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# Conditional Punishment: A Comparative Analysis of the Electoral Consequences of Welfare State Retrenchment in OECD Nations, 1980–2003

KLAUS ARMINGEON and NATHALIE GIGER

*Do incumbent parties that retrench the welfare state lose votes during the next election? That is the guiding question for our paper. We analyse elections and social policy reforms in 18 established OECD democracies from 1980 to 2003. We show that there is no strong and systematic punishment for governments which cut back welfare state entitlements. The likelihood of losing votes is the same for governments that retrench the welfare state as for those that do not. Rather, electoral punishment is conditional on whether governments have the chance to stretch retrenchment over a longer period of time, and whether social policy cuts are made an issue in the electoral campaign. If other political parties and the mass media do not put the theme on the agenda of the campaign, and if the retrenchment can be carried out in small steps during a longer governmental term, governments may considerably reduce welfare state effort without fear of major electoral consequences.*

The welfare state has created its own battalions. In European democracies, large numbers of voters strongly support welfare state expenditure and an active role for the state in providing social security. They do so because they highly value social security, because they already receive transfers and services, or because they have contributed to the welfare state by social security contributions and taxes and expect to get something back in return. Therefore, welfare state retrenchment is risky in electoral terms. For many politicians and journalists this is taken as a truism. Its apparent validity is also reflected in the theories of political business cycles. The basic assumption of this literature is that politicians use social and fiscal policy to ensure re-election. If social policy benefits most of the voters, voters tend to punish

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governing parties at elections if these parties cut back on the social policy effort (Alesina *et al.* 1997; Persson and Tabellini 2000; Rogoff 1990). While social policy retrenchment may hurt the interests of incumbent parties as vote-seekers in elections, parties may opt for cutting back the welfare state due to their role as policy-seekers. Because of external constraints such as the Maastricht criteria of the EU, or a strategic calculation, it may pay off to reduce the size of certain, unpopular social policies. Governments aiming at retrenchment may reduce political costs by strategies of obfuscation, division of the potential political opposition, or by strategies of compensation. Welfare state retrenchment becomes the art of blame avoidance for any political actor. In contrast, welfare state expansion may be conceived as the result of functional requirements of modern societies and political power distributions (Pierson 1994). Other authors have suggested that political opportunities for retrenchment may depend on the constellations of political parties and the coalitions of parties that reduce welfare state effort (Green-Pedersen 2002; Kitschelt 2001).

Welfare state containment was already an important political issue in most democracies during the 1980s. But since the 1990s, the issue has become even more prominent, since the demographic challenges to and contradictions of the existing welfare state structure have become obvious. At the same time, the manoeuvring room of governments has shrunk. The dominance of neo-liberal ideas, the invisible fist of globalised markets, and the rules of the Maastricht treaty with its caps on deficits and debts (in force since 1993), all favour substantial reduction in state intervention. With this as a backdrop, some governments have retrenched the welfare state (Allan and Scruggs 2004; Korpi and Palme 2003).

What happened to these governing parties during the following election? This is the guiding question of this paper. We argue that a general punishment for retrenchment of entitlements did not take place. Rather, the response has been conditional on whether governments could distribute major cuts over a longer period of time, and whether the retrenchment entered the agenda of the electoral campaign.

The first point addresses whether governments had the opportunity either to distribute cuts over a longer period, or to compensate for the unfavourable electoral effect of social policy reform in one year by offering more favourable policies in other fields in the other years. This opportunity is only available to governments who hold power for longer periods of time. In contrast, governments with a short term of office cannot stretch out major retrenchment, nor can they compensate for the electorally unfavourable effects of social policy-making in one year with attractive and popular policies in the following years. There is ample evidence that such 'reform stretching' occurred by limiting the extent of retrenchment of entitlements, and, in particular, by dealing gently with pension systems and their large clienteles (Scruggs 2005). But even a reduced risk is still a risk. One would still expect incumbent vote losses after social policy cuts, since we have

strong evidence that a large share of citizens strongly support the welfare state (Arts and Gelissen 2001; Gelissen 2000; Roller 1995; Svallfors 1997). However, we find little evidence of related electoral failures.

Under what conditions, then, has retrenchment of entitlements led to the electoral punishment of governing parties, and when did voters support parties that cut citizens' transfer income? We show that in addition to the unavailability of the stretching opportunity, a crucial precondition for punishment was the elevation of this issue to the top of the electoral campaign agenda by a combination of opposition parties, interest groups, and the mass media. This process of making the issue extremely salient happened rather infrequently though. Voters were alerted only when the dogs barked. And in most instances, the dogs did not bark.

In a nutshell, we argue that electoral punishment for welfare state retrenchment is likely to occur if a short-term government reduces welfare state expenditure considerably or if a (long-term) government introduces major cutbacks during its term and these enter the agenda during the next electoral campaign.

Our basic theoretical orientation is that with regard to welfare state policies, voters have a utilitarian orientation and tend to lack information. Information is provided by the discourse between conflicting elites and by the mass media. The availability of information is also dependent on the transparency of the political decision-making process, which is shaped by the politico-institutional context (Powell and Whitten 1993).

Empirically, we base our analysis on data from 18 established democracies of the OECD country group in the period 1980–2003.<sup>1</sup> We restricted the analysis to these 18 countries since a stable democracy with a long tradition of party competition over social and economic policies is a major precondition for our argument. In addition, comparable data on welfare schemes are available only for these nations. We selected the period 1980–2003 due to data availability and due to the changing discourse on the welfare state during that time. This change, from broad support to a much more critical stance, was already felt in the 1980s and became very strong in most nations during the 1990s (Allan and Scruggs 2004; Huber and Stephens 2001; Korpi and Palme 2003; Schmidt 2002a, 2002b).

We provide the evidence for our argument in three steps. Section two discusses the research design. In the third section we test the simple model that electoral losses are a function of welfare state retrenchment. We show that changes in average replacement rates are not strongly correlated with votes for governing parties, even if other variables are controlled for. In section four we provide evidence for our guiding hypothesis of conditional punishment. We show that retrenchment is likely to lead to a loss of votes if the opportunity for reform stretching is not available. More important, however, is a second condition: if reform stretching is possible, electoral punishment occurs only if the issue has been made salient in the electoral campaign – and that does not happen very frequently.

## **Research Design**

We start from the assumption that voters reach electoral decisions from a retrospective position. They evaluate the last term of a party in government in order to decide whether to continue to vote for that party (see e.g. Fiorina 1981). Our dependent variable is the change in the vote share of the incumbent parties (party). This is the sum of the differences in the vote share for every governing party ( $p1 \dots pn$ ) at the parliamentary election at time  $t$  minus the vote share at the previous ( $t - 1$ ) election, i.e.  $\Delta \text{ vote share}_t = \Sigma (\text{vote}_{t, p1 \dots pn} - \text{vote}_{t-1, p1 \dots pn})$ .<sup>2</sup> We only consider governments which were in power for at least 12 months, as there is little reason to assume that shorter term governments are really responsible for the welfare state changes that happened during their few months of holding office.

Our major independent variable is the extent of welfare state reform, for which we use two operationalisations: the change of social policy entitlements – i.e. the cash benefits received by the clientele of the welfare state – and the change of social security expenditures. We will argue that on several counts the benefits are a much better measure of welfare state change than overall expenditure.

The change rate of benefits is calculated as the difference between the level of social security schemes entitlement net of taxes at election  $t$ , minus the level of entitlement at the time when the incumbent parties took office.<sup>3</sup> The underlying assumption is that the government has actually either caused the reforms, or will be held responsible by the voters for the changes regardless of responsibility. In calculating these change rates, we relied on a database produced by Lyle Scruggs covering welfare state entitlements in the 18 countries under consideration (Scruggs 2005). It contains data for net average replacement rates for four schemes: minimum pension (i.e. the pension for a person with no work history), standard pension (i.e. the pension for an employee with an uninterrupted employment history), unemployment, and sickness. Data is given both for a single person and for a couple with a single earner and two dependent children (or no children in the case of pensioners). We averaged the rates for both household types. We then calculated the difference between the average replacement rates at the time of the election and the rates at the time when the governing parties took office. As a summary measure, we calculated the sum of all differences over the four schemes.

This composite index has certain advantages. If we were to focus only on pension reforms, there would be two problems. One could argue that pension reforms are frequently phased in over a long period so that it will be hard for citizens to identify and punish the government that introduced this measure because the decisions were made a long time ago. There may be many cases in which this particular government may have already left office for quite some time. The second argument is that pension reforms are empirically rare events. In contrast, reforms of unemployment insurance

and sickness pay happen more frequently and in general take effect shortly after the decision has been made. Therefore, we are on much safer ground if we do not concentrate on one particular field of social policy, but rather cover various schemes.

Figure 1 depicts the change in entitlements by governments and by time in the period 1980–2003. The entries are the size of welfare state reforms brought about by a given government. For example ‘SWE 98’ denotes the Swedish governing party (Social Democracy Party) that stood for election on 20 September 1998. This government had reduced the average replacement rate for sickness by 2.3 percentage points, for unemployment by 9 percentage points, and for minimal and standard pensions by 2.8 and 2.5 percentage points respectively. This adds up to a decline of 16.6 percentage points (or, as indicated here, to  $-.166$ ).

Figure 1 confirms the findings of retrenchment in entitlements, particularly since the end of the 1990s (Allan and Scruggs 2004; Hicks and Zorn 2005; Korpi and Palme 2003). The distribution of the cases in Figure 1 shows that there was a huge degree of variation between governments. Considering entitlements, the governments in Norway, Italy, Netherlands, and New Zealand expanded the welfare state even around the

FIGURE 1  
CHANGE IN TOTAL REPLACEMENT RATES (FOUR SCHEMES) BY GOVERNMENT,  
1980–2003

year 2000,<sup>4</sup> while at the same time in Switzerland, Germany, and France the welfare state was cut back. It is notable that retrenchment of entitlements has been limited. If we arbitrarily set a cut-off point for a major retrenchment at 5 percentage points, this happened only 10 times in 99 cases of governments in the field of sickness, 12 times in the field of unemployment, once in schemes providing minimum pensions, and three times in schemes providing standard pensions. Across all four schemes governments achieved an accumulated reduction of  $4 * 5$  percentage points, i.e. 20 percentage points, in only three out of 99 cases.

A second indicator is the development of social security spending (percentage of GDP). It is considered to be a good indicator of the size of the welfare state, and hence it is very relevant in electoral campaigns. It is, however, not an exact indicator of welfare state effort or redistribution, as it is also driven by developments outside the immediate reach of politics. For example, all other things being equal, an increasing share of citizens beyond retirement age or a rise in unemployment causes an increase in social security expenditure without any additional political decision, since unemployment benefits or pensions have to be paid to a larger number of recipients. Hence, there may be cases where social security expenditures grow despite there having been massive cuts in pensions or unemployment benefits – albeit not to an extent that offsets the expansionary effects of labour market crises and the ageing of society on public expenditures (Esping-Andersen 1990; Green-Pedersen 2004; Siegel 2002, 2005). Finally, the same amount of social security spending has different effects depending on the extent to which these benefits are taxed, i.e. to what extent there is claw-back by the state (Adema and Ladaïque 2005). As in the case for replacement rates, we calculated the change rate of social security expenditure as the difference between the expenditures at the time of the election and at the time when the governing parties took office. Since social security spending includes pensions, and since the size of spending on pensions depends on the share of elderly, we also calculated social security spending net of expenditures for pensions. This did not alter the results.

For this reason, it comes as no surprise that change in social security expenditure is not significantly correlated with changes of entitlements ( $r = .14$ ). In addition to the ‘automatic’ effects of a changing number of welfare state recipients under the condition of invariant benefits per case, another reason for this statistical independence of both indicators is the different effects of a given percentage change in various welfare state schemes on the aggregate level of spending: a one percentage point reduction in pension replacement rates has a much larger impact on spending than the same reduction in schemes that cover a much smaller group of citizens, such as unemployment insurance. In addition, the development of the level of spending is also driven by demographic factors and by the growth rates of GDP.



So far, these measures do not take into account the opportunity to stretch welfare state retrenchment over a longer period of time. Therefore, we standardised welfare policy change – measured both as entitlements and as aggregate spending – by the length of the governmental term, i.e. the change per year of government. This variable was calculated as change in entitlements over the term divided by the duration of the governmental term, i.e. the number of years between the two consecutive elections.<sup>5</sup> If ‘stretching’ works, welfare state cuts standardised by length of term should be correlated more strongly with electoral losses. An example may clarify this argument. Imagine two governments that both cut entitlements by 5 per cent. Government A is in office for one year and it loses 10 per cent of its previous vote share. Government B is in office for five years and stretches these cuts over the five years. It loses 1 per cent of its previous vote share. Government C does not retrench and does not lose votes. If we compare the unstandardised amount of cuts with vote losses, there is little correlation. The correlation becomes much stronger, however, if cuts are standardised by the length of the term of office. This standardisation is superfluous if the amount of retrenchment is a function of the length of government. We did not, however, find a significant correlation between these two variables.

We include these independent variables in a standard model of electoral change, such as the one which has been developed in the research on economic voting (see Alvarez and Nagler 1998; Dorusson and Taylor 2002; Lewis-Beck 2006; Lewis-Beck and Paldam 2000; Nannestad and Paldam 1994; Powell and Whitten 1993). This is motivated by the need to control for other effects on the electoral fate of governments. According to these analyses, popularity of the parties is a function of the previous vote, of economic circumstances such as growth and unemployment, of aspects of the clarity of responsibility of the governing parties for policy outputs and outcomes, and of alternatives open to voters.

A first variable of this standard model is the size of the previous vote share. The likelihood of losing votes increases with the magnitude of previous votes (Powell and Whitten 1993).

Another set of variables concerns the state of the economy, such as economic growth and unemployment, as voters are assumed to blame governing parties for poor economic performance (Anderson 2000; Lewis-Beck and Paldam 2000; Nadeau *et al.* 2002; Nannestad and Paldam 1994).

Aspects of clarity of responsibilities are measured in different ways. Our ‘index of clarity of responsibility’ is ‘0’ if either the government is a minority government or if there are three or more governing parties. Otherwise it is ‘1’. The underlying reason for this is the dependence of minority governments on changing majorities in the parliament, which often makes it very unclear which of the governing parties or which of the supporting parties in parliament are responsible for policies. Likewise, coalition governments with many parties make it difficult to assign responsibility to



single-member parties. An alternative – and only weakly correlated – indicator of clarity is an index developed by Powell and Whitten (1993: 406). It is based on the cohesiveness of political parties, the sharing of committee chairs by the opposition, the control of the policy-making institutions by the opposition, the existence of a minority government, and the number of competing parties in the governing coalition. It runs from '0' (New Zealand) to '3' (Switzerland). The Powell–Whitten indicator is time-invariant, while the clarity indicator may vary by time. In this analysis, which combines longitudinal and cross-sectional dimensions, we consider the time-variant clarity-indicator superior.<sup>6</sup>

Finally, the likelihood of withdrawing support depends on alternatives. If there are many other parties, voters have a choice. However, one could argue that a large number of parties indicate the existence of several socio-cultural cleavages, which restrict the options available to voters embedded in their socio-cultural milieu. In a similar vein, Anderson (2000: 155) claims that 'fragmented party systems – identified by a large effective number of parties – should make it more difficult for voters to identify a clear alternative to the incumbent government'. Accordingly, we integrate a control for the effective number of parties as measured by the Laakso/Taagepera index. Our index of clarity, the Powell–Whitten index, and the effective numbers of parties are correlated, but these correlations are well beyond the level of multicollinearity.

The core of our argument is the notion that retrenchment leads to electoral punishment, conditional on whether the welfare state is a major theme in the electoral campaign. We developed two operationalisations for the saliency of the welfare state in a campaign. First, we selected from our pool of 99 cases of change of entitlements those cases where the overall retrenchment was at a minimum 5 per cent. In our sample, 29 cases fulfilled this condition. For these cases, we re-analysed the reports on electoral campaigns<sup>7</sup> in order to identify whether welfare state issues were highly prominent in the campaign and whether the intensity of the campaign was high.

In the best case we have two electoral reports from the experts at our disposal. We have to bear in mind that their reports reflect varying qualifications and normative orientations. Accordingly, the focus of the evaluation of the situation may differ and make it harder to distinguish whether the welfare state was a major issue or not. But we can still state that these are approved national experts who know the electoral and the media contexts very well, and consequently are able to make reasonable judgements. Of course, it would be preferable to expand our preliminary analysis by a study of the campaign's media coverage in order to complete and objectify our image of the specific electoral context. Unfortunately, we lack the data for this step, and furthermore the added value of such a fine-grained analysis remains unclear. The analyses based on this indicator are presented in Table 3.

Secondly, we established an indicator which gives an idea of the importance of the welfare state issue in the campaign. Party Manifesto Data (Budge *et al.* 2001) of all the significant parties<sup>8</sup> have been aggregated into a single dummy measure. The idea behind this is that in instances where all major parties elevated the topic of welfare into a prominent position, the chances are high that the welfare issue was also considered highly important by the media. The measure in this case is of course also only a proxy, as the link between party programmes and media portrayal is far from being confirmed. Our indicator though is conservative enough<sup>9</sup> to depict a tendency. This indicator of welfare state saliency in the campaign is used in all regression models since it covers all cases in the dataset (not only 29 as above) and presents a balanced assessment of the saliency based on sound and solid data.

We built our data base from these sources: for the dependent variable, the change in the vote share of the incumbent parties, we used Woldendorp *et al.* (2000) and various issues of the *Political Data Yearbook* (*European Journal of Political Research*). Scruggs (2005) and OECD (OECD 2005) data is the source for the calculation for the extent of welfare state reform. Budge *et al.* (2001) and the reports on recent elections in *Electoral Studies* and in the *Political Data Book* of the *European Journal of Political Research* served as sources for the saliency in the campaign analysis. The data for other political and economic variables<sup>10</sup> are taken from Armingeon *et al.* (2005) and Huber *et al.* (2004).

Our basic technique is to regress the dependent variable (change in vote share for the incumbent party/parties) on the retrenchment and campaign indicators, controlling for the other standard variables which electoral research identifies as reasons for vote change between two consecutive elections. Since we have clusters defined by nations, we calculated standard errors, which are controlled for the clustering of the data. There is some evidence that the political logics of welfare state retrenchment are different from the logics of welfare state expansion. Therefore, the file was split into cases of welfare state growth and reduction.

### **Are Governing Parties Punished if they Retrench the Welfare State?**

What are the electoral consequences of welfare state retrenchment? According to the standard view of politicians and political scientists, these governments will experience a loss of votes at the next election. This is clearly not supported by the cross-tabulation of cases of welfare state retrenchment of entitlements and vote gains or losses reproduced in Table 1.

Roughly every second government cut back entitlements, and about seven out of ten governments lost votes. However, the share of governments that lost votes is about the same in the group of expansive (74 per cent) as in the group of retrenching governments (70 per cent).

TABLE 1  
CROSS-TABULATION OF VOTE LOSS AND CUTBACKS IN SOCIAL POLICY, 99  
GOVERNMENTS WHICH STOOD FOR ELECTION, 1980–2003

		Was there a cut of total replacement rates?		
		No	Yes	Total
Did incumbent parties lose votes?	Yes	12 26.1%	16 30.2%	28 28.3%
	No	34 73.9%	37 69.8%	71 71.7%
	Total	46 100%	53 100%	99 100%

*Note:* Vote loss is any negative change of incumbent vote shares compared to the previous election.

This is confirmed by the OLS regression analyses reproduced in Table 2. In order to account for the clustering of the data (countries), the standard errors are clustered. Multicollinearity did not occur in the regression analyses presented in Tables 2 and 4.<sup>11</sup>

The major independent variable of the first four models is the change of entitlements, and of the last three models the change of social security expenditure. The most obvious finding is that changes in social security expenditures (percentage of GDP) do not correlate with electoral fortunes in any model specification (models 5 to 7). The coefficients are in the expected direction (the more/the less spending the more/the less votes), but are far from significant.<sup>12</sup> This confirms the hypothesis that voters do not react to changes in social security spending as a share of GDP; since this measure is also strongly influenced by changes to the denominator (GDP), since it is not correlated with changes of what the citizens get in terms of cash benefits net of taxes (such as in the case of entitlements), and since it carries little information for the ordinary citizens. Even well informed and sophisticated voters may have problems recalling or developing a clear overview of the changes to social security spending over recent years. We could not find significant coefficients for standardised and non-standardised social security expenditures in any further regression analyses. Therefore, we do not present the regression statistics for this variable in the ensuing documentation of the analyses. Instead, we focus on change of entitlements.

The findings of Table 2 show that there is a different electoral logic of welfare state change in periods of expansion and retrenchment. Welfare state policy and electoral success do not seem to be correlated (model 1). During expansion, an increase of entitlements does not lead to larger vote shares (model 2). In contrast, there is a weak correlation between the decrease of welfare state effort and the loss of votes (model 3). Excluding all variables of model 3 with  $p < .20$  (i.e.  $p < .10$  one-tailed) leads to a simple model (model 4). A decrease of welfare state entitlements is correlated – albeit only on the 10 per cent level – with a decrease of vote share. Two of

TABLE 2  
OLS-REGRESSION ESTIMATES FOR CHANGE OF VOTES FOR GOVERNING PARTIES, 1980–2003

	Model 1 (change)	Model 2 (expan.)	Model 3 (retrench.)	Model 4 (retrench.)	Model 5 (change)	Model 6 (expan.)	Model 7 (retrench.)
Change in total replacement rates	4.36 (4.92)	−0.16 (6.67)	24.53* (13.24)	22.94* (11.80)			
Change in social expenditure					0.23 (0.52)	0.94 (1.41)	0.04 (0.68)
Vote share of government	−0.09 (0.09)	−0.18 (0.13)	−0.05 (0.07)		−0.12 (0.08)	−0.19 (0.13)	−0.08 (0.08)
Growth rate	0.23 (0.23)	0.39 (0.36)	−0.03 (0.24)		0.32 (0.26)	0.46 (0.43)	0.21 (0.26)
Unemployment rate	−0.61* (0.31)	−0.04 (0.72)	−0.66** (0.28)	−0.66** (0.27)	−0.75 (0.46)	−0.61 (1.13)	−0.93** (0.40)
Effective number of parties	−0.71 (0.45)	−0.77 (0.63)	−0.93** (0.29)	−0.86** (0.20)	−0.75* (0.42)	−0.59 (0.66)	−1.05** (0.34)
Powell/Whitten index of clarity	1.73** (0.44)	1.66 (1.66)	1.47 (0.99)	1.45 (1.04)	1.69** (0.43)	1.40 (1.66)	1.86 (1.22)
Clarity of responsibility	1.03 (1.23)	3.43 (3.37)	−1.21 (1.86)		1.34 (1.35)	3.61 (3.64)	−0.56 (1.91)
Constant	0.22 (4.04)	2.95 (7.21)	3.23 (4.70)	−0.53 (2.23)	1.27 (4.01)	2.45 (6.74)	1.31 (5.02)
R <sup>2</sup>	0.13	0.15	0.34	0.32	0.15	0.16	0.27
N	99	46	53	53	92	43	49

\*p < 0.10; \*\*p < 0.05 (two-tailed).

Note: Estimates are unstandardised coefficients with robust standard errors in parentheses (using the option ‘cluster’ in Stata).

the remaining control variables have significant coefficients in the expected direction: The higher the unemployment, the lower the government's vote share. And in periods of retrenchment a large number of effective parties correlate with vote losses for the incumbent parties. Having a larger number of alternatives, citizens tend to vote for these other parties.

The most remarkable finding of the bivariate analysis of Table 1 and the analyses documented in Table 2 with control variables is the contradiction to the generally accepted assumption of many journalists, politicians and social scientists. According to that view, welfare state retrenchment is almost inevitably connected to electoral losses. The findings, however, significantly contradict this.

### **Welfare State Reform, the Art of Stretching, and the Avoidance of a Public Debate**

These findings come as a surprise. For example, the politics of recent pension reforms in Germany strongly suggest that social policy-making is driven by electoral concerns. The same applies to the most recent German election (2005) (Korte 2005). Likewise, there are clear utilitarian concerns among the welfare state clientele (Kitschelt and Rehm 2006). Experiences with the Swiss direct democracy show that even in this non-statist country, citizens tend to reject proposals for retrenchment if they do not come with compensations or are not obfuscated by further measures (Bonoli 2000).

We argue that the explanation for this puzzle lies in the way welfare state retrenchment is conducted and within the role of the electoral campaign. If retrenchment can be managed gently over a longer period of time and if this is not a major theme of the electoral campaign, the reform may be overlooked by citizens or considered to be of less importance when making decisions at the ballot box. Citizens are unlikely to accurately identify small retrenchment of entitlements. They frequently lack the necessary information and are unaware about the context of the reform.

Ordinary voters are only able to link retrenchment of entitlements to their electoral decision if social policy becomes a major and contested issue in the electoral campaign. Priming theory states that voters evaluate parties/candidates only on those issues which are on the 'top of their mind' at the precise moment of the decision. Party/candidate evaluation then is not the result of a complete information and integration process across all policy fields (Iyengar and Kinder 1987; Krosnick and Brannon 1993; Krosnick and Kinder 1990). This means that if the attention of voters is not drawn to social security, or if there are other issues that trump welfare state concerns, welfare state reforms will not be taken into consideration for the actual vote decision, and consequently incremental retrenchment will not lead to loss of votes.

The electoral campaigns in Germany in 2002 and 2005 are good examples of this. In 2002 the question of the German position with regard to the

imminent war in Iraq and the efficient actions and generous help of the government after the flooding in the east of Germany were arguably more important in the electoral campaign than the cutback of welfare state entitlements. In direct contrast are the massive protests of the trade unions against welfare state retrenchment in combination with the media coverage of these conflicts in 2005, which ensured that the topic was on the top of the campaign agenda. In addition, the accusations by the CDU (Christian Democratic Union) that the SPD (Social Democrats, incumbent party) had failed to bring down unemployment and create sound fiscal and tax policies further highlighted the welfare issue (Brettschneider 2005; Brettschneider and Rettich 2005; Falter 2005).

We found evidence for the crucial role of agenda-setting and priming effects in an analysis of electoral campaigns. This analysis was done in a twofold design. The first strategy was a simple bivariate procedure on the basis of the secondary analyses of expert reports on electoral reforms (Table 3).

The underlying assumption of our analysis is that a highly conflictual campaign, centred on welfare state reform (retrenchment) issues, is a sufficient condition for an electoral loss (= more than 5 per cent loss of votes for the incumbent parties). It is not a necessary condition though. Governing parties may lose votes for other reasons, such as fielding weak candidates, failures in other fields – for example foreign policy – or shrinking numbers of voters with strong party affiliation. Accordingly, in a 2 × 2 cross-tabulation, we do not expect cases where the condition (welfare

TABLE 3  
RE-ANALYSIS OF ELECTORAL CAMPAIGN IN CASE OF RETRENCHMENT (MORE THAN 5% CUT BACK)

		Was welfare state (retrenchment) an important issue in the campaign?	
		No	Yes
Did incumbent parties lose votes? (more than 5%)	Yes	IRE 87	NZ 90
		BEL 91	SWE 91
		AUL 96	NZ 93
		IRE 97	NET 94
		AUL 98	SWE 94
		GER 98	NZ 99
		FRA 02	SWE 98
	No	CH 83	UK 92
		UK 83	DK 98
		USA 84	CH 99
		JAP 86	FIN 99
		DK 87	USA 00
		UK 87	AUS 02
		USA 88	GER 02
		CH 91	IRE 02

*Note:* For details on the re-analysis of the reports on electoral campaigns, see section 2 and the appendix.

state most prominent issue) was present, but the outcome (governmental loss of votes) was not.

We do find some instances where welfare state retrenchment led to a loss of votes by the governing parties (party), giving evidence for a conditional link between electoral punishment and the prominence of welfare state issues in the campaign. More often, though, the dogs did not bark: welfare state reform was not the most prominent issue in the campaign, and for this reason there was also no punishment on election day. The cases in the upper-left hand corner, where the incumbent parties did lose votes, but not because of a debate on welfare state issues; they do not falsify our theory. They do call for another explanation though. Vote losses may be caused by other reasons alone. In the Appendix we list the major issues of each electoral campaign in order to give more information about other possible reasons for the result of the election.

Campaigns are not the only conditional factor for electoral punishment. The availability of the stretching option is the other major explanatory factor. Table 4 displays the results of regression analyses based on

TABLE 4  
OLS-REGRESSION ESTIMATES FOR CHANGE OF VOTES FOR GOVERNING  
PARTIES INCLUDING CONDITIONAL EFFECTS, 1980–2003, ONLY CASES WITH  
CUT BACKS OF SOCIAL POLICY

	Model 1 (full)	Model 2 (reduced)	Model 3 (interaction)	Model 4 (reduced interaction)
Change in total replacement rates (standardised by governing years)	76.87** (27.23)	65.57** (25.04)	41.28* (22.74)	32.03 (25.57)
Vote share of government	−0.07 (0.07)		−0.07 (0.07)	
Growth rate	−0.07 (0.23)		−0.03 (0.25)	
Unemployment rate	−0.63** (0.28)	−0.64** (0.24)	−0.58** (0.26)	−0.57** (0.21)
Effective number of parties	−1.01** (0.31)	−0.89** (0.22)	−1.21** (0.34)	−0.97** (0.25)
Powell/Whitten Index of clarity	1.25 (0.84)	1.30 (0.91)	1.09 (0.77)	1.40 (0.85)
Clarity of responsibility	−1.62 (1.87)		−3.03 (1.82)	
Campaign over welfare state issues			1.08 (1.86)	1.67 (1.72)
Interaction between change in total replacement rates and campaign			104.13* (56.91)	88.41* (49.06)
Constant	4.95 (4.70)	−0.15 (2.10)	6.74 (4.68)	−0.53 (2.10)
R <sup>2</sup>	0.35	0.31	0.43	0.36
N	53	53	53	53

\* $p < 0.10$ ; \*\* $p < 0.05$  (two-tailed).

Note: Estimates are unstandardised coefficients with robust standard errors in parentheses (using the option 'cluster' in Stata).



retrenchments per year of government. Based on our finding of the previous sections we focus only on the cases where retrenchment occurred. Model 1 contains all control variables, model 2 only those of model 1 whose coefficients have  $p < .20$ . The clear result is that the larger the average annual cutbacks of entitlements, the greater the electoral losses. In other words, governments may be able to afford even major welfare state retrenchment if they can stretch it over many years. In contrast, if a government cuts back entitlements considerably during, for example, its first two years of office-holding, the likelihood of major vote loss is considerable if it has to, for whatever reason, face an election after this short time in office.

What happens if the electoral campaign, as the second factor of conditional punishment, is entered into the regression model? Based on the dummy variable of whether the welfare state was salient in the electoral campaign (Party Manifesto data), we calculated an interaction term with the annual average of reduction of entitlements. Model 3 of Table 4 displays the results of the estimation of the model with all variables; model 4 contains only the variables whose coefficients had a  $p < .20$ .<sup>13</sup>

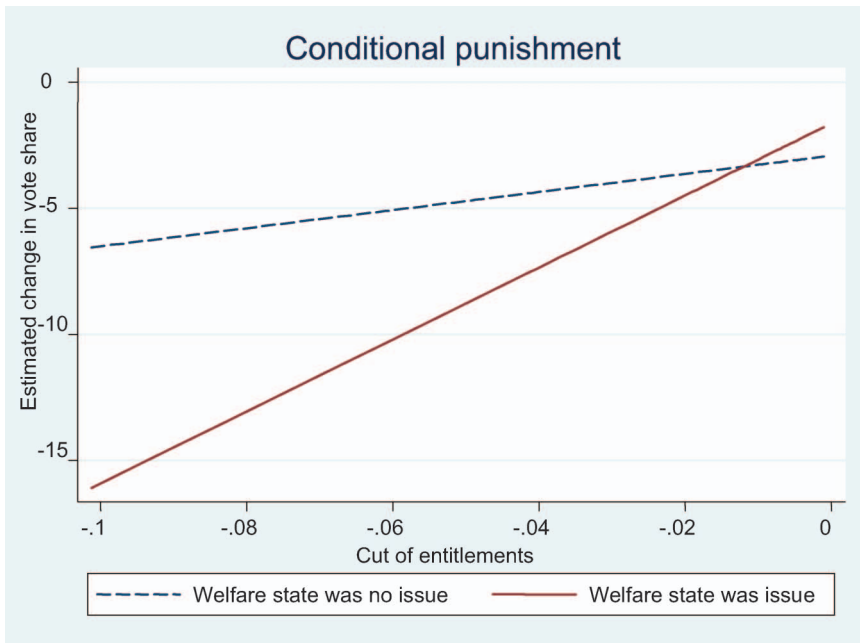
Table 4, models 3 and 4, reveals that welfare state change has a significant influence on the vote share of the governing parties, given that the topic was relevant in the campaign. It is this interaction term, rather than the sheer amount of annual reduction or the existence of a campaign, that is an important explanatory variable. If we consider only the governments with a tenure of at least two years, the significance of the coefficient of this interaction term increases to  $p = .033$ , supporting our main argument that the opportunity for stretching and the absence of a public debate are the major conditions under which punishment for retrenchment is avoided.

However, we cannot detect any influence of social policy change when the issue was not brought up in the electoral campaign. These findings were also confirmed with a calculation of interaction terms.

Figure 2 visualises these statistical findings. It shows the slope of the regression model for two different contexts: when the welfare state was an issue in the electoral campaign, and for situations where the welfare state was not an important issue. All other variables in the model are kept on their average position.

If there is no public debate, the relation between retrenchment and vote losses is indicated by the upper line. The slope of this line is much less steep than that for the case of public debate. The graph demonstrates that there is smaller loss of votes when benefits have been reduced but retrenchment was not an issue in the campaign. In contrast, when the welfare state was an issue, the same cut is associated with a much stronger loss of votes for the government. In addition, the upper slope (welfare state was not an issue) is not significantly different from zero while the lower slope (welfare state was an issue) is statistically significantly different.

FIGURE 2  
VISUALISATION OF THE INTERACTION TERM IN TABLE 4



The control variables of the full and reduced models produce coefficients that are in the expected direction: in periods of welfare state retrenchment, increases in unemployment and the existence of electoral alternatives are the major variables that explain whether and to what extent governments are punished on election day.

## Conclusion

Do governing parties that retrench the welfare state lose votes during the following election? This was the guiding question of our paper. We showed that in general there was no strong electoral punishment in the period 1980–2003 in 18 established democracies of the OECD country group.

Why did retrenchment of entitlements not lead to electoral punishment? We advanced two arguments. (a) In general, retrenchment has been designed so that it minimised the danger of electoral repercussion. Massive cuts have been confined to small segments of the electorate. This was possible if the option of stretching welfare state reforms existed. In that case governments could take the electoral risk of cuts if they could distribute these cuts over many years or – much less frequently – if they introduced a major cut in one year and did not harm the interests of the welfare state

recipients in the following years. (b) In order to become a major cause of loss of votes, retrenchment has to be a major issue in the electoral campaign. In many cases though, opposition parties, media, and interest groups did not consider this issue very important. Hence the dogs that could have alerted the electorate did not bark.

This article provides strong empirical hints that policy advisers and journalists are wrong if they see welfare state retrenchment as the first step to electoral failure. Rather, governments may take the risk of retrenchment if they do it gently in small annual steps over a longer period of time and if they are able to ensure that competing parties and the mass media will not put the theme on the agenda of the electoral campaign.

Our main argument rests on the assumption of retrospective voting, and on the assumption that voters generally hold incumbent parties responsible for reforms, regardless of their actual degree of responsibility. This is a problematic assumption though. Voters may be able to effectively trace present reforms back to governments that have long before left office. They may also evaluate competing parties not so much for what they have done, but rather for what they claim they will be able to do in the future (prospective voters). It is an ongoing debate in electoral studies whether voters rely more on retrospective or prospective judgements (see e.g. MacKuen *et al.* 1996, 1992; Norpoth 1996). From a theoretical point of view, it is more reasonable to expect voters to rely on retrospective evaluation, especially in the case of government performance, due to information availability and reliability (Fiorina 1981; Popkin 1991; Popkin *et al.* 1976). Most governments in Western democracies are coalition governments, and therefore voters need to judge with whom their preferred party will build a government. In addition, voters have to know the party programmes and need to have information about what these governments intend to do in the field of social policy reform. And finally they must be able to extrapolate the difference between party programmes and political practice of the past into the future. In total, the expectations placed on the average voters are high, and it is more reasonable to assume that they rely on the more easily accessible information about government performance as it is portrayed by the media.

Being agnostic, though, we also tested an alternative operationalisation with prospective voters: we checked whether governmental parties are punished for future actions (in the next legislature).<sup>14</sup> We did not find any evidence for this, further solidifying our assumption that retrospective voting is more appropriate for the purposes of our study.

For us, one of the most interesting aspects of this paper is the important role of the agenda setting and priming effects of the media in the electoral context. Our admittedly rather crude measures of issue saliency in the electoral campaign and policy reform point to the importance of the social construction of policy reform, even in fields where citizens should be able to make a rational decision simply by looking at their social security payments.

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## Notes

1. Australia (AUL), Austria (AUS), Belgium (BEL), Canada (CAN), Denmark (DK), Finland (FIN), France (FRA), Germany (GER), Ireland (IRE), Italy (ITA), Japan (JAP), Netherlands (NET), New Zealand (NZ), Norway (NOR), Sweden (SWE), Switzerland (SWI), UK, USA.
2. Our argument is based on parliamentary systems. For the USA we apply the procedure by Powell and Whitten (1993: 393). They measure the performance of the presidential party in the House of Representatives over the last four years in order to allow comparison with other parliamentary systems.
3. To account for the fact that a new administration needs some time before effectively taking control, we only include the indicator for the year of the election in cases where the election took place in the second half of the year. Otherwise, the calculation is based on the indicator of the year previous to the election under consideration.
4. We have to state, however, that New Zealand experienced a phase of retrenchment in the 1990s.
5. The results without standardisation do not differ substantially from the results presented in this paper. Standardising the change of entitlements by the total length of office - i.e. the number of years which elapsed between the election under study and the date when a particular government took office for the first time - would account for the 'costs of ruling'. We found some slightly significant results (on the 10 per cent level) for this variable in various model estimations, however, we do not focus on the costs-of-ruling problem in this paper. Rather, we are interested in what happened between two consecutive elections and, due to the number of cases in the models, we have to keep the models as parsimonious as possible.
6. We also tried to make the Powell-Whitten indicator time-variant by adding elements of an index as suggested by Nadeau et al. (2002). These elements are ideological cohesion (is government composed of parties with different ideologies in terms of left-centre-right), number of governing parties (low vs. high), and effective number of parliamentary parties. Another element of the Nadeau et al. index is duration of participation in government and this is already an independent variable. While this new indicator produced coefficients in the expected direction, the results were insignificant. Therefore we stuck with the simple, time-invariant Powell-Whitten indicator, which is connected with significant coefficients in the expected direction.
7. We used the 'Notes on Recent Elections' section of *Electoral Studies* plus the European Data Yearbook by the *European Journal of Political Research* (since 1991). For the USA Dinkin (1989) was also used, as was *Année Politique Suisse* for Switzerland.
8. The categories p.503 (social justice), p.504 (welfare state expansion) and p.505 (welfare state retrenchment) have been taken into account. For more details concerning the categories, etc, see Budge et al. (2001).
9. There are 56 categories in the manifesto programme; the entries in the data base are the number of (quasi-) sentences in each category standardised by taking the total number of

(quasi-) sentences in the documents as a basis. The mean average coverage of the three categories is 4.2 per cent. Our threshold is an average issue-coverage of 7 per cent for social justice and welfare state. This procedure leads to a dummy variable which is '1' if the value of the issue-coverage is substantially above the mean, indicating that the welfare state is an important issue. Otherwise it is '0'.

10. These are the variables 'majority/minority government'; 'number of parties in government'; 'effective number of parties'; 'economic indicators'.
11. Variance Inflation Factors (VIF) always stayed below 2.
12. The elections during 1980-2003 are not a random sample. However, we use tests of significance to take account of non-systematic errors in data collection and as a heuristic means for evaluation of the robustness of the model estimation. We do not consider any coefficient that does not meet the criteria of  $p < .10$  in a one-tailed test. We speak of a robust finding if  $p < .025$  in case of a one-tailed test.
13. Since 'campaign' is a main effect of the interaction term we also kept this variable in the reduced model.
14. This analysis was conducted in a similar manner to the ones shown previously. We used the same operationalisations for the dependent and independent variables. While we still used the change in the vote share of the government between time  $t$  and  $(t - 1)$ , the size of welfare state change was now measured between time  $t$  and  $(t + 1)$ .

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APPENDIX  
ANALYSIS OF CAMPAIGNS FOR ELECTORAL CONTEXTS WITH MORE  
THAN 5% RETRENCHMENT

Nr.	Date	Country	Issues in the campaign	Importance WS	Intensity of the campaign	Electoral punishment
1	23.10.83	Switzerland	environment; social security; unemployment	1	Medium	no
2	09.06.83	United Kingdom	nuclear disarmament; unemployment; social spending	1	Aggressive	no
3	06.07.86	Japan	taxes/fiscal policy; confidence in politicians; social security	1	Medium	no
4	08.09.87	Denmark	economics; social security; immigration	1	Medium (very short)	no
5	17.02.87	Ireland	economic crisis; social security; unemployment	1.5	Medium	yes
6	11.06.87	United Kingdom	foreign policy; social security (housing and health); negative campaigning	1	Medium	no
7	08.11.88	USA	Personal campaign against Dukakis and focusing on his qualities as a governor	0	Low	no
8	27.10.90	New Zealand	economics; social security (health); education	2	Low	yes
9	24.11.91	Belgium	Flemish/Walloon conflict; immigration and racism; values/abortion; social security	1	Medium	yes
10	20.10.91	Switzerland	immigration; Europe; pension; tax on gas	1	Low	no
11	15.09.91	Sweden	deregulation; social security; pension; immigration	2	High	yes
12	09.04.92	United Kingdom	tax policy; Health system	1	Medium	no
13	06.11.93	New Zealand	social security; economy	2	Medium	yes
14	03.05.94	Netherlands	pension, unemployment; social security; immigration	2	High	yes

(continued)

APPENDIX  
(Continued)

Nr.	Date	Country	Issues in the campaign	Importance WS	Intensity of the campaign	Electoral punishment
15	18.09.94	Sweden	economy; health care; family policy	2	Medium	yes
16	02.03.96	Australia	environment; race and immigration; status as republic; electoral system	0	Medium	yes
17	06.06.97	Ireland	Crime; tax for top end; Northern Ireland; economy; social security	0.5	Low	yes
18	03.10.98	Australia	taxation (VAT), immigration; aborigines	0	Low	yes
19	11.03.98	Denmark	immigration; housing policy	0.5	Medium	no
20	27.09.98	Germany	unemployment, change in general	0.5	High	yes
21	20.09.98	Sweden	social security; unemployment	2	Medium	yes
22	24.10.99	Switzerland	unemployment; immigration; social security	1	Medium	no
23	21.03.99	Finland	unemployment; taxes; public spending	1	Low	no
24	27.11.99	New Zealand	economic growth; social security (employment; health; education; housing)	2	Medium	yes
25	07.11.00	USA	economy/jobs; taxes; education; social security (health)	1	Medium	no
26	24.11.02	Austria	taxation; Europe; social security (health; scholarships)	1	High	no
27	16.06.02	France	security; cohabitation; presidential elections	1	Low	yes
28	22.09.02	Germany	unemployment; economy; social protection	1	Medium	no
29	17.05.02	Ireland	well doing economy; crime, social security (health)	1	Medium	no

Importance of social security 0 (no importance) – 2 (high importance).