information on the state education grant reform.
'Gentle' broken promises (e3a9f613)	BT	February 20	The presentation of the state education grant reform.	Specific information on the state education grant reform.
Debate: Morten vs. Morten (e3a9ee3e)	Arbejderen	February 20	The presentation of the state education grant reform.	Information on the state education grant reform.
Debate: New presents for business (e3a9f4b1)	Arbejderen	February 21	Information on retrenchment plans of the state education grant system.	The government is preparing an education reform.
Demonstration against the state education grant reform (e3aa60f8)	Arbejderen	February 21	The presentation of the state education grant reform.	Specific information on the state education grant reform.
A lot of retrenchments (e3aa6102)	Arbejderen	February 21	The presentation of the state education grant reform.	Specific information on the state education grant reform.
Students blocked the Faculty of Theology in Copenhagen (e3aafb7)	Kristeligt Dagblad	February 22	The presentation of the state education grant reform.	Specific information on the state education grant reform.
Debate: A happy student's secret about the state education grant reform (e3aaf5c0)	Information	February 22	Information on retrenchment plans of the state education grant system.	The government is preparing an education reform. (Only the headline mention the reform.)
Debate: "Æh, bæh, buh" - student protests now (e3ab6d8a)	Politiken	February 23	The presentation of the state education grant reform.	Specific information on the state education grant reform.
Analysis: The spoiled youth - intergenerational unity is in flux (e3ab5aec)	Berlingske	February 23	The presentation of the state education grant reform.	Information on the state education grant reform.
Morten Østergaards' beard (e3ab767e)	Information	February 23	General political analysis not related to the reform.	No information.
Debate: State education grant reform (e3abaf85)	Politiken	February 24	Two letters to the editor arguing for and against the state education grant reform, respectively.	Information on the state education grant reform.
Is there a spindoctor? (e3abac35)	Politiken	February 24	The presentation of the state education grant reform.	Information on the state education grant reform.
The welfare steamer reverses (e3abb0fe)	Jyllands-Posten	February 24	The presentation of the state education grant reform.	Specific information on the state education grant reform.

Week 9

Harsh criticism of the governments' reverse Robin Hood (e3ac4946)	Berlingske	February 26	The reform strategy of the government	Information on the state education grant reform.
Mette's attack on the poor (e3ac57a4)	Ekstra Bladet	February 26	General political analysis not related to the reform.	No information.

It's classical Social Democratic policy (e3acc93e)	Jyllands-Posten	February 27	General political analysis not related to the reform.	No information.
Students in full combat for the state education grant (e3ad3803)	Arbejderen	February 28	The presentation of the state education grant reform.	Specific information on the state education grant reform.
Sleep is replaced with big meetings (e3ad380a)	Arbejderen	February 28	The presentation of the state education grant reform.	Information on the state education grant reform.
The battle does not end here (e3ae46ec)	Arbejderen	March 1	The presentation of the state education grant reform.	Information on the state education grant reform.
Debate: The youth of today are a generation of complainers (e3aeb6bc)	Politiken	March 2	The presentation of the state education grant reform.	Information on the state education grant reform.
Daylight robbery (e3aebc4b)	Arbejderen	March 2	The presentation of the state education grant reform.	Specific information on the state education grant reform.
Debate: Don't include the teaching (e3aec1e3)	Jyllands-Posten	March 2	The presentation of the state education grant reform.	No information.
Debate: Reject the state education grant impairments (e3aebc0a)	Arbejderen	March 2	The presentation of the state education grant reform.	Specific information on the state education grant reform.
Analysis: When internal confrontations took the life of an offensive (e3af1bd2)	Politiken	March 3	General political analysis not related to the reform.	No information.
Chaos is restored (e3af1d0d)	Politiken	March 3	General political analysis not related to the reform.	No information.
Week 10				
Debate: The state education grant and growth (e3af45f4)	Jyllands-Posten	March 4	The presentation of the state education grant reform.	Information on the state education grant reform.
Debate: You are perceived as heartless in you are in favor of the state education grant reform (e3afb846)	Politiken	March 5	The presentation of the state education grant reform.	Information on the state education grant reform.
Week 11				
You will get more protests (e3b4a9ac)	Arbejderen	March 15	The presentation of the state education grant reform.	Specific information on the state education grant reform.
Juggling with the students' livelihood (e3b4a9af)	Arbejderen	March 15	The presentation of the state education grant reform.	Information on the state education grant reform.
The state education grant reform costs the tip of a jet fighter (e3b50573)	Arbejderen	March 16	The reform strategy of the government	Information on the state education grant reform.

Note: All articles are collected through the media database Infomedia (infomedia.dk).

Prior to the presentation of the reform, there was articles mentioning the potential for reforms in relation to the state education grant, and in particular articles discussing the reforming the state

education grant in order to address a deficit of two billion Danish kroner. However, there was no specific information on the content of the reform or details on when and how a reform would be presented or implemented.

After the presentation of the reform, there was not only an increase in the coverage of the state education grant reform, but also a more detailed coverage with information on the specific details on the reform. This included coverage on a shortened period of state education grant, additional requirements of progression in study programs in order for students to get the state education grant, requirements for the universities to improve student completion times, changed state education grants for people living with their parents, regulation of the state education grant as a transfer payment and more. In other words, the qualitative nature of the coverage changed after the presentation with the coverage being substantially more detailed and specific about an actual reform.

The coverage after the presentation of the reform was about the specific reform and events in relation to the reform, e.g. student demonstrations against the content of the reform, in sharp contrast to the less specific coverage about potential reforms prior to the presentation. Importantly, and as described in the main text, there was coverage about potential reforms of the state education grant prior to February 19, but not in specific details (i.e. the qualitative aspects of the coverage) or to an extent as after the presentation (i.e. the quantitative aspects of the coverage).

Last, two additional points are worth mentioning based on the analysis. First, some articles discussed the reform in relation to the general reform agenda of the government. Thus, it is likely that the public will not react to the specific reform, but an increase in the coverage of the overall reform agenda. Second, and as a caveat to the content analysis, a majority of the articles are from centre-left outlets (Politiken) and leftish outlets (Information and Arbejderen). One explanation might be that other outlets are less likely to mention the reform in relation to retrenchments, but rather frame the reform as rationalizations and improvements.

C. Data: Dates of interviews

Figure C.1: Date of interview, ESS, Denmark

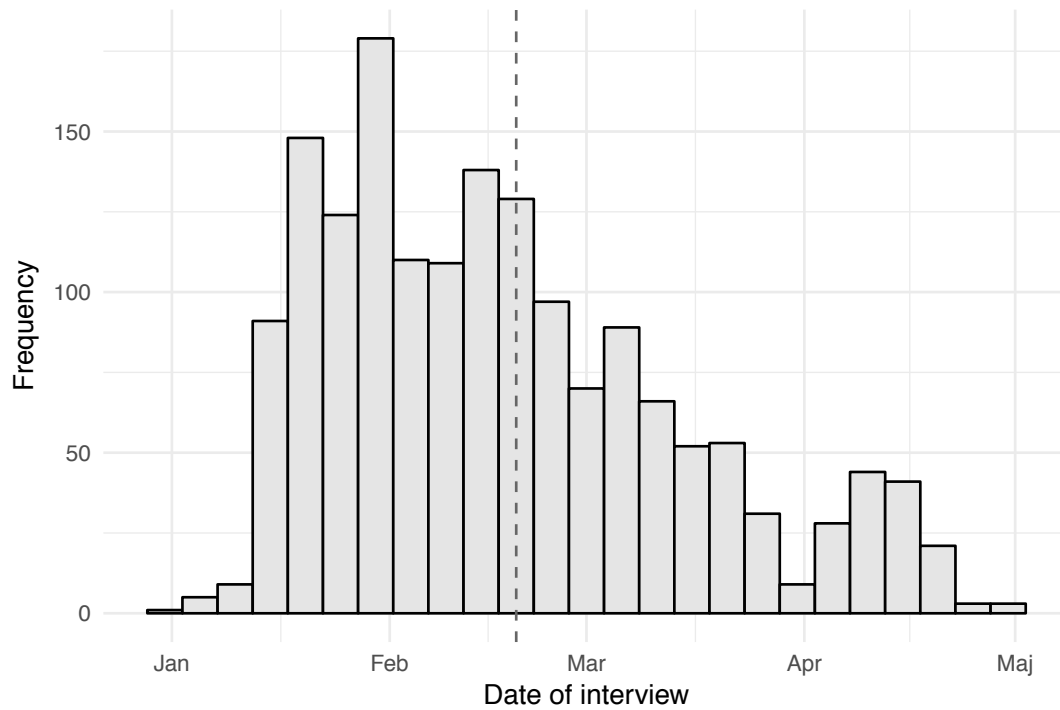


Figure C.1 shows the day of interview in the 6th round of the European Social Survey for Denmark. The first interview was conducted on January 11 and the last on May 2. The dashed line in the figure indicates the day of the announcement of the reform, February 19.

D. Data: Question wording

Government satisfaction. Question: Now thinking about the Danish government, how satisfied are you with the way it is doing its job? Values: Extremely dissatisfied [0], Extremely satisfied [10]

Education. Question: Which of these descriptions applies to what you have been doing for the last 7 days? In education, (not paid for by employer) even if on vacation.

Male. Values: Male [1], Female [0].

Age. Derived from year born.

Education level. Question: What is the highest level of education you have successfully completed?

Values: ES-ISCED I, less than lower secondary [0], ES-ISCED II, lower secondary [1], ES-ISCED IIIb, lower tier upper secondary [2], ES-ISCED IIIa, upper tier upper secondary [3], ES-ISCED IV, advanced vocational [4], ES-ISCED V1, lower tertiary education [5], ES-ISCED V2, higher tertiary education [6].

Subjective class. Question: There are people who tend to be towards the top of our society and people who tend to be towards the bottom. On this card there is a scale that runs from top to bottom. Where would you place yourself on this scale nowadays? Values: Bottom of our society [0], Top of our society [10].

Political interest. Question: How interested would you say you are in politics – are you...? Values: Very interested [1], quite interested [2], hardly interested [3], or, not at all interested? [4].

Political news. Question: On an average weekday, how much of your time watching television is spent watching news or programmes about politics and current affairs? Values: No time at all [0], Less than ½ hour [1], ½ hour to 1 hour [2], More than 1 hour, up to 1½ hours [3], More than 1½ hours, up to 2 hours [4], More than 2 hours, up to 2½ hours [5], More than 2½ hours, up to 3 hours [6], More than 3 hours [7].

Religiosity. Question: Regardless of whether you belong to a particular religion, how religious would you say you are? Values: Not at all religious [0], Very religious [10].

Ideology. Question: In politics people sometimes talk of “left” and “right”. Using this card, where would you place yourself on this scale, where 0 means the left and 10 means the right? Values: Left [0], Right [10].

Life satisfaction. Question: All things considered, how satisfied are you with your life as a whole nowadays? Values: Extremely dissatisfied [0], Extremely satisfied [10].

Economy satisfaction. Question: On the whole how satisfied are you with the present state of the economy in Denmark? Values: Extremely dissatisfied [0], Extremely satisfied [10].

Democracy satisfaction. Question: On the whole, how satisfied are you with the way democracy works in Denmark? Values: Extremely dissatisfied [0], Extremely satisfied [10].

Family proximity. Question 1: Which of the descriptions on this card applies to what he/she has been doing for the last 7 days? In education, (not paid for by employer) even if on vacation. Question 2: Including yourself, how many people – including children – live here regularly as members of this household? And in what year were you/was he/she born?

Prospective family proximity. Question: Including yourself, how many people – including children – live here regularly as members of this household? And in what year were you/was he/she born?

Paid work. Question: Which of these descriptions applies to what you have been doing for the last 7 days? In paid work (or away temporarily) (employee, self-employed, working for your family business).

Unemployed. Question: Which of these descriptions applies to what you have been doing for the last 7 days? Unemployed (and actively looking for a job), unemployed (wanting a job but not actively looking for a job).

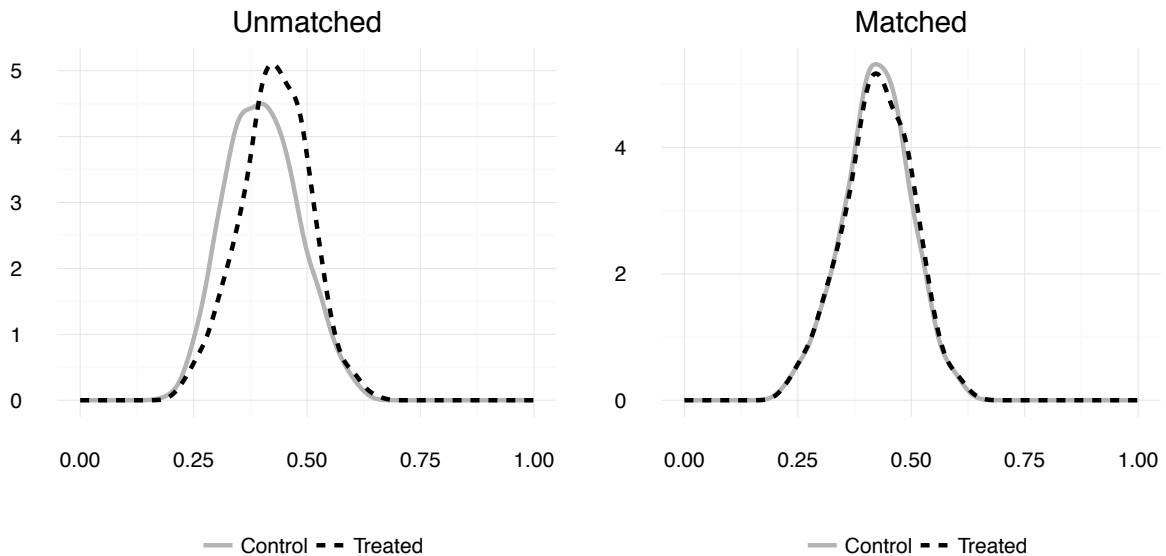
Housework. Question: Which of these descriptions applies to what you have been doing for the last 7 days? Doing housework, looking after children or other persons.

E. Data: Description of the matching procedure

The matching procedure ensures that the two groups are of equal size and that the people interviewed before and after were comparable on a set of covariates, namely gender, age, education status, education level, subjective class, ideology, religiosity, political interest and political news consumption. The matching procedure results in a greater number of unmatched control cases (267 subjects) as a greater number of people were interviewed prior to the reform than after. For the treated group, 10 cases were left unmatched. This provides a matched sample of 593 subjects interviewed before the reform and 593 interviewed after the reform.

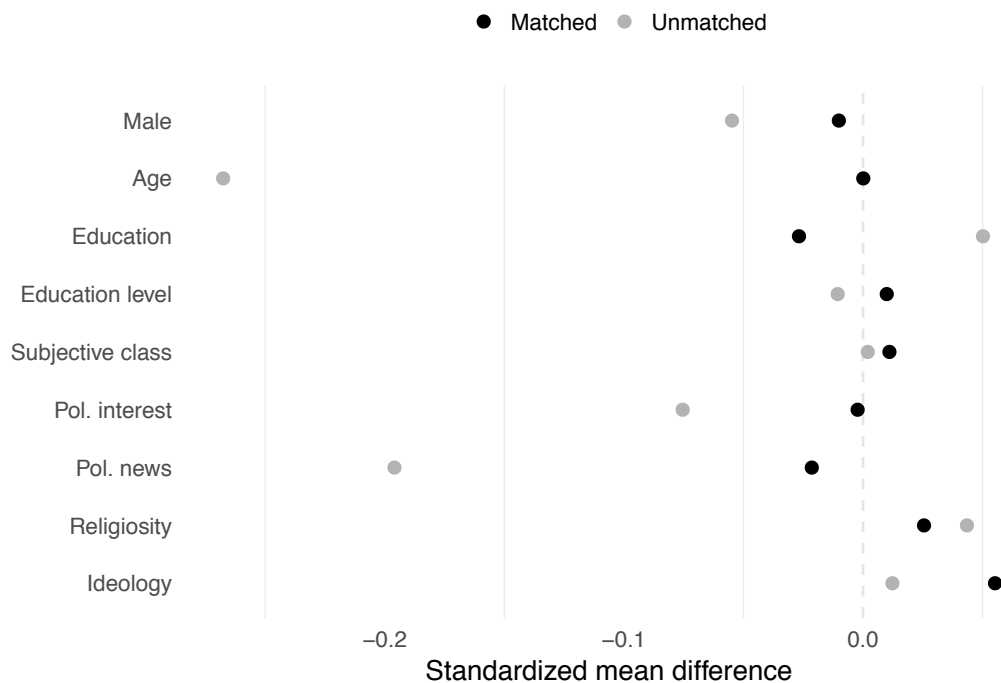
Figure E.1 shows the distribution of propensity scores before and after the matching procedure. If people interviewed after the presentation of the reform are comparable to the people interviewed before, there will be a complete overlap between the treatment and control group, and while there is a satisfactory overlap in the unmatched sample, there is a better overlap in the matched sample.

Figure E.1: Distribution of propensity scores, unmatched and matched data



In order to examine the balance, Figure E.2 shows the standardized mean differences on the covariates prior to and after the matching procedure. The standardized mean differences are greater prior to the matching procedure, and there are some significant differences on age and political news consumption. Noteworthy, none of the differences are significant after the matching procedure. The likelihood ratio chi-square test for the unmatched data is 34,36 (df = 9, $p < 0.01$) and 1,58 (df = 9, $p = 0.9965$) for the matched data. The Nagelkerke R^2 is 0.031 for the unmatched data and 0.002 for the matched data.

Figure E.2: Balance between unmatched and matched sample



The process and statistical software used to produce the matches are described in Ho *et al.* (2007; 2011).

F. Data: Descriptive statistics

Table F.1: Summary statistics, full data

Statistic	N	Mean	St. Dev.	Min	Max
Government satisfaction	1,613	4.797	2.264	0	10
Reform	1,613	0.411	0.492	0	1
Education	1,650	0.166	0.372	0	1
Male	1,650	0.505	0.500	0	1
Age	1,650	48.701	19.016	16	97
Education level	1,645	3.960	1.907	1	7
Subjective class	1,617	6.434	1.545	0	10
Political interest	1,648	2.868	0.764	1	4
Political news	1,603	2.295	1.329	0	7
Religiosity	1,643	4.197	2.684	0	10
Ideology	1,585	5.338	2.372	0	10

Table F.2: Summary statistics, matched data

Statistic	N	Mean	St. Dev.	Min	Max
Government satisfaction	1,186	4.773	2.234	0	10
Reform	1,186	0.500	0.500	0	1
Education	1,186	0.172	0.378	0	1
Male	1,186	0.500	0.500	0	1
Age	1,186	46.277	17.929	16	93
Education level	1,186	4.024	1.870	1	7
Subjective class	1,186	6.470	1.527	0	10
Political interest	1,186	2.866	0.745	1	4
Political news	1,186	2.187	1.258	0	7
Religiosity	1,186	4.209	2.645	0	10
Ideology	1,186	5.302	2.322	0	10

G. Results: Matching specifications

To test the sensitivity of the results to the choices of the matching procedure, several models were estimated with different specifications. First, Table G.1 provide the results on the full data, i.e. the results prior to any preprocessing of the data. The table show that the results hold prior to any matching procedure, substantiating that the estimates provided in the main text is not conditional upon a specific model.

Table G.1: Welfare retrenchment and government satisfaction, full data, OLS regression

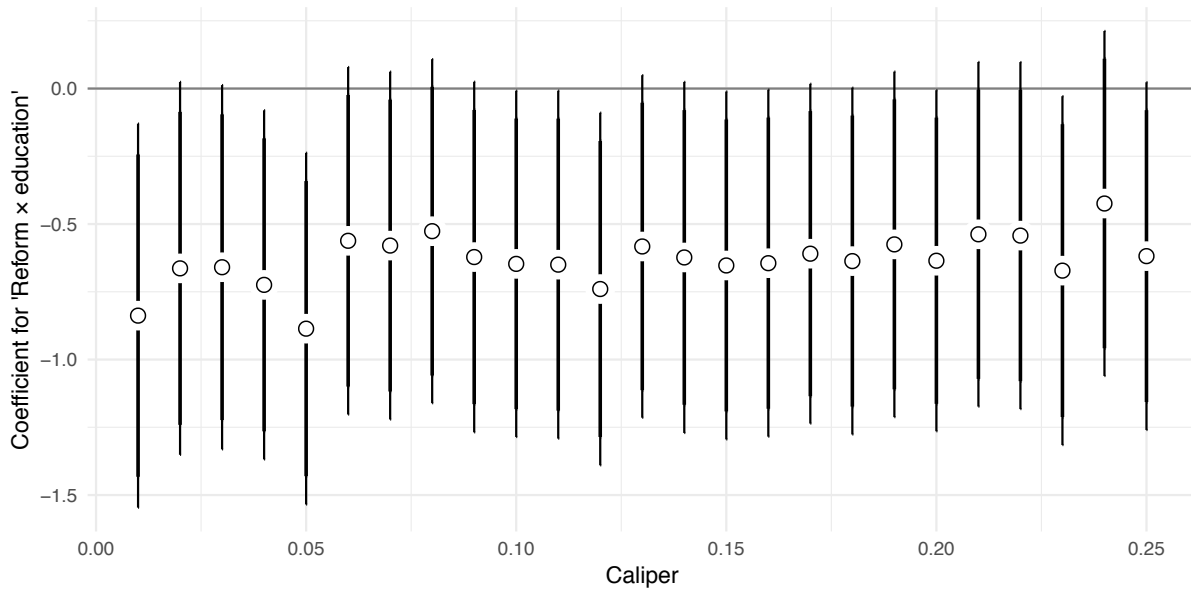
	(1)	(2)	(3)	(4)
Reform	-0.28** (0.12)	-0.28** (0.11)	-0.19 (0.13)	-0.18 (0.12)
Education		0.07 (0.12)		0.06 (0.12)
Reform×Education			-0.55* (0.31)	-0.62** (0.31)
Male		0.01 (0.003)		0.01 (0.004)
Age		0.15*** (0.03)		0.15*** (0.03)
Education level		0.18*** (0.04)		0.18*** (0.04)
Subjective class		-0.11 (0.08)		-0.11 (0.08)
Pol. interest		-0.04 (0.05)		-0.04 (0.05)
Pol. news		0.11*** (0.02)		0.12*** (0.02)
Religiosity		-0.18*** (0.02)		-0.18*** (0.02)
Ideology			-0.05 (0.20)	0.31 (0.24)
Constant	4.93*** (0.07)	3.73*** (0.35)	4.94*** (0.08)	3.65*** (0.39)
Observations	1,577	1,473	1,577	1,473
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 to 10), with greater values indicating greater levels of satisfaction with the government. * p < 0.1, ** p < 0.05, *** p < 0.01

The models presented in the main text use a single caliper choice. The caliper choice determines which control units are matched to treated units, and to examine the robustness of the caliper choice, Figure G.1 show the interaction coefficients from several models with different calipers. The models further controls for the covariates described and used in the main text. Overall, the figure shows that the results are robust, although the level of significance vary slightly. Accordingly, some models have

0 in the 95% confidence interval and a single model also in the 90% confidence interval. However, this is what to be expected based on the number of models being estimated.

Figure G.1: Results using different calipers with nearest neighbor matching



Note: The heterogeneous effect of the reform using different calipers (from 0.01 to 0.25). Matched on the variables described in the main text and with the covariates used in the full model in the manuscript (Column 5, Table 1). The thin vertical lines indicate 95% confidence interval and the thick vertical lines indicate 90% confidence intervals.

An additional concern is related to balance in the covariates between people undergoing education. More specifically, the people undergoing education in the sample might differ on several characteristics before and after the presentation of the reform with implications for the propensity to being treated. To take this possibility into account, the matching procedure was conducted with each covariate interacted with education status, i.e. a more flexible functional form. Table G.2 show the results when estimating the key parameters on the sample with the covariates-education matching data. Overall, this provide more conservative estimates, mostly due to a stronger effect in the general population, but an interaction substantially identical to the results presented in the main text.

Table G.2: Welfare retrenchment and government satisfaction, covariates-education matching

	(1)	(2)	(3)	(4)
Reform	-0.27** (0.13)	-0.23* (0.12)	-0.16 (0.14)	-0.12 (0.14)
Education			0.03 (0.24)	0.19 (0.27)
Reform×Education			-0.65* (0.34)	-0.67** (0.33)
Male		0.08 (0.13)		0.06 (0.13)
Age		0.003 (0.004)		0.001 (0.005)
Education level		0.15*** (0.04)		0.14*** (0.04)
Subjective class		0.19*** (0.04)		0.19*** (0.04)
Pol. interest		-0.16* (0.09)		-0.16* (0.09)
Pol. news		-0.04 (0.06)		-0.04 (0.06)
Religiosity		0.11*** (0.02)		0.11*** (0.02)
Ideology		-0.18*** (0.03)		-0.18*** (0.03)
Constant	4.91*** (0.09)	3.91*** (0.39)	4.91*** (0.10)	3.97*** (0.43)
Observations	1,194	1,194	1,194	1,194
R ²	0.004	0.09	0.01	0.09

Note: Unstandardized regression coefficients with standard errors in parentheses. The dependent variable is government satisfaction (0 to 10), with greater values indicating greater levels of satisfaction with the government. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Last, the results were estimated using optimal and full matching techniques. Table G.3 show the results using the data sets with optimal and full matching for a model with the interaction between the reform and education status and a model with statistical control for the covariates as well. In sum, the results are substantially identical to the results presented in the main text.

Table G.3: Welfare retrenchment and government satisfaction, optimal and full matching

Matching	Optimal (1)	Optimal (2)	Full (3)	Full (4)
Reform	-0.17 (0.14)	-0.17 (0.14)	-0.19 (0.13)	-0.19 (0.12)
Education	0.12 (0.23)	0.37 (0.26)	0.03 (0.21)	0.30 (0.24)
Reform×Education	-0.72** (0.34)	-0.70** (0.33)	-0.62* (0.32)	-0.60* (0.31)
Male		0.02 (0.13)		0.07 (0.12)
Age		0.005 (0.005)		0.01 (0.004)
Education level		0.16*** (0.04)		0.16*** (0.03)
Subjective class		0.18*** (0.04)		0.18*** (0.04)
Pol. interest		-0.11 (0.09)		-0.13 (0.08)
Pol. news		-0.04 (0.05)		-0.03 (0.05)
Religiosity		0.10*** (0.02)		0.12*** (0.02)
Ideology		-0.17*** (0.03)		-0.18*** (0.02)
Constant	4.90*** (0.10)	3.74*** (0.43)	4.92*** (0.08)	3.67*** (0.39)
Observations	1,206	1,206	1,463	1,463
R ²	0.01	0.09	0.01	0.10

Note: Unstandardized regression coefficients with standard errors in parentheses. The dependent variable is government satisfaction (0 to 10), with greater values indicating greater levels of satisfaction with the government. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

H. Results: Time trend control

To test whether the results hold when taking the day of the interview into account, Table H.1 show a series of regressions mimicking the key results presented in the manuscript with the addition of a statistical control for the distance in days to the presentation of the reform (February 19). The results are substantially identical to the results presented in Table 1 in the manuscript. In other words, taking the specific distance in the day of interview into account have no implications for the results.

Table H.1: Welfare retrenchment and government satisfaction, time trend control, OLS regression

	(1)	(2)	(3)	(4)
Reform	-0.24* (0.13)	-0.23* (0.12)	-0.13 (0.14)	-0.11 (0.14)
Time trend	-0.01*** (0.004)	-0.01*** (0.004)	-0.01*** (0.004)	-0.01*** (0.004)
Education			0.10 (0.24)	0.39 (0.27)
Reform×Education			-0.69** (0.34)	-0.70** (0.33)
Male		0.13 (0.13)		0.12 (0.13)
Age		0.004 (0.004)		0.004 (0.005)
Education level		0.17*** (0.04)		0.18*** (0.04)
Subjective class		0.20*** (0.04)		0.20*** (0.04)
Pol. interest		-0.12 (0.09)		-0.11 (0.09)
Pol. news		-0.04 (0.05)		-0.04 (0.05)
Religiosity		0.10*** (0.02)		0.10*** (0.02)
Ideology		-0.17*** (0.03)		-0.17*** (0.03)
Constant	5.16*** (0.12)	3.83*** (0.40)	5.13*** (0.13)	3.70*** (0.44)
Observations	1,186	1,186	1,186	1,186
R ²	0.01	0.10	0.02	0.11

Note: Unstandardized regression coefficients with standard errors in parentheses. The dependent variable is government satisfaction (0 to 10), with greater values indicating greater levels of satisfaction with the government. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

I. Results: Regression discontinuity design estimates

In order to test whether there was a discontinuity in government satisfaction after the presentation of the reform, a regression discontinuity design was used to estimate the local average treatment effect identified at the cutoff point, c , i.e. the day the reform was presented. The design utilizes a sharp discontinuity, i.e. a conditional probability of treatment at the threshold value going from 0 to 1.¹

The limitation of the design in this context is the requirement of a large N to estimate the local effect. In the sample, we are substantially limited by the small number of interviews, i.e. 274 persons undergoing education over the full period with several days with no observations at all. As a result, the RDD estimates are only estimated for the full sample. Furthermore, the bandwidth choice is crucial as a lower bandwidth reduces the risk of bias but further reduces the number of observations (Green et al. 2009; Robinson et al. 2009). Accordingly, the models are estimated on the full data set as well as a limited period excluding people interviewed more than 45 days before or after the presentation of the reform.

To estimate the RDD effects we rely on the robust nonparametric procedure described in Calonico *et al.* (2014). These procedures result in conventional, bias corrected and robust estimates. Accordingly, we have six estimates (two samples with three estimates for each sample). Table I.1 shows the local average treatment effect of the reform for the full and limited data and the different estimates.

Table I.1: Welfare retrenchment and government satisfaction, RDD estimates

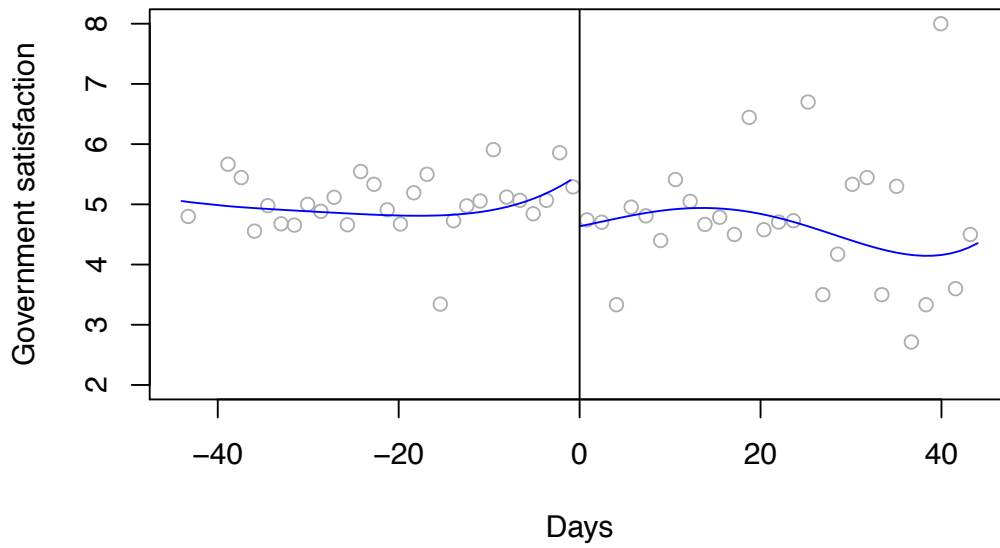
	Full			Limited		
	Conventional	Bias-corrected	Robust	Conventional	Bias-corrected	Robust
Reform	-0.69* (0.36)	-0.79** (0.36)	-0.79** (0.42)	-0.72** (0.35)	-0.83** (0.35)	-0.83** (0.42)
N	1613	1613	1613	1493	1493	1493

Note: RDD estimates with standard errors in parentheses. The dependent variable is government satisfaction (0 to 10), with greater values indicating greater levels of satisfaction with the government. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

¹ Formally, the parameter of interest is: $\tau = \lim_{x \downarrow c} E[Y(1)|X = c] - \lim_{x \uparrow c} E[Y(0)|X = c]$.

Across all models, we find that there is a discontinuity in the satisfaction with the government when the reform is presented. Figure I.1 visualize this discontinuity based on the robust model with the limited data.

Figure I.1: Welfare retrenchment and government satisfaction, RDD estimate

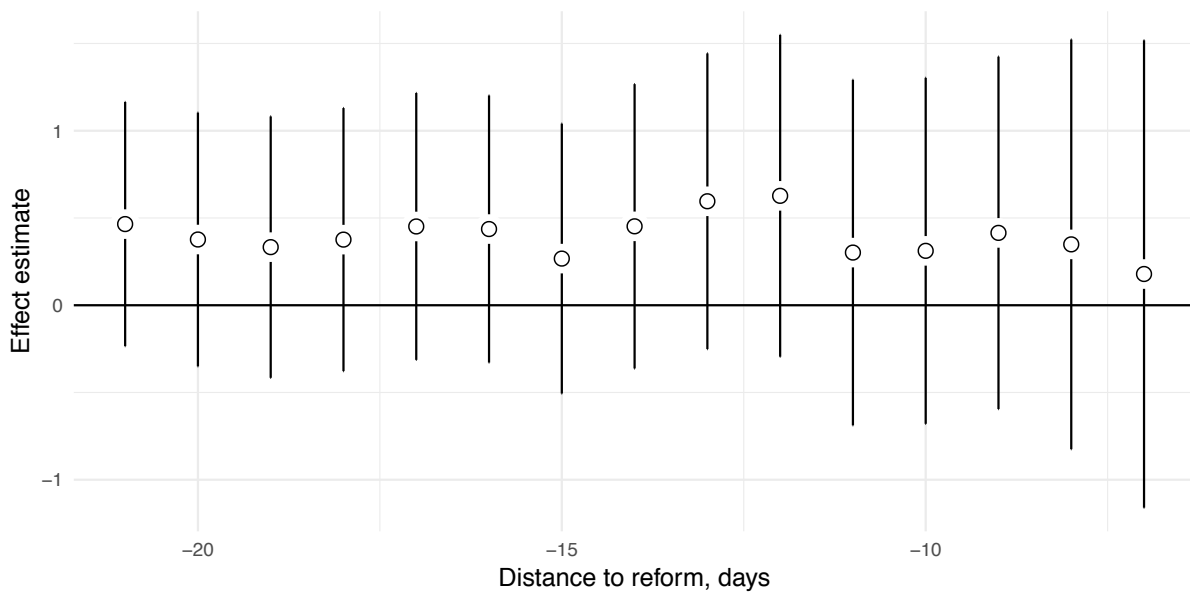


J. Results: Announcement of reform

Although there was no evidence in the media coverage that the reform was presented in detail prior to February 19, we examine the possibility that people reacted to the reform prior to the presentation. In order to do this, we estimate differences in government support creating groups using the respondents interviewed prior to the reform. The first group consists of people interviewed within the week (seven days) before the reform was presented which we compare to the remainder of the people interviewed prior to the reform was presented. The procedure is then repeated using other days as announcement dates, resulting in a series of estimates using groups within one week (seven days) and three weeks (21 days) prior to the reform.

Figure J.1 shows the effect estimates of the 15 estimated models. The results show two interesting aspects. First, we find no significant effects in any of the models, substantiating that there were no announcement effects in the period prior to the presentation of the reform. Second, all estimates, while non-significant, are positive, and thus in the opposite direction of what should be expected, had there been an announcement effect.

Figure J.1: Welfare retrenchment and government satisfaction, announcement effects



K. Results: Proximity models

In the main text, the models provided evidence on four different types of proximity. One of the models tested whether being proximate to family most likely to receive the state education grant reacted more strongly to the reform. In Table K.1, we test an additional type of family proximity, namely prospective proximity, i.e. whether the respondent was living with kids below the age threshold of the state education grant, and as a result, likely to receive the state education grant at some point in the future. The results show that prospective family proximity did not condition the impact of the reform.

Table K.1: Welfare retrenchment and government satisfaction, prospective family proximity

	(1)	(2)	(3)
Reform	-0.23 (0.16)	-0.23 (0.15)	-0.23 (0.15)
Prospective family	0.19 (0.19)	0.13 (0.19)	0.09 (0.20)
Reform×Prospective family	-0.20 (0.27)	-0.16 (0.27)	-0.17 (0.27)
Male		0.01 (0.13)	0.01 (0.13)
Age		0.01 (0.004)	0.002 (0.01)
Subjective class		0.24*** (0.04)	0.24*** (0.04)
Pol. interest		0.03 (0.09)	0.03 (0.09)
Pol. news		-0.07 (0.06)	-0.07 (0.06)
Religiosity		0.09*** (0.02)	0.09*** (0.02)
Ideology		-0.19*** (0.03)	-0.19*** (0.03)
Education			-0.26 (0.22)
Constant	4.86*** (0.11)	3.75*** (0.40)	3.97*** (0.45)
Observations	1,186	1,186	1,186
R ²	0.01	0.08	0.08

Note: Unstandardized regression coefficients with standard errors in parentheses. The dependent variable is government satisfaction (0 to 10), with greater values indicating greater levels of satisfaction with the government. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In the main text, it was also examined whether socioeconomic characteristics conditioned the impact of the reform. More specifically, a model tested whether having been doing paid work within the last

seven days mattered for the impact of the reform. Here, we expand this analysis in two ways. First, by including other types of characteristics (being unemployed and doing housework). Second, by running a full model with all characteristics. Table K.2 show the different models and provide evidence that the other characteristics did not shape the response to the reform, and that the direct proximity mattered across all models.

Table K.2: Welfare retrenchment and government satisfaction, socioeconomic proximities

	(1)	(2)	(3)	(4)
Reform	-0.42* (0.23)	-0.11 (0.14)	-0.10 (0.17)	-0.27 (0.26)
Education	-0.08 (0.30)	0.15 (0.27)	0.11 (0.27)	0.002 (0.32)
Reform×Education	-0.58* (0.35)	-0.76** (0.33)	-0.74** (0.33)	-0.65* (0.35)
Paid work	-0.32 (0.20)			-0.25 (0.22)
Reform×Paid work	0.39 (0.27)			0.31 (0.29)
Unemployed		0.58 (0.37)		0.40 (0.41)
Reform×Unemployed		-0.74 (0.55)		-0.55 (0.58)
Housework			0.06 (0.18)	0.09 (0.18)
Reform×Housework			-0.16 (0.26)	-0.20 (0.26)
Male	-0.003 (0.13)	0.01 (0.13)	-0.01 (0.13)	0.002 (0.13)
Age	-0.0002 (0.01)	0.002 (0.005)	0.002 (0.005)	0.001 (0.01)
Subjective class	0.25*** (0.04)	0.24*** (0.04)	0.24*** (0.04)	0.25*** (0.04)
Pol. interest	0.04 (0.09)	0.04 (0.09)	0.04 (0.09)	0.04 (0.09)
Pol. news	-0.07 (0.06)	-0.07 (0.06)	-0.07 (0.06)	-0.07 (0.06)
Religiosity	0.09*** (0.02)	0.09*** (0.02)	0.09*** (0.02)	0.09*** (0.02)
Ideology	-0.19*** (0.03)	-0.19*** (0.03)	-0.19*** (0.03)	-0.19*** (0.03)
Constant	4.19*** (0.46)	3.82*** (0.44)	3.89*** (0.43)	4.04*** (0.51)
Observations	1,186	1,186	1,186	1,186
R ²	0.09	0.09	0.09	0.09

Note: Unstandardized regression coefficients with standard errors in parentheses. The dependent variable is government satisfaction (0 to 10), with greater values indicating greater levels of satisfaction with the government. * p < 0.1, ** p < 0.05, *** p < 0.01.

L. Results: Standardized regression coefficients

Table L.1: Welfare retrenchment and government satisfaction, OLS regression

	(1)	(2)	(3)	(4)
Reform	-0.07 (0.13)	-0.06 (0.12)	-0.04 (0.14)	-0.04 (0.14)
Education			0.02 (0.24)	0.07 (0.27)
Reform×Education			-0.09 (0.34)	-0.09 (0.33)
Age		0.03 (0.004)		0.03 (0.005)
Education level		0.15 (0.04)		0.15 (0.04)
Subjective class		0.14 (0.04)		0.14 (0.04)
Pol. interest		-0.04 (0.09)		-0.03 (0.09)
Pol. news		-0.02 (0.06)		-0.02 (0.05)
Religiosity		0.12 (0.02)		0.12 (0.02)
Ideology		-0.18 (0.03)		-0.18 (0.03)
Constant	0.00 (0.09)	0.00 (0.39)	0.00 (0.10)	0.00 (0.43)
Observations	1,186	1,186	1,186	1,186
R ²	0.004	0.10	0.01	0.10

Note: Standardized regression coefficients with standard errors in parentheses. The dependent variable is government satisfaction (0 to 10), with greater values indicating greater levels of satisfaction with the government. See Table 1 in the main text for the statistical tests (i.e. p-value asterisks).

M. Results: Matching on education status

An important concern is that it is not education status that explain the change in government support as a result of the reform, but factors which correlate with education status. More specifically, the results might be caused by the endogenous exposure to education in particular with regard to political preferences and support. Thus, we want to ensure that education status is conditionally exogenous.

This aspect was tested empirically in two different ways. First, by interacting education status with all the covariates in explaining government support. This is a direct way to test whether the relevance of education status is conditional upon the covariates in explaining government support. Second, by applying the propensity score matching procedure on education status. More specifically, to further make education status (the sectional dimension) a function of the covariates and interactions between the covariates and reform exposure (the longitudinal dimension).

Figure M.1: Balance between unmatched and matched sample, education status

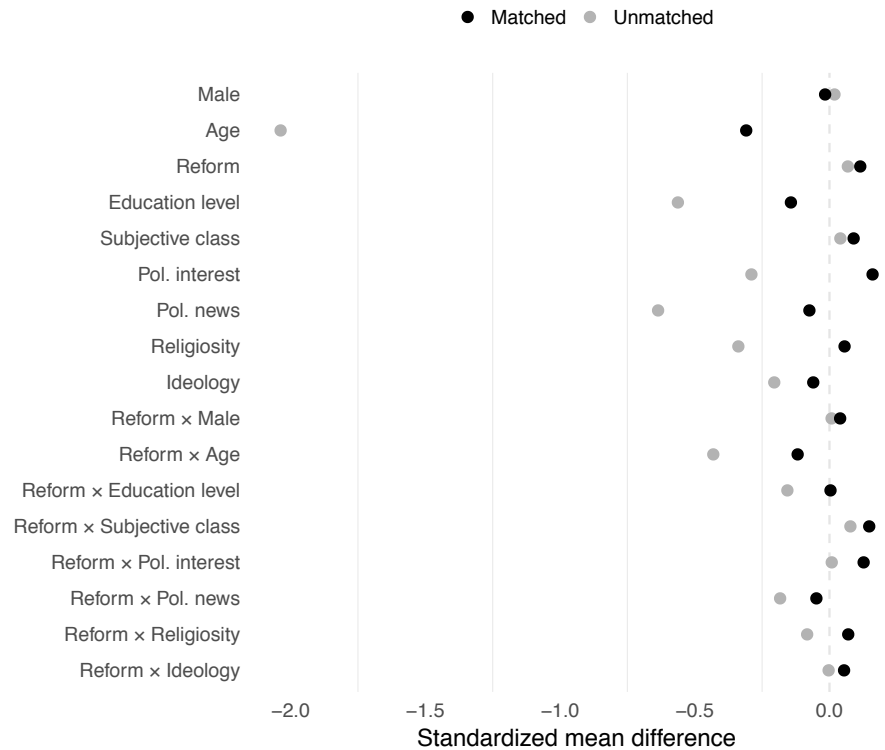


Figure M.1 shows the balance before and after matching on education status. Importantly, there is no difference in reform exposure. While there are some differences in the overall level of education and political news consumption, the greatest difference between people undergoing education and not is age. Not surprisingly, people undergoing education are substantially younger than the general public. Overall, the matching procedure provides comparable groups while also reducing the sample. Accordingly, after the matching procedure, we have 230 respondents.

Table M.1: Welfare retrenchment and government satisfaction, education matched sample, OLS

	(1)	(2)	(3)	(4)
Reform	-0.16 (0.14)	0.31 (0.45)	0.44 (0.44)	0.48 (0.44)
Education	3.12*** (1.17)	0.87** (0.39)	0.84** (0.39)	2.88 (1.87)
Reform×Education	-0.71** (0.33)	-1.56** (0.62)	-1.51** (0.61)	-1.60** (0.62)
Male	0.19 (0.15)		0.23 (0.32)	0.54 (0.47)
Age	0.01 (0.01)		0.02 (0.02)	0.03 (0.02)
Education level	0.19*** (0.04)		0.12 (0.10)	0.16 (0.16)
Subjective class	0.24*** (0.05)		0.08 (0.09)	0.19 (0.13)
Pol. interest	-0.09 (0.10)		-0.16 (0.21)	-0.25 (0.28)
Pol. news	-0.01 (0.06)		0.02 (0.14)	0.05 (0.20)
Religiosity	0.09*** (0.03)		0.07 (0.06)	-0.01 (0.10)
Ideology	-0.18*** (0.03)		-0.25*** (0.06)	-0.27*** (0.10)
Education×Male	-0.44 (0.34)			-0.63 (0.66)
Education×Age	-0.02 (0.03)			-0.03 (0.03)
Education×Education level	-0.02 (0.13)			-0.07 (0.21)
Education×Subjective class	-0.22** (0.11)			-0.27 (0.19)
Education×Pol. interest	-0.15 (0.25)			0.19 (0.43)
Education×Pol. news	-0.21 (0.16)			-0.06 (0.29)
Education×Religiosity	0.02 (0.06)			0.12 (0.13)
Education×Ideology	0.04 (0.07)			0.04 (0.13)
Constant	3.07*** (0.46)	4.43*** (0.27)	4.21*** (0.91)	3.49*** (1.18)
Observations	1,186	230	230	230
R ²	0.11	0.04	0.13	0.16

Note: Unstandardized regression coefficients with standard errors in parentheses. The dependent variable is government satisfaction (0 to 10), with greater values indicating greater levels of satisfaction with the government. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table M.1 shows the results. In the first model, we let education status interact with all the covariates in explaining government support. In the second model, we test whether people similar to each other with the exception of education status reacted differently to the reform. Again, we find that people undergoing education were substantially and significantly less supportive of the government as a result of the reform. The third model includes the covariates and the fourth model includes the covariates and the covariate-education interactions. In the models, people undergoing education are still substantially and significantly less supportive of the government.

Overall, this suggest that it is education status and not other factors, e.g. age differences between groups, that account for the response to the reform.

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