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Promises, policies and pocketbook voting

Mikael Elinder [a,b], Henrik Jordahl [b,c], Panu Poutvaara [c,d,e,f,*]

[a] Department of Economics, Uppsala University, Sweden
[b] The Research Institute of Industrial Economics (IFN), Sweden
[c] IZA, Germany
[d] Department of Economics, University of Munich, Germany
[e] Ifo Institute, Poschingerstr. 5, 81679 Munich, Germany
[f] CESifo, Germany

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ABSTRACT

Do voters respond to political parties' promises or to their past actions? We use a suitable sequence of events in Swedish politics to provide the first answer to this question. In the 1994 election campaign the Social Democrats proposed major cuts in transfers to parents with young children, whereas in the 1998 campaign they promised to increase transfers. The Social Democrats won both elections and delivered on both promises. Using voting among parents with slightly older children as counterfactual, we find that voters with young children responded markedly to economic promises rather than to implemented policies.

1. Introduction

Political promises attract a lot of attention in election campaigns, but their effects on voting remain largely unknown. Following the classification of Persson and Tabellini (2000), models of pre-election politics assume that election promises are binding. Consequently, citizens are predicted to vote for the party with the platform that would give them the highest expected utility. Models of post-election politics, on the other hand, assume that election promises are non-binding, implying that citizens vote based on the government's behavior and accomplishments. While a large empirical literature on economic voting tests post-election models, for instance how macroeconomic outcomes and targeted transfers affect support for the incumbent, our study is the first to estimate the causal effect of election promises on voting.[1]

We compare voter responses to election promises with responses to past actions of political parties. We refer to voting based on electoral promises as *prospective* and voting based on implemented policies as *retrospective*. We examine individual voting responses to two high-stake reforms with substantial economic consequences for a specific group of voters. Both reforms appeared as campaign promises in Swedish parliamentary elections and were subsequently

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* Corresponding author at: Department of Economics, University of Munich, Ifo Institute, CESifo, Poschingerstrasse 5, 81679 Munich, Germany. Tel.: +49 8992241372.

E-mail address: poutvaara@ifo.de (P. Poutvaara).

[1] For comprehensive reviews of the economic voting literature, see Lewis-Beck and Stegmaier (2007, 2013).

implemented. Hence, we test for both prospective and retrospective *pocketbook voting*. Pocketbook voting is defined as voting for the political candidate or party that benefits the voter the most financially. Prospective pocketbook voting refers to votes that are cast based on how electoral promises would affect the voter's disposable income if implemented. Retrospective pocketbook voting refers to voting based on how implemented policies affect the voter's disposable income.

In the 1994 election campaign, the Social Democratic Party surprised pundits and voters alike when it suggested major cuts in financial support for parents with young children as a way to fight the budget deficit. In the next election campaign, in 1998, the Social Democrats came up with another eye-opening turnaround: a promise to put a cap on child-care fees in order to reduce the child-care costs of families with young children substantially. The Social Democrats won both elections and delivered on both promises. This sequence of events allows us to test both prospective and retrospective voting. Since parents with older children were largely unaffected by the reforms—and had voted nearly identically to parents with young children in elections before 1994—we use a difference-in-differences strategy to identify the voting responses to the reforms.

The emerging voting pattern provides evidence of prospective pocketbook voting, as parents with young children responded to the two campaign promises rather than to the later implementation of the reforms. The pattern fits the logic of median voter models (Downs, 1957, ch. 8) and probabilistic voting models (Lindbeck and Weibull, 1987, 1993; Dixit and Londregan, 1996). In our interpretation, the voting pattern also indicates that the campaign promises were largely seen as credible. This makes sense since it has been demonstrated that the Swedish Social Democrats have kept a majority of their election promises (Naurin, 2011).

Our analysis is complementary to a large related literature, mostly published in political science journals, which asks whether voters react more strongly to politicians' past performance or expectations concerning future performance. Both in that previous literature and in our paper, retrospective voting refers to voting that is based on past performance, although there are different interpretations on whether voters simply reward or punish the incumbent (Key, 1966) or use past performance as a guidance on what to expect in the future (Fiorina, 1981). As for prospective voting, the political science literature typically asks how voters expect the incumbent to perform in terms of managing the economy (in contrast to our definition in terms of electoral promises that have redistributive implications). Such expectations can be derived from national election studies (Lanoue, 1994), surveys of consumer sentiment (MacKuen et al., 1992) or directly asking voters how they expect competing candidates to perform in managing the economy or in other policy areas (Hsieh et al., 1998). Although such expectations could be affected by electoral promises, none of these papers has tested to what extent voters react to electoral promises. The literature has been divided in its judgment on whether voters vote retrospectively or prospectively. Lanoue (1994) and Norpoth (1996) find that voting is mostly retrospective, whereas MacKuen et al. (1992) and Hsieh et al. (1998) find that it is mostly prospective. Lewis-Beck (1988) concludes that prospective and retrospective evaluations have about the same impact on the vote in six western countries.

When it comes to pocketbook voting, Lewis-Beck and Stegmaier (2007) conclude in their review of about 400 studies that the massive literature on economic voting has not provided much evidence in favor of this hypothesis.[2] However, most of those studies suffered from severe identification problems and often assumed that voters attribute all the changes in their financial situation to the policies of the incumbent government. A major challenge in estimating the effects of government spending on voting is identifying a proper comparison group. The few studies that have succeeded with this generally find that realized public spending increases the government's support among recipients. This emerging consensus comes from the United States (Levitt and Snyder, 1997), Russia (Richter, 2006), as well as from a series of studies of poverty reduction programs in Latin America (Manacorda et al., 2011; De La O, 2013; Zucco, 2013) and in Romania (Pop-Eleches and Pop-Eleches, 2012). All of these studies estimate the effect of implemented policies. We add to this literature by comparing voter responses to promises and to realizations of targeted transfers. We find that Swedish voters respond to promises of targeted transfers without any additional voting responses after the policies are realized.

Can we expect our findings to apply to other countries? We find it plausible that prospective pocketbook voting could be important in countries where a majority of election promises are fulfilled. Since Mansergh and Thomson (2007) report that a majority of election promises are kept in Canada, Great Britain, Greece, Ireland, the Netherlands and the United States, our findings could well be generalizable to other Western democracies. Furthermore, findings that politicians choose to target spending to informed and swing voters imply that they believe that people vote their pocketbooks (Dahlberg and Johansson, 2002; Strömberg, 2004; Snyder and Strömberg, 2010).

The rest of the paper is organized as follows. Section 2 presents the theoretical framework and our hypotheses to be tested. Section 3 describes the events in Swedish politics which allow us to test for retrospective and prospective pocketbook voting. Section 4 presents the data, Section 5 the empirical strategy and Section 6 the results. Section 7 concludes and presents a calculation of the empirical cost of the vote in Sweden.

2. Theory and our hypotheses

In this section we put our study into theoretical perspective and motivate the hypotheses to be tested. We focus on pocketbook voting in response to promises (prospective voting) and implemented policies (retrospective voting). A formal model that incorporates both prospective and retrospective pocketbook voting is presented in Appendix A.

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[2] See e.g. Kinder and Kiewiet (1979), Lewis-Beck (1985), Markus (1988) and Sigelman et al. (1991) for early tests and discussions of pocketbook voting.

Pocketbook voting is a cornerstone in several influential models of electoral politics, e.g. Meltzer and Richard (1981), Lindbeck and Weibull (1987) and Rogoff (1990). In the political science literature, pocketbook voting has traditionally been contrasted with sociotropic voting (defined as voting based on national economic conditions). While it is reasonable to assume that a pocketbook voter is motivated by economic self-interest, a sociotropic voter may but need not share this motivation (Kinder and Kiewiet, 1981).[3] Regardless of the underlying motivation, voting can be prospective, retrospective or both. As described in Section 1, we base our definition on the information set used by voters, defining voting based on economic performance and implemented policies as *retrospective* and voting based on promises and election platforms as *prospective*. As formulated by Lewis-Beck (1988), "a voter, in evaluating parties and candidates, weighs what they say that they *will do*, as well as what they *have done*." In the setting of targeted transfers, voters can directly observe the link between government policies and their economic benefits. Unlike previous papers on prospective and retrospective economic voting, we do not have to make assumptions about how voters attribute economic performance measures such as changes in GDP or individual income to the government policies.

In Persson and Tabellini's (2000) classification, the distinction between prospective and retrospective voting is a fundamental difference between models of pre- and post-election politics.[4] Models of pre-election politics focus on competition between the government and the opposition in election campaigns. Election promises are assumed to be binding, and voting to be prospective. Citizens vote for the party with the platform that would give them the highest expected utility. Pre-election models come in two forms: median voter models (in line with Downs, 1957, ch. 8) and probabilistic voting models (e.g. Lindbeck and Weibull, 1987, 1993; Dixit and Londregan 1996). Citizen–candidate models, pioneered by Osborne and Slivinski (1996) and Besley and Coate (1997), assume that political candidates cannot pre-commit to a platform before an election and will always implement their preferred policies if elected. But as voters are assumed to know the candidates' ideal points, their choice problem is analogous to that in pre-election politics models. Models of post-election politics, on the other hand, focus on the government's policymaking. The opposition is portrayed as passive and anonymous, and is typically modeled as a random draw that is available as an alternative to the government. Since electoral promises are unenforceable and voters cannot predict future policies from candidates' ideal points, voting is retrospective.

Post-election models can be further divided into sanctioning and selection models. Following Key (1966), early sanctioning models assumed that voters reward or punish the incumbent government based on its performance in office. Barro (1973) and Ferejohn (1986) develop a sanctioning model focusing on the conflict between voters and politicians. Voters are able to discipline incumbents by using a threshold requirement for re-election. Voters in selection models, on the other hand, try to find out which party they prefer to have in office for the next period. The government's competence is signaled through economic outcomes (Rogoff and Sibert, 1988; Rogoff, 1990; Persson and Tabellini, 1990).[5] Besley (2006) incorporates both sanctioning and selection processes into a comprehensive discussion and development of post-election models.

While the difference between sanctioning and selection models is theoretically important, these two types of retrospective voting models have proved difficult to test against each other. Our empirical setting is no exception; voting based on the "purely retrospective" sanctioning model would closely mirror voting based on the rational–predictive logic of the selection model. In contrast, our definition separates prospective and retrospective voting both in theoretical models and in empirical investigations. In the empirical investigation we will test the following two hypotheses:

Hypothesis 1. (Prospective pocketbook voting)
If a party promises increases (cuts) in transfers to a group of voters, it will gain (lose) votes from this group, all else being equal.

Hypothesis 2. (Retrospective pocketbook voting)
If a party has increased (cut) transfers to a group of voters while in government, it will gain (lose) votes from this group, all else being equal.

The two hypotheses are derived from the theoretical model presented in Appendix A. Our starting point for the model is a probabilistic voting model which we modify to allow voters to form expectations of future policies based on both promises and on past policies. The model makes clear that both prospective and retrospective voting can occur in the same election. By putting weights on promises and implemented policies in the formation of voter expectations, the model also helps us to understand when to expect prospective or retrospective voting to dominate. In the spirit of Fiorina (1981, ch. 4), we conjecture that the parties' track record of keeping promises determines the extent to which people vote prospectively and retrospectively. Voters will regard a party that has kept most of its past promises as credible.

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[3] For evidence of sociotropic voting, see the original contribution by Kinder and Kiewiet (1979), the Nordic studies by Nannestad and Paldam (1997) and Jordahl (2006), as well as the review by Lewis-Beck and Stegmaier (2007).

[4] Alesina (1988) and Harrington (1993) link pre-election and post-election politics in models in which repeated elections influence the incentive for parties to keep their promises. Otherwise, there have been very few theoretical attempts to bridge models of pre-election and post-election politics.

[5] Drazen and Eslava (2008) develop a selection model in which voters use policy outcomes to infer politicians' preferences for targeted redistribution towards different groups.

3. Promises and policies

During our period of study, Swedish politics was dominated by two blocs of parties. The center-right bloc consists of four parties: the Moderate Party, the Centre Party, the Liberal Party, and the Christian Democrats. The left bloc consists of the Social Democratic Party and the Left Party. The Social Democratic Party has been the country's largest party for decades, usually forming a minority government supported by the Left Party. In our period of study, the left bloc (i.e. the Social Democratic Party and the Left Party) had a combined vote share of around 50% throughout the 1980s. In 1991, their vote share fell to 42% as they were defeated by the center-right parties, which then formed a majority coalition. Then, in the midst of an economic crisis with a surge in unemployment and huge budget deficits, the left bloc made a strong recovery and returned to power having won 51% of the votes in the 1994 election. Although experiencing small backlashes, the left bloc managed to stay in power after the 1998 and 2002 elections.

In this study, we make use of the fact that the Social Democratic Party promised parents with young children diametrically different schemes in the 1994 and 1998 parliamentary campaigns, while parents with older children remained largely unaffected by the proposals. In our empirical tests, it is crucial that parents with young children and parents with somewhat older children were treated similarly in terms of other promises or policy changes. After examining the election manifestos of all the parliamentary parties from 1982 to 2002, we verified that this was in fact the case. Apart from the promises described in this section, the election manifestos contain no significant promises that would lead to different treatments of the two groups of parents.[6] We also systematically investigated records of implemented policy changes to ensure that no other policy change affected the two groups differently.

Given that Sweden has a proportional electoral system with several parties, an important concern to address is strategic voting. There is a threshold of 4% of all votes that parties need to reach to be represented in the parliament. As the smallest parties in the center-right bloc have been close to the threshold in opinion polls before some elections, strategic voting cannot be ruled out within the center-right bloc. However, there is no reason for a supporter of Social Democrats or the Left Party to strategically support any party from the opposing bloc. To account for a possibility of strategic voting within the left bloc, we analyze both vote shares for Social Democrats and Left Party together and for Social Democrats alone. Next, we give a brief description of the electoral campaigns before the elections in 1994, 1998 and 2002. We focus on two reforms that appeared as election promises in 1994 and 1998 as well as their implementation. Parents whose youngest child was 4 years old or younger and parents whose youngest child was between 6 and 11 years old were affected differently by the proposals in 1994 and 1998. Both reforms were at the center of the national election campaigns in which they appeared and were extensively covered by the mass media, making it likely that most citizens were well aware of them. Voters may of course like or dislike the reforms for non-economic reasons. For instance, conservative and progressive voters may have different ideological views on the specific proposals. As long as such non-economic evaluations of the reforms are unrelated to the age of voters' children they do not confound our estimates of pocketbook voting.

3.1. Financing the budget deficit—the key issue of the 1994 election

In the early 1990s, the Swedish economy plunged into a severe crisis that triggered a surge in unemployment and huge budget deficits. The political parties worked hard to try to persuade voters during the 1994 election campaign that they had the ability to handle the budget crisis. The center-right coalition government's main proposals involved cuts in pension benefits and a promise to crack down on tax evasion and fraudulent behavior in the social welfare system. Apart from a traditional, unspecific proposal of tax hikes for high-income earners, the Social Democratic Party also proposed an unexpected way to fight the budget deficit—major cuts in financial support to parents with young children:

(1) *Abolition of the child-raising allowance.* The ruling center-right coalition introduced a child-raising allowance in 1994. Families with children between one and three years old who did not use publicly subsidized child care received a tax-free transfer of 220 euros per month and child (1 euro equals about 9 Swedish kronor). Both the Social Democratic Party and the Left Party opposed the introduction of the child-raising allowance and declared that they would repeal this law if they won the next election, which they did. As a result, the child-raising allowance was abolished, effective from 1 January 1995.

(2) *Reduction of parental insurance.* In 1994 parents could claim compensation for about one year's loss of earnings when staying at home with a newborn child. The Social Democrats unexpectedly advocated in the summer of 1994 to reduce parental leave compensation from 90 to 80%. After the election, the compensation was reduced to 80%, starting in January 1995. For a typical parent working in the private sector, this implied a loss of 107 euros per month (net of tax) during parental leave. While parents with children aged 0–7 are eligible for the insurance, most of it (60%) is taken up by parents during the first year of the child and almost all of the insurance (90%) is taken up by parents with children aged 0–4.[7]

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[6] During our period of study (1982–2002), typical economic promises in the election manifestos of the Social Democratic Party included investment in infrastructure, spending on public services and measures to reduce income differences. Typical economic promises of the largest center-right party (the Moderate Party) included a reduction of the total tax burden and the privatization of state-owned companies and of public service provision.

[7] The statistics are collected from The Swedish Social Insurance Agency.

(3) *Introduction of a qualifying day for compensation when caring for sick children.* Parents who care for sick children are entitled to compensation for lost labor income. In the election campaign of 1994, the Social Democrats declared that they would introduce a day of qualification before such compensation could be claimed; the first day of stay-at-home care of sick children would thus not be compensated. On average, a worker with young children stays at home 2.5 times per year and would have lost about 130 euros per year (net of tax) if this change had been implemented. This proposal was, however, not put into place. Fig. 1 shows the average number of qualification days that would have been imposed on parents with one child. Clearly, parents with young children had more to lose from a day of qualification, since young children are sick more often than older children.[8]

Taken together, these three proposals constituted a reform with a strong negative effect on the economic situation of parents with young children. A family with two children between 0 and 4 years old could expect to lose approximately 150 euros per month from all three proposals together.[9] In his description of the 1994 election, Widfeldt (1995) notes that promises of spending increases were absent from the campaign and that the Social Democratic Party's proposal to cut financial support for families with young children led to an intense debate.

The proposal to cut transfers to parents with young children provided a prospective reason for this group of parents to reduce their support for the left bloc. The introduction of the child-raising allowance in 1994 provided a retrospective reason to vote for the incumbent center-right coalition.[10] Thus, both prospective and retrospective voting models predict that parents with young children would decrease their support for the left bloc in the 1994 election. If voting is retrospective, we would also expect the left bloc to lose votes from parents with young children in the 1998 election, due to the cuts in transfers that were implemented between 1994 and 1998.

3.2. Lower child-care fees—SDP's turnaround in the 1998 election

In the 1998 election campaign, the Social Democrats promised to introduce a fee cap on child-care services that would substantially reduce child-care fees for most families with young children. The promise became one of the most important issues in the 1998 election campaign (Arter, 1999). The center-right parties on the other hand held on to suggestions to cut taxes on labor income, to improve business conditions and to increase the possibilities for citizens to choose between different schools and between different providers of health care and elderly care.

The promise to put a gap on child-care fees targeted a well-defined group of voters with a high take-up rate: about 73% of all children aged 1–5 participated in publicly subsidized child care in 1998. Children up to 5 years old are eligible for full time child care. However, parents with children aged 0–4 at the time of the September election stood to benefit the most from the fee cap as a substantial part of the children who were 5 years old would turn 6 before the cap could be introduced (which could at the earliest be done in January).

Although child-care fees had been discussed for some time, the electorate and even most Social Democratic ministers were surprised when a reform proposal was presented as an election promise in the party's election manifesto. The manifesto had been prepared in secret by the prime minister and a few confidants, and was published one month before the election day. It included the following promise:

> … we would like to implement a cap on child-care fees in the entire country at 700 Swedish kronor [78 euros] per month for the first child and lower fees for subsequent children. The reduction in child-care fees should be at least 200 Swedish kronor [22 euros] for everyone. [Our translation.]

The opposition did not support the idea of a cap on child-care fees. The left bloc won the election, and in November 2000, the parliament voted in favor of introducing the reform. The reform was implemented on 1 January 2002. The fee cap was set to a maximum of 3% of gross family income for the first child, but with a maximum payment of 127 euros, 2% for the second child, with a maximum payment of 84 euros, and 1% for the third child, with a maximum payment of 42 euros, granted that they were all in child care. Child care was made free of charge for any additional children.

The fee cap substantially reduced many families' child-care costs. A typical family with 2 children in child care 33 h per week gained a total of 113 euros per month in the municipality that had median fees and 238 euros per month in the municipality that had the highest fees before the reform.[11] These gains constituted approximately 5% and 10% of the family's monthly net income.

It is evident that the fee cap reform substantially improved the financial situation of the vast majority of families with young children. If parents with young children vote prospectively, we would therefore expect the Social Democrats to have gained votes from this group in the 1998 election. If parents instead vote retrospectively, we would expect the Social

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[8] The reason why there are so few sickness periods for children up to one year old is that one parent is typically staying home financed by the parental insurance and hence is not entitled to any additional compensation when caring for a sick child.

[9] Our calculation of the approximate loss is based on the share of children in different age groups and modes of child care (as reported by the Swedish National Agency for Education). The day of qualification, which was not implemented, made up 11% of the total expected loss from the reform.

[10] The opponents of the child-raising allowance argued that it would have negative effects on female labor market participation. As such an ideological opposition would affect both parents in the treatment and in the control group, it should not affect our estimates of pocketbook voting.

[11] The calculated gains are based on figures from the Swedish National Agency for Education.
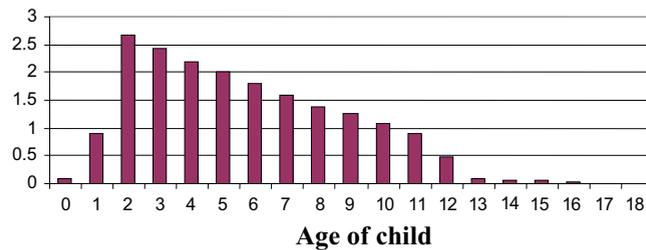
Fig. 1. Average number of sickness periods with compensation per child in 1994.
Source: The Swedish Social Insurance Agency

Democrats to have lost votes from parents with young children in the 1998 election, and gained votes from them in the 2002 election after the implementation of the fee cap reform.

3.3. Focus shifting away from family policies—the 2002 election

Immigration became the most salient issue in the 2002 election campaign (Widfeldt, 2003). The Liberal Party suggested a new program for immigration and integration, which included a five-year qualification period for a permanent residence permit and full entitlement to welfare benefits as well as a language test as a criterion for Swedish citizenship. Several politicians, in particular from the Moderate Party, were accused of being xenophobic in a TV documentary by the Swedish National Public TV Broadcaster. While discussions about immigration overshadowed most other issues in the election campaign there were still important differences in the proposals of the Social Democrats and the center-right parties, as well as within the center-right parties, also in other policy domains. The Social Democrats put forward traditional suggestions to increase spending on education, health care and elderly care. The Moderate Party opposed increased spending and instead suggested broad tax cuts. Although the Moderate Party explicitly criticized the fee cap reform as a mistake, both the Liberal Party and the Christian Democrats changed their position and accepted the reform whereas the Center Party did not mention it in their election manifesto. In this situation it must have been difficult to say what would happen to the reform after a center-right victory. Parents with young children who believed that the fee cap would probably be kept in place after a center-right victory would now have a weaker prospective reason to support the left bloc than in the 1998 election. At the same time, parents who believed that the center-right government would abolish the fee cap would not have any reason to reduce their support for the left bloc. Prospective voting thus implies that the support for the left bloc among parents with young children should either fall or stay constant. Note also that if citizens vote retrospectively, then a larger fraction of parents with young children would vote for the left bloc since the fee cap reform had been implemented during the previous parliamentary period.

Fig. 2 describes the timing of the election campaign promises and their implementation.

4. Data

The availability of high-quality data at the individual level is a prerequisite for our empirical strategy. The Swedish Election Studies sample about 3000 individuals drawn from the population of eligible voters. This data set contains a multitude of background questions about the respondents and their partners, including the number and age of all the children in the household, which allows us to identify an ideal sample of parents for both the treatment and the control group.[12] Table B1 in Appendix B contains variable definitions; Tables B2 and B3 contain descriptive statistics. While the secrecy of the ballot introduces unavoidable uncertainty into the individual voting data, the Swedish Election Studies have at least two features that make them attractive in international comparisons. First, turnout data from the electoral register are used to verify whether respondents who claimed to have voted actually did so. By excluding the respondents who incorrectly claimed to have voted, we eliminate problems associated with over-reporting (Belli et al., 2001). Second, the response rates are high in comparison with other election studies (between 69% and 81% in the 1991–2002 Swedish Election Studies).

5. Empirical strategy

The two reforms that affected families with young children are well suited to testing the pocketbook voting hypotheses. First, both reforms were described with unusual clarity in the election manifestos of the Social Democratic Party. Second, since both reforms were implemented, we can test whether voters responded to the reform proposals when they appeared

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[12] The election studies are made in the form of a two-step panel in which each respondent is interviewed twice and half of them are replaced in each study. We use this information to cluster standard errors on individuals in the statistical analysis. While a fixed effects model might seem appealing, a major drawback is that it would use voting responses of people who were planning or expecting to have children as the counterfactual.
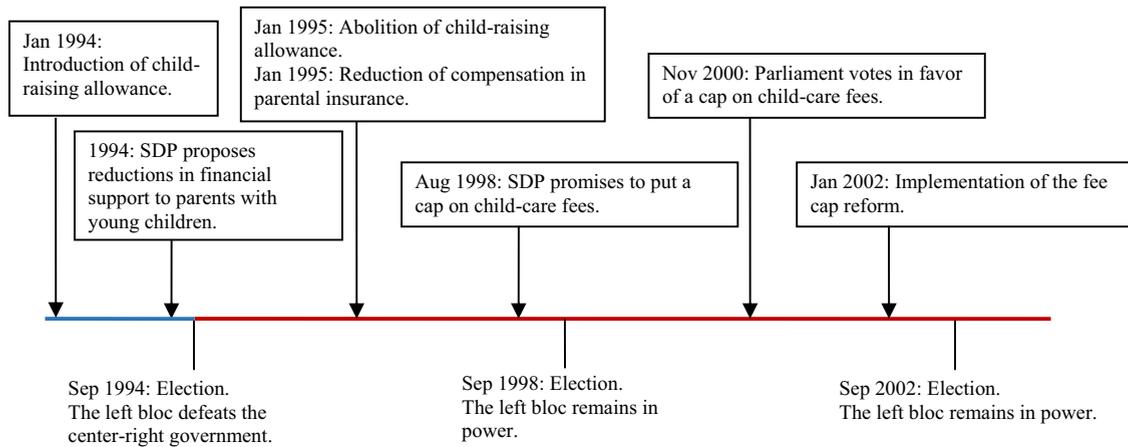
Fig. 2. Timing of election promises and their implementation. (SDP=Social Democratic Party.)

as campaign promises or after they had actually been implemented. A third attractive feature of the reforms is that significant individual benefits were at stake. Fourth and finally, parents with young and somewhat older children voted nearly identically in several elections before the reforms under study (as we will demonstrate in connection with Fig. 3), which makes it reasonable to assume that the reforms explain later voting differences.

We test the pocketbook voting hypotheses by estimating difference-in-differences models separately for 1991–1994, 1994–1998 and 1998–2002. Our dependent variable is a dummy for individual vote choice, coded one for the left bloc of parties—the Social Democratic Party and the Left Party. The Social Democratic Party and the Left Party made up the opposition in 1991–1994, whereafter the Left Party supported the Social Democratic minority government for the rest of our period of study. We also present the results for the vote share of the Social Democratic Party alone.

The basis of our difference-in-differences specifications is that the new child-care policies affected the economic situation of parents with young children (the treatment group) to a much larger extent than parents with older children (the control group).[13] Our basic difference-in-differences regressions can be written as

$$Left\ Vote_{i,t} = \alpha + \beta T_i + \gamma p_t + \delta T_i p_t + \varepsilon_{i,t}, \tag{1}$$

where $Left\ Vote_{i,t}$ is a dummy variable that equals 1 if respondent i votes for the left bloc in election t. T_i is a dummy variable taking the value 1 if the respondent belongs to the treatment group. The time period dummy p_t takes the value 0 in the election before the proposal or policy change and 1 in the election after it. The parameter of interest is δ, which measures the effect of the policy changes on the treated group; α measures the fraction of voters in the control group who vote for the left bloc in the first period; β accounts for differences in support for the left bloc in the first election between the treatment and the control group; γ captures the change in voting between the first and the second election in the control group; and $\varepsilon_{i,t}$ is an error term. We calculate standard errors that are robust to heteroscedasticity and correlation between individuals who appear twice in the data.[14]

In line with the incidence of the reforms, we assign parents whose youngest child is aged 0 to 4 to the treatment group and parents whose youngest child is aged 6–11 to the control group. We do not use respondents whose youngest child is aged 5 as one could argue that they could belong either to the treatment or to the control group.[15]

Table 1 displays the predicted voting responses under prospective and retrospective voting. In the 1994 election, the treatment group should have decreased their support for the left bloc regardless of whether voting is prospective

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[13] An alternative identification strategy would be to use variation, in voting, around an age threshold in a regression discontinuity model. However, such a strategy would use much less variation in treatment and only a small fraction of our sample. Hence the tests of our hypotheses would suffer from having very low power.

[14] An alternative specification would use all elections simultaneously. We prefer our specification based on two elections at a time since it uses the same identifying information but makes the coefficients easier to interpret and provides more reliable standard errors. As pointed out by Wooldridge (2003) and Donald and Lang (2007), the standard errors are likely to be downward biased in the presence of unaccounted group-specific shocks and a small number of groups (two in our case). Such a problem could arise if the treatment and control groups were hit by shocks that are correlated within but not across the two groups. The time pattern of vote shares looks comforting in this respect, as the early time periods give no reason to believe that our treatment and control groups are exposed to different shocks (see Fig. 3). Since Wooldridge's (2003) test does not reject the absence of group-specific shocks, we are confident that the presented standard errors of the treatment effects are appropriate. On the other hand, when using all the elections simultaneously, we risk introducing another source of bias in the standard errors arising from serial correlation (Bertrand et al., 2004).

[15] By including respondents whose youngest child is five years old in the control group, we obtain slightly higher treatment effects, while the opposite happens if we include them in the treatment group.
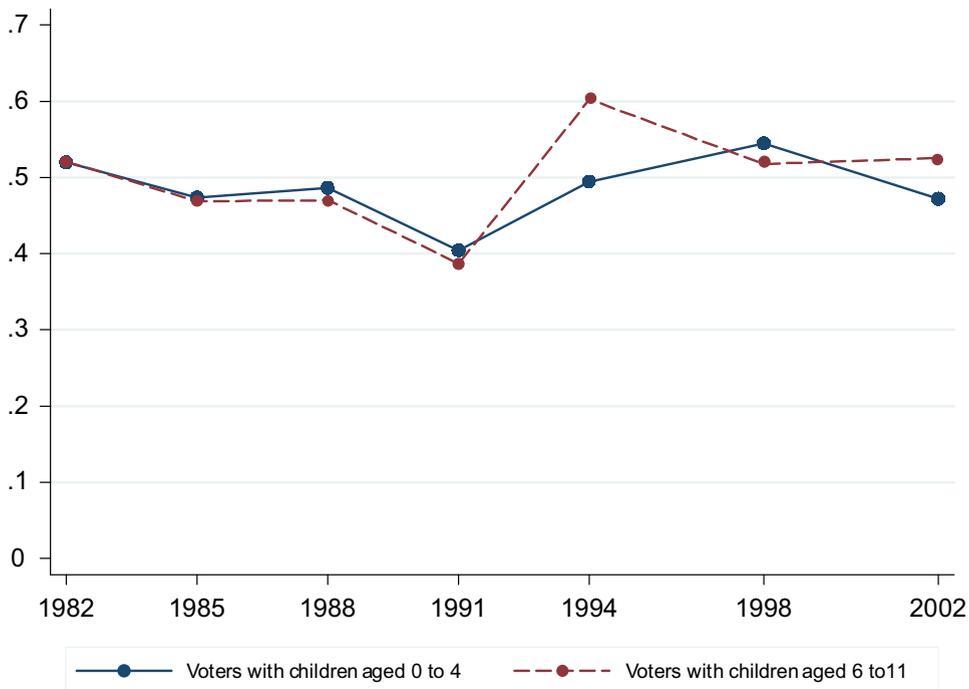
Fig. 3. Vote shares of the left bloc 1982–2002. (The left bloc consists of the Social Democratic Party and the Left Party. Respondents in both groups are allowed to have older children as well, but respondents in the control group are not allowed to have younger children.)

Table 1
Predicted treatment effects for parents with young children.

Election year	1994	1998	2002
Age of youngest child	0−4 vs. 6−11	0−4 vs. 6−11	0−4 vs. 6−11
Type of voting			
Prospective	Negative	Positive	Negative/no response
Retrospective	Negative	Negative	Positive

Note: In each election, the table gives the predicted prospective and retrospective voting responses among parents with young children, relative to parents with somewhat older children.

or retrospective. Consequently, we expect a negative δ in 1994. The 1994 election thus serves as a test of pocketbook voting even though we cannot discriminate between the prospective and the retrospective type from this election alone. In the 1998 election, prospective voting predicts a positive δ, and retrospective voting a negative δ. In 2002, retrospective voting predicts a positive response. Prospective voting predicts a negative response for voters who believed that the center-right bloc would keep the fee cap and no response for those who believed that the center-right bloc would abolish it.

We treat our difference-in-differences equations as linear probability models and estimate the parameters with ordinary least squares (OLS). The main advantage of this approach is that δ is easily interpreted as the average treatment effect. A potential drawback with the linear model, as compared with the logit model for instance, is that the individual responses to the reforms are assumed to be uncorrelated with the probability of voting for the left bloc in the absence of the reforms. Although this assumption could be criticized, we would still obtain unbiased estimates if the treatment effects are symmetric around the average baseline probability of voting for the left bloc (which seems reasonable as this probability is close to 50% in our sample). To investigate the possible bias of the average treatment effects, we have compared our OLS estimates with estimates obtained from the logit model. The results are nearly identical in terms of both size and statistical significance.

A more fundamental condition for obtaining unbiased estimates of the treatment effects is the parallel trend assumption, which requires that in the absence of the policy changes under study, the treatment group would have changed its voting pattern in the same way as the control group did. To be confident that this assumption is not violated, we have selected the control group to be as similar to the treatment group as possible. This minimizes the risk that ideological evaluations of the reforms, unrelated policy changes or other events affect the two groups differently. While there are certain life-cycle differences between the two groups—parents with older children are for example somewhat older on average—these differences can be expected to influence voting only marginally. To investigate further the
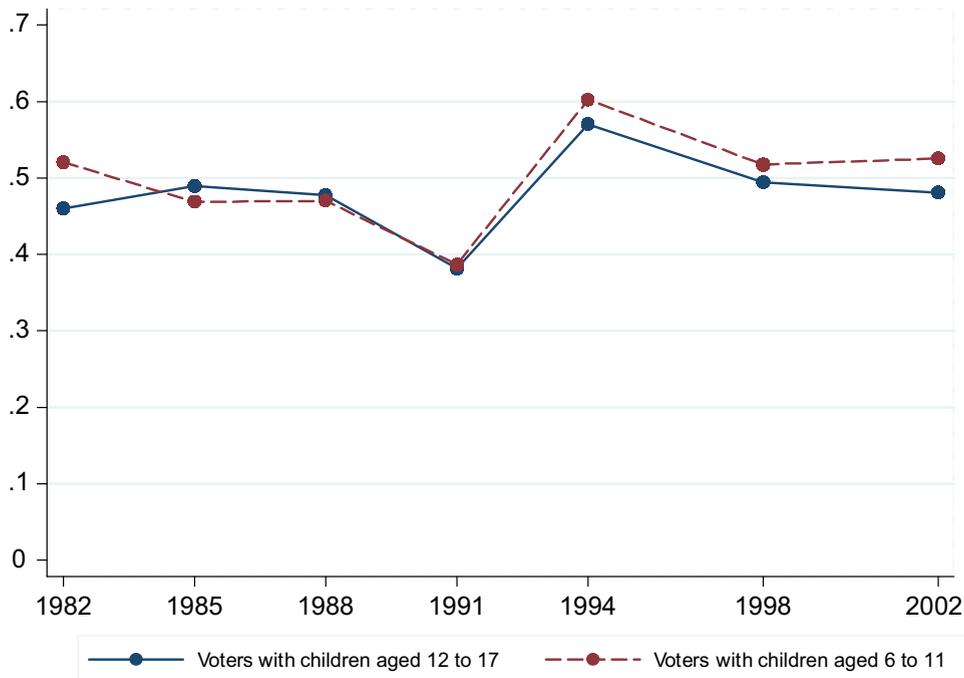
Fig. 4. Vote shares of the left bloc 1982–2002. (The left bloc consists of the Social Democratic Party and the Left Party. The figure shows voting among our control group of parents with children aged 6–11 years and among parents with children aged 12–17 years. None of the respondents in the two groups are allowed to have younger children.)

plausibility of the parallel trend assumption, we also report results from regressions that include control variables that could affect voting and that differ somewhat between the treatment and the control group. For the parallel trend assumption to be justified, the estimated treatment effects should be insensitive to including the controls, while the precision of the estimates could increase. Finally, the two groups voted nearly identically in the elections before 1994 and placebo tests on the election pairs 1982–1985, 1985–1988 and 1988–1991 all produce statistically insignificant treatment effects (p-value > 0.1).

6. Results

6.1. Visual investigation

We begin by presenting our results visually. Fig. 3 displays the vote share for the left bloc in the parliamentary elections from 1982 to 2002 among parents with young children and parents with older children.

Fig. 3 is quite suggestive of prospective pocketbook voting. The support for the left bloc among parents with young children (relative to parents with older children) decreased in 1994 when they were asked to pay for the budget deficit and increased in 1998 when the Social Democrats promised a cap on child-care fees. The support was somewhat reduced in 2002, when no new promises were targeted at this group by the Social Democrats, and two of the center-right parties had changed their position to accept the fee cap reform.

It is evident that retrospective pocketbook voting is incompatible with the pattern displayed in Fig. 3. While retrospective voting may fit the 1994 election, it fails to explain the voting in 1998 and in 2002.

According to Fig. 3, the voting patterns of the two groups seem to follow the same trend—in fact, the groups voted nearly identically in all four elections before 1994. The difference in the left bloc vote share between the treatment and the control group varies between − 1.7% and 0.1% in these four elections and is always statistically insignificant. In the absence of the policy changes under consideration, it seems plausible to assume that our treatment group would have continued to vote in a similar way to the control group. In addition, as shown in Fig. 4, the voting pattern of parents in the control group (with children between 6 and 11 years old) closely follows the voting pattern of parents with even older children (between 12 and 17 years old). The voting pattern of the control group is also similar to that of the electorate at large as described at the beginning of Section 3. This suggests that we do not identify our treatment effect by unobserved factors affecting our control group only. Fig. 4 also demonstrates that the parents in our control group in 1998 and 2002 do not seem to be affected by having been treated in the preceding elections.

Table 2
Estimates of pocketbook voting for the left bloc of parties. (Dependent variable: vote for left bloc.)

	(1)	(2)	(3)
Election year	1994	1998	2002
Age of youngest child	0−4 vs. 6−11	0−4 vs. 6−11	0−4 vs. 6−11
Treatment effect (δ)	−0.126** (0.060)	0.135** (0.067)	−0.080 (0.069)
Time effect (γ)	0.215*** (0.047)	−0.085* (0.050)	0.008 (0.048)
Treatment group difference (β)	0.017 (0.043)	−0.108** (0.046)	0.027 (0.050)
Control group (α)	0.387*** (0.035)	0.602*** (0.038)	0.518*** (0.036)
Type of voting			
Retrospective voting	Consistent	Inconsistent	Inconsistent
Prospective voting	Consistent	Consistent	Consistent
Observations	1071	908	830

Notes: Linear probability model corresponding to Eq. (1). The left bloc includes the Social Democratic Party and the Left Party. The election year of 1994 (1998, 2002) refers to a difference-in-differences estimation of 1994 vs. 1991 (1998 vs. 1994, 2002 vs. 1998). Time effect is an estimate of the change in support for the left bloc in the control group. Treatment group difference is an estimate of the difference in the vote share of the left bloc between the treatment and control group in the first of the two elections that are compared in each column. Control group is an estimate of the fraction of parents in the control group who voted for the left bloc in the first of the two elections that are compared in each column. Robust standard errors clustered on individuals in parentheses.
* Statistically significant at 90% confidence level.
** Statistically significant at 95% confidence level.
*** Statistically significant at 99% confidence level.

6.2. Main results

Having visually inspected the development of voting, we now move on to formal statistical testing. By estimating Eq. (1), we analyze the responses to the promises and policy changes in the elections of 1994, 1998 and 2002.

Table 2 contains our results. Column 1 is based on changes in voting behavior between 1991 and 1994. The estimated *Treatment effect* shows the effect of the reform on the treatment group. The point estimate of −0.126 is statistically significant at the 5% level. This estimate implies that in 1994, the support for the left bloc was 12.6 percentage points lower in the treatment group than it would have been in the absence of any promises or changes in policy. The estimate for the *Control group* (0.387) is in essence an estimate of the fraction of respondents in the control group who voted for the left bloc in 1991. The estimate for *Treatment group difference* shows that in 1991 the vote share of the left bloc was 1.7 percentage points higher in the treatment group than in the control group. *Time effect* is an estimate of the change in support for the left bloc in the control group. While the 1994 treatment effect is consistent with pocketbook voting, it is not possible to distinguish between prospective and retrospective voting from this election only.

Column 2 presents an examination of the 1998 election. The estimated treatment effect is 0.135 in this election and is statistically significant at the 5% level. The estimate is consistent with prospective pocketbook voting, but not with voting that is retrospective only. If voting were both prospective and retrospective, or if some voted prospectively and others retrospectively, the estimate would capture the net effect of prospective voting, implying that retrospective voting is less important.

Column 3 focuses on the election in 2002. Since the fee cap reform was implemented at the beginning of this election year, retrospective voting would increase the support for the left bloc in the treatment group. If pocketbook voting were prospective, voters should already have responded in 1998 when the Social Democrats made the promise to reduce child-care fees. The statistically insignificant treatment effect of −0.080 in 2002 is consistent with prospective but not with retrospective voting. Although the Moderate Party explicitly criticized the fee cap reform as a mistake, both the Liberal Party and the Christian Democrats changed their position and accepted the fee cap reform whereas the Center Party did not mention it in their election manifesto. It is therefore possible that the negative point estimate in 2002 reflects to some extent parents' expectation that they would not have to pay higher child-care fees even if the opposition won the election.

As illustrated in Fig. 5, a striking pattern of prospective pocketbook voting emerges over the elections in question. Pocketbook voting of the prospective type is consistent with the estimated treatment effect in every election. In contrast, retrospective voting is inconsistent with the voting pattern over the last two elections. Although the negative treatment effect in 1994 could be due in part to retrospective voting, the positive treatment effect in 1998 contains at most a small retrospective effect, and the voting behavior in the 2002 election is inconsistent with retrospective voting.

6.3. Robustness

If there are significant differences between the treatment and the control group, or if the composition of the groups changes over time, the estimated treatment effect could be biased. We therefore examine demographic and socioeconomic characteristics that could affect voting but are plausibly unaffected by the treatment (see Tables B2 and B3 in Appendix B). In our sample, the fraction of single parents increases over time in both the treatment and the control group. Furthermore, the share of unemployed
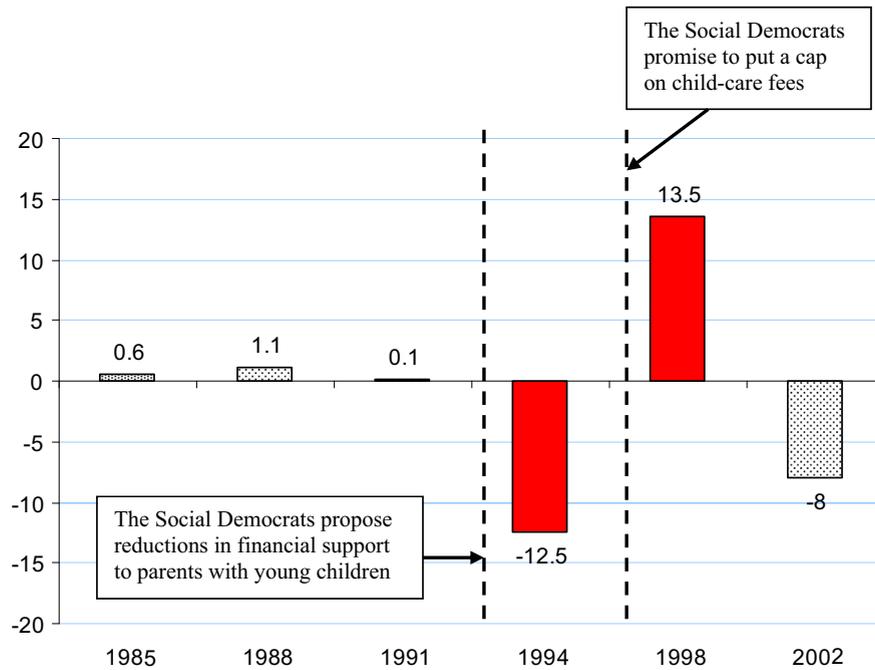
Fig. 5. Difference-in-differences estimates of left bloc vote share between the treatment group and the control group in percentage points. (The red bars indicate statistically significant estimates. The left bloc includes the Social Democratic Party and the Left Party. The youngest child of the parents in the treatment group is 0–4 years. The youngest child of the parents in the control group is 6–11 years old. The elections before 1994 serve as placebo treatments and show that the treatment group voted nearly identically to the control group.)

Table 3
Robustness tests. (Dependent variable: vote for left bloc/SDP.)

	(1)	(2)	(3)
Election year	1994	1998	2002
Age of youngest child	0−4 vs. 6−11	0−4 vs. 6−11	0−4 vs. 6−11
Dep. Var: vote for left bloc			
Main specification	−0.126** (0.060)	0.135** (0.067)	−0.080 (0.069)
With controls	−0.135** (0.060)	0.154** (0.067)	−0.071 (0.069)
With controls and excluding the unemployed	−0.123** (0.062)	0.149** (0.070)	−0.052 (0.072)
With controls and unemployment risk	−0.158** (0.062)	0.153** (0.067)	−0.060 (0.071)
Dep. Var: vote for SDP			
Main specification	−0.144** (0.059)	0.138** (0.066)	−0.066 (0.067)
With controls	−0.152** (0.060)	0.158** (0.067)	−0.056 (0.068)

Notes: Linear probability model corresponding to Eq. (1). The election year of 1994 (1998, 2002) refers to a difference-in-differences estimation of 1994 vs. 1991 (1998 vs. 1994, 2002 vs. 1998). The control variables are: dummies for cohorts (five year intervals), sex, relationship status, unemployed, two children, three or more children, and income (in €1000). SDP=Social Democratic Party. Robust standard errors clustered on individuals in parentheses.
* Statistically significant at 90% confidence level.
** Statistically significant at 95% confidence level.
*** Statistically significant at 99% confidence level.

parents is higher in the treatment group. Parents in the treatment group are also younger and have slightly lower incomes.[16] To account for these differences, we include the demographic and socioeconomic characteristics in the regressions.

The results, presented in Table 3, are robust to adding the control variables including a set of cohort dummies. However, unemployment increased more in the treatment than in the control group from 1991 to 1994. We do control for unemployment at the individual level, but our estimates could be confounded if the treatment and control group reacted differently to the downturn in the labor market. We address this concern by analyzing two additional specifications. First, we exclude all unemployed voters from our sample and re-estimate our model. Second, using the full sample of both

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[16] Out of the four elections underlying our difference-in-differences estimates, the income difference between the treatment and the control group is only statistically significant in 1994.

employed and unemployed voters, we add a measure of the risk of becoming unemployed to the set of controls. We obtain the variable *Unemployment risk* by estimating yearly models of unemployment on income, and dummies for sex, relationship status, union membership, university education, eight age groups, and having two or at least three children. We then use these models to predict the election year specific unemployment risk for each voter and add this variable to our voting models.

As can been seen in Table 3, our coefficients of interest are largely unaffected by these alterations of the empirical specification. We thus conclude that it is unlikely that our main finding—that voters respond to election promises—is confounded by voting responses to changes in labor market conditions. In addition, we have used votes for the Social Democratic Party as an alternative dependent variable. The coefficients of interest do not change much from this change (see Table 3). The robustness of our results strengthens the evidence in favor of prospective pocketbook voting.

6.4. Potential concerns

Is it reasonable to believe that the observed voting pattern is a result of prospective pocketbook voting? In this section we provide support for our interpretation by examining two different dose response tests, heterogeneous treatment effects, the effects of a stricter definition of the control group, stated vote motives, and alternative voting theories.

First, we perform a dose response test for each of the three elections. The tests are based on the fact that the benefits and costs of all of the policy proposals and implemented policies were decreasing in the age of the children. As we discussed in Section 3.1, the annual cuts in benefits that were proposed in the 1994 election campaign were largest for parents with children no older than two years. In addition, parents with younger children would be affected by the policies during more years. This logic applies also to the fee cap reform. Only children between 1 and 5 years old are eligible for full time child care. In our treatment group, parents with younger children benefit from reduced child-care fees for a longer period than parents with older children. The risk of late implementation of the election campaign promise also means that the expected value of the fee cap reform should decrease with the age of children. In the tests we estimate different treatment effect for parents with children who are 0–2 and 3–5 years old. Since we divide the treatment group into two parts and estimate more parameters, the power of these tests will unavoidably be quite low. We, therefore, include five year olds in the older treatment group to increase the size of the sample and to balance the size of the two groups. The differences in the treatment effects between the two groups are not statistically significant, but the point estimates of the dose response tests are consistent with our previous results, as we find larger responses among parents with children aged 0–2 years than among parents with children aged 3–5 years, in all three elections (see Table C1 in Appendix C).

In an additional dose response test, we make use of the fact that some voters have more than one young child and are therefore more strongly affected by the policies. We divide the treatment group into two groups: those with one child aged 0–4 and those with two or more children aged 0–4. The point estimates are again consistent with our main results as we find stronger responses among parents who have two or more children (see Table C2 in Appendix C). The differences in treatment effects between the two groups are, however, statistically insignificant.

Second, we investigate whether "swing voters", i.e. voters with a weak ideological conviction, respond stronger to the reforms. Our measure of ideological conviction is based on the self-reported positioning of respondents on an ideological left-right scale. The scale ranges from 0 to 10 and we define swing voters as those who place themselves between 3 and 7 on this scale. The point estimates are again in line with expectations as the voting response to the 1994 reform appears to be concentrated among swing voters, and the response to the 1998 reform is about twice as high among swing voters (see Table C3 in Appendix C). In general, the precision of the estimates is too low to conclude that they are different from each other at conventional levels of statistical significance. However, the voting response of swing voters in the 1994 election differs from the response of other voters at the 10% significance level.[17]

A further potential concern is that parents with children aged 6–8 (i.e. a part of the control group) could react negatively to the Social Democrats in 1998 since they did not get any benefit from the fee cap reform even though they had been paying for financing the budget deficit since 1994. The concern is that a negative reaction in this group could confound our estimates. To investigate this concern, we have tested to drop parents with children aged 6–8 from the control group. Notably, the estimated treatment effects then become slightly larger—not smaller (see Table C4 in Appendix C). In addition, Fig. 4 shows that the drop in support for the Social Democrats was not unique to our control group. Parents with even older children voted almost identically as our control group in 1998. In consequence, our results do not seem to be confounded by this concern.

While doubts have been raised about people's willingness to state truthfully that they vote based on self-interest, Carlsson and Johansson-Stenman (2010) report survey evidence that people admit that they vote as they do partly because of self-interest and that they believe that others do so to an even larger extent. Also, our survey data indirectly suggest that this is the case. The Swedish Election Studies ask the following open question: "Think about the election this year. Are there any issues that you regard as important when it comes to choosing which party to vote for in the parliamentary election this year?" In 1994 and 1998, 35.9% and 32.0% of the respondents with children aged 0–4 stated that issues related to family policies were important for

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[17] We have also tested for heterogeneous treatment effects with respect to the sex of respondents, which could be an indication that either women or men viewed parts of the studied reforms mainly as an ideological issue and did not vote their pocketbooks. As we find only small differences in the point estimates of men and women, it seems that they responded similarly to the studied reforms.

their party choice. The corresponding numbers for the control group were 16.6% and 18.9%. These figures indicate two interesting patterns. First, a large fraction of parents state that policies directed at themselves are important for their party choice. Second, respondents with older children appear to care less about policies that would have been important to them only a few years ago (when their children were younger). Both observations strengthen our interpretation that pocketbook voting is the driving force behind the voting differences between parents with young children and parents with older children.

Our results are also unlikely to be confounded by macroeconomic conditions. To start with, the treatment and the control group face the same conditions. As long as the distribution of individual reactions to the macroeconomic development does not differ between the treatment and the control group, any macroeconomic influence on voting is captured by the general popularity coefficients α and γ. However, according to partisan theory (Hibbs, 1977), macroeconomic voting responses partly reflect diverging group interests between occupational classes. While this means that macroeconomic voting responses should not be written off a priori, there is no reason to assign our treatment and control groups to different occupational classes. Moreover, the minimal voting differences between the treatment group and the control group in the elections before 1994 indicate that the two groups do not react differently to macroeconomic events. However, it is possible that the treatment and the control group react differently to an election-specific macroeconomic event. For example, the fact that the parents in the control group are slightly older than the parents in the treatment group may have given rise to different responses to the mounting budget deficit in the 1994 election. Yet, judging from Fig. 4, this particular objection seems far-fetched, as there is no voting difference between parents in the control group and parents with even older children in any election. We also compare how respondents in the treatment and in the control groups answer a survey question on their views of recent changes in the state of the national economy. We find no statistically significant differences between their stated views in any of the surveys from 1991 to 2002. This further supports our assumption that the treatment and the control group responded similarly to macroeconomic conditions in the 1994 and 1998 elections.

Lastly, group voting is not a viable interpretation of the results when group membership is determined by economic gains. In addition, our treatment group—parents with children up to four years old—does not resemble any of the groups that Mutz and Mondak (1997) work with. Their groups are demarcated by sex, race, labor market participation, income and self-identification with a certain social class.

7. Conclusions

Our empirical investigation of individual-level data from several elections provides evidence that voters respond to electoral promises. We find no additional responses after the policies have been implemented. At a more general level, our results are consistent with theoretical models of pre-election politics (Lindbeck and Weibull, 1987, 1993; Dixit and Londregan, 1996).

Our findings suggest that promises of targeted transfers can be important for election outcomes. According to the Swedish Election Studies, our treatment group made up about 12% of the voters. Together with our estimates of the 1994 and 1998 treatment effects (−12.6 and 13.5 percentage points), the shares suggest that the promise to cut financial support to parents with young children reduced the support for the left-bloc parties by approximately 1.5 percentage points in the 1994 election and that the promise to put a cap on child-care fees increased support for the left-bloc parties by approximately 1.6 percentage points in the 1998 election.[18] Although a reform effect of about 1.5 percentage points would not have tipped the scales in the 1994 or the 1998 election, an effect of this size has the potential to affect the balance of power in closer races—the margin between the left and the center-right party bloc was less than 1.5 percentage points in 6 out of 20 post-war elections.

The financial stakes of the two reforms are quite similar. The cuts in financial support to parents with young children as suggested in 1994 would have cost parents in the treatment group an estimated €370 million per year.[19] The estimated cost to taxpayers of the fee cap reform as suggested in 1998 was €360 million per year.[20] By dividing the effect on the government budget with the number of votes that were swayed by these election promises, we obtain crude estimates of the cost per vote per year of about €4400 in the 1994 election and about €4200 in the 1998 election, corresponding to €16,800–€17,600 over a four-year electoral period. Previous studies of pork-barrel spending have analyzed voting at a district level as opposed to our individual-level analysis and have estimated the cost of a vote at about $14,000 (Levitt and Snyder, 1997) and between $2000 and $13,000 (Leigh, 2008). Therefore, the estimated costs of a vote over an electoral term seem to be somewhat smaller in the United States and Australia than in Sweden.

If the government actively tries to target benefits to the most responsive group (as suggested by models of tactical redistribution), the treatment effect cannot be generalized to other groups. In our setting, however, the fact that parents

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[18] These estimates should obviously be taken with great caution. There may for instance be a counterweighing effect if other voters expect their tax burden to change as a consequence. Our estimates are based on the assumption that voting responses to the diffuse costs and benefits to other voters are negligible. In any case, qualifications along these lines do not affect our interpretation that people vote their pocketbooks.

[19] Our cost calculation is based on budgetary motions, complemented with numbers from Statistics Sweden and the Swedish Social Insurance Agency.

[20] The cost calculations are from the Ministry of Education and Research. This cost refers to the changes in child-care fees that were promised in the 1998 election manifesto and relies on the assumptions that labor supply and child-care demand were unaffected by the reform. Since Lundin et al. (2008) show that the reform had no effects on the labor supply, the cost of the reform is probably underestimated since the reform seems to have increased the demand for child care.

with young children were treated negatively before they were treated positively makes it less likely that they were targeted because they were unusually responsive to transfers. Neither do the economic resources and the political influence of the treatment group suggest that it should be disproportionately able or unable to tilt redistribution in its favor (Karabarbounis, 2011). Furthermore, most voters belong to our treatment group of parents with young children at some point in their lives; indeed, the results for such a group are important due to its sheer size.

At the end of the day, we expect promises to influence voting when they are regarded as credible. In Sweden, both the Social Democrats and the four center–right parties in the Alliance for Sweden have fully or partly implemented between 80% and 90% of their promises (Naurin, 2014). A clear majority of election promises (50–84%) have also been kept in many other countries, including Canada, Great Britain, Ireland, the Netherlands and the United States (Mansergh and Thomson, 2007). These numbers suggest that our results could plausibly apply to many other Western democracies. Future research should examine whether this is the case.

Acknowledgments

Appendix A. Theoretical model of prospective and retrospective pocketbook voting

In this appendix, we present a simple theoretical model that clarifies the definitions of prospective and retrospective pocketbook voting as well as the hypotheses to be tested. We take as our starting point a probabilistic voting model, and modify it to allow voters to form their expectations based on both promises and past and present policies, while the canonical probabilistic voting model (see Persson and Tabellini (2000, pp. 52–58)) assumes that the winning candidate has to implement his or her platform.

There are two parties, L and R. We analyze the likelihood that citizen j, belonging to an identifiable group m, votes for party L. The group index captures the fact that some transfers are targeted to specific groups, such as pensioners, people registered as unemployed, or—as in our empirical investigation—parents of young children. A period is defined so that it includes both an electoral campaign and the subsequent parliamentary term. The political parties present their platforms at the beginning of the period. After this, citizens vote and the winner forms the government. Group-specific monetary benefits for members of group m in period t are denoted by $B_{m,t}$. The benefits can be negative if the group is a net contributor to government spending. The benefits that members of group m expect to receive if party K, $K \in \{L, R\}$, would win are denoted by $E(B_{m,t}^K)$. This distinction between expected and realized benefits allows us to analyze non-binding promises and to define prospective and retrospective pocketbook voting. We also include a general popularity parameter in favor of party L in period t, denoted by ρ_t. Popularity may thus vary between elections and can take either positive or negative values. It reflects issues like perceived competence or likability of current party leaders. Finally, citizens have individual-specific ideological preferences. Without loss of generality, we denote ideological preference of citizen j in favor of party L by σ_j, negative values implying an ideological preference in favor of party R. The ideological preference incorporates issues such as foreign policy, environmental regulation and civil liberties. Voter j votes for party L if

$$E(B_{m,t}^L - B_{m,t}^R) + \rho_t + \sigma_j > 0. \tag{A1}$$

We allow citizens' expectations concerning policies targeting their group to depend on promises and past policies. Party K's promised benefits to group m are denoted by $P_{m,t}^K$. Past policy refers to the historical record of benefits implemented by the party during its most recent period in office. For formal definition, we introduce an indicator variable θ_t^K which is equal to one if party K formed the government in period t and equal to zero otherwise. We then denote the historical record of group-specific benefits in period t by $H_{m,t}^K = B_{m,t-1}$ if $\theta_{t-1}^K = 1$ and $H_{m,t}^K = H_{m,t-1}^K$ if $\theta_{t-1}^K = 0$. To facilitate empirical

investigation, we assume an additive structure of expectations as a function of the described components:

$$E(B_{m,t}^K) = \lambda_{m,t} P_{m,t}^K + (1 - \lambda_{m,t}) H_{m,t}^K, \tag{A2}$$

where citizens belonging to group m attach weight $\lambda_{m,t}$ to the platform of party K in period t, and the weight $(1 - \lambda_{m,t})$ to the past policies of party K, with $0 \le \lambda_{m,t} \le 1$. Inserting (A2) for parties L and R into (A1), we obtain the following condition for citizen j voting for party L in period t:

$$\lambda_{m,t}(P_{m,t}^L - P_{m,t}^R) + (1 - \lambda_{m,t})(H_{m,t}^L - H_{m,t}^R) + \rho_t + \sigma_j > 0. \tag{A3}$$

According to the inequality in (A3), party choice is determined both by economic and by other considerations, and also depends on how expectations are formed. Prospective pocketbook voting can now be defined as $\lambda_{m,t} > 0$ and retrospective pocketbook voting as $1 - \lambda_{m,t} > 0$, in line with Hypotheses 1 and 2. Thus, prospective and retrospective pocketbook voting may occur at the same time; their relative importance needs to be determined by data.

In a political system with credible party promises, $\lambda_{m,t}$ would approach unity, and citizens would vote based on promises, as in models of pre-election politics. If political promises are instead viewed as cheap talk, $\lambda_{m,t} = 0$, and citizens would base their expectations about future policies on past and present policies, as in models of post-election policies.

In this model, we have taken $\lambda_{m,t}$ to be exogenous. In a richer model, $\lambda_{m,t}$ would be endogenous, and arguably depend on the extent to which promises have been kept in the past. Political institutions such as division of power and rules for government formation could in turn influence the frequency of promise keeping. To address this issue, we would need to model the actions of the government, which is a complication that we can avoid as long as we focus on voting behavior only.

Appendix B. Variables and descriptive statistics

The variables in our data set are taken from the Swedish Election Studies, which are available from the Swedish National Data Service (SND). The data in the Swedish Election Studies were originally collected in a research project at the Department of Political Science at the University of Gothenburg, under the guidance of Sören Holmberg, Mikael Gilljam and Henrik Oscarsson. Neither the SND nor the primary researchers are responsible for the analyses and interpretations presented in this paper. Variable names and definitions are given in Table B1.

Variable means and confidence intervals for the treatment and control groups are given in Tables B2 and B3.

Table B1
Description of variables.

Variable	Definition
Vote for left bloc	1 if voted for the Social Democratic Party or the Left Party; 0 otherwise.
Sex	1 if female; 0 if male.
Relationship status	1 if single; 0 if married or cohabiting with another adult.
Unemployed	1 if the respondent was unemployed or in an active labor market program; 0 otherwise.
Two children	1 if there are two children under age 18 living in the household; 0 otherwise.
Three or more children	1 if there are three or more children under age 18 living in the household; 0 otherwise.
Income (€1000)	Gross earnings, referring to the income year two years prior to the survey year. Register-based. Primary source: Statistics Sweden.
Age	Years of age (used to construct cohort dummies).

Table B2
Variable means for the treatment groups (parents whose youngest child is aged 0–4).

	1991	1994	1998	2002
Vote for left bloc	0.404 (0.353–0.457)	0.494 (0.440–0.549)	0.545 (0.473–0.615)	0.472 (0.404–0.541)
Vote for SDP	0.382 (0.332–0.435)	0.397 (0.345–0.451)	0.420 (0.351–0.492)	0.375 (0.310–0.443)
Sex (1=female)	0.48 (0.43–0.53)	0.53 (0.47–0.58)	0.53 (0.45–0.60)	0.52 (0.45–0.59)
Relationship status (1=single)	0.031 (0.015–0.054)	0.047 (0.027–0.075)	0.055 (0.028–0.096)	0.074 (0.043–0.118)
Unemployed	0.033 (0.017–0.057)	0.103 (0.073–0.140)	0.085 (0.050–0.133)	0.069 (0.039–0.11)
Two children	0.41 (0.36–0.46)	0.41 (0.36–0.46)	0.51 (0.43–0.58)	0.42 (0.35–0.49)
Three or more children	0.25 (0.21–0.30)	0.26 (0.22–0.31)	0.18 (0.13–0.24)	0.22 (0.17–0.28)
Income (€1000)	18.4 (17.2–19.6)	18.0 (17.0–19.1)	20.8 (18.7–22.9)	27.0 (19.0–35.0)
Age	32.1 (31.5–32.7)	32.6 (31.9–33.2)	33.0 (32.2–33.9)	34.4 (33.6–35.1)
Observations	361	340	200	216

Notes: 95% confidence intervals in parentheses. The higher mean income in 2002 is driven by one observation with income $> €900,000$. Excluding this observation reduces the mean income in 2002 to 23.1 (in €1000).

Table B3
Variable means for the control groups (parents whose youngest child is aged 6–11).

	1991	1994	1998	2002
Vote for left bloc	0.387 (0.319–0.458)	0.602 (0.524–0.676)	0.518 (0.446–0.589)	0.525 (0.457–0.593)
Vote for SDP	0.327 (0.262–0.397)	0.485 (0.408–0.563)	0.371 (0.303–0.442)	0.392 (0.326–0.460)
Sex (1=female)	0.48 (0.41–0.55)	0.40 (0.33–0.48)	0.47 (0.40–0.54)	0.49 (0.42–0.56)
Relationship status (1=single)	0.060 (0.032–0.103)	0.099 (0.059–0.154)	0.157 (0.110–0.216)	0.198 (0.147–0.257)
Unemployed	0.020 (0.005–0.051)	0.047 (0.020–0.090)	0.036 (0.014–0.071)	0.051 (0.026–0.089)
Two children	0.50 (0.43–0.57)	0.54 (0.36–0.61)	0.50 (0.43–0.57)	0.51 (0.44–0.58)
Three or more children	0.24 (0.18–0.31)	0.20 (0.15–0.27)	0.26 (0.20–0.33)	0.28 (0.22–0.34)
Income (€1000)	19.9 (18.6–21.2)	20.6 (18.9–22.3)	21.1 (19.5–22.6)	23.7 (20.9–26.4)
Age	41.4 (40.6–42.3)	40.5 (39.5–41.5)	39.3 (38.2–40.3)	40.0 (39.0–41.0)
Observations	199	171	197	217

Note: 95% Confidence intervals in parentheses.

Appendix C. Additional tests

See Tables C1–C4.

Table C1
Dose response test using variation in the age of children in the treatment group. (Dependent variable: vote for left bloc.)

	(1)	(2)	(3)
Election year	1994	1998	2002
Treatment effect (δ)	−0.050 (0.077)	0.068 (0.084)	0.018 (0.087)
Additional treatment effect, children 0−2 ($\Delta\delta$)	−0.097 (0.076)	0.031 (0.085)	−0.067 (0.094)
Time effect (γ)	0.215*** (0.047)	−0.085* (0.050)	0.0076 (0.048)
Pre period diff. treatment gr. 3–5 and control gr. (β_1)	0.016 (0.055)	−0.034 (0.054)	−0.044 (0.056)
Pre period diff. treatment gr. 0−2 and control gr. (β_2)	0.0045 (0.046)	−0.143*** (0.068)	0.033 (0.062)
Control group	0.387*** (0.035)	0.602*** (0.038)	0.518*** (0.036)
Observations	1155	992	897

Notes: Linear probability model. The left bloc includes the Social Democratic Party and the Left Party. The treatment effect for parents whose youngest child is aged 0–2 (3–5) is $\delta+\Delta\delta$ (δ). Since the test divides the treatment group into two parts and estimates two more parameters, the power will unavoidably be quite low. For this reason we include respondents whose youngest child is 5 years old in the treatment group. The control group consists of respondents whose youngest child is aged 6 − 11. The election year of 1994 (1998, 2002) refers to a difference-in-differences estimation of 1994 vs. 1991 (1998 vs. 1994, 2002 vs. 1998). Robust standard errors clustered on individuals in parentheses.
* Statistically significant at 90% confidence level.
** Statistically significant at 95% confidence level.
*** Statistically significant at 99% confidence level.

Table C2
Dose response test using variation in the number of children in the treatment group. (Dependent variable: vote for left bloc.)

	(1)	(2)	(3)
Election year	1994	1998	2002
Treatment effect (δ)	−0.102 (0.065)	0.104 (0.072)	−0.073 (0.074))
Additional treatment effect, 2 or more children aged 0–4 ($\Delta\delta$)	−0.103 (0.084)	0.128 (0.106)	−0.039 (0.122)
Time effect (γ)	0.215*** (0.047)	−0.085* (0.050)	0.0076 (0.048)
Pre period diff. treatment gr. 1 child and control gr. (β_1)	−0.025 (0.046)	−0.077 (0.049)	0.027 (0.054)
Pre period diff. treatment gr. 2 or more child. and control gr. (β_2)	−0.0001 (0.059)	−0.205*** (0.066)	0.028 (0.083)
Control group	0.387*** (0.035)	0.602*** (0.038)	0.518*** (0.036)
Observations	1071	908	830

Notes: Linear probability model. The left bloc includes the Social Democratic Party and the Left Party. The treatment effect for parents with 2 or more children (1 child) is $\delta+\Delta\delta$ (δ). The control group consists of respondents whose youngest child is aged 6 − 11. The election year of 1994 (1998, 2002) refers to a difference-in-differences estimation of 1994 vs. 1991 (1998 vs. 1994, 2002 vs. 1998). Robust standard errors clustered on individuals in parentheses.
* Statistically significant at 90% confidence level.
** Statistically significant at 95% confidence level.
*** Statistically significant at 99% confidence level.

Table C3

Heterogeneous treatment effects with respect to swing voters. (Dependent variable: vote for left bloc.)

	(1)	(2)	(3)
Election year	1994	1998	2002
Age of youngest child	$0-4$ vs. $6-11$	$0-4$ vs. $6-11$	$0-4$ vs. $6-11$
Treatment effect (δ)	-0.007 (0.088)	0.091 (0.097)	-0.124 (0.102)
Treatment effect diff. swing ($\Delta\delta$)	-0.182** (0.086)	0.103 (0.099)	0.071 (0.106)
Time effect (γ)	0.229*** (0.049)	-0.108** (0.051)	-0.008 (0.049)
Treatment group difference (β)	-0.073 (0.078)	-0.145* (0.083)	-0.077 (0.084)
Treatment group diff. swing ($\Delta\beta$)	0.094 (0.087)	-0.002 (0.089)	0.132 (0.095)
Swing	0.034 (0.064)	-0.053 (0.062)	-0.084 (0.057)
Control group (α)	0.375*** (0.059)	0.670*** (0.062)	0.585*** (0.055)
Observations	963	836	805

Notes: Linear probability model. The left bloc includes the Social Democratic Party and the Left Party. Swing voters are defined as voters with a weak ideological conviction. Ideological conviction is measured as the self-reported positioning of respondents on an ideological left-right scale. The scale ranges from 0 to 10 and we classify voters who place themselves between 3 and 7 on this scale as swing voters. The treatment effect for swing voters (non swing voters) is $\delta+\Delta\delta$ (δ). The election year of 1994 (1998, 2002) refers to a difference-in-differences estimation of 1994 vs. 1991 (1998 vs. 1994, 2002 vs. 1998). Robust standard errors clustered on individuals in parentheses.
* Statistically significant at 90% confidence level.
** Statistically significant at 95% confidence level.
*** Statistically significant at 99% confidence level.

Table C4

Estimates of pocketbook voting for the left bloc of parties, excluding respondents with children aged 6–8 years from the control group. (Dependent variable: vote for left bloc.)

	(1)	(2)	(3)
Election year	1994	1998	2002
Age of youngest child	$0-4$ vs. $9-11$	$0-4$ vs. $9-11$	$0-4$ vs. $9-11$
Treatment effect (δ)	-0.197** (0.080)	0.173** (0.088)	-0.083 (0.084)
Time effect (γ)	0.287*** (0.072)	-0.122 (0.077)	0.010 (0.070)
Treatment group difference (β)	0.047 (0.053)	-0.151** (0.061)	0.0223 (0.064)
Control group (α)	0.358*** (0.046)	0.645*** (0.055)	0.523*** (0.053)
Type of voting			
Retrospective voting	Consistent	Inconsistent	Inconsistent
Prospective voting	Consistent	Consistent	Consistent
Observations	886	704	626

Notes: Linear probability model corresponding to Eq. (1). The left bloc includes the Social Democratic Party and the Left Party. The election year of 1994 (1998, 2002) refers to a difference-in-differences estimation of 1994 vs. 1991 (1998 vs. 1994, 2002 vs. 1998). Time effect is an estimate of the change in support for the left bloc in the control group. Treatment group difference is an estimate of the difference in the vote share of the left bloc between the treatment and control group in the first of the two elections that are compared in each column. Control group is an estimate of the fraction of parents in the control group who voted for the left bloc in the first of the two elections that are compared in each column. Robust standard errors clustered on individuals in parentheses.
* Statistically significant at 90% confidence level.
** Statistically significant at 95% confidence level.
*** Statistically significant at 99% confidence level.

Appendix D. Supplementary information

Supplementary data associated with this article can be found in the online version at http://dx.doi.org/10.1016/j. euroecorev.2015.01.010.

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