Do Voters Blame Governments for Social Spending Cuts?
Evidence from a Natural Experiment

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Abstract

The idea that citizens reward or punish incumbents for past performance at election time is one of the central tenets of modern conceptions of representative democracy. This rests on a core assumption that voters are rational and self-interested actors who have the ability to link the outcomes of government policies to their evaluation of performance and in turn to vote choice. Yet, only few studies have explicitly tested this assumption. Existing work often fails to disentangle the complex causal relationships between policy outcomes, performance evaluations, and incumbent support. This study utilizes actual social spending cuts in child care in the Netherlands from 1995 as a natural experiment to examine the effect of policies on vote choice. We thus circumvent the endogenous relationship between policy evaluations and incumbent support. What is more, our case selection provides a conservative test of the reward-punishment model of voting model as Dutch politics is characterized by power-sharing institutions clouding responsibility for policy outcomes. Our results suggest that citizens who are financially adversely affected by the spending cuts are less likely to approve of the government. However, we also find that these effects are moderated by political sophistication.

Key Words: Accountability, government policy, natural experiment, reward-punishment model, pocketbook voting, political sophistication.

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**Introduction**

Voters are meant to care about policies that affect them directly and punish governments if they implement policies that adversely affect them. Notions such as ‘political business cycles’ and ‘pork barrel politics’ rest on the assumption that voters will respond positively to governments stimulating the economy and that constituents will favor politicians that ‘bring home the bacon’, but punish those who do not deliver the goods (see e.g. Mayhew 1974; Cox and McCubbins 1986; Stein and Bickers 1994; Alesina and Rosenthal 1995; Brender and Drazen 2008). The idea that citizens reward or punish incumbents for past performance at election time is a central tenet of representative democracy and one of the most studied subjects within political science. In particular, there is a large literature on the impact of the economy in elections, which has shown that when economic conditions are improving, incumbents are more likely to be reelected (see Kramer 1971; Lewis-Beck 1988; Nannestad and Paldam 1991; Lewis-Beck and Stegmaier 2000; Duch and Stevenson 2008). Nevertheless, there is uncertainty as to the extent to which citizens actually realize and respond to specific policy changes. According to some studies, citizens consider national economic conditions rather than individual financial circumstances. Scholars have suggested that policies of governments are dimly understood by citizens and that pocketbook-based concerns are of minimal importance to opinions and vote choices, which are instead shaped by general predispositions (Kinder and Kieweit 1979, 1981; Feldman 1982; Sears 1993; Soss and Schram 2007). This raises the important question of when do government policies matter? Specifically we ask: do government policies that adversely affect citizens’ financial self-interest shape their opinions of the government and, in turn, their voting behavior? The answer to this question has important implications for our understanding of opinion formation on the one hand and strategic behavior of politicians on the other.

Surprisingly, only few studies have examined the effect of specific policies on political attitudes and vote choice, and most have relied on the effect of policy preferences rather than actual measures of policies (see e.g. Alvarez and Nagler 1995, 1998; Lewis-Beck and Paldam 2000; Mughan and Lacy 2002; recent exceptions include Healy and Malhotra 2010; De la Calle and Orriols 2010; Erikson and Stoker 2011; Bechtel and Hainmueller 2011). Yet, such studies make it difficult to disentangle the potentially endogenous relationship between policy preferences, partisanship and vote choice. To study the effect of specific policies on citizens’ attitudes and behavior, we therefore make use of a natural experiment. Natural experimentation has a long
tradition in program evaluation in psychology and economics (see for example Campbell 1969: Angrist and Krueger, 2001), but is also increasingly employed within the study of political participation (Lassen 2005: Krasno and Green 2008), political attitude formation (Erikson and Stoker 2011) or electoral behavior (Carman, et al. 2008: Healy and Malhotra 2010: Bechtel and Hainmueller 2011). The virtue of a natural experiment is that it utilizes a specific event, like a policy change in child benefits in the Netherlands in 1995 in our case, that "happens to affect some subjects but not others", and where "the naturally occurring [policy] intervention was assigned as-if at random" (Sekhon and Titiunik 2012: 1, italics in original, see also Dunning 2008). Social spending cuts in Dutch child benefits represents such an exogenous policy change that was, as-if randomly, assigned to parents with children within specific age brackets, and we can thus use this treatment to determine whether the adversely affected individuals changed their attitudes and behavior towards the government accordingly. In many ways, this case provides us with a conservative test of policy voting, since it had an effect on household income, but not a transformative effect on livelihood (see Healy and Malhotra 2010: Erikson and Stoker 2011: Margalit 2011). Moreover, Dutch politics is characterized by power-sharing institutions and a grand coalition with left- and right-wing parties, which cloud responsibility for policy outcomes.

Our expectation is that ‘policy voting’ does occur: that public spending cuts influence the attitudes of affected voters, making them more critical of the incumbent government and less likely to vote for incumbent parties. However, we also expect significant heterogeneity in policy voting across citizens. Following the literature on political sophistication, we hypothesize that political sophisticates will be more responsive to policy changes, as they are able to make the associative linkage between government policy and changes in their personal financial status, and better able to divide blame and credit across multiple coalition partners (Gomez and Wilson 2001, 2006).

This study proceeds as follows. First, we briefly review existing work on the effect of policies on attitudes and voting and present our hypotheses. We thereafter discuss the advantages to using a natural experiment design to make causal inferences, and describe the particular details of the case of policy reform in the Netherlands. Analyzing data from the Dutch sociological survey from 1996, we find considerable support for our hypotheses. Finally, we consider the implications of these findings.
Policy Effects on Attitudes and Behavior

There is plenty of anecdotal evidence and academic work suggesting that politicians use public budgets strategically to keep their constituents happy (Mayhew 1974; Shespe and Weingast 1981; Lindbeck and Weibull 1987; Cox and McCubbins 1986; Levitt and Snyder 1997). This basic model of strategic politicians using public spending to win votes rests on the assumption that voters also behave in a self-interested manner and reward governments when they benefit from public transfers, whereas they punish them when public spending cuts affect them. 'Pork barrel politics' where incumbents gain electoral advantages by distributing private goods only works if citizens vote with their pocketbooks. Yet, remarkably little work has been dedicated to testing this assumption. That is, to what extent do voters respond to changes in public transfers by altering their evaluations of politicians and their vote choice?

Elections are generally regarded as a sanctioning mechanism where voters can decide on whether to elect the incumbent on the basis of past performance (Key 1966; Kramer 1971; Fearon 1996). Empirically, it has been shown that voters are more likely to reelect incumbents when economic conditions are improving and more likely to punish them when conditions are worsening (Fiorina 1981; Lewis-Beck 1988). Performance voting in elections is particularly pronounced in systems with concentrated power and high clarity of responsibility (Powell and Whitten 1993; Nadau et al. 2002; Duch and Stevenson 2008). Yet, while there is little doubt that changing economic conditions affect vote choices, there is less consensus about whether voters respond to changes in specific government policies that affect them financially, or whether they are primarily responding to their perceptions of general economic conditions. Most of the studies that have examined the effect of public policy on vote choice have studied the impact of policy preferences rather than actual policy changes (see e.g. Fiorina 1981; Kinder and Kiewiet 1979; Alvarez and Nagler 1995; Mughan and Lacy 2002; Lewis-Beck et al. 2008). This raises the issue of endogeneity, since policy preferences and evaluations could be shaped by vote choice. In recent years, several studies have argued that both economic evaluations and policy preferences are shaped by vote choice, rather than vice versa (Wilcox and Wlezien 1993; Wlezien, Franklin, and Twiggs 1997; Evans and Andersen 2006; Evans and Pickup 2010). If policy preferences and economic evaluations are partly endogenous to vote choice, this casts doubt on whether there is a strong causal effect of policies, such as cuts in social transfers, on voter opinions and
vote choice. Some aggregate-level work has demonstrated that increases in public spending helps incumbents to win voters (see Levitt and Snyder 1997). But this still leaves open the question of whether voters respond to policy changes that affect them personally in the way that most rational models of strategic politicians would assume. On the one hand, there is evidence suggesting that self-interested concerns are of minimal importance to opinions and vote choices, which are instead shaped by general predispositions (Kinder and Kieweit 1979, 1981; Sears 1993; Soss and Schram 2007). On the other hand, recent studies have suggested that policy changes can have significant effect on attitudes and voting behavior, in a disparate cases such as the Vietnam Draft Lottery in the US (Erikson and Stoker 2011), flood responses in Germany (Bechtel and Hainmueller 2011), and investment in urban public transport in Spain (De La Calle and Orriols 2010).

Building on this recent work concerning the causal relationship between policy and voter behavior, this study examines the effect of changes in core area of public spending, namely social transfers, on political attitudes and behavior. To overcome problems of endogeneity in survey-based studies of performance voting, we make use of a natural experiment, where a policy change in child benefits affected the social transfers to some voters (those with children within specific age brackets), but not others. Using this case, we can examine whether voters affected by the policy reform responded to the change, and whether this response was moderated by political sophistication.

Following the vast rational choice literature on party competition and vote choices, our baseline assumption is that voters are rational and self-interested. This implies that "each citizen casts his vote for the party he believes will provide him with more benefits than any other" (Downs 1957, 36; see also Kramer 1971). This also implies that voters will respond to policy changes that directly affect their economic situation. However, the literature has so far been inconclusive about the extent to which individuals take into account their personal financial situation when deciding which party to vote for. Studies on economic voting have been divided between those arguing that people vote according to changes in their own personal circumstances, pocketbook-voting (Campbell et al. 1960: Kramer 1971, 1983), and those arguing that voters are 'sociotropic' and consider the overall state of the national economy (Kinder and Kiewiet 1970: Fiorina 1981: Alvarez and Nagler 1995). Typically, sociotropic variables have emerged as stronger predictors of vote choice than individual economic circumstances, but that does not necessarily imply that voters are not self-interested, or that policies
that affect them personally are irrelevant. Indeed, it has been argued that pocketbook voting may only emerge when the policy effect and the attribution of responsibility is clear (Feldman 1982; Gomez and Wilson 2001): that is when voters can identify both the effect of specific government policies on their income (or general well-being) and can identify who is responsible for these changes.

Hence, before rejecting the notion of pocketbook voting, we need to test whether voters respond to policies that clearly affect their financial circumstances. If the pocketbook model of performance voting works, citizens who are affected by a policy change should first relate this to their own income prospects (i.e. citizens who are adversely affected by a cut in social spending should report a negative change in their personal economic prospects). Thereafter, affected voters should change their evaluation of the government (i.e. they blame the government for any deterioration in their economic prospects). Finally, this would then translate into incumbent support (i.e. they would be less likely to vote for the government).

This mechanism of ‘policy voting’ can be tested in a natural experimental set-up, where the policy change is exogenous to voters’ predispositions, with the following hypotheses:

**H1a:** Citizens who are adversely affected by a government reform to social spending are more likely to report that their economic prospects have deteriorated since the policy change took effect.

**H1b:** Citizens who are adversely affected by a government reform to social spending are more likely to be dissatisfied with the performance of the government.

**H1c:** Citizens who are adversely affected by a government reform to social spending are less inclined to vote for an incumbent party.

Yet, this simple mechanism of pocketbook or policy voting treats all voters as the same and does not consider important heterogeneity in voting behavior. Several recent studies have suggested heterogeneity in economic or performance voting, depending for example on political sophistication (Gomez and Wilson 2001, 2006; Duch 2001; De Vries and Giger 2012). Variation in voters’ levels of political sophistication is indeed well-documented, and recent work has also argued that this can have important implications
for the degree of economic voting. When it comes to the effect of political sophistication on pocketbook voting, the literature however has presented clearly contrasting expectations. On the one hand, studies focusing on information acquisition have argued that pocketbook voting requires minimal information: "[p]ocketbook politics requires little in the way of political expertise. Knowing who the incumbents are, where the polling place is located, and a few other details are all that is needed" (Kinder and Kiewiet 1981, 130). As a consequence, pocketbook voting should be more pronounced among less politically sophisticated voters (see Fiorina 1981; Delli Carpini and Keeter 1996; Duch 2001). On the other hand, Gomez and Wilson (2001, 2006) have put forward a theory of heterogeneous attribution, which posits that pocketbook voting is dependent on voters’ ability to make causal associations between policy outcomes and the actors politically responsible, which in turn hinges on their level of political sophistication. Consequently, only voters with a certain level of political sophistication will be able to make the associative linkages between personal economic circumstances and politicians. In contrast, "[h]igh sophisticates should be more able to make some associative linkages between their own economic situation and national political actors…and thus should be more likely than low sophisticates to ‘vote with their pocketbooks’" (Gomez and Wilson 2001, 903).

A crucial point in the heterogeneous attribution theory is that political sophistication enables voters to link their own economic situation to the actions of multiple governmental actors, whereas for low sophisticated voters attribution of responsibility is largely restricted to proximate causes, such as taking personal responsibility (see Feldman 1982; Gomez and Wilson 2001). In case of specific policy voting, the informational demands are even higher, since voters need to first identify a change in their personal circumstances as a result of the policy change and then attribute this to the relevant governmental actor. The demands are even greater when the government in question is a coalition government where there are multiple actors that could be held responsible. We therefore expect political sophistication to moderate the extent to which voters can divide blame (and credit) for policies among multiple government actors. This is highly relevant in parliamentary systems where most governments consist of a coalition of parties (Hobolt and Karp 2010). In such systems, responsibility is divided across multiple parties, which complicates the process of blame attribution and policy voting (Powell and Whitten 1993), and this suggest that only voters with some level of political sophistication are able to attribute blame for policies among multiple actors. This leads to our second hypothesis.
**H2:** Politically sophisticated citizens are more likely to punish the government for policies that adversely affect them. In cases where multiple governmental actors are responsible for a policy change, sophisticates are also more likely to attribute blame differently across parties.

This raises further questions of how political sophisticates differentiate between parties in a coalition government when attributing blame. They may, for instance, attribute more blame to larger parties, or parties that are responsible for the particular portfolio associated with the policy change. We also expect partisan differences in the effects of policy reforms on vote choice. In certain instances, such as the Dutch case examined in this study, attribution of responsibility is further complicated as citizens are faced also with a government that spans the ideological spectrum, i.e. includes both the left and the right, a so-called grand coalition. The literature on welfare retrenchment differs in its expectations concerning whether the left or the right will be punished more for cuts in social spending (see Giger and Nelson 2010 for an overview). One prominent expectation is that parties that 'own' the welfare issue, i.e. have a strong historical reputation for building and protection the welfare state and correcting social injustice, are less likely to be punished for welfare retrenchment policies (Ross 2000; Levy 1999). Hence, this so-called Nixon-goes-to-China argument suggests that "whereas rightist governments run the risk of being perceived as mean-spirited and socially negligent when they impose losses, the left may even be viewed as fiscally prudent and economically responsible" (Ross 2000, 165). Yet, empirical work testing this thesis has found limited effects of social policy cuts across the board, suggesting that voters may fail to place blame for cuts in the first place (Giger and Nelson 2010; Giger 2011). This lack of evidence for policy voting in response to welfare entrenchment could be because only sophisticated voters can make the associative linkages between policies, personal situation and blame attribution.

These propositions are tested in the ensuing section. First we outline the specific case of social spending cuts in the Netherlands and elaborate why this can be used to make causal claims about the effect of policies on attitudes and vote choice.
Dutch Child Care Reform of 1995 as a Natural Experiment

To test our hypotheses, we utilize a policy reform of child benefits in the Netherlands in 1995 as a natural experiment on policy voting. Before discussing the treatment and control groups, we provide some more background on this reform.

The Dutch state has provided families with children with financial benefits since the early post-war period. In 1962, a legal framework of generous universal child benefits was established for every child below the age of 18 regardless of household characteristics such as income, number or material status of parents (Algemene Kinderbijslagwet 1962; Van Daalen 2002; Tweede Kamer 1994). This law included two types of differentiation in the amount of benefits provided. First, the premium per child was designed in such a way that its overall level increased as a function of the number of children. In other words, bigger families received higher monetary benefits per child. Second, the premium system included an automatic increase when children reached certain age brackets coinciding with primary and high school enrolment. Children in the pre-school age bracket between the ages of 0 and 5 received 70 per cent of the premium, children in the primary school age bracket between 6 and 11 100 per cent, while households with children aged between 12 and 17 in the high school age bracket received 130 per cent of the benefits¹ (Van Daalen 2002: 304; Tweede Kamer 1994: 9).

The reform of these latter two elements of the Dutch child support system during the second half of 1994 is the core of our natural experiment.

The reforms were initiated by a new coalition government formed after the elections on 3 May 1994. This government included the Social Democrats (PvdA), the left-wing Liberals (D66), and the right-wing Conservative Liberals (VVD), thus spanning the ideological spectrum from left to right. This was a novelty in Dutch politics as it excluded the Christian Democrats (CDA) from government for the first time in the postwar period (Andeweg and Irwin, 2005). During the election campaign all major parties proposed cuts in social welfare provisions, but the reform of the child care system was not on the media or party agenda during the campaign (Hippe et al. 1995).

What is more, the PvdA (1994: 90) dedicated only one paragraph of their over 100 page long 1994 electoral manifesto to discussing possible changes in the legal framework of

¹ This feature ensured that the possible increased household costs due to schooling expenses, such as transportation or the purchase of school materials for example, were partially compensated for
child benefits, and the D66 and VDD manifestos included no specific references to child benefits (D66 1994; VVD 1994). As a result, the proposed policy change in child benefits laid out in the coalition accord of 22 August 1994 was unexpected, but it was proposed by the new coalition government and agreed by Parliament in November 1994. The fast-track legislative process meant it came into force already on 1 January 1995 (Van Praag et al. 1997).

The goals of the reform were twofold (Tweede Kamer 1994: 1-2). First, it served as a tool to reduce overall collective social spending and control government finances. Second, the government stressed that having children constituted a choice made by individuals. Consequently, the role of government in providing incentives to secure population enhancement through increased benefits for larger families should be reduced (Tweede Kamer 1994: 2). The two specific policy changes aimed at reducing the differentiation in the level of benefits due to family size and the age of children within the household: First, the reform standardized the premium per child irrespective of family size (henceforth standardization of child benefits), and second, it reduced the incremental increase of benefits to level out cost increases due to schooling by half, from 70·100·130 per cent increase for children in the 0·5/6·11/12·17 age brackets to 70·85·100 (henceforth leveling of child benefits). Research suggests that these policy changes led to a substantial decrease in the household income of families with children affected and it was therefore highly contentious with the public (Van Praag et al. 1997: 137-155; Van Daalen 2002).

To illustrate how it affected the individual families differently, imagine the following hypothetical example: At the time of the policy change, Emma and her friend Anna both had a pre-school child and a newborn baby, but while Emma’s second child was born on 1 January 1995, Anna’s was born one day earlier on 31 December 1994. Due to the child benefit reforms that came into effect on 1 January 1995, Anna thus received almost 40 per cent more child benefits for her newborn compared to Emma. Table 1 below shows the financial ramifications of the standardization of child benefits per number of children residing in the household.
Table 1: Overview of Financial Ramifications of Standardization of the Child Care Premium

<table>
<thead>
<tr>
<th>Number of Children</th>
<th>Child born before 01.01.1995</th>
<th>Child born after 01.01.1995</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>188.57</td>
<td>188.57</td>
</tr>
<tr>
<td>2</td>
<td>309.40</td>
<td>188.57</td>
</tr>
<tr>
<td>3</td>
<td>322.73</td>
<td>188.57</td>
</tr>
<tr>
<td>4</td>
<td>349.11</td>
<td>188.57</td>
</tr>
<tr>
<td>5</td>
<td>364.94</td>
<td>188.57</td>
</tr>
<tr>
<td>6</td>
<td>375.50</td>
<td>188.57</td>
</tr>
<tr>
<td>7</td>
<td>383.03</td>
<td>188.57</td>
</tr>
<tr>
<td>8</td>
<td>396.59</td>
<td>188.57</td>
</tr>
<tr>
<td>9</td>
<td>407.13</td>
<td>188.57</td>
</tr>
<tr>
<td>10</td>
<td>415.56</td>
<td>188.57</td>
</tr>
</tbody>
</table>

Notes: Table entries provide the level of the child benefits for children born before and after 1 January 1995 in Euros adjusted for inflation to reflect price levels in 2012. The calculations were provided by the Dutch Ministry for Social Affairs through the Sociale Verzekeringsbank, see http://www.svb.nl/int/nl/kinderbijslag/betaling/hoeveel_kinderbijslag_krijgt_u/.

Now imagine that both Emma and Anna have common friend, Sophie, who has two children born before January 1995. While Emma’s daughter will turn six on the 5 February 1995, Sophie’s daughter already reached that age in late December of 1994. Although both daughters will enroll in primary school, the small age difference has financial implications: while Sophie received an increase in child benefits of 30 per cent as her daughter turned six before 1 January 1995, Emma obtained only half of that increase as her second child turned six after the reform was implemented. Recall, that in addition to the standardization of child benefits, the reform also entailed a leveling of the premium, halving the automatic age bracket increase from a ratio of 70-100-130 per cent to 70-85-100. For Emma, the reforms of child benefit thus meant that she received 40 per cent less child support for her newborn compared to Anna, as well as 15 per cent cut in the premium for her second child compared to Sophie, simply due to the birth dates of her children.

Examining the effects of the 1995 childcare reform in the Netherlands not only provides us with a measure of policy affect that is exogeneous to citizens’ political preferences and behavior, it also is particularly well-suited to test the reward-punishment model of voting. Child support in the Netherlands is an apposite example of a universal state benefit. Unlike employment benefits which are targeted at a subset of the population, child benefits affect all households with children cutting across class and ideological lines.
Treatment and Control Groups

Both the standardization and leveling of child benefits introduced in the Netherlands on 1 January 1995 provides us with an unique opportunity to examine if citizens adversely affected by cuts in child care spending, like Emma in our example above, were more likely to change their attitudes and behavior towards the government compared to citizens that were not (yet) affected, like Anna and Sophie. This reform constitutes a natural experiment as the assignment of the treatment was haphazard and satisfies the condition of *as-if* randomness. The exogeneity of the treatment assignment is secured in two ways. First, with respect to the leveling of child benefits, parents with children falling into the age brackets affected by the reform could not self-select into the treatment as they cannot modify the age of their existing children. Second, relating to the standardization of child benefits, it is very unlikely that parents could have anticipated the reform and adjusted their child planning accordingly as only 4 months and 9 days elapsed before the government reform initiative, first announced in the coalition accord of 22 August 1994, became official law.\(^2\)

Given the fact that the policy reform fulfills the *as-if* randomness condition, we can distinguish three treatment groups as well as a control group that allow us to compare if these groups differ in terms of their economic and government evaluations. On the basis of a large-scale sociological survey from 1996, *Culturele Veranderingen*, which asks randomly selected respondents both about the number of children residing in their household as well as the respective ages of these children at the time of the fieldwork, we identify four groups of subjects on which we base our analyses:

1) **Treatment group 1**: Respondents with children eligible for child support that were affected by the standardization, i.e. who have a second, third etc. child aged 0 or 1 at the time the survey was conducted.
2) **Treatment group 2**: Respondents with children eligible for child support that were affected by the leveling of child benefits, i.e. who have a child aged 6 or 7, and/or 12 or 13 at the time the survey was conducted.
3) **Treatment group 3**: Respondents who have children eligible for child support who are neither affected by the standardization nor leveling of child care treatment, but might be confronted with the effects of leveling in the future since they have underage children, i.e. those respondents who have a second,

\(^2\) Although the theoretical possibility exists that those parents expecting a second, third etc. child could artificially induce labor to circumvent cuts in their child benefits this is a highly unlikely event due to the risks involved.
third etc. child aged older than 1, and/or a child not aged 6 or 7, and/or not 12 or 13 at the time the survey was conducted.

4) **Control group**: Respondents with children that are no longer eligible for child support, i.e. who have children 18 and older at the time the survey was conducted.

Overall, our sample includes 321 respondents in treatment group 1, 104 in treatment group 2, 459 in treatment group 3, while the control group is comprised of 233 respondents.  

A crucial element of natural experiment is concerns the precise demarcation and comparison of different treatment groups (Sekhon and Titiunik, 2012). First, ideally we would have been able to distinguish the respondents within different groups outlined above based on the exact birth date of their children, but unfortunately our survey data fails to provide this information. As a consequence, we rely on items included in the survey asking respondents about the age of their respective children at the time the field work of the survey was conducted, November 1996, to provide us with the relevant information (SCP 1997). Second, differentiating the three treatment groups outlined above provides a slightly conservative measurement of the treatment effects as we were not able to construct a fourth treatment group including subjects that were affected by both treatments and thus experienced the harshest financial ramifications for their household income possible (like Emma in our example). For these respondents we would expect the effects of the policy reform on economic and government evaluations to be most pronounced. Unfortunately, our sample merely includes merely nineteen respondents that were exposed to both treatments which is too small of a group size for statistical testing. Consequently, since these subjects experienced a roughly 65 per cent cut in child benefits (40 per cent due to standardization plus 15 per cent due to leveling) we grouped them together with the respondents that experienced the standardization treatment only.

Finally, since our main quantity of interest is the effect of experiencing a cut in child benefits on a respondent’s economic evaluations as well as attitudes and behavior towards the government we need to define the relevant control group for comparison. Borrowing the language from causal inference in statistics (Rubin 1974; Holland 1986), we are interested in the difference between the potential outcomes for the political

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3 We both compared the means and balance statistics of these groups in terms of a set of relevant political and socio-demographic variables. These results suggest that the treatment and control groups are indeed comparable (results of these analyses are available upon request from the authors).

4 This is not particularly surprising since in order to be treated twice respondents both had to have a second, third, etc. child aged 0 or 1 and one of more existing children at the age of 6, 7, 12 or 13 at the time the survey field work was conducted.
attitudes under treatment, i.e. Y1, and under control, i.e. Y0, for each respondent. Holland (1986) suggests that the "fundamental problem of causal inference" stems from the fact that we can never observe both potential outcomes for each respondent, i.e. (Y1 - Y0) for each respondent is an unknown quantity. Relating this to our specific case, for each individual respondent we cannot observe the counterfactual level of personal economic evaluations, government satisfaction or vote propensity for incumbent parties that she would have obtained in the absence of the reform of child benefits, and vice versa. In order to arrive at unbiased estimates of the treatment effects, we thus need to compare our treatment group to a relevant control group that is sufficiently similar to the treated group in all other relevant characteristics except for the treatment (see Sekhon and Tituunik 2012). As outlined above we have a control group consisting of respondents with children no longer eligible for child support. As a result, we restrict our analyses to respondents with children only in order to circumvent that the examination of the causal effect of treatment is diluted by the decision of having children. What is more, by solely including respondents with children we are sensitive to the fact that not the entire population was subjected to as-if randomization due to the reform of child benefits, but only those with children. If the treated respondents differ from this control group and if we find differences in the size of the treatment effects based on the amount with which child benefits were reduced between our three different groups, we increase our confidence that the degree to which citizens are adversely affected by cuts in child benefits indeed affects their economic and government evaluations.

**Variables and Estimation**

We are interested in the effect of treatment on three different outcome variables: personal economic evaluations, government satisfaction and the propensity to vote for an incumbent party. Our survey includes three questions that we use to operationalize these three different political attitudes. First, we gauge each respondent's future outlook on her personal income by using an item tapping into the expectation of her income situation in the future. The variable *income prospects* ranges from 1 "deteriorate" to 3 "improve". Second, we measure the degree to which a respondent is satisfied with the current coalition government of PvdA, D66 and VVD responsible for the reform in child benefits by creating an item labeled *government satisfaction*. It is based on a four-point scale asking respondents about the degree to which they are satisfied with the actions of
the current government where 1 indicates "very dissatisfied" and 4 "very satisfied". Finally, we operationalize the propensity of voting for the incumbent government by utilizing a vote intention question within the survey asking respondents which party they would vote for if elections were held today. We construct three different variables. First, the variable incumbent vote takes on a value of 1 if respondents intend to vote for one of the three parties currently in government, i.e. PvdA, D66 or VVD, and 0 for all other parties. Second, we constructed a measure leftwing incumbent vote for which 1 indicates that a respondent is likely to vote for one of the leftwing coalition partners, PvdA or D66, and 0 if a respondent intends to vote for another party. Finally, we created an item entitled rightwing incumbent vote which reflects the extent to which respondents will vote for the rightwing party in the coalition, the VVD, versus all other parties. The items leftwing and rightwing incumbent vote allow us to examine the possibility that treated respondents punish coalition partners differently. 

Hypothesis H2 highlights that the effect of our policy treatment variable might be moderated by a respondent’s level of political sophistication. We understand political sophistication as the store of political information about politics available to an individual to be called upon when making judgments or decisions (see for example Zaller 1992). When studying the moderating influence of political sophistication, scholars usually distinguish between voters’ subjective assessment of political interest and objective measures (Luskin 1987). Objective measures are often based on responses to specific knowledge questions which we unfortunately do not have at our disposal. We use a respondent’s level of education as a proxy for political sophistication (Zaller, 1992; Delli Carpini and Keeter 1996). The education variable ranges from 1 "primary school or specialized professional education only" to 4 "tertiary education".

Finally, we included several control variables. Although our treatment and control group assignment were based on as-if randomization we do control for potential confounders that could possibly cloud the causal effect of being affected by the child care

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5 Note that the vote intention question entails a lot of missing data points, in effect we lose about 300 respondents who did not report a vote intention (specifically 37 per cent of respondents in the treatment groups and 39 per cent in the control group). Due to the fact that we cannot be sure that these missing observations are randomly distributed, they could dilute our as-if randomness condition. Consequently, we used imputation to circumvent this issue. Due to the fact that imputing values on the dependent variable solely on the basis of the predictors employed in the model is potentially problematic, we included a wide range of variables that could affect voters’ ballot choices, such as religious denomination, education and an array of different policy attitudes relating to the environment, immigration and law and order. More information is available upon request from the authors.
reform on our three different outcome variables outlined above. First, since our design entails two treatments that affect children at a different age respondents with more children could have an increased probability of being treated. Consequently, we introduce the number of children residing in the household as a control. Second, since older respondents have a greater probability to also have children no longer eligible for child benefits, we add each respondent’s age as a control. Third, as our treatment groups 1 and 2 include parents with second, third etc. child born in 1995 or 1996, thus affected by the standardization treatment, and/or those of children aged six or seven or twelve or thirteen at the time of our survey, born in 1989, 1990, 1983 or 1984 and affected by the leveling treatment, there is the possibility of period effects that could affect the levels of our outcome variables. Since these years coincide with important events such as the fall of the Berlin Wall and the founding of the Dutch Green party, one could for example imagine that post-materialist values, like personal lifestyle choices, are particularly salient for these respondents (Inglehart 1990, Dalton 2006). In addition to controlling for socio-demographics – gender and household income · we therefore also include control variables tapping into post-materialist values like a respondent’s attitude towards abortion and an item capturing a respondent’s religiosity measured by church attendance.

In order to estimate the effect of treatment on our three outcome variables whilst controlling for moderators and possible confounders, we utilize multivariate regression models. In the case of income situation and government satisfaction, we use ordinary least squares (OLS) regression analysis. As both variables are not strictly continuous, but rather based on a 3- and 4-point ordinal scale we also employed an alternative estimation method based on an ordinal logistic regression model. These analyses yield substantively identical results compared to the OLS findings presented in the subsequent section. We opted for the presentation of OLS estimates as it eases interpretation. Our third outcome variable of interest, a respondent's vote intention for incumbent parties, is dichotomous in nature which rules out the use of OLS. For the models estimating the effect of treatment on incumbent vote as well as those in which we allow for the moderation of treatment by political sophistication, we rely on a logistic regression model.

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6 Results available upon request from the authors.
Results

Our expectations posit that voters who are adversely affected by a government reform, in our case cuts in child benefits, are more likely to hold negative views about their personal financial situation (H1a), be less satisfied with the government (H1b) and less inclined to support the incumbent parties (H1c). Table 2 presents the results of the empirical examination of the treatment effect for all three outcome variables. We provide three sets of results. In model 1 we examine the effects of being in the three different treatment groups on respondents' evaluations of their prospective household income situation, model 2 presents the treatment effects on government satisfaction and finally model 3 examines the degree to which electoral support for incumbent parties is affected by the different treatments. All models include several variables that control for possible confounders of the causal effect of treatment. The control group including respondents with children no longer eligible for child support serves as the reference category.

Regarding our first outcome variable, the evaluation of one's income situation, the results for model 1 suggest that parents of underage children affected by the standardization of child benefits and thus faced the largest reduction of child support (treatment group 1) are significantly more negative about their household income than parents who were not affected by the policy change (as their children were ineligible for child support) and that this treatment effect holds when we control for possible confounders. For respondents exposed to a reduction of child benefits due to standardization the satisfaction with their household income situation deteriorated with -.177 on a 3-point scale compared to respondents with children ineligible for child support. Respondents in treatment group 2 are also more pessimistic about their future household income situation, but the coefficient with an associated p-value of .054 is just borderline statistically significant. Although the coefficient for parents with underage children that may be affected by leveling in the future (treatment group 3 respectively) is also in the expected direction, it fails reach conventional levels of statistical significance (p=.094). This suggests that respondents in treatment group 3 are not significantly different from the baseline control group in terms of their income evaluations. Consequently, we find partial evidence for hypothesis 1a. Only those respondents for which child benefits were directly cut due to either standardization or leveling hold significantly more negative views about their personal income prospects compared to parents that were not (yet) affected by the policy reform.
### Table 2: Effects of Child Care Reform Treatment on Citizens’ Economic and Government Evaluations and their Support for Incumbent Parties

<table>
<thead>
<tr>
<th>Main Predictors</th>
<th>Income Prospects</th>
<th>Government Satisfaction</th>
<th>Incumbent Vote</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Model 1</td>
<td>Model 2</td>
<td>Model 3</td>
</tr>
<tr>
<td>Standardization (treatment group 1)</td>
<td>-.177**</td>
<td>-.224**</td>
<td>-.382</td>
</tr>
<tr>
<td></td>
<td>(.077)</td>
<td>(.058)</td>
<td>(.312)</td>
</tr>
<tr>
<td>Leveling Treatment (treatment group 2)</td>
<td>-.119*</td>
<td>-.161**</td>
<td>.053</td>
</tr>
<tr>
<td></td>
<td>(.069)</td>
<td>(.052)</td>
<td>(.257)</td>
</tr>
<tr>
<td>Future Leveling Treatment (treatment group 3)</td>
<td>-.092</td>
<td>-.131**</td>
<td>-.209</td>
</tr>
<tr>
<td></td>
<td>(.055)</td>
<td>(.045)</td>
<td>(.226)</td>
</tr>
<tr>
<td>Control Variables</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Left/Right Ideology</td>
<td>-.029</td>
<td>.012</td>
<td>.174</td>
</tr>
<tr>
<td></td>
<td>(.026)</td>
<td>(.021)</td>
<td>(.103)</td>
</tr>
<tr>
<td>Abortion Attitude</td>
<td>-.004</td>
<td>-.040**</td>
<td>.261**</td>
</tr>
<tr>
<td></td>
<td>(.018)</td>
<td>(.015)</td>
<td>(.072)</td>
</tr>
<tr>
<td>Number of Children in Household</td>
<td>.027</td>
<td>.032</td>
<td>-.129</td>
</tr>
<tr>
<td></td>
<td>(.024)</td>
<td>(.020)</td>
<td>(.091)</td>
</tr>
<tr>
<td>Gender</td>
<td>.101**</td>
<td>-.024</td>
<td>.238</td>
</tr>
<tr>
<td></td>
<td>(.041)</td>
<td>(.034)</td>
<td>(.161)</td>
</tr>
<tr>
<td>Age</td>
<td>.004*</td>
<td>-.001</td>
<td>.039**</td>
</tr>
<tr>
<td></td>
<td>(.002)</td>
<td>(.001)</td>
<td>(.008)</td>
</tr>
<tr>
<td>Religiosity</td>
<td>-.008</td>
<td>.039**</td>
<td>-.537**</td>
</tr>
<tr>
<td></td>
<td>(.016)</td>
<td>(.013)</td>
<td>(.056)</td>
</tr>
<tr>
<td>Household Income</td>
<td>.027**</td>
<td>.034**</td>
<td>-.004</td>
</tr>
<tr>
<td></td>
<td>(.007)</td>
<td>(.006)</td>
<td>(.029)</td>
</tr>
<tr>
<td>Constant</td>
<td>1.844**</td>
<td>2.924**</td>
<td>-.223</td>
</tr>
<tr>
<td></td>
<td>(.159)</td>
<td>(.131)</td>
<td>(.636)</td>
</tr>
<tr>
<td>F-Test/ LR Chi²</td>
<td>3.92**</td>
<td>5.53**</td>
<td>225.82**</td>
</tr>
<tr>
<td>Adj. R² / Correctly classified cases</td>
<td>.003</td>
<td>.039</td>
<td>79.32</td>
</tr>
<tr>
<td>N</td>
<td>1117</td>
<td>1117</td>
<td>1117</td>
</tr>
</tbody>
</table>

**Notes:** Table entries in the columns “Income Prospects” and “Government Satisfaction” are OLS regression coefficients with standard errors in parentheses, and in column “Incumbent Vote” logistic regression coefficients with standard errors in parentheses. The control group, i.e. respondents with children ineligible for child benefits, serves as the reference category for the effects of respondents with treated underage children (treatment groups 1-3). We performed several robustness checks using alternative estimation strategies as well as alternative model specifications. Since the indicators used to tap into respondents’ evaluations of their income prospects and the government entailed three versus four categories respectively, we estimated ordered logistic regression models next to the OLS analyses reported here which yields substantively identical results. In addition, we also ran the models presented above using additional controls such as items tapping respondents’ social class identification, education level or attitudes towards crime and law & order. These analyses corroborate the findings presented here and are available upon request from the authors.

**significant at p ≤ .01 (one-tailed); *significant at p ≤ .05 (one-tailed).**

In the case of government evaluations, we find that all treated respondents are less satisfied with government performance compared to respondents not affected by the policy change. The results displayed in the column Model 2 show that the treatment effect for subjects exposed to the standardization treatment is the most pronounced.
These respondents (treatment group 1) are -0.224 points (on a 4-point scale) less likely to be satisfied with the government compared to respondents in the control group. We also find a more sizable effect for respondents affected by the leveling treatment (treatment group 2). Respondents that were directly affected by the leveling of child benefits are roughly -0.161 points more negative about the performance of the government compared to respondents with children no longer eligible for child benefits. Finally, also parents of underage children that are not yet affected by the cuts in child benefits due to leveling but will be in the future are just over -0.131 points less satisfied with the government compared to the control group. In line with hypothesis 1b we find that respondents that are adversely affected by the reform of child benefits hold more negative evaluations of government performance. What is more, this effect is most pronounced among parents most severely touched by the spending cuts. Overall, the two sets of results presented so far suggest that citizens that who experienced the largest reduction of their child benefits are the least satisfied with their household income situation and with the government responsible for these cuts, which support the core assumptions of the pocketbook voting model.

Crucially to the pocketbook voting thesis, we would also expect that treated citizens are more likely to punish the incumbent. The last column in Table 2 tests this expectation. These results do not lend support for hypothesis 1c. We fail to find a significant and negative effect of the different treatments on incumbent vote intention. The coefficients for all three treatment group are negligible in size, wrongly signed for treatment group 2 and accompanied by a large standard error. These results suggest that while citizens adversely affected by the 1995 child care reform do update their evaluations of the government and their income, they do not punish the incumbent accordingly.

It may of course be particularly hard for adversely affected citizens to not only adjust their political preferences due to their experiences, but also to let them inform their behavior. Especially within this particular case as Dutch citizens are faced with a government coalition comprised of parties that span much of the ideological left/right spectrum of the party system. Consequently, it seems likely that a heterogeneous effect in treatment due to citizens’ level of political sophistication exists (see hypothesis H2). These are the results we turn to now.

Table 3 below inspects the possibility of a heterogeneous treatment effect on incumbent vote intention based on political sophistication. On the basis of a median
split we compare the treatment effects for respondents with lower levels of education, so-called novices, and those with higher education, i.e. experts. In line with the theory of heterogeneous attribution, we expect sophisticates to make stronger associative links between policy outcomes and the government, and therefore to be more likely to punish incumbent parties. In addition, we hypothesized that highly sophisticated voters attribute blame differently across parties as they are more aware of differing degrees of policy responsibility. The literature on welfare retrenchment suggests that parties who 'own' the welfare issue, i.e. have a strong historical reputation for building and protection the welfare state, are less likely to be punished for welfare retrenchment policies, while rightwing governments strongly associated with balancing budgets and fiscal discipline may face more electoral penalties (Ross 2000; Levy 1999). It seems likely that highly sophisticated voters due to their larger store of knowledge and interest in politics are more aware of these specific policy reputations. Consequently, we would expect highly sophisticated citizens who were affected by the reduction of child benefits to be more likely to punish the rightwing coalition partner for their harsh retrenchment, while refrain from penalizing the left.

The results in Table 3 suggest that lower sophisticated citizens who were negatively affected by the reform of child benefits due to the standardization or leveling of child care assistance neither punish the leftwing nor rightwing incumbent more compared to equally low sophisticated respondents in the control group. Yet, we do find treatment effects for the high sophisticates. The last column of Table 3 shows that highly sophisticated voters affected by a reduction of child benefits either due to standardization or leveling are much more likely to punish the rightwing incumbent than equally informed voters that were not affected (due to having children who are no longer underage). Again, this effect is most pronounced among parents with underage children that faced harshest financial ramification of the reform on the basis of the standardization of child benefits. These respondents are roughly 25 percentage points less likely to express a vote intention for the rightwing incumbent compared to parents of children who are no longer eligible for child benefits. The treatment effect for highly sophisticated respondents in treatment group 2, those that experienced a cut in child benefits due to leveling, amounts to 16.5 percentage points, while the effect is roughly 20 percentage points for respondents that are not affected by leveling yet but may be in the future (treatment group 3). These findings suggest that although we do not find a direct effect of our policy treatment on incumbent support as the theory of pocketbook voting would suggest, there is evidence of electoral punishment due to reduced child
benefits for the rightwing incumbent. Yet, this is only the case when a voter has a large enough store of political information to relate the respective cuts to the party most likely responsible for welfare retrenchment.

### Table 3: Heterogeneous Treatment Effects of Child Care Reform on Citizens’ Support for Incumbent Parties, by Political Sophistication

<table>
<thead>
<tr>
<th>Main Predictors</th>
<th>Incumbent Vote</th>
<th>Leftwing Incumbent Vote</th>
<th>Rightwing Incumbent Vote</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Novices</td>
<td>Experts</td>
<td>Novices</td>
</tr>
<tr>
<td>Standardization Treatment</td>
<td>-.499 (1.42)</td>
<td>-.699 (.571)</td>
<td>-.443 (.363)</td>
</tr>
<tr>
<td>(treatment group 1)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Leveling Treatment</td>
<td>.577 (1.328)</td>
<td>-.958 (.509)</td>
<td>.495 (.282)</td>
</tr>
<tr>
<td>(treatment group 2)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Future Leveling Treatment</td>
<td>.052 (1.273)</td>
<td>-.875 (.477)</td>
<td>-.016 (.241)</td>
</tr>
<tr>
<td>(treatment group 3)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>.623 (1.793)</td>
<td>-2.209 (1.163)</td>
<td>2.036** (.707)</td>
</tr>
<tr>
<td>LR Chi²</td>
<td>155.32** 155.32**</td>
<td>90.33** 90.33**</td>
<td>133.01** 133.01**</td>
</tr>
<tr>
<td>Correctly classified cases</td>
<td>79.80</td>
<td>80.52</td>
<td>72.18</td>
</tr>
<tr>
<td>N</td>
<td>696</td>
<td>409</td>
<td>696</td>
</tr>
</tbody>
</table>

**Notes:** Table entries are logistic regression coefficients with standard errors in parentheses. These are results of a fully-specified model including all the control variables included in Table 2 (full results are available upon request from the authors). The control group, i.e. respondents with children ineligible for child benefits, serves as the reference category for the effects of respondents with treated underage children (treatment groups 1-3). As a robustness check, we also ran the models presented above using additional controls such as items tapping respondents’ social class identification or attitudes towards crime and law & order. These analyses yield substantive identical results to those presented here. Both batches of results available upon request from the authors.

**significant at p≤.01 (one-tailed); *significant at p≤.05 (one-tailed).**

The results in Table 3 also indicate that treated citizens regardless of their level of sophistication do not punish the left for a reduction in child care spending. The moderating treatment effect due to political sophistication demonstrates that adversely affected citizens at a high level of sophistication are more likely to punish the rightwing incumbent, but refrain from penalizing the left. These findings are line with the Nixon-goes-to-China argument put forward in the welfare retrenchment literature: while parties on the right are more likely punished for retrenchment policies as they may be perceived as socially negligent when introducing social spending cuts, leftwing parties avoid blame for introducing these measures as they are likely to be viewed as fiscally prudent and economically responsible (Ross 2000; Levy 1999).
Discussion

It is widely believed by academics, politicians and the media that incumbent politicians seek to allocate public spending to their constituents for electoral advantage (Mayhew 1974; Shepsle and Weingast 1981; Lindbeck and Weibull 1987; Cox and McCubbins 1986; Levitt and Snyder 1997). Consequently, governments are reluctant to cut social transfers, such as university benefits, especially in the run-up to elections, as they fear that the voters adversely affected by such cuts will punish them in the election. The core assumption of these models of strategic incumbent behavior is that voters are self-interested 'pocketbook' voters that respond to changes in public spending by rewarding or punishing the incumbent. However, there is only weak empirical evidence supporting the basic assumptions of policy voting, namely that voters respond to changes in policies that affect them by changing their attitudes and behavior. While there is a large literature on the importance of performance voting, most notably economic voting, such studies rarely examine the actual effects of policy changes on attitudes and behavior, but rely instead of subjective measures of policy preferences or economic evaluations.

When it comes to the effect of economic evaluations on vote choices, most studies have found stronger effects of 'sociotropic' variables than of pocketbook considerations.

The objective of this study has been to test the basic assumptions of the policy voting thesis by examining government reforms of direct social transfers. To circumvent issues of endogeneity in purely survey-based methods, we take advantage of a natural experiment of reforms to the Dutch childcare benefit system. The virtue of this specific policy event is that provides us with an as-if randomness condition where some Dutch citizens (parents of children born after the reforms and in certain age brackets) experienced significant reductions in their social transfers which they could neither have anticipated nor circumvented. Our study has assessed whether citizens adversely affected by this 'policy condition' responded to it as assumed by most rationalist models of political behavior: realizing a change in income prospects, adjusting their evaluations of the government and withdrawing electoral support for the incumbent. We thus assess the existence of policy voting in a core area of public spending, namely social transfers, where we would expect citizens to take notice.

Our findings lend some empirical support to the 'policy voting' model. While we find that only the most adversely affected citizens responded by adjusting their evaluations of their household income prospects suggesting that they realized that the policy reform negatively affects their personal financial situation, we do find that all
parents that faced negative financial ramifications due to the reform were more likely to be dissatisfied with the incumbent government. However, we find less evidence of an effect on voting behavior: adversely affected parents were no less likely to support the government in an upcoming election. This suggests that changing vote choice is a higher hurdle for voters than simply changing their political attitudes. This should perhaps come as no surprise given the large literature suggesting that vote choice is shaped by long-term psychological attachments, notably partisanship (Campbell 1960; Bartels 2000, 2002; Gerber et al. 2010). Moreover, general ideological considerations and sociotropic concerns may play a greater role in influencing party choice. Even voters who are personally adversely affected by cuts may consider that such austerity measures are necessary and prudent policy changes for the country’s economy. Normatively, it is therefore not clear-cut that a rational voter would always want to engage in pocketbook ‘policy voting’. That said, our findings do suggest that relating policy outcomes to one's financial situation or to incumbent support is an easier task for politically sophisticated voters. What is more, the varying electoral punishment for leftwing and rightwing incumbents among sophisticates seems largely in line with the theory of heterogeneous attribution as put forward by Gomez and Wilson (2001, 2006). Highly sophisticated voters are more able to transform their political preferences or behavior in accordance to changes in their own financial situation, and do so by blaming the incumbent party most likely responsible.

By exploiting an actual policy change in Dutch social spending, this study has been able to circumvent the complex causal relationships between policy outcomes, performance evaluations and incumbent support to test the core assumptions of rational models of vote choice and incumbent strategies. The advantage of this research design is that it gives us more confidence is our causal inferences: policy reform does appear to have an effect on affected individuals’ evaluation of their financial situation and of the incumbent. These findings have important implications for our understanding of both vote choices and government strategic behavior. They show that voters respond to policy changes in a largely rational manner, yet that attitudes only inform and change the behavior the politically sophisticated voters. What is more, our findings suggest that when reforming social benefits incumbents, especially those on the right, face a tough choice: While they may have to pursue reforms to satisfy the demands for fiscal responsibility among their constituents, they are likely going to face the electoral penalties of highly politically aware citizens.
The disadvantage of such a natural experiment is that it explores an isolated case of policy reform, and this raises the question of whether these findings can be generalized to other instances of policy reforms and other political systems. In many ways, we would argue that the case of reforms to child care benefit provides an apposite test, since it is a form of direct social transfers employed by most Western welfare states. That said, the Dutch case only explores voter responses to cuts in benefits. Some research suggests that voter punishment due negative policy outcomes may be more pronounced than rewards on the basis of improvements (Nannestad and Paldam 1994, Duch 2001). It seems important for future work to explore this potential asymmetry. We also regard it as a relatively conservative test due to the power-sharing nature of Dutch politics in general and the specific incumbent in particular. We would thus expect greater support for the policy voting model in systems with higher clarity of responsibility. Whether this expectation holds, is a question for future research.
References


Krasno and Green 2008


