Economic Studies 113

Mikael Elinder
Essays on Economic Voting, Cognitive Dissonance, and Trust

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Abstract

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This thesis consists of four self-contained essays.

Essay 1: (with Henrik Jordahl and Panu Poutvaara) We present and test a theory of prospective and retrospective pocketbook voting. Focusing on two large reforms in Sweden, we establish a causal chain from policies to sizeable individual gains and losses and then to voting. The Social Democrats proposed budget cuts affecting parents with young children before the 1994 election, but made generous promises to the same group before the 1998 election. Since parents with older children were largely unaffected we use a difference-in-differences strategy for identification. We find clear evidence of prospective pocketbook voting. Voters respond to campaign promises but not to the later implementation of the reforms.

Essay 2: This essay presents a detailed analysis of voters' response to municipality and regional level unemployment and economic growth, in Swedish general elections from 1985 to 2002, using data on 284 municipalities and 9 regions. The preferred specification suggests that an increase in regional growth or a reduction in regional unemployment by one percentage point is associated with an increase in the support for the national government by about 0.6 and 1.0 percentage points. Changes in unemployment and growth at the municipality level seem to have much smaller effects on government support.

Essay 3: One prediction from cognitive dissonance theory is that the act of voting makes people more positive toward the party or candidate they have voted for. Following Mullainathan and Washington (2008), I test this prediction by using exogenous variation in turnout provided by the voting age restriction. I improve on previous studies by investigating political attitudes, measured just before elections, when they are highly predictive of voting. In contrast to earlier studies I find no effect of voting on political attitudes. This result holds for a variety of political attitudes and data from both Sweden and the United States.

Essay4: (with Niclas Berggren and Henrik Jordahl) We conduct an extensive robustness analysis of the relationship between trust and growth by investigating a later time period and a bigger sample than in previous studies. In addition to robustness tests that focus on model uncertainty, we systematize the investigation of outlier influence on the results by using the robust estimation technique Least Trimmed Squares. We find that when outliers (especially China) are removed, the trust-growth relationship is no longer robust. On average, the trust coefficient is half as large as in previous findings.

To my grandmothers

Mary-Ann and Viola

– for introducing me to research and politics

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On August 13, 2003, I made two big investments. I started the Ph. D. program and I bought a house. In terms of monetary returns it still seems as if buying a house was the better investment. But, as everyone knows maximizing utility is not equivalent to maximizing revenue.

Did I maximize my utility by writing a thesis in economics? Yes, in fact, I think so. The reason is that I find it hard to imagine that I could have met so many thought-provoking, funny, loving, and trustworthy people anywhere else. A countless number of colleagues and friends have generously done their best to help me with my research.

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Most of this thesis has been written under surveillance of my roommate Håkan. You have provided me with all the small pieces of advice, facts, critical comments, and pushes in the right direction that I have needed. But, most importantly, our daily discussions of every giant and Lilliputian aspect of life make my days.

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My brother, Martin, taught me early on to don't go with the flow, something that I have tried to bring with me. My final thank goes to my parents Christer and Birgitta. Your trust and support provide me with more opportunities than I can make use of.

A sunny day in Länna, October 2008

Mikael Elinder

Contents

Introduction

As the title of this thesis suggests, the four essays address research questions in three different fields. All four essays use empirical approaches or econometric methods that are novel to the problems they attempt to shed light on. The essays address questions such as why citizens vote as they do, how political attitudes are formed, and if trust among people leads to higher economic growth. Below, I briefly introduce the three research fields to which the essays belong, together with a few words about how this thesis contributes to these fields.

Economic voting

About 50 percent of the total value of the yearly production in Sweden is collected in taxes. Although, this share is high by international standards, all governments in modern democracies collect substantial amounts of taxes. Through elections, the power to tax and to spend the tax revenue is given to elected politicians. How much taxes governments collect and how they spend the revenue ultimately depends on how citizens vote.

The term economic voting refers to the study of how economic concerns influence voting decisions. Essays 1 and 2 contribute to this literature by investigating to what extent citizens are motivated by self-interest, if they respond to election promises, and if economic conditions at the municipal and regional levels influence how citizens vote.

This field of research owes much to the contribution of Anthony Downs. In *An Economic Theory of Democracy* (1957) he provided the analytical framework, that is still used today, for analyzing elections as an interplay between politicians and voters. Downs also emphasized the role of economic concerns in voting decisions, as opposed to regarding voting decisions as determined solely by socioeconomic background or by ideology.

Self-interested voting is a key assumption in many theoretical models in political economics. But, critics argue that the empirical support for self-interested voting is weak overall. To evaluate the relevance of models based on self-interested voting, and to understand why we see some public policies but not others, we need to know to what extent self-interest explains how citizens vote.

Essay 1 (together with Henrik Jordahl and Panu Poutvaara) investigates to what extent citizens tend to vote for parties that provide policies that are beneficial to themselves. By focusing on two large reforms in Sweden, we establish a causal chain from policies to sizeable individual gains and losses and then to voting. Before the 1994 election, the Social Democrats proposed budget cuts with negative economic effects for parents with young children, but made generous promises to the same group before the 1998 election.

Since parents with older children were largely unaffected by the policies, we can estimate the extent of selfish, or pocketbook, voting by comparing voting responses by these two groups of parents.

We find clear evidence that people vote for the party that is most beneficial to their personal finances. Since the parents respond already to the campaign promises but show no additional response after the reforms have been implemented, we can also conclude that election promises influences voting behavior.

Essay 2 investigates a somewhat different matter, how the state of the economy influences the reelection probabilities of the incumbent government. Politicians who handle the economy competently should, if elections work well, face a higher reelection probability than politicians who handle the economy incompetently.

In 1971, Gerald Kramer showed that, in the elections to the U.S. House of Representatives, the party of the President gets an electoral boost if the national economy has improved during the previous year. Following Kramer, numerous studies show that improved macroeconomic conditions benefit governments in many countries and political systems. Ron Johnston and Charles Pattie (2001), however, show that in the 1997 General Election, British voters seemed to respond more strongly to economic conditions at the regional level than at the national and individual levels. Their results indicate that previous studies of the impact of national economic conditions on voting may have been confounded by responses to economic conditions at lower geographical levels. Furthermore, it raises the question why voters would care more about how their own region is doing than how the national economy is doing. In their analysis, the regional level is not defined and the analysis is based on the respondents' subjective assessment of economic conditions, rather than on real economic conditions. Each respondent in their data is asked to rate the economic conditions in "their part of Britain".

I investigate how regional and municipal economic conditions influence voting in the national elections in Sweden between 1985 and 2002. The geographic levels are clearly defined as the regions consist of the areas covered by the regional news, broadcast by the Swedish public television (*Regionalnyheterna,* SVT). Furthermore, the analysis is based on objective economic conditions rather than subjective assessments. I find that the vote share for the central government increases in regions that experience a decrease in unemployment or an increase in economic growth. Changes in economic conditions at the municipal level seem, however, to have much smaller or no effects on the vote.

Cognitive dissonance

In 1957, Leon Festinger published *A Theory of Cognitive Dissona*nce, a book that came to have a dramatic influence on social psychology. Over the years it has been cited more than 7,000 times. For a long period however, the

theory did not attract much interest among economists. But, after George Akerlof and his colleague William Dickens published an article in 1982, in which they argue that cognitive dissonance theory could provide important insights into several economic problems, the theory began to attract the interest of economists as well.

The theory states, in essence, that when you do something that you do not think is right, your behavior is dissonant with your attitudes. Dissonance is experienced as a negative feeling. According to Festinger's theory, people often reduce dissonance by changing their attitudes instead of their behavior. To give an example, suppose you are a smoker and a new study shows that smoking is even worse for your health than you previously believed. How do you react? According to cognitive dissonance theory, you could convince yourself that life is anyway much more fun when you are young, and that smoking is part of the reason. As a result you become more positive toward smoking than you were before the report. Economists, on the other hand, would typically refer to rational choice theory and say that the report makes you more aware of the negative sides of smoking and that you tend to reduce your smoking. The fact that cognitive dissonance theory and rational choice theory often predict opposite reactions makes it very interesting to test which of the theories that gets support in the data in different contexts.

Essay 3 investigates whether the act of voting makes people more positive to the party they have voted for. If this is the case, as cognitive dissonance theory suggests, then it is reasonable to assume that citizens tend to vote for the same party in the future even if that party has proved to be worse than previously thought. To give a specific example, in his overview of cognitive dissonance research, Joel Cooper argues that Bill Clinton increased his popularity in 1998, partly because of the Lewinsky scandal. According to Cooper, those who had voted for Clinton experienced dissonance and justified their previous vote by convincing themselves that Clinton was indeed very competent as President and that private life and politics should not be mixed up. As a consequence, they became more positive toward Clinton. This story suffers from the same two problems as previous studies that have investigated whether voting has an effect on political attitudes and on future voting. The first problem is that it is difficult to know how Clinton's popularity had developed if the Lewinsky scandal had not happened. It is plausible, to think that it had improved even more and that the causal effect of the scandal on Clinton popularity was negative. This problem is credibly accounted for in a study by Sendhil Mullainathan and Ebonya Washington (2008). They find that, two years after Presidential elections in the United States, those who were eligible to vote in the previous election and are affiliated with the President's party are more positive toward the President than those who are also affiliated with his party but were ineligible to vote.

The second problem is that we do not know if cognitive dissonance theory has any implications for future voting. For cognitive dissonance

theory to be of relevance to voting and elections, we would like it to explain political attitudes when it is time to vote. If the act of voting does not influence the political attitudes that are subsequently expressed in the next vote, then cognitive dissonance theory has limited relevance in the context of voting and elections.

In essay 3, I try to jointly address both of these problems, by using a similar empirical strategy as Mullainathan and Washington. However, unlike them, I analyze political attitudes just a few weeks before elections instead of two years before elections. At this time political attitudes are highly predictive of party choice. I do not find any effect of voting on political attitudes, as measured just before elections. This holds both for parliamentary elections in Sweden and for Presidential elections in the United States, and the result is not sensitive to what measure of political attitudes that is analyzed. All specifications indicate that previous voting does not have an effect on political attitudes just before the subsequent election. These results, therefore, cast some doubt on the relevance of cognitive dissonance theory to voting and elections.

Trust

The term social capital was rarely used in the social sciences before James Coleman published a seminal article in 1988. Since then, there has been an intense academic discussion of what social capital is, its causes, and its consequences.

Economists have come to pay substantial attention to the role of social capital in the economy. Social capital often refers to the ability of people to cooperate, even when no one has a narrow incentive to do so. A particular aspect of social capital that has attracted much interest among economists is the extent to which people trust others – especially people they know little or nothing about.

Paul Zak and Stephen Knack (2001) formulated an economic model in which they showed that when people trust others, economic transactions work smoother, and less resources are spent on checking and controlling the actions of others. As a consequence, more investments can be made, leading to higher future production, and more resources can be devoted to production.

If more people come to trust each other in a society, we would expect that society to experience an increase in economic growth. In a pioneering study, Stephen Knack and Philip Keefer (1997) showed that there seems to exist an empirical relationship between trust and economic growth, indicating that trust could be an important growth factor. The same relationship has later been found also by other researchers, using slightly different methods. These studies, however, all use data from the 1970s or 80s, and from a small number of countries. Many relationships between potential growth factors and economic growth have been found to rely on quite specific assumptions

in the empirical analyses that once identified the relationships. As a consequence, these relationships are often sensitive to which countries that are included in the analysis, or to which confounding factors that are addressed, or both.

In essay 4 (together with Niclas Berggren and Henrik Jordahl), I investigate the robustness of the trust–growth relationship with respect to the time frame of the investigation, the choice of included countries, and the choice of control variables. We find that the relationship is not as robust as claimed earlier. The lack of robustness stems to some degree from instability over time, but especially from sensitivity to which countries that are included in the analysis. It should, however, be pointed out, that this does not necessarily imply that trust is unimportant for economic growth.

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Essay I

Selfish and prospective:
Theory and evidence of pocketbook voting

(together with Henrik Jordahl and Panu Poutvaara)

1. Introduction

The idea that voting behavior is determined by material self-interest is a fundamental assumption in numerous economic models, with the Meltzer and Richard (1981) paradigm of redistribution as a prominent example. In contrast, the empirical evidence in favor of self-interested voting is surprisingly weak. Lewis-Beck and Stegmaier (2007) note that there are about 400 published studies on economic voting, and conclude that the cumulative support for "pocketbook voting" is marginal at best. Pocketbook voting usually refers to self-interested economic voting in general, but the empirical tests have typically relied on the specific assumption that voters attribute all changes in their financial situations to the policies of the incumbent government.[1] While judging the incumbent government by one's own pocketbook can be a useful information shortcut in some cases, there are plenty of confounding factors. For example, it appears implausible that a voter should be more inclined to support the incumbent government if she earns more money after having graduated, or for some other reason that is unrelated to government policy.[2] In this paper we introduce a new approach to investigate the empirical relevance of pocketbook voting.

We start out by presenting a theory of pocketbook voting in which citizens respond to the influence of proposed or implemented policies on

[1] Empirical studies in this literature include Fiorina (1978), Kinder and Kiewiet (1979), Markus (1988, 1992), Alvarez and Nagler (1995, 1998), Nadeau and Lewis-Beck (2001), and Jordahl (2006).

[2] Gomez and Wilson (2001) acknowledge the diffuse attribution in traditional tests of pocketbook voting, but rather than focusing on how policies affect personal finances, they argue − somewhat paradoxically − that only the politically sophisticated let the development of their personal finances determine how they vote.

their personal economic situation. The model allows for both prospective and retrospective voting. Prospective voting means that voters base their expectations of future policies on electoral platforms. Retrospective voting means that they base them on past policies.[3]

In the empirical investigation we establish a causal chain from policy proposals to sizeable individual gains and losses, and in a second step from individual gains and losses to voting. As mentioned above, this link from expected policies to personal finances has been missing in previous empirical studies. We identify a group of voters who are affected by two significant policy shifts. Parents with young children were singled out for a relatively unfavorable treatment according to the electoral platform of the Swedish Social Democratic Party before the 1994 election, but were given especially generous promises by this party just before the 1998 election. By comparing parents with young and parents with older children (who were largely unaffected by the policy changes) we obtain exogenous variation in the gains and losses from the reforms.

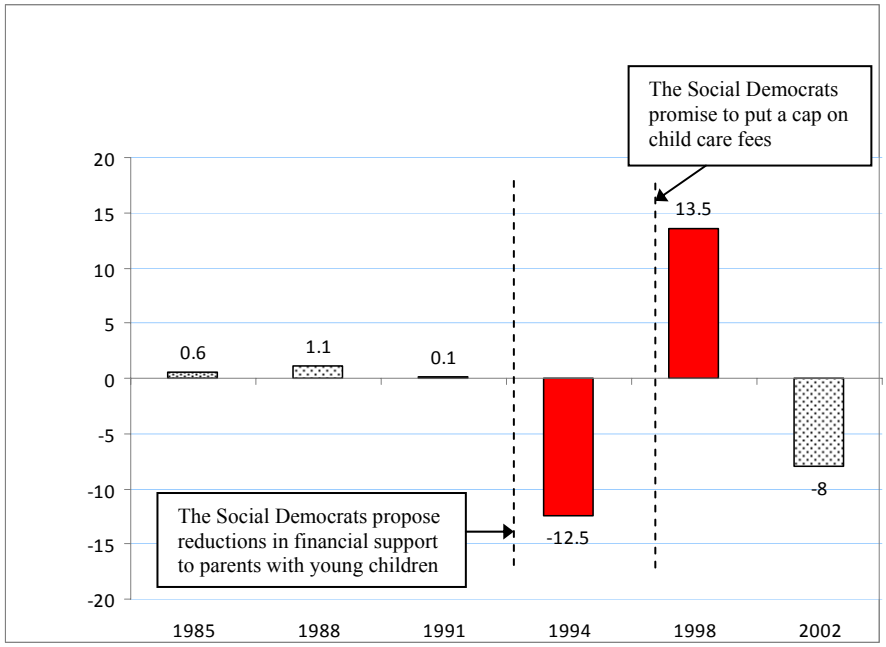
Since both reforms were implemented during the following parliamentary term we discriminate between prospective and retrospective voting by tracking the votes of parents with young and parents with older children over several elections. The emerging voting pattern provides striking evidence of prospective pocketbook voting. Parents with young children respond when the reforms appear as election campaign promises, and not when the reforms are later implemented.

Our main findings are summarized in Figure 1. In the figure, we depict difference-in-differences estimates for each Swedish national election between 1985 and 2002. The elections before 1994 serve as placebo treatments and show that the treatment group of parents with children aged 0−4 voted almost identically as the control group of parents with children aged 6−11. In the 1994 election campaign, the Social Democrats proposed major cuts in the financial support to parents with young children. This group was then significantly less likely to vote for the political left, the estimated treatment effect being −12.6 percentage points. In the 1998 election campaign the Social Democrats instead made a generous promise to the same group in the form of a cap on child care fees. Parents with young children responded to this promise with a positive treatment effect of 13.5 percentage points. Taken together, the negative response in 1994 and the positive response in 1998 provide evidence of prospective pocketbook voting. If parents would have voted retrospectively, the treatment group should have reduced its support for the political left in 1998, in response to

[3] Hibbs (2006) provides an informative overview of retrospective and prospective voting in macroeconomic models. MacKuen et al. (1992) show that the time series of presidential approval can be accounted for by the prospective economic expectations of American voters, but not by their retrospective economic experiences.

the cuts implemented after the 1994 election. Also the negative, but statistically insignificant point estimate in 2002 is inconsistent with retrospective voting. Since the cap on child care fees was implemented in the election year of 2002, parents with young children should have increased their support for the political left in this election if they would have voted retrospectively.

Figure. 1. Difference-in-differences estimates of left bloc vote share between the treatment group (youngest child 0−4 years old) and the control group (youngest child 6−11 years old) in percentage points. The left bloc includes the Social Democratic Party and the Left Party.



While our study of two groups of parents is primarily designed to identify the causal effect of public expenditures on individual voting, some tentative calculations show that pocketbook voting may be an important determinant of election outcomes. At face value, our estimates suggest that the generous promise to parents with young children resulted in an electoral gain of approximately 1.5 percentage points for the left bloc of parties in the 1998 election. Such an increase exceeds the margin of victory in 5 out of 19 Swedish postwar elections.

A few previous studies provide related evidence from varying applications and settings. To start with, self-interested voting is a keystone in theoretical models of tactical redistribution (Lindbeck and Weibull, 1987; Dixit and Londregan, 1996). Recent empirical contributions provide empirical support for the prediction that governments tactically target redistribution to swing voters (Dahlberg and Johansson, 2002) and to

informed voters (Strömberg, 2004), suggesting that influential politicians believe that citizens vote their pocketbooks. Levitt and Snyder (1997) find that federal spending on assistance programs in a U.S. congressional district benefits the congressional incumbent electorally. However, they do not find any effect from direct transfers to individuals. From our theoretical perspective of pocketbook voting, one explanation for this could be that they do not control for how incumbents have voted on entitlement programs. Edlund and Pande (2002) provide a motivation for studying pocketbook voting based on the U.S. political gender gap. They trace the rise of this gender gap to the decline in marriage and argue that the rising support for the Democratic Party among women can be explained by selfish preferences for redistribution driven by changes in personal income.

In a recent paper, Richter (2006) analyzes economic voting in the Russian presidential election of 1996, which was branded as a fundamental choice between capitalism and communism. Despite repeated orders by President Yeltsin to pay off wage arrears, every other worker in the private and public sector experienced nonpayments of wages at the time of the election. Richter uses a matching model to demonstrate that workers whose wages were not paid in time were less likely to vote for the incumbent president. Unlike Richter, we study a mature democracy and compare prospective with retrospective voting.

Other explanations of voting behavior include group voting (Mutz and Mondak, 1997), social background (Campbell et al., 1960), beliefs about the causes of income (Fong, 2001; Alesina and Angeletos, 2005), and voting responses to the macroeconomy (Fair, 1978). While we cannot compare the importance of pocketbook voting with other voting models
– macroeconomic voting in particular – an advantage with our empirical strategy is that none of the alternative explanations confounds our estimates.

2. Theoretical framework

Previous empirical studies have been quite vague when it comes to defining prospective and retrospective voting. To formally define those central concepts and to provide structure for the empirical investigation, we start by developing a theoretical framework of pocketbook voting.[4]

We analyze voter choices between two political parties, denoted by L for the party on the left and R for the party on the right. We take as our starting point Persson and Tabellini's (2000) workhorse model of probabilistic voting in which it is assumed that promises made by political parties are always fulfilled. We extend the model by allowing voters to regard promises

[4] The major previous contributions are Downs (1957), Key (1966), and Fiorina (1981). A common feature of these models is that voters do not care about promises. Similarly, the agency approach to elections and politics championed by Besley (2006) leaves no room for promises.

as non-binding. As our focus is on introducing a theoretical framework that allows for empirical tests of different hypotheses about voting behavior, we take party platforms as given. Our approach is compatible with different views on party behavior, including both office-motivations and ideological considerations.

2.1 Voting with given expected policies

For simplicity, we analyze the likelihood that citizen *j*, belonging to an identifiable group *m*, votes for party L. The group index is meant to capture the fact that large amounts of transfers are targeted to specific groups, like parents of young children, pensioners, or people registered as unemployed. We denote the gross income that citizen *j* receives in period *t* by $Y_{j,t}$. 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. During the rest of the period, the government decides on tax schedules and income transfers, and the incomes of the citizens are realized.

We denote the after-tax income that citizen *j* would receive in period *t* if party K, $K \in \{L, R\}$, were in power by $N_t^K(Y_{j,t})$. This net income is calculated in the absence of group-specific transfers, and it does not have group-specific indices, to capture the idea that the income tax schedule is typically independent of age, and many other demographic characteristics that affect transfers. The benefits that members of group *m* would receive if party K were in power are denoted by $B_{m,t}^K$. We also allow for a general popularity parameter in favor of party L in period *t*, denoted by κ_t. Popularity is allowed to vary between elections and could take either positive or negative values. It reflects issues like perceived competence or likability of current party leaders.

Finally, citizens have (unobservable) individual-specific ideological preferences. Without loss of generality, we denote the ideological preference of citizen *j* in favor of party L by z_j, negative values implying an ideological preference in favor of party R. The ideological preference incorporates different ways in which the parties may differ. In addition to economic policy, voters may care, to varying extents, about social issues, environmental regulation, and foreign policy. In line with Persson and Tabellini (2006), we further assume that utility has a linearly additive structure. We can now write the expected utility of citizen *j* in group *m* of having party L in power as

$$E(U_{j,t}^L) = E(N_t^L(Y_{j,t})) + E(B_{m,t}^L) + \kappa_t + z_j. \quad (1)$$

Likewise, the expected utility of having party R in power is

$$E(U_{j,t}^{R}) = E(N_t^{R}(Y_{j,t})) + E(B_{m,t}^{R}). \quad (2)$$

We assume that citizens vote for the political party giving the higher expected utility. From Equation (1) and (2), citizen *j* in group *m* prefers party L if:

$$E(N_t^{L}(Y_{j,t})) + E(B_{m,t}^{L}) + \kappa_t + z_j > E(N_t^{R}(Y_{j,t})) + E(B_{m,t}^{R}). \quad (3)$$

We next define a shorter term to denote the expected monetary difference in after-tax income from party L being in office (relative to party R) for voter *j*:

$$E(M_{j,t}) = E(N_t^{L}(Y_{j,t}) - N_t^{R}(Y_{j,t})). \quad (4)$$

Rearranging (3) and using (4), we obtain

$$E(M_{j,t}) + E(B_{m,t}^{L} - B_{m,t}^{R}) + \kappa_t + z_j > 0. \quad (5)$$

The inequality in (5) tells us that the choice between party L and party R depends on the difference in after-tax income between the L and R platforms, the difference in transfers between the L and R platforms, the general popularity parameter in favor of L, and the citizen-specific ideological preference for L. Citizen *j* votes for L if and only if the sum of these terms is positive. The term $E(M_{j,t}) + E(B_{m,t}^{L} - B_{m,t}^{R})$ measures the pocketbook voting motivation in favor of party L. We simplify the analysis by assuming that the expected monetary difference in after-tax income between the L and R platforms is the same for all individuals, denoted by $\overline{M}$. Under this assumption, we can obtain a cutoff ideological preference parameter $\hat{z}_{m,t}(\overline{M})$ that denotes the group-specific ideological threshold above which a citizen votes for L:[5]

$$\hat{z}_{m,t}(\overline{M}) = -\overline{M} - E(B_{m,t}^{L} - B_{m,t}^{R}) - \kappa_t. \quad (6)$$

[5] At the cost of notational complexity, we could specify groups corresponding to different income ranges. The subsequent analysis would then be written separately for individuals belonging to each income range. In the empirical part of the paper, we first present results when ignoring income differences (arguing that we compare two groups with similar incomes), and then show in appendix B that controlling for income differences does not change the qualitative results.

If we further follow Persson and Tabellini (2000) and assume that the ideological preference parameter z follows a uniform distribution on the interval $\left[\underline{z}, \overline{z}\right]$, we can use equation (6) to calculate the vote share that party L receives. To allow for different ideological preferences in different groups, we assume a group specific uniform distribution of z_m on $\left[\underline{z_m}, \overline{z_m}\right]$. The probability that a citizen belonging to group m votes for party L in the election in period t is given by equation (7):

$$Q_{m,t} = \frac{\hat{z}_{m,t}(\overline{M}) - \underline{z_m}}{\overline{z_m} - \underline{z_m}}. \tag{7}$$

2.2 Policy expectations

So far, we have taken citizens' expectations concerning policies to be implemented as given. The next step is to formalize how expected policies are related to party platforms and previous policies. We allow for four components in citizens' expectations. First, the two parties have an underlying ideology. We denote the monetary benefits that group m expects from party K, based on the latter's ideology, by I_m^K. This permanent component takes into account that some groups of citizens may be associated with one of the parties, like labor unions with the left party and entrepreneurs with the right party, or that some groups expect equally generous transfers from both parties, as might be the case with pensioners.

Second, citizens may react to electoral promises. We measure the promise-dependent expectation component, $G_{m,t}^K$, as the difference between party K's platform, denoted by $P_{m,t}^K$, and permanent ideological component, I_m^K: $G_{m,t}^K = P_{m,t}^K - I_m^K$. The variable $G_{m,t}^K$ thus measures how generous promises party K makes to group m in period t, relative to what group m could expect, based on party ideology.

Third, citizens may at least partly base their expectations on a party's track record of implemented policies. If party K formed the government in period $t-1$, we denote by $H_{m,t-1}^K$ the difference between the transfers that it gave to group m, and what the party's ideology gives reason to expect: $H_{m,t-1}^K = B_{m,t-1}^K - I_m^K$. If party K was in opposition in period $t-1$, we define $H_{m,t-1}^K = 0$. As an opposition party cannot implement policies, citizens cannot observe its realized generosity toward their group.

Fourth and finally, we allow for the possibility that citizens may respond to differences between implemented policies by the governing party and its

previous electoral promises. If party *K* formed the government in period *t−1*, we define as a term measuring deviation between implemented policies and electoral promises: $J^K_{m,t-1} = B^K_{m,t-1} - P^K_{m,t-1}$. If K was not in the government, we define $J^K_{m,t-1} = 0$.

To facilitate the empirical investigation, we assume a simple additive structure of expectations as a function of the described components:

$$E(B^K_{m,t}) = I^K_m + \lambda G^K_{m,t} + \mu H^K_{m,t-1} + \chi J^K_{m,t-1}, \quad (8)$$

where λ is the weight that the citizen attaches to the platform of party *K*, relative to what the citizen expects from party *K* based on ideology, μ measures the extent to which expectations of the governing party's future policies react to its generosity, relative to its ideological standpoint, during the concluding term, and χ measures the extent to which expectation of the governing party's future policy reacts to difference between its past policy and past platform. It is reasonable to expect that $0 \leq \lambda, \mu, \chi \leq 1$, and that $0 \leq \lambda + \mu \leq 1$.[6] It is natural to interpret λ as the likelihood that the winning party will implement its promises. In a political system with a high level of trust and credible party promises, λ would approach unity, while it would be close to zero in the opposite case.

Equation (8) is general enough to allow as special cases different views on political competition and the formation of expectations. If $\lambda = \mu = \chi = 0$, then citizens would ignore both platforms and implemented policies, and base their expectations about future policies solely on the characteristics of the party's permanent ideology. If political platforms are viewed as cheap talk, then λ=0. We find it reasonable that under such circumstances χ=0; if current electoral promises are considered to be cheap talk, so are past promises. If μ=0, then citizens do not change their expectations about future policies of the governing party based on its current policies, while if μ>0 voters expect that future policies are positively correlated with past policies. One way to motivate μ>0 is that citizens may update their beliefs about the competence of the government, based on its past policies. Transfers which are more generous than expected from party ideology may signal either that the governing party is actually ideologically more favorable toward the targeted group than previously thought, or that the current party leadership is exceptionally generous. Exceptional generosity can result from competence, tactical considerations or a combination of the two.

[6] Allowing λ+μ to be larger than one could result in a peculiar case in which the political party in government could change a transfer toward a group and then state that it will maintain the new level of transfers, but with citizens expecting that the party would again change the transfer in the same direction.

Inserting equation (8) into equation (5), we obtain the following condition for voting party L:

$$z_j + E(M_{j,t}) + (I_m^L - I_m^R) + \lambda(G_{m,t}^L - G_{m,t}^R) + \mu(H_{t-1}^L - H_{t-1}^R) + \chi(J_{m,t-1}^L - J_{m,t-1}^R) + \kappa_t > 0. \quad (9)$$

The inequality in (9) implies that citizens have both ideological and economic motivations, and that party choice also depends on how expectations are formed.

2.3 Formalizing the pocketbook voting hypotheses

For the empirical analysis, we can now specify two types of pocketbook voting. We view pocketbook voting as retrospective if $\mu>0$, and as prospective if $\lambda>0$. Pocketbook voting is only retrospective if $\lambda=0$ and $\mu>0$. Note that the idea of prospective pocketbook voting can be expected to imply also that $\chi>0$, i.e. that citizens roll back their support for political parties that do not deliver on their promises.[7] We define prospective voting in a general form; the value of χ determines the degree to which citizens react to deviations from past promises by the government. We formalize the definitions of prospective and retrospective pocketbook voting as:

Definition 1. Citizens engage in retrospective pocketbook voting if $\mu>0$.

Definition 2. Citizens engage in prospective pocketbook voting if $\lambda>0$.

According to definition 1, retrospective pocketbook voting means that citizens react to implemented policies.[8] According to definition 2, prospective pocketbook voting means that citizens are responsive to

[7] If $\lambda>0$ and $\mu = \chi = 0$, voters would be naive in the sense that a political party could increase its vote share by making generous promises to a certain group without ever implementing any promises.

[8] Conceptually, we can distinguish two types of retrospective voting: predictive and reactive. Our theoretical model describes retrospective voting of the predictive type. A predictive retrospective voter − as the name indicates − tries to find out which party she prefers to have in office for the next period. The prediction is based on current policies and not on electoral promises. A reactive retrospective voter is "purely retrospective" in the sense that she rewards or punishes the incumbent government based on how she is affected by policies, regardless of what she expects for the next period of office. As the predictions of the two types of retrospective voting are similar in our empirical setting, we lump them together under the label of retrospective voting throughout our empirical investigation.

promises by political parties.[9] The definitions imply that prospective and retrospective voting may coexist. It is left for the empirical analysis to evaluate their relative strength in cases where they push for different voting behavior. Therefore, definitions 1 and 2 translate into two testable hypotheses:

Hypothesis 1. Assume that the government cuts (increases) benefits targeting group *m*, while it does not change benefits targeting group *n*. Then the electoral support for the government party decreases (increases) in group *m*, relative to group *n*, in the subsequent election.

Hypothesis 2. Assume that party K proposes cuts (increases) in benefits targeting group *m*, while it does not propose changes in benefits targeting group *n*. Then the electoral support for K should decrease (increase) in group *m*, relative to the support in group *n*, in the current election.

2.4 From theory to empirics

To start with, the fact that politics is multi-dimensional provides a major empirical challenge. In order to test our hypotheses and estimate the impact of a given policy, one should compare voting behavior between individuals who are similarly affected by other policies than the one under study. Furthermore, in the counterfactual absence of the policy change, the affected citizens should have voted exactly as the citizens in the comparison group did. We have noted that the total pocketbook motivation in favor of party L is $E(M_{j,t}) + E(B^{L}_{m,t} - B^{R}_{m,t})$. Our empirical approach will be to focus on the second term (transfers), and try to hold the first term (taxes) constant. This is accomplished by investigating voting responses to child care reforms among parents with young children compared with responses of parents with somewhat older children who were largely unaffected by the same reforms. The underlying idea 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). From now on we denote the treatment group by T and the control group by C. Without loss of generality, and in line with the empirical analysis, we focus on the

[9] Parties have to earn credibility by keeping previous promises, and if they do not we expect λ=0. Although this reasoning implies that previous promises and implemented policies will feed into λ, we keep this parameter exogenous in the model. Credibility may change slowly and is in any case fixed in a given election. In consequence, we expect retrospective voting to be relatively more important in an environment of broken promises. An intriguing topic would be to estimate responses to broken promises. Since we study reforms that were implemented as promised, this has to be left for future research.

case in which party L changes its platform and subsequently implements this change.

To connect our theoretical framework with the empirical difference-in-differences specification, we assume that the vote share of party L developed similarly in the treatment and in the control group before the policy change appeared as a new part of an election platform.[10] Therefore, the ideological cutoff parameter that the individuals belonging to these groups face is the same before the treatment, for any given level of monetary difference in after-tax earned income: $\hat{z}_{T,t-1}(\overline{M}) = \hat{z}_{C,t-1}(\overline{M}) = \hat{z}_{t-1}(\overline{M})$. We also assume that the individual-specific ideological preference parameters follow the same uniform distribution in the treatment and control groups, so that $\underline{z}_C = \underline{z}_T = \underline{z}$ and $\overline{z}_C = \overline{z}_T = \overline{z}$. We can now write the probability that a voter belonging to the treatment (or control) group votes for L in period t−1 as

$$Q_{T,t-1} = Q_{C,t-1} = \frac{\hat{z}_{t-1}(\overline{M}) - \underline{z}}{\overline{z} - \underline{z}}. \tag{10}$$

As we analyze elections in which party R did not change its electoral platforms toward the treatment group or control group, and party L did not change its policies toward the control group, we assume that $E(B^R_{T,t-1}) = E(B^R_{T,t})$, $E(B^R_{C,t-1}) = E(B^R_{C,t})$, and $E(B^L_{C,t-1}) = E(B^L_{C,t})$. The probability that a citizen belonging to the control group votes for L in period *t* is now

$$Q_{C,t} = \frac{\hat{z}_{t-1}(\overline{M}) - \underline{z}}{\overline{z} - \underline{z}} + \frac{\kappa_t - \kappa_{t-1}}{\overline{z} - \underline{z}}. \tag{11}$$

The second term in equation (11) captures the effect of change in the general popularity parameter in favor of L. The probability that a voter belonging to the treatment group votes for L is now

$$Q_{T,t} = \frac{\hat{z}_{t-1}(\overline{M}) - \underline{z}}{\overline{z} - \underline{z}} + \frac{\kappa_t - \kappa_{t-1}}{\overline{z} - \underline{z}} + \frac{E(B^L_{T,t} - B^L_{T,t-1})}{\overline{z} - \underline{z}}. \tag{12}$$

The third term of equation (12) captures the effect of changes in the expected benefit from L. Our basic difference-in-differences model can be written as

[10] Note, in passing, that this assumption is satisfied in our empirical analysis.

$$Vote_{i,t} = \alpha + \beta \cdot T_i + \gamma \cdot p_t + \delta \cdot T_i \cdot p_t + \varepsilon_{i,t}\,, \tag{13}$$

where $Vote_{i,t}$ is a dummy variable which equals 1 if at time period t (t=1,2), respondent i votes for L. T_i is a dummy variable taking the value 1 if the respondent belongs to the treatment group, and p_t is a time period dummy that takes the value 0 in the election before and 1 in the election after the proposal or the policy change under study. 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 that vote for L in the first period, β accounts for differences in support for L in the first election between the treatment and control group. γ captures the change in voting between the first and second election in the control group, and $\varepsilon_{i,t}$ is an error term. The parameters α, β, γ and δ arise from equations (10), (11), and (12) in the following way:

$$\alpha = \frac{\hat{z}_{t-1}(\overline{M}) - \underline{z}}{\overline{z} - \underline{z}}; \beta = 0; \gamma = \frac{\kappa_t - \kappa_{t-1}}{\overline{z} - \underline{z}}; \delta = \frac{E(B^L_{T,t} - B^L_{T,t-1})}{\overline{z} - \underline{z}}. \tag{14}$$

Inserting equation (8) into the expression for δ in equation (14) gives

$$\delta = \lambda \frac{G^L_{m,t} - G^L_{m,t-1}}{\overline{z} - \underline{z}} + \mu \frac{H^L_{t-1} - H^L_{t-2}}{\overline{z} - \underline{z}} + \chi \frac{J^L_{m,t-1} - J^L_{m,t-2}}{\overline{z} - \underline{z}}. \tag{15}$$

In equation (15), the first term measures the prospective voting response to changes in party L's platform. The second term measures the retrospective response to the generosity of party L if it were in the government. Note that H^L_{t-2} refers to the generosity before the previous election, if L was in government at that time. This term captures the idea that voters reacted to what happened in period *t−2* already in the election at the beginning of period *t−1*, and no longer in the election in period *t*. One potential explanation for such a limited memory is that politicians change over time, so that voters might not react to what happened in the more distant past. The third term refers to potential voter reactions in case L has deviated from its promises when being in government.

3. Two reforms affecting families with young children

Sweden is a welfare state characterized by high taxes and generous welfare benefits even compared with other Nordic countries. For decades, the Social Democratic Party has been the largest party and has usually formed a minority government supported by the (formerly Communist) Left Party. Since World War II, the centre-right parties received enough seats in

Parliament to form a government only in four general elections (1976, 1979, 1991, and 2006), most of the time as a majority coalition.

We make use of the fact that parents with young children were promised diametrically different treatments in the 1994 and 1998 election platforms of the Social Democratic Party, whereas parents with older children were largely unaffected by these proposals. The details of this empirical strategy are spelled out in section 4. Next, we give a brief description of the two reforms that appeared as election promises in 1994 and 1998, focusing on how the incidence differed between parents with younger and parents with older children. Both reforms were at the center of the national election campaigns in which they appeared, making it very likely that most citizens were aware of them.

3.1 Financing the budget deficit

In the early 1990s the Swedish economy ended up in a severe crisis combining a surge in unemployment and huge budget deficits. As a result, the political parties tried hard to persuade voters in the 1994 election campaign that they were able to deal with the budget crisis. The main proposals of the centre-right coalition government were cuts in pension benefits and a promise to be tougher on tax evasion and fraudulent behavior in the social welfare systems. Apart from a traditional, unspecific proposal of tax increases for high income earners, the Social Democratic Party also proposed an unexpected and detailed way of fighting the budget deficit: major cuts in financial support to parents with young children. Three of the proposals were particularly disadvantageous for parents with young children.

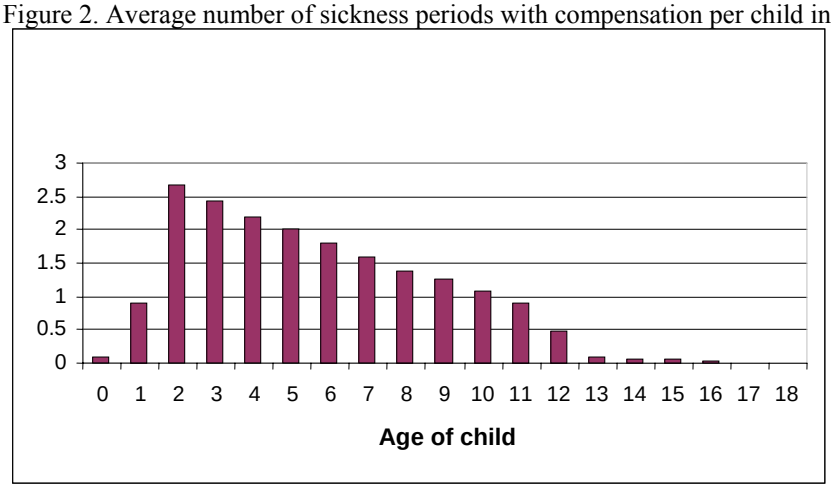
First, the centre-right government coalition had introduced a child-raising allowance in 1994. Families with children between one and three years old who did not use publicly subsidized child care received a cash transfer of € 220 (net of tax) per month.[11] Both the Social Democratic Party and the Left Party opposed the introduction of the child-raising allowance and declared that they would repeal this law if they won the election. They won and the child-raising allowance was abolished already from January 1, 1995.

Second, parents who care for sick children are entitled to compensation for lost labor income from the social insurance system. In the election campaign of 1994 the Social Democrats declared that they would introduce a day of qualification before compensation could be claimed so that the first day of caring would not be compensated. The proposal met severe criticism since it was claimed that lone mothers would suffer hard from this change. But the Social Democrats insisted that it was needed to balance the budget. At the time of the election in 1998 this part of the reform had not been implemented.

[11] 1€ equals about 9 SEK. Families with children who participated part time in public child care were entitled to a smaller subsidy.

Figure 2 shows the average number of qualification days that would be imposed on parents with one child if the sickness pattern of children would be the same, after the introduction of the qualification day, as it was before. Clearly, parents with young children have more to lose from a day of qualification, since young children are sick more often than older children.[12] A typical worker who stays at home 2.5 times per year would loose about € 130 per year (net of tax) if this change would be implemented.[13]

Figure 2. Average number of sickness periods with compensation per child in 1994.



Source: The Swedish Social Insurance Agency.

Third, in the summer of 1994 the Social Democrats unexpectedly proposed to reduce the compensation level in the parental insurance from 90 to 80 percent after the election.[14] Such an initiative was at odds with a long left bloc tradition of generous compensations to parents but was well in line with most of the centre-right parties' policies, and can definitely be seen as a change in relative positions.[15] After the election, the compensation was reduced to 80 percent from January 1995. For a parent working in the private

[12] The reason why there are so few sickness periods for children up to one year old is that one parent is typically not working and hence not entitled to compensation when caring for a sick child.

[13] Based on the average daily salary for private sector workers in 1995, which was € 76, and a 30 percent tax rate.

[14] The Swedish parental insurance is very generous compared with other European countries. In 1994 parents could claim compensation for about one year's loss of earnings when staying at home with a newborn child. The compensation can be spread out over many years. In practice, however, a large majority of the compensation is claimed during the child's first or second year.

[15] The Liberal Party initially opposed the reduction in the compensation level, but after the Social Democrats had reached an agreement with the other centre-right parties they finally also supported the agreement.

sector this implied a loss of € 107 per month net of tax, during parental leave.[16]

Together these three proposals constituted a reform which would significantly worsen the economic situation of parents with young children. The repeal of the child-raising allowance affected parents with children between 1 and 3 years. The introduction of a qualifying day for benefits when taking care of sick children would also primarily affect parents with young children. The reduction of benefits in the parental insurance almost exclusively affected parents with children aged 0 to 2. There are strong reasons to believe that the described proposals mattered to voters. In a description of the 1994 election, Widfeldt (1995) highlights that promises of spending increases were absent in the campaign and that especially the Social Democratic Party's proposal to cut the financial support for families with young children led to an intense debate.

3.2 The cap on child care fees

In the election campaign of 1998, the Social Democratic Party promised to introduce a fee cap on child care services that would substantially reduce the child care fees for most families with young children. This reform targeted a well defined group of voters with a high take-up rate: in 1998 about 73 percent of all children aged 1 to 5 participated in publicly subsidized child care.[17] Although child care fees had been discussed for some time, the electorate and even most Social Democrat ministers were surprised when a tangible reform proposal was presented as an election promise in the party's election manifesto. The manifesto had been prepared secretly by the Prime Minister and a few confidants, and was published one month before Election Day. It included the following promise:

> No other group has carried the burden of the economic crisis as heavily as families with children. Not even the most unprotected and innocent – the children – were unaffected by the economic crisis. We have therefore increased child benefits and introduced free medical services for children. To further improve the situation of families with children, and to encourage more parents to start working or to increase their hours of work, we would like to implement a cap on child care

[16]Based on a daily salary of € 76, 20 working days per month, and 30 percent tax rate,

[17] This statistic comes from the Swedish National Agency for Education (1999). Note however that the fraction of children who participate in publicly subsidized child care between the age of 2 and 5 is even larger than 73 percent since most of the children who do not participate in child care at a given time are under two years old. We do not take a stance on the welfare effects of child care. For a recent analysis on the labor supply and family welfare effects of the introduction of a highly subsidized child care in Quebec, see Baker et al. (2008).

> fees in the entire country at SEK 700 [€ 78 per month] for the first child and with lower fees for subsequent children. The reduction in child care fees should be at least SEK 200 [€ 22] for everyone. A fee cap should also be introduced in after school recreation centers at SEK 500 [€ 56]. The municipalities will be compensated for their loss of revenues.[18]

The opposition did not support the idea of a cap on child care fees in the election campaign of 1998. In November 2000, the parliament voted in favor of introducing the fee cap reform.[19] The parties belonging to the centre-right bloc presented different alternatives but did not agree on any counter-proposal. In his note on the 1998 election, Arter (1999) brings up the promise to reduce day care fees as one of the most important issues of the campaign.

The reform was implemented on January 1, 2002. All municipalities but two implemented the fee cap from the beginning and the remaining two did so one year later.[20] The fee cap was set to a maximum of three percent of family gross income or € 127 for the first child, two percent or € 84 for the second child, and one percent or € 42 for the third child, granted that they were all in child care. For additional children, child care was made free of charge.[21]

The fee cap substantially reduced the child care costs of many families. A family with a "normal" income and two children in child care 33 hours per week gained € 113 per month in the municipality with median fees and € 238 in the municipality with the highest fees in 1999.[22] These gains constitute approximately five and ten percent of the family's monthly net income.

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[18] Taken from the 1998 election manifesto of the Social Democratic Party, "Med omtanke om framtiden – Socialdemokraternas politik inför 2000-talet". (Our translation, the original text is in Swedish.) The mentioned changes in child benefits and medical fees had marginal economic consequences and also did no differ much between parents with young and parents with older children.

[19] Although the Left Party and the Green Party initially were against the reform, they voted with the Social Democrats.

[20] Child care is the responsibility of municipalities, but by compensating for lost revenues, the central government gave each municipality a strong incentive to adopt the cap on child care fees.

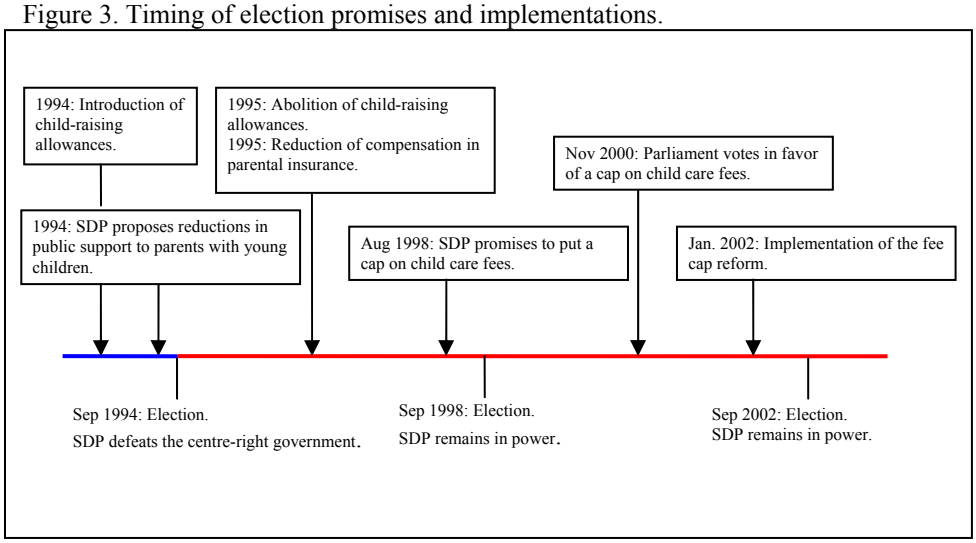
[21] Parents with children in after-school care also benefited from the reform, but to a considerably smaller degree. The reduction in after-school care fees was much smaller, and in addition fewer children attend after-school care.

[22] Fees before and after the reform for a family with a total monthly gross income of € 4,120 in 1999 and € 4,630 in 2002 and with two children in child care 33 hours per week. Figures are taken from The Swedish National Agency for Education (1999, 2003). Figures from 2002 are transformed to 1999 years prices (factor 0.946 from the CPI of Statistics Sweden).

It is evident that the fee cap reform would substantially improve the financial situation of a vast majority of families with young children. Specifically, parents with children aged 0 to 4 at the time of the election in 1998 would benefit the most from this election promise.

Figure 3 describes the timing of the election campaign promises and their implementation. Note the change of government in the 1994 election.

Figure 3. Timing of election promises and implementations.



Note: SDP = Social Democratic Party.

4. Empirical strategy

The two Swedish reforms affecting families with young children (described in section 3) are exceptionally well suited for testing the pocketbook voting hypothesis. First of all, since both reforms were described with unusual clarity in the election manifestos of the Social Democratic Party they provide a link from policies to individual gains and losses. Second, both reforms were implemented, allowing us to test whether voters responded to reform proposals already when they appeared as campaign promises, or only after they were implemented.[23] A third attractive feature of the reforms is that significant individual benefits were at stake. Since elections are

[23] Several studies show that election promises are often delivered on. In the United Kingdom about 80 percent of the election promises of the winner are implemented, while in the United States the Democrats and Republicans implement about 60 percent of their election promises (Royed, 1996). For Sweden and in our period of study (1994–2002), Naurin (2008) reports that the Social Democrats have fulfilled about 60 percent of their election promises and partially fulfilled another 25 percent of them.

multidimensional, one should not expect to find empirical relationships between voting and minor economic issues.

We test the pocketbook voting hypotheses by estimating difference-in-differences models separately for 1991 to 1994, for 1994 to 1998, and for 1998 to 2002, using equation (13). In our baseline specification our dependent variable is the vote share of the left bloc of parties consisting of the Social Democratic Party and the Left Party.[24] But we also present all empirical results for the vote share of the Social Democratic Party alone.

Our difference-in-differences model is given by equation (13), described in section 2, where δ measures the effect of the policy changes on the treated group. Equation (14), which demonstrates how δ and the other parameters in the estimated equation (13) arise from the underlying theoretical model, is based on the assumption that there has not been any change in left bloc policies toward the treatment group (relative to the control group) in the previous election. This assumption is satisfied in the 1994 election.[25] However, equation (13) holds also when there has been past policy changes, as in the elections of 1998 and 2002. The only difference in the empirical analysis is that in these latter elections, the terms α, β, and γ also reflect past policy changes, so that the prediction $\beta=0$ need not be satisfied. However, what matters is the estimated size and statistical significance of δ, so that equation (13) can be used also for those periods.

We follow the reform descriptions in section 3 and assign parents with children aged 0 to 4 to the treatment group and parents with children aged 6 to 11 to the control group. In both groups we include respondents who also have older children, but in the control group we do not include respondents who have younger children. We do not use respondents with children who are 5 years old since one could argue that they could belong to either the treatment or to the control group.[26]

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[24] The Left Party is a small party which has consistently supported Social Democratic minority governments. In the election campaign of 1994, the Left Party made the same promise as the Social Democratic Party to repeal the child-raising allowances and in 1995 they helped the Social Democratic government to do so. In 2000, the Left Party voted with the Social Democratic government to introduce the cap on child care fees.

[25] The centre-right parties had consistently been in favor of the benefits they introduced in 1994, but having been in opposition since 1982 they had no opportunity to implement them in any of the previous elections covered in our study. The fact that we present the model in section 2 only for the policy changes by party L is without loss of generality. Any reaction in retrospective voting in favor of new benefits that the centre-right parties introduced implies an opposite reaction in the support for the left bloc.

[26] By including respondents with five year old children in the control group, we obtain slightly higher treatment effects, while the opposite happens if we include them in the treatment group.

Studying two consecutive reforms that were both implemented after the election makes it possible to test if pocketbook voting is prospective or retrospective. Prospective pocketbook voting predicts the support for the Social Democratic Party to decrease among parents with young children in the election of 1994 (due to the proposal to let them finance the budget deficit), to increase in 1998 (due the promise to put a cap on child care fees), and to stay the same in 2002 (since the fee cap reform was implemented as promised). Retrospective voting also predicts a decrease in support for the Social Democratic Party in 1994 (due to the child-raising allowance introduced by the centre-right government). In 1998 a further reduction is predicted (due to the abolition of child-raising allowances and the reduction of compensation in the parental insurance), and in 2002 the support is predicted to increase (due to the implementation of the fee cap).

Table 1 displays the predicted voting responses for the prospective and for the retrospective hypotheses. In 1994 both hypotheses predict that parents with children aged 0 to 4 should decrease their support for the left bloc (and for the Social Democratic Party) relative to parents with children aged 6–11. In the election of 1998, the prospective prediction is positive and the retrospective prediction negative. In 2002, only retrospective voting predicts a response, which is positive.

Table 1: Predicted treatment effects

Election year	1994	1998	2002
Age of children	0−4 vs. 6–11	0−4 vs. 6–11	0−4 vs. 6–11
Type of voting			
Prospective	Negative	Positive	No response
Retrospective	Negative	Negative	Positive

For our estimations to give unbiased estimates of the treatment effects, two assumptions need to be fulfilled. First, the additive structure of equation (13) must be correct. This assumption is much weaker than assuming a fully linearly additive data generating process, which has been the standard specification in previous empirical studies of economic voting (Lewis-Beck and Stegmaier, 2007). Second, the error term should be uncorrelated with treatment status. The most critical part of this assumption is known as the parallel trend assumption. This means that in absence of the policy changes, the treatment group would have changed its voting pattern just in the same way as the control group did. In practice this means that demographic and socioeconomic characteristics that determine party support must be stable within each group over time and that no other policy changes which affect the two groups differently occur at the same time.

To ensure that we can be reasonably confident that these two assumptions are not violated, we have selected the control group to be as similar to the treatment group as possible (more on this in section 5 and 6). This minimizes the risk that unrelated policy changes or other events affect the two groups

differently. Importantly, the control group can be seen as being identical to the treatment group but moved forward in time. As a consequence, we can consider the assignment of parents to the treatment group or to the control group to be as good as random, since it is determined by the year in which their children are born. In section 5 we compare the voting pattern of the treatment and the control group over seven elections to make sure that the parallel trend assumption is not violated. Furthermore, we compare socioeconomic characteristics that are likely to influence voting, both between the groups and within the groups over time, to ensure that they are similar in other relevant respects and also have a stable composition over time. Finally, judging from the election platforms of all Swedish parties from 1982 to 2002, there are no comparable proposals affecting parents belonging to the treatment or to the control group.

We calculate standard errors that are robust to heteroskedasticity and correlation between individuals that appear twice in the data. But, as pointed out by for example Donald and Lang (2007) and Wooldridge (2003), in the presence of unaccounted group-specific shocks and a small number of groups (two in our case), the standard errors are likely to be downward biased. In our case such a problem could arise if the treatment and control groups would be hit by shocks that are correlated within but not across the two groups. In this respect the time pattern of vote shares looks comforting as the early time periods give no reason to believe that our treatment and control groups are exposed to different shocks (see Figure 4 in section 5). Wooldridge (2003) proposes a test for whether group-specific shocks are present. Since the test does not reject the null hypothesis that such shocks are absent, we are confident that the presented standard errors of the treatment effects are accurate.[27]

5. Data and illustrative results

The availability of high quality data at the individual level constitutes a prerequisite for our empirical strategy. The Swedish Election Studies sample about 3,000 individuals drawn from the population of eligible voters with two exceptions: those living abroad and those older than 80 years.[28] This data

[27] When performing Wooldridge's (2003) test we include all years simultaneously in the regressions, giving us seven elections and two groups. We estimate 11 parameters and therefore obtain 3 degrees of freedom. The test statistic is chi-square distributed and equals 0.14 when we study voting for the left bloc and 3.14 when we study voting for the Social Democratic Party alone. We are clearly on the safe side as the critical value at the 10 percent level is 6.25. On the other hand, by including all elections we risk introducing another source of bias in the standard errors arising from serial correlation, see Bertrand et al (2004). To avoid this problem we present estimates based on two elections at a time.

[28] The election studies are made in the form of a two-step panel in which each respondent is interviewed twice and one half of them are replaced in each study. In

set contains a multitude of background questions about the respondents and their partners, including the number and age of all children in the household. While the secrecy of the ballot unavoidably introduces uncertainty to 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 is used to verify whether respondents who claimed to have voted actually did.[29] Being able to exclude respondents who incorrectly claim to have voted eliminates problems associated with over-reporting of turnout, which has been widely discussed in connection with the American National Election Studies; see e.g. Belli et al. (2001). Second, the response rates are high in comparison with other election studies (between 69 and 81 percent in the 1991–2002 Swedish Election Studies).

We first present the support for the left bloc in the general elections from 1982 to 2002, among parents with young and parents with older children. This visual inspection illustrates our empirical strategy and is quite suggestive of prospective pocketbook voting. The formal econometric analysis follows in section 6.

Figure 4 displays a pattern 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 to put a cap on child care fees. The support is more stable in 2002 when no new promises were targeted at this group.

It is evident that retrospective voting is incompatible with the pattern displayed in Figure 4. While the retrospective voting model (as described in section 4) can be compatible with the 1994 election, it fails to explain voting in 1998 (when it predicts that the benefit cuts will lead to decreasing support for the left bloc among parents with younger children) and in 2002 (when it predicts that the implementation of the fee cap will lead to increasing support for the left bloc among parents with younger children).

According to Figure 4, the voting patterns of the two groups seem to follow the same trend before the 1994 election. This means that in absence of the investigated policy changes it seems plausible to assume that our treatment group would have continued to vote in a similar way as the control group. Figure 4 also suggests that the change in the support for the left bloc differ between parents with younger and with older children only in 1994 and 1998. This impression is supported by statistical tests. All three placebo tests on the election pairs 1982–1985, 1985–1988, and 1988–1991 (using the difference-in-differences approach as described in section 4) produce statistically insignificant treatment effects ($P>0.1$), regardless of whether we

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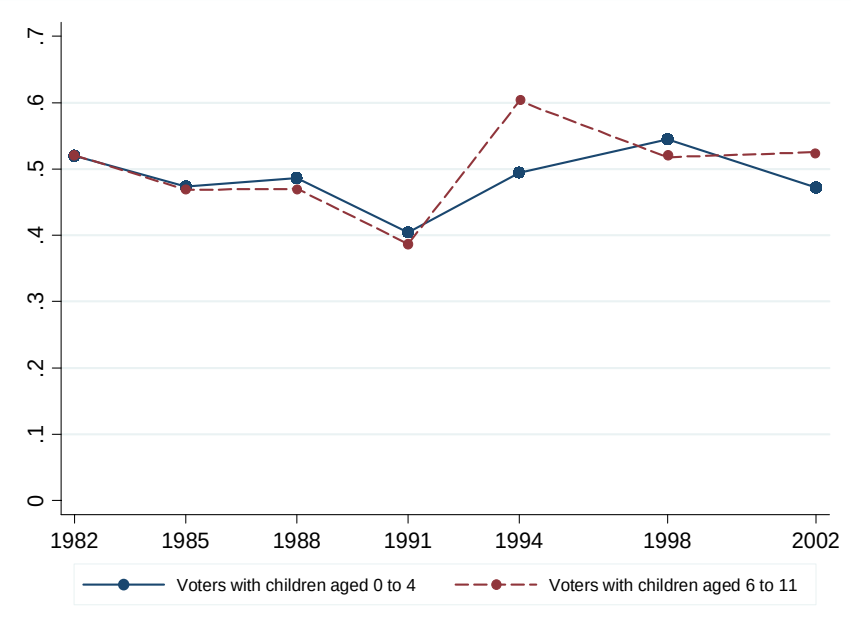
the econometric analysis we use this information to cluster standard errors on individuals.

[29] Turnout in the Swedish national elections was 87 percent in 1991 and in 1994, 81 percent in 1998, and 80 percent in 2002.

study votes for the left bloc or votes for the Social Democratic Party. In addition, Appendix C shows that 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). This suggests that we do not identify our treatment effect by unobserved factors affecting our control group only. It also demonstrates that the parents in our 1998 and 2002 control groups do not seem to be affected by having received treatment in the preceding elections.

Figure 4. Vote shares of the left bloc (Social Democratic Party and Left Party) 1982−2002.



Note: respondents in both groups are allowed to have older children as well, but the respondents in the control group are not allowed to have younger children.

Is it reasonable to believe that the voting pattern displayed in Figure 4 is a result of pocketbook voting? While it is typically not fruitful to ask people whether their vote choice is based on selfish motives, 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 percent of the respondents with children aged 0 to 4 stated that issues related to family policies were important for their party choice. Interestingly, the corresponding numbers for the control group were 16.6 and 18.9 percent. These numbers point at 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 and parents with older children. The formal testing of this hypothesis is the subject of the next section.

6. Econometric analysis

Having inspected the development of the support for the left bloc visually, we now move on to formal econometric testing. By estimating Equation 13 (described in section 2) we analyze responses to the policy changes (described in section 3) in the elections of 1994, 1998, and 2002. Recall, that before the 1994 election, the Social Democratic Party proposed several reductions in benefits for parents with young children. The reductions were, with one exception, implemented during the 1994–1998 term of office. Similarly, in the election year of 2002, the Social Democratic Party delivered on their promise from the 1998 election campaign to put a cap on child care fees.

Table 2 contains the difference-in-differences estimates for the left bloc of parties. 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 five percent 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 *Control group* (0.387) is in essence an estimate of the fraction of respondents in the control group that voted for the left bloc in 1991. The estimate for *Treatment group difference* shows that the fraction of respondents in the treatment group that voted for the left bloc in 1991 was 0.404 (0.387+0.017). *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.

In column 2, we look at the 1998 election. In this election the estimated treatment effect equals 0.135 and is again statistically significant at the five percent level. The estimate is consistent with prospective pocketbook voting, but not with voting that is retrospective only. If voting is both prospective and retrospective, the estimate captures the net effect of prospective voting (and implies that retrospective voting is less important).

Column 3 focuses on the election in 2002. Since the fee cap reform was implemented before this election, retrospective voting predicts an increase in the support for the left bloc in the treatment group, whereas no voting response is predicted by the prospective voting hypothesis. If pocketbook

voting is prospective, the voters should already have responded to the promise to reduce child care fees in 1998. The estimated treatment effect of −0.080 is not statistically different from zero. Prospective but not retrospective voting is consistent with the 2002 election.

When looking at the three elections together, a striking pattern of prospective pocketbook voting emerges. Pocketbook voting of the prospective type is consistent with the estimated treatment effect in all three elections. In contrast, retrospective voting is inconsistent with the voting pattern over the last two elections. Although the negative treatment effect in 1994 could (partly) be due 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. The same prospective pattern emerges when we use votes for the Social Democratic Party as an alternative dependent variable, as reported in table 3.

Table 2: Estimates of pocketbook voting for the left bloc of parties

Dep. var. Vote for left bloc	(1)	(2)	(3)
Election year	1994	1998	2002
Age of children	0−4 vs. 6−11	0−4 vs. 6−11	0−4 vs. 6−11
Treatment effect (δ)	-0.126**	0.135**	-0.080
	(0.060)	(0.067)	(0.069)
Time effect (γ)	0.215***	-0.085*	0.008
	(0.047)	(0.050)	(0.048)
Treatment group diff. (β)	0.017	-0.108**	0.027
	(0.043)	(0.046)	(0.050)
Control group (α)	0.387***	0.602***	0.518***
	(0.035)	(0.038)	(0.036)
Socioeconomic controls	No	No	No
Type of voting			
Retrospective voting	Consistent	Inconsistent	Inconsistent
Prospective Voting	Consistent	Consistent	Consistent
Observations	1,071	908	830

Notes: 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). Robust standard errors clustered on individuals in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Estimates of pocketbook voting for the Social Democratic Party (SDP)

Dep. var. Vote for SDP	(1)	(2)	(3)
Election year	1994	1998	2002
Age of children	0−4 vs. 6−11	0−4 vs. 6−11	0−4 vs. 6−11
Treatment effect (δ)	-0.144**	0.138**	-0.066
	(0.059)	(0.066)	(0.067)
Time effect (γ)	0.159***	-0.115**	0.021
	(0.047)	(0.050)	(0.046)
Treatment group diff. (β)	0.056	-0.088*	0.049
	(0.042)	(0.047)	(0.049)
Control group (α)	0.327***	0.485***	0.371***
	(0.033)	(0.038)	(0.034)
Socioeconomic controls	No	No	No
Type of voting			
Retrospective voting	Consistent	Inconsistent	Inconsistent
Prospective Voting	Consistent	Consistent	Consistent
Observations	1,071	908	830

Notes: 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. * significant at 10%; ** significant at 5%; *** significant at 1%.

If there are significant differences between the treatment and control groups, or if the compositions of the groups change over time, the estimated treatment effect may be biased. We therefore compare demographic and socioeconomic characteristics both between the groups and within the groups over time (see table A2 and A3 in Appendix A). In our sample the fractions of singles increase over time in both the treatment and control groups. Furthermore, the fraction of unemployed is higher in the treatment groups, which as expected also are younger and have slightly lower income.[30] As a robustness check we therefore include the demographic and socioeconomic characteristics in the regressions. It turns out that the results are robust to this inclusion. Detailed regression results are found in table B1 and B2 in Appendix B. The stability of our results indicates that our choice of control group is appropriate and strengthens the evidence in favor of pocketbook voting.

The time one child can be in publicly subsidized child care is limited to about five years, as children can attend child care only when they are between one and six years old. This means that when the children have grown old enough to start school, the parents no longer benefit from reduced child care fees. This implies that parents with younger children will benefit

[30] 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.

from reduced child care fees for a longer period than parents with older children. Also the possibility of a late or even no implementation of the election campaign promise means that the expected value of the reform should decrease in the age of the children. As a further test of our pocketbook voting hypothesis, we have performed a dose response test, using such variation. The point estimates of the dose response test, reported in Appendix D, are in line with our previous results.

Compared with previous studies our analysis embodies several advantages. To start with, most previous studies of pocketbook voting have just assumed a diffuse attribution mechanism: that improved individual finances (or the expectation of this) can be ascribed to the government. This assumption that perceptions about economic outcomes are related to differences in policy between the government and the opposition will at the very least introduce substantial measurement error, and in consequence attenuation bias. The assumption that changes in people's finances are uncorrelated with omitted determinants of voting is even more problematic.

Having a negative treatment followed by a positive treatment for the same group also makes it somewhat more likely that the estimated treatment effect can be generalized to other groups. 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. Strictly speaking, the estimated treatment effect of each reform can only be generalized to the group of parents who received treatment – but the fact that this group first received a negative and then a positive treatment arguably makes it less likely that it was targeted because its members were unusually responsive.

Our empirical strategy also rules out any confounding influence of the four alternative explanations of voting mentioned in the introduction. First, 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 Mutz and Mondak (1997) work with. Their groups are demarcated by sex, race, labor market participation, income, and self-identification with, or perceived closeness to, a social class.

Second, social background is another possible source of bias. In a seminal contribution, Campbell et al. (1960) claim that party identification is formed early in life through socialization. In our setting, it is conceivable that age differences between the treatment and the (slightly older) control group could capture cohort effects that show up exactly in the elections in which treatment is assigned. While we view such an interpretation as quite far fetched a priori, we address this concern, nonetheless, as it still appears to be the strongest alternative explanation for our findings.

In particular, the estimated treatment effect in 1994 could be a spurious cohort effect if the treatment group is unusually right-wing or if the control group is unusually left wing. We know from Figure C1 in Appendix C that the control group is not unusually left-wing when compared with parents

with even older children aged 12–17. Figure 4 demonstrates the same thing as the "treatment group" of parents with young children in 1991 largely corresponds to the control group of parents with older children in 1994, and there is no difference between the "treatment" and "control" group in 1991. Thus the control group in 1994 does not seem to be an unusual cohort. But the treatment group in 1994 could still be special. In fact, both the estimated treatment effect in 1994 and in 1998 could be spuriously explained by an unusually right-wing treatment group in 1994, since the treatment group in 1994 enters the control group in 1998 as their children grow older. If this were the case, the same group of parents would also stand out in 2002 when their children have grown even older. But Figure C1 displays no such difference in 2002 between parents with children aged 12–17 and parents with children slightly younger than that. To conclude, there is no reason to believe that our empirical results are driven by cohort effects.

Third, we focus on reforms where the voters' attribution problem should be minimal. This leaves little room for different beliefs about the causes of income. Fong (2001) and Alesina and Angeletos (2005) argue that such beliefs shape political preferences for redistribution. While our findings do not conflict with their argument in other contexts, we show that individual pocketbook motivations play a role independently of beliefs about the causes of income.

Fourth, our results are not confounded by macroeconomic conditions; see Fair (1978) for an early test of the electoral impact of such conditions. The reason is simply that the treatment and the control group face the same macroeconomic conditions. So 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 parameter. And given the minimal voting differences between the treatment group and the control group in all elections before 1994, we see no reason to suspect that the two groups should react differently to the same macroeconomic events. The four alternative theories could obviously play an important role in certain political settings, but none of them should confound our identification of pocketbook voting.

7. Concluding remarks

We have presented a theoretical framework for pocketbook voting which results in two testable hypotheses. Our empirical analysis provides clear evidence of prospective pocketbook voting over several elections. This finding stands in sharp contrast to the previous literature where the support for pocketbook voting has been weak overall.

The size of our treatment group implies that the pocketbook effect could be important for election outcomes. According to the Swedish Election Studies, our treatment group made up about 10.8 percent of the electorate

(and 11.2 percent of the voters) in 1998. Together with our estimate of the 1998 treatment effect (which is 13.5 percentage points), the share suggests that the promise to put a cap on child care fees increased the support for the left bloc of parties by approximately 1.5 percentage points in the 1998 election.[31] Although a reform effect of 1.5 percentage points would not have tipped the scales in the 1998 election, it is clear that an effect of this size could affect the balance of power in closer races. The margin between the bloc of left parties and the bloc of centre-right parties was less than 1.5 percentage points in five out of 19 postwar elections. The picture that emerges from our investigation is that pocketbook voting can influence election outcomes, providing a temptation for politicians to tactically redistribute tax revenues to certain groups of voters.

The fee cap reform has been estimated to cost the taxpayers about €360 million per year.[32] By dividing this cost with the roughly 80,600 votes (1.5 percent of the total votes) that were swayed by this election promise, we get a crude cost estimate of about €4,500 per vote annually, corresponding to €18,000 over a four-year electoral period. Previous studies of pork-barrel spending have estimated the cost of a vote at about $14,000 (Levitt and Snyder, 1997) and between $2,000 and $13,000 (Leigh, 2008). Despite the fact that these studies have analyzed voting at district level, while we use individual-level data, the estimated costs over an electoral term are quite similar in Sweden, the United States, and Australia.

It is important to emphasize that we have only investigated pocketbook voting responses to two salient reforms, and not to other specific policies. As a next step, we believe that our theoretical framework and empirical strategy can be applied to reforms in other countries. Only by directly linking policies with individual gains and votes can pocketbook voting be credibly identified. Finally, since we have studied the case of election campaign promises that were later implemented, it is natural to study two alternative cases in our theoretical model. The first case is that of a campaign promise that is not implemented (or that is implemented to a larger or to a smaller extent than promised). The second case is that of an unexpected implementation of a policy that has not previously appeared as an election campaign promise.

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[31] The 1.5 percentage point estimate should obviously be taken with great caution. There may for instance be a counterweighing effect if other voters expect their tax burden to increase as a consequence. Note, however, that this qualification doesn't affect our main finding that citizens vote their pocketbooks.

[32] According to calculations made by the Ministry of Education and Research (1999) 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) have shown that the reform had no effects on labor supply, the cost of the reform is probably underestimated as the reform seems to have increased the demand for child care (Swedish National Agency for Education, 2007).

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Appendix A: Description of data

The Swedish Election Studies

The variables in our data set are available at 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 Göteborg University, under the guidance of Sören Holmberg, Mikael Gilljam, and Henrik Oscarsson. Neither SND nor the primary researchers are responsible for the analyses and interpretations presented in this paper. The sample of the Swedish Election Studies is drawn from a population of 18- to 80-year old Swedish citizens entitled to vote in the general election. Citizens living abroad are not included in the sample. The election studies are made in the form of a two-step panel in which each respondent is interviewed twice and one half of them are replaced in each study. The response rates were 82 percent in 1982, 78 percent in 1985, 75 percent in 1988, 73 percent in 1991, 80 percent in 1994, 81 percent in 1998, and 69 percent in 2002.

Description of variables

Table A1: Description of variables used in the analysis

Variable	Definition
Vote for left bloc	1 if voted for the Social Democratic Party or the Left party; 0 otherwise.
Vote for the Social Democratic Party	1 if voted for the Social Democratic Party; 0 otherwise.
Sex	1 if female; 0 if male
Marital status	1 if single; 0 if married with or cohabiting with another adult.
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 (€ 1,000)	Gross earnings, referring to the income year two years prior to the survey year. Register based. Source: Statistics Sweden.
Age	Years of age

Source: The Swedish Election Studies.

Descriptive statistics

Table A2. Variable means for the treatment groups (parents with children aged 0 to 4 years old)

	1991	1994	1998	2002
Vote for left bloc	40.4 (35.3–45.7)	49.4 (44.0–54.9)	54.5 (47.3–61.5)	47.2 (40.4–54.1)
Vote for SDP	38.2 (33.2–43.5)	39.7 (34.5–45.1)	42.0 (35.1–49.2)	37.5 (31.0–44.3)
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)
Marital 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 (€ 1,000)	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	360	340	200	216

Note: 95 % confidence intervals in parentheses.

Table A3. Variable means for the control groups (parents with children aged 6 to 11 years old)

	1991	1994	1998	2002
Vote for left bloc	38.7 (31.9–45.8)	60.2 (52.4–67.6)	51.8 (44.6–58.9)	52.5 (45.7–59.3)
Vote for SDP	32.7 (26.2–39.7)	48.5 (40.8–56.3)	37.1 (30.3–44.2)	39.2 (32.6–46.0)
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)
Marital 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 (€ 1,000)	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 B: Robustness checks

Including demographic and socioeconomic characteristics in the difference-in-differences estimations

Table B1. Pocketbook voting: 1994, 1998 & 2002 including demographic and socioeconomic controls

Dep. var. Vote for left bloc	(1)	(2)	(4)
Election year	1994	1998	2002
Age of children	0-4 vs. 6-11	0-4 vs. 6-11	0-4 vs. 6-11
Treatment effect (δ)	-0.128**	0.151**	-0.063
	(0.060)	(0.066)	(0.069)
Time effect (γ)	0.208***	-0.091*	0.009
	(0.047)	(0.050)	(0.048)
Treatment group diff. (β)	-0.022	-0.150***	-0.005
	(0.050)	(0.051)	(0.054)
Control group (α)	0.621***	0.866***	0.714***
	(0.126)	(0.129)	(0.123)
Sex (1= female)	0.005	-0.006	0.000
	(0.037)	(0.040)	(0.037)
Marital status (1=single)	0.059	0.119**	0.068
	(0.066)	(0.061)	(0.055)
Unemployed	0.157**	0.084	0.112
	(0.066)	(0.062)	(0.072)
Two children	-0.008	0.061	0.063
	(0.036)	(0.040)	(0.041)
Three or more children	-0.054	-0.015	0.044
	(0.044)	(0.050)	(0.050)
Income (€ 1,000)	-0.005***	-0.005***	-0.001**
	(0.001)	(0.001)	(0.001)
Age	-0.003	-0.005*	-0.006**
	(0.003)	(0.003)	(0.003)
Type of voting			
Retrospective voting	Consistent	Inconsistent	Inconsistent
Prospective voting	Consistent	Consistent	Consistent
Observations	1,070	908	830

Notes: 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). Robust standard errors clustered on individuals in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

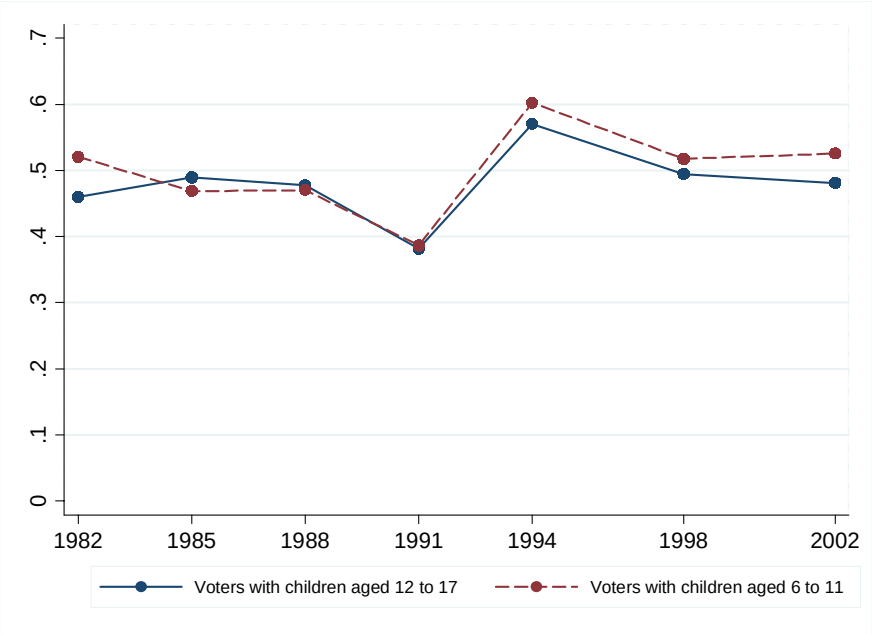
Table B2. Pocketbook voting: 1994, 1998 & 2002 including demographic and socioeconomic controls

Dep. var. Vote for the Social Democratic Party	(1)	(2)	(4)
Election year	1994	1998	2002
Age of children	0-4 vs. 6-11	0-4 vs. 6-11	0-4 vs. 6-11
Treatment effect (δ)	-0.150**	0.132**	-0.055
	(0.060)	(0.066)	(0.067)
Time effect (γ)	0.155***	-0.110**	0.028
	(0.047)	(0.050)	(0.047)
Treatment group diff. (β)	0.028	-0.108**	0.010
	(0.049)	(0.052)	(0.054)
Control group (α)	0.501***	0.626***	0.540***
	(0.124)	(0.129)	(0.124)
Sex (1= female)	0.002	-0.014	-0.004
	(0.036)	(0.039)	(0.036)
Marital status (1=single)	-0.037	-0.024	-0.093*
	(0.069)	(0.063)	(0.052)
Unemployed	0.134*	0.049	0.058
	(0.070)	(0.065)	(0.072)
Two children	-0.005	0.062	0.071*
	(0.037)	(0.040)	(0.041)
Three or more children	-0.054	-0.040	0.021
	(0.043)	(0.047)	(0.049)
Income (€ 1,000)	-0.004***	-0.002	-0.001**
	(0.001)	(0.001)	(0.000)
Age	-0.002	-0.003	-0.005
	(0.003)	(0.003)	(0.003)
Type of voting			
Retrospective voting	Consistent	Inconsistent	Inconsistent
Prospective voting	Consistent	Consistent	Consistent
Observations	1,070	908	830

Notes: 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). Robust standard errors clustered on individuals in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix C: Voting among parents in the control group and among parents with even older children

Figure C1. Vote shares of the left bloc (Social Democratic Party and Left Party) 1982–2002



Note: The figure shows voting among our control group of parents with children 6 to 11 years old and among parents with children 12 to 17 years old. None of the respondents in the two groups are allowed to have younger children.

Appendix D: Dose response test

As a natural extension of our empirical tests of pocketbook voting we test for a dose response relationship. If we find that voters with children aged 0 to 2 respond more strongly to the promise of a cap on child care fees than voters with children aged 3 to 5, it would be strong evidence in favor of a causal relationship between pocketbook promises and voting.[33] Table 4 presents the results from such a test. Since this test divides the treatment group into two parts and estimates two more parameters, the power of the test will unavoidably be quite low. For this reason we include respondents with five year old children in the treatment group. The point estimates of the treatment effects follow the pattern predicted by prospective pocketbook voting. The point estimate is higher among parents with the youngest children who received the largest benefits from the fee cap. The precision of the estimates is, however, too low to conclude that they are different from each other at normal levels of statistical significance. Thus, the dose response test doesn't provide strong evidence in favor of prospective pocketbook voting, but neither does it contradict our previous findings.

Table D1: Dose response test for prospective voting in 1998

Dep. var. Vote for the left bloc	(1) 0−2 vs. 3−5 vs. 6−11
Treatment effect 3–5	0.068
	(0.084)
Treatment effect 0−2	0.098
	(0.074)
Past difference between treatment	-0.034
group 3–5 and control group	(0.054)
Past difference between treatment	-0.143***
group 0−2 and control group	0.068
Time effect (1994–1998)	-0.085*
	(0.050)
Control group	0.602***
	(0.038)
Observations	992

Notes: The election year of 1998 refers to a difference-in-differences estimation of 1998 vs. 1994. Robust standard errors clustered on individuals in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

[33] Comparing parents with one and parents with two or more children in child care would be a less convincing dose response test. On the one hand, additional children mean higher momentary gains from the reform. On the other hand, since many parents with one child plan to have more children, they can expect to enjoy the fruits of the reform for a longer period of time in the future.

Essay II

Local economies and general elections:

The influence of municipal and regional economic conditions on voting in Sweden 1985–2002

1. Introduction

Numerous empirical studies have shown that voters respond to economic conditions. These studies typically focus on national or individual level economic conditions.[1] Very few studies have estimated responses to local economic conditions. This is surprising for two reasons. First, a large theoretical literature uses the assumption that voters are sensitive to local economic conditions and second, because estimates of responses to other levels may be misleading if the response to local level variables is substantial. This paper improves the understanding of voting behavior by conducting a detailed analysis of responses to two different local levels of economic conditions. In particular, voters' response to municipality and regional level unemployment and economic growth are estimated, using data from six Swedish general elections between 1985 and 2002 covering 284 municipalities.

It is not a novel idea that voters consider local economic conditions when deciding whether to support the government or to vote for the opposition. Models by e.g. Lindbeck and Weibull (1993) and Dixit and Londregan (1996) suggest that governments tactically redistribute resources to regions with many swing voters. Cox and McCubbins (1986) argue that politicians redistribute to regions where they have many supporters and Weingast et al. (1981), discuss how political effort and spending may be geographically uneven (and inefficient) if politicians favor their own constituencies. These

[1] For surveys of the economic voting literature, see Lewis-Beck and Stegmaier (2000), Lewis-Beck and Paldam (2000), and Nannestad and Paldam (1994).

models all assume that voters are sensitive to the amount of resources spent in their area and that resources are geographically unevenly redistributed.

A few empirical studies have found that voters respond to regional spending. Levitt and Snyder (1997) show that the outcomes of U.S. House elections are influenced by federal spending and Jordahl (2002) shows that voters respond to specific grants to Swedish municipalities from the national government. These tests have focused on spending rather than measures of economic outcomes. Funds come in a variety of forms and may have very different effects on unemployment and economic growth. I, therefore, take a more general approach to this problem by estimating the impact of economic growth and unemployment, at local levels, on the support for the national government.

Knowing if voters respond in systematic ways to economic conditions at local levels is important for understanding how voters respond to national and individual level economic conditions as well. Omitting local level variables may result in misspecified models and therefore to misleading estimates and conclusions. Earlier studies, including only national and individual level economic indicators, may therefore be missing important aspects of voting behavior.

Johnston and Pattie (2001), address this issue by including a local level in their analysis of the British general election of 1997, arguing that such an intermediate level is necessary for understanding voting behavior. They estimate the effect of individual, local, and national level economic conditions on individual voting behavior. Their study uses survey data on individuals' perceptions of economic conditions, and find that voters are less influenced by perceptions of changes in national level economic prosperity than by perceptions of changes in their local area's prosperity. They also find that the prosperity of the local area influences voting behavior more than changes in the personal financial situation.

In a related study, Auberger and Dubois (2005) find that local economic circumstances play an important role in forecasting election outcomes in French legislative elections, using a measure of labor market performance as an indicator of local economic conditions. The objective of their study is mainly to make accurate predictions of election outcomes, while the objective of this paper is to understand at which levels voters respond to real changes in objective economic conditions. Furthermore, a key difference between the French and Swedish parliaments is that in the French National Assembly, representatives administrate local territories, while representatives in the Swedish Riksdag do not have such a clear local connection. This raises the question whether local economic conditions have an impact on the vote also for political systems in which national politicians do not have a direct responsibility for specific local areas.

Leigh (2005) investigates among other things how neighborhood characteristics influence citizens' partisan choice in Australian elections and finds that living in a richer neighborhood increases the probability of voting

for the right-wing party. While that study focuses on a left−right choice this paper focuses on a government−opposition choice.

Eisenberg and Ketcham (2004) asks a number of questions regarding voting in the United States. One question they try to answer is whether local economic conditions matter in U.S. presidential elections. They find that county level economic conditions have a relatively small impact on the election outcomes relative to state and national level economic variables. The results are not so surprising, given the great vertical distance between the national (federal) and the county level in the United States. This also makes it hard to draw inference from this study to much smaller European countries.

Veiga and Veiga (2008) find that national economic conditions play a role in national elections in Portugal, and they also find some influences of economic conditions at the municipality level.

Johnston and Pattie (2001), Auberger and Dubois (2005), Leigh (2005), and Veiga and Veiga (2008) all indicate that economic conditions between the national and the individual level influence how citizens vote.

Although the objective of Johnston and Pattie's (2001) study and that of this paper are similar, a few differences are worth pointing out. First, Johnston and Pattie analyze only one election, whereas I use data from six elections with much more variation in government performance. This also allows me to use a difference approach which effectively reduces bias stemming from omitted variables.

While Johnston and Pattie use subjective perceptions of economic conditions and survey responses of how the respondents have voted, I use objective measures of economic conditions and real voting data. This avoids problems associated with measurement error and non-response in survey data. A possible advantage of their approach is that they use individual level data and perceptions of economic conditions. This has, however, also some drawbacks. Since their respondents are asked to answer how their part of the country has developed, two problems arise.[2] First, the respondents may differ in how large the area they think of is. Second, even if the respondents have concordant perceptions of what they mean with *this part of Britain*, the researcher does not know if most people are referring to the village they live in or if they think of regions with many millions of people. I avoid these problems by using economic variables at well defined levels of aggregation. The levels of aggregation I use are municipalities and regions based on the area covered by the regional news, as broadcasted by the Swedish public television.

[2] The question Johnston and Pattie (2001) use to measure perceptions of local economic development is: "Compared with other parts of Britain since the last general election in April 1992, would you say that (this part of Britain/Scotland/Wales) has been getting more prosperous than average, stayed about the average, or been getting less prosperous than average?"

Another well known problem of using subjective perceptions of economic changes as explanatory variables is that a large proportion of the variation may be unrelated to variation in their objective counterparts. As a consequence estimates based on subjective perceptions may suffer from severe attenuation bias, making it difficult to know if real changes in the objective variables influence voting decisions (Kramer, 1983). Subjective perceptions of economic conditions have also been shown to be related to many confounding factors such as political attitudes, education and media exposure (Duch et al., 2000) making it even more difficult to know how real economic changes affect the support of the incumbent government. For further discussions on these questions, see e.g. Kramer (1983), Nannestad and Paldam (1997), and Duch et al. (2000). Although voters must base their decisions on their perceptions of economic conditions, it may be more interesting to know how they respond to real economic conditions.

This paper improves upon the existing literature on the influences of local economic conditions by analyzing responses to objective measures of economic conditions at two different local levels, using panel data covering six elections. In addition, I expose the results to extensive robustness tests. Furthermore, previous analyses of responses to local economic conditions have used British, French, Portuguese, Australian, and American data, but no study has investigated Swedish data.

The empirical results of this paper indicate substantial responses to both economic growth and unemployment at the regional level. The preferred specification suggests that a reduction of regional unemployment or increase in growth by one percentage point is associated with an increase in the support for the government by about 1.0 and 0.6 percentage points. Exposing the model to extensive robustness tests, provides further insights, but does not change the conclusion that voters seem to respond to economic conditions at the regional level. At the municipality level the effects of unemployment and economic growth are much smaller than at the regional level in all specifications.

2. Theoretical background

In addition to the papers by Weingast et al. (1981) and Dixit and Londregan (1996), which directly suggest that the support for the government may be influenced by local economic conditions, there are many models of voting and political behavior that also suggest that local economic conditions may be important.

The economic voting literature builds on early contributions by Downs (1957) and Key (1966). They both argue that voters hold the incumbent government responsible for economic conditions in the sense that they reward a government that has been successful in its economic policies by reelecting it. Downs (1957) motivated this retrospective economic voting theory with the observation that the policies of political parties tend to be

rather stable over time. Models of rational retrospective voting have been formalized by Rogoff and Sibert (1988) and Persson and Tabellini (1990), showing that past outcomes of economic variables can be used as signals of the economic competence of the government.

Economic models of voting typically assume that voters are selfish[3], while many political scientists instead argue that voters have more altruistic voting motives. It is, however, hard to test to what degree voters are selfish or altruistic. Empirical studies typically suggest that voters respond to national level indicators of economic conditions, while only a few find important responses to microeconomic conditions.[4] Responses to macroeconomic conditions are, however, consistent with both altruistic concerns and selfish motives. For example, a voter that responds to macroeconomic variables may do so because she believes it to be a good indicator of how her own personal financial situation will develop if the incumbent government is reelected, or because she is concerned about the economic situation of all citizens in the country. Furthermore, Brennan and Lomasky (1993) argue that citizens that are otherwise selfish may very well express altruism when casting their vote. The reason is that the cost of altruistic voting is negligible, since the probability of affecting the outcome of the election is very small.

As the discussion shows, no theoretical consensus of why citizens vote as they do has been reached and the empirical literature has not been able to clearly discriminate between the models. Most of the models do, however, suggest that citizens base their voting decisions at least partly on the success of the government's economic policy.

Few of the models discussed above explicitly suggest which economic indicators voters should base their decision on and none of them seem to conflict with the hypothesis that voters also consider local economic conditions. Some of them may actually work better with local economic indicators since voters that are purely selfish may find that local economic conditions could better predict personal financial outcomes than national economic conditions. Altruistic voters may on the other hand care relatively more about people living closer to them and, therefore, also have reason to consider local economic conditions. In line with this, I specify the econometric model so as to allow voters to respond to past economic conditions at two different local levels.

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[3]There are, however, models that do not rely on this assumption. In papers by for example Blomquist and Christiansen (1999), and Dixit and Londregan (1998) citizens that are allowed to have altruistic preferences vote over the supply of publicly provided private goods and over redistribution.

[4] For a review, see Lewis-Beck and Stegmaier (2000) and Nannestad and Paldam (1994). Examples of papers finding responses to macroeconomic variables are: Kramer (1971), Kinder and Kiewiet (1979), and Markus (1988). Examples of papers finding responses to microeconomic variables are: Nannestad and Paldam (1997), Markus (1988), and Jordahl (2006).

A necessary condition for voters to respond to economic conditions at the local level is that they collect relevant information and are able to distinguish differences in economic conditions at different levels. Analyzing which economic indicators voters respond to, therefore, sheds light both on how well-informed voters are and how sophisticated they are in using this information. The aim is not to discriminate between the different models but purely to investigate the empirical relationships between economic conditions at local levels and government support.

3. Institutional and geographical setting

The Swedish political system is organized into three levels: municipalities, counties and the national level. All levels set and collect taxes as well as supply services to the citizens. The municipalities supply services such as primary and secondary education, child care, and social assistance, while the counties' main task is to supply medical service. Elections, at all levels, were held on the same day every three years from 1970 until 1994, and every four years since 1994.

The focus of this study is on the general election, i.e. the election to the Swedish parliament.[5] A proportional system is used to allocate seats in parliament, which then appoints a Prime Minister to form a government. The government can consist of a single party or of a coalition of parties and the parties are usually divided into a left and a centre-right bloc. During the time period for this analysis, the centre-right bloc formed a coalition government between 1991 and 1994 and the Social Democratic Party (Socialdemokraterna) ruled as a minority government during five terms (1982-1991 and 1994-2002). The Social Democratic Party has generally been supported by the Left Party (Vänsterpartiet). For extended periods, the Green Party (Miljöpartiet) and the Centre Party (Centerpartiet) also cooperated with the Social Democratic Party and other parties have at times lent the Social Democratic Party pivotal support on specific issues.

It is natural to investigate responses to municipality level economic conditions, since these are likely to have a direct impact on the well-being of many citizens. It is also reasonable to believe that many voters are informed about economic conditions in their own municipality, since many sources for that kind of information are available, such as local newspapers and regular interactions with other people in the municipality. If voters hold the central government responsible for municipal economic conditions is, however, less clear and will be investigated in this paper.

It would be desirable to investigate responses also to regional economic conditions, to be able to assess at which levels voters respond to economic conditions. Ideally, one would like regions to be large enough so that

[5] Several studies have shown that economic voting plays an important role in Sweden, for a brief survey of these studies see Jordahl (2006).

economic conditions in the municipalities may differ from those of the region. One would also like the region to be a natural region in the sense that citizens have common perceptions of what constitutes the region. Furthermore it is important that citizens have easy access to information about economic conditions in the region.

One type of region that meets the above criteria are the regions that are defined by the coverage areas of 'the regional news' (Regionala nyheter). The Swedish public television (Sveriges Television AB) broadcasts regional news on a daily basis and has done so during the entire period covered in this paper. The regional news are an important source of information about the regional economy. Every day, about ten percent of the population watches the regional news. This means that citizens within each region have a common important source of information about the economic situation of the region. During the period of study nine different programs were broadcasted, each covering a geographically connected area consisting of a specified set of municipalities. This makes the coverage areas of the regional news natural areas to use as regions in this investigation.[6] Consequently, these regions will serve as the regional level in the empirical analysis. For further details about the regions see appendix B.

An alternative to use media regions would be to use the counties as regions. At least one aspect of the counties makes them less appealing to use as regions in this analysis. Since each county encompass fewer municipalities than the media regions the variables of economic conditions are correlated to a very high degree between the municipalities and the county. This makes it difficult to estimate responses to both levels with a reasonable degree of precision. As a robustness test, I will, however, also investigate responses to county level economic conditions.

4. Data

This paper uses a dataset consisting of a panel of 284 out of 290 Swedish municipalities[7], covering six general elections from 1985 to 2002.[8] The time period is constrained backwards because of lack of unemployment data for municipalities. The fundamental variables needed for the regression analysis are election results at the municipality level for the general election and measures of economic conditions at the municipality and regional level.

The dependent variable is defined as the change in the vote share (since the preceding election expressed in percentage units) of the governing party

[6] The nine regions used encompass on average about 32 municipalities and 2.5 counties.

[7] The six newly founded municipalities that have been left out are (year founded in parentheses): Bollebygd (1995), Gnesta (1992), Knivsta (2003), Lekeberg (1995), Nykvarn (1999) and Trosa (1992).

[8] The elections occurred in 1985, 1988, 1991, 1994, 1998 and 2002.

or parties. As mentioned above, the Social Democratic Party has been ruling as a minority government but has been collaborating with other parties. This makes it difficult to fully ascertain which parties that bear the responsibility for the economic policy. I will therefore also, as most studies of economic voting in Sweden, use the change in the vote share of the governing bloc as dependent variable.[9]

Although there are different ways to measure economic conditions I will follow most researchers in the economic voting literature and use measures of income growth and unemployment. The variables measured at the municipality level are real growth in the per capita tax base[10] and the unemployment rate[11]. The measures of unemployment and economic growth at the regional level are aggregated from data at the municipality level and calculated as the averages of their counterparts in all municipalities in the region, weighted by population. For detailed descriptions of all variables and descriptive statistics see Appendix A.

5. Empirical investigation

5.1 Model and empirical strategy

Most estimations in the economic voting literature are based on linear equations specified either in levels or in changes.[12] Two particular features of this study make a model specified in changes preferable. First, one advantage of estimating a first difference equation is that covariates that are time invariant cancel out and hence reduce potential bias stemming from unobserved time-invariant characteristics. Second, the different governments in Sweden have been of different sizes because the Social Democratic Party has been ruling as a minority government, while the centre-right bloc has

[9] See e.g. Alesina et al. (1997) and Jordahl (2006). The left bloc consists of the Social Democratic Party (Socialdemokraterna), the Left Party (Vänsterpartiet) and the Green Party (Miljöpartiet). The centre-right bloc consists of the Moderate Party (Moderaterna), the Centre Party (Centerpartiet), the Liberal Party (Folkpartiet), the Christian Democrats (Kristdemo-kraterna) and the New Democrats (Ny demokrati).

[10] Change in tax base is the most commonly used measure of economic growth in Swedish municipalities. However it should be noted that there was as change in the definition of the tax base in conjunction with the tax reform of 1991. After the reform only labor income is included in the tax base measure, while before both capital and labor incomes were included. It is still nevertheless the best measure available and in section 4.5 I show that the estimates are not sensitive to excluding the election in 1994.

[11] This measure is the best available measure of unemployment at the municipality level, since it is measured in the same way both across municipalities and time. It is calculated as the number of unemployed divided by the population aged 16-64, and does therefore not correspond to the unemployment rate traditionally used by labor economists, but is closer connected to the inverse of the employment rate.

[12] See, Nannestad and Paldam (1994).

formed coalition governments consisting of three or four parties. By focusing on the changes in the vote share of the incumbent government, instead of the level of the support, the problem that the support for left and the centre-right governments have fluctuated around different levels becomes less problematic. This latter aspect make a first difference model preferable to a fixed-effects model, which would utilize deviations from the mean support of both left and centre-right governments to estimate the coefficients. It also makes a random effects model inappropriate to use, since the distribution of municipality specific effects are unlikely to be normally distributed when both left and centre-right governments are included in the dependent variable.

While the dataset I use is well suited for estimating responses to local level economic conditions it is unfortunately not suited for estimating responses to national level economic conditions. The problem is that with only six elections there is only six different observations of macro variables, which leads to low precision in the estimates. The standard errors become large and the estimates become sensitive to unusual values in national level variables. Furthermore since national level variables per definition are constant across municipalities it is only possible to include at maximum one macro variable per election in the specification. This is troublesome since many factors at the macro level such as inflation, evolution of government debt, election specific issues, candidate effects etc. have been shown to affect election outcomes. For such an analysis, either a longer time-series or perceptions of economic conditions would be needed. In this case the most general way to control for macroeconomic conditions is to include year dummies. Wooldridge (2002, p. 129) suggests that year dummies should generally be used to account for aggregate changes over time when pooled cross sections are analyzed. If any variables at the national level are included in the regressions then some year dummies need to be dropped. It is, however, hard to argue why one year dummy should be included but not another. I argue that the best way to control for these factors is to add a full set of year dummies as explanatory variables. Year dummies capture the average effect of omitted macro variables and since the macro variables by definition take the same values for all cross-sectional units the effect of them is uniform and hence coincides with the average effect. Adding year dummies instead of specific macro variables will therefore not bias the estimates of the effects at the municipality and regional level. Unfortunately this comes with the cost that it is no longer possible to include the macro variables of interest, i.e. unemployment and growth. While a comparison with the effect of national level variables will not be feasible, the effects of municipality and regional level variables will not be biased due to omission of variables at the macro level.

Specifying the model in first difference and adding year dummies yield the following regression equation:

$$\Delta V_{i,t} = \beta^{M} \Delta G_{i,t}^{\ M} + \gamma^{M} \Delta U_{i,t}^{\ M} + \beta^{R} \Delta G_{i,t}^{\ R} + \gamma^{R} \Delta U_{i,t}^{\ R} + YD_t + Z_{i,t}^{'} \Phi + \varepsilon_{i,t},$$

where subscript *i* refers to municipality *i*, and *t* to the election at year *t*. $\Delta V_{i,t}$ hence denotes the change in the vote share of the incumbent government from municipality *i* at election *t*, since last election. ΔG refers to change in economic growth and ΔU to change in unemployment. Superscript *M* and *R* refer to the municipality and regional level. YD_t are election specific dummies, Z' is a row vector of time variant covariates, and ε an error term.

The municipalities differ in the number of voters. Therefore, if each municipality is given the same weight in the estimations, voters in large municipalities are implicitly treated as less important in the regressions. Since each citizen's vote is given equal weight in the election and we are equally interested in voting behavior of people in all municipalities it is natural to give each vote equal weight in the regressions as well. To solve this problem I estimate the model by applying different weights to the municipalities. The weights are calculated as each municipality's share of the number of votes in the election. By doing so, larger municipalities will be given higher weights than small ones. It should be noted that if voting behavior is homogeneous across municipalities of different sizes, which is implicitly proposed by the empirical specification, the weighted and the unweighted regressions should produce similar results.

5.2 Main results

This section presents estimates of the effects of unemployment and growth at the regional and municipality level, based on the model described in the previous section. The results are based on regressions with the change in the governing parties' vote share as dependent variable. The change in economic growth is measured as the difference between the average growth rate over present term of office and the previous term of office. The change in unemployment is calculated in the same way.

Since the measures of economic conditions at the regional level by construction are correlated with their counterparts at the municipality level, multicollinearity may potentially cause problems in the estimations. Table 1 shows a matrix of correlation coefficients for changes in unemployment and growth at the regional and municipality level.

Table 1: Correlation coefficients

	ΔG_R	ΔU_R	ΔG_M	ΔU_M
ΔG_R	1.000			
ΔU_R	-0.521	1.000		
ΔG_M	0.901	-0.459	1.000	
ΔU_M	-0.525	0.962	-0.462	1.000

As can be seen, the correlations between corresponding measures at the different levels are very high, while the correlation between changes in unemployment and growth within each level is more modest.[13] I, therefore, compare the results obtained when only municipality level measures and when only regional level measures are included with results when measures at both levels are included in the regressions. If multicollinearity causes severe problems, I expect the estimates to vary excessively between the regressions and to be imprecisely estimated.

Table 2 shows the results from five different specifications.[14] Column 1 shows the estimates when the model is estimated with only regional level measures and year dummies. In column 2, municipality level measures are used instead. In column 3, both the regional and municipality level are included and in column 4, three demographic control variables are added. In column 5, I include economic variables that are multiplied with one if the municipality government is of the same type (left/right) as the national government and zero otherwise.[15]

[13] All correlations are statistically significant at the one percent level.

[14] These results are obtained using averages of the economic variables over the full term of office. The literature on political business cycles suggests that voters respond more to economic conditions closer to the election. One could therefore argue that the regression model should be specified with economic variables that give a higher weight to conditions close to the elections. I have replicated the results from this table using averages over the election year and the year before instead of full period averages. The results are qualitatively similar and available upon request.

[15] If the share of the parties that form the governing bloc on the national level has received more than 50 percent of the votes in the municipality election, that municipality is coded as same as the national incumbent.

Table 2: Effects of regional and municipality level economic conditions on government support

	(1)	(2)	(3)	(4)	(5)
Dep. Var.	ΔGov	ΔGov	ΔGov	ΔGov	ΔGov
ΔG_R	0.481**		0.640***	0.634***	0.515**
	(0.212)		(0.234)	(0.217)	(0.250)
ΔU_R	-1.421***		-0.983***	-0.960***	-0.679***
	(0.210)		(0.196)	(0.210)	(0.214)
ΔG_M		-0.059	-0.147**	-0.116*	-0.133*
		(0.055)	(0.066)	(0.065)	(0.080)
ΔU_M		-0.492***	-0.377***	-0.335***	-0.332***
		(0.069)	(0.066)	(0.061)	(0.120)
Δyoung				0.283***	
				(0.101)	
Δold				-0.402*	
				(0.220)	
Δforeign				-0.018	
				(0.115)	
Municipality incumbent					0.275
					(0.220)
ΔG_R×municipality incumbent					-0.204
					(0.172)
ΔU_R×municipality incumbent					-0.024
					(0.269)
ΔG_M×municipality incumbent					-0.172
					(0.148)
ΔU_M×municipality incumbent					-0.084
					(0.242)
y85	-1.388**	-0.020	-1.458**	-1.303**	-0.607
	(0.676)	(0.225)	(0.678)	(0.613)	(0.685)
y88	-3.626***	-1.203***	-3.676***	-3.551***	-2.780***
	(0.762)	(0.178)	(0.724)	(0.686)	(0.737)
y91	-5.356***	-5.883***	-5.223***	-5.327***	-5.469***
	(0.397)	(0.178)	(0.392)	(0.380)	(0.375)
y94	3.280**	-3.435***	2.870**	2.676**	0.533
	(1.393)	(0.398)	(1.273)	(1.332)	(1.391)
y98	-11.38***	-9.360***	-11.405***	-11.080***	-10.577***
	(0.864)	(0.346)	(0.854)	(0.775)	(0.871)
y02	-1.405	2.098***	-1.233	-0.979	-0.123
	(1.136)	(0.574)	(1.058)	(1.062)	(1.108)
Constant	No	No	No	No	No
Elections	6	6	6	6	6
Observations	1685	1675	1675	1675	1675
Adj R^2	0.89	0.89	0.89	0.90	0.90

Notes: Estimates based on WLS estimations. Giving all municipalities the same weight yields qualitatively similar results. Standard errors clustered on municipalities in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%. U and G refer to unemployment and growth, subscript M and R to municipality and region.

The estimates in column 1 indicate a rather large and statistically significant effect of both economic growth and unemployment at the regional level. A one percentage point increase in growth or decrease in unemployment is associated with a 0.5 and 1.4 percentage points increase in the support for the incumbent government. Column 2 shows the corresponding results for the municipality level. The estimate for the effect of growth is virtually zero but the effect of changes in unemployment is statistically significant and about -0.5. When all measures are included at the same time, in column 3, the effect of growth at the regional level increases to 0.6 and the effect of unemployment decreases to -1.0. At the municipality level the effect of unemployment decreases somewhat but the effect of growth becomes negative and statistically significant. The impression of including all measures at the same time is that most of the estimates are rather stable and precisely estimated indicating that multicollinearity may not be too big a problem. Three of the coefficients take expected signs, implying that improving economic conditions is beneficial for the incumbent government. The only estimate that is somewhat surprising is the effect of growth at the municipality level that is negative and statistically significant when all four measures are included. The data contain five elections in which social democrat governments have been evaluated and only one election in which a centre-right government has been evaluated. A possible explanation of why this estimate is negative could be that two different effects may be at work. When municipalities get richer they may be more likely to support centre-right governments, with less redistributive policies, but also to be more satisfied with the incumbent government. This would mean that only centre-right governments should unambiguously benefit from increased growth at the municipality level. This issue will be examined further in section 5.4.

The year dummies, capturing the effects of changes at the macro level, explain a lot of the variation in the change of government support indicating that we cannot rule out important effects of national level economic conditions. The largest estimate is found at the election in 1998, an election in which the Social Democratic Party received the lowest support since the unicameral system was introduced in 1970.

Column 4 shows the results from a regression in which three demographic control variables are included: the change in the fraction of citizens of age 18 or younger (Δyoung), the change in the fraction of people age 65 or older (Δold), and the change in the fraction of foreign citizens (Δforeign). Demographic variables are commonly used to predict party choice rather than government support and therefore all three demographic variables are multiplied with minus one for the 1994 election when there was a centre-right government. The reason to include demographic control variables is to make sure that the estimates of unemployment and growth neither at the municipality level nor at the regional level are biased due to different trends in the demographical structure of the municipalities. Adding these variables

do not lead to any significant change in any of the estimates from column 3. An increase in the fraction of foreign citizens or the fraction of old people by one percentage point is associated with a change in the support for a social democrat (centre-right) governments by +0.28 (-0.28) and -0.4 (+0.4) percentage points. A change in the fraction of young people do not have a statistically significant impact in the regression. Since excluding or including these variables does not alter the coefficients of interest in this paper, demographic controls will be omitted in the remainder of the analysis. Furthermore, since covariates that are time-invariant cancel out in a first difference model, I believe that the estimates of unemployment and growth are not severely biased as a result of potentially omitted variables.

Column 5 tries to capture the idea that voters may decide to punish or reward the national incumbent for local economic conditions depending on whether the municipality government is of the same type as the national government or not. For example, if local economic conditions are improving and the Social Democrats are incumbents both at the national and municipality level, then some voters may conclude that policies by the Social Democrats are beneficial for their municipality or region. If the municipality and national governments are of different types then it may be less clear what type of policy that has caused the economic outcomes. None of the estimates of these interaction terms are, however, statistically significant and the point estimates are all rather small. This indicates that when voting in the national election, voters do not condition their response to local economic conditions on whether the municipality government is of the same type as the national government or not. As a consequence these interaction terms will be omitted in the remainder of the analysis.

What do these results say about the importance of local economic circumstances for voting behavior? Clearly, effects at the regional level are larger than at the municipality level. At the regional level, a one percentage point increase in the growth rate or decrease in unemployment is associated with 0.6 and 1.0 percentage point higher support for the incumbent government. As can be seen in Table A1, the mean change (over regions and between elections) in regional growth varies between -2.4 and 3.7 percentage points and for unemployment the corresponding numbers are -2.8 and 5.7. The standard deviation of change in regional growth is 2.5 percentage points and for unemployment 2.6 percentage points. A one percentage point change in either growth or unemployment can therefore not be considered an unusually large change in these variables.

Clearly, these results suggest that voters respond to economic conditions at the regional level. To investigate whether it makes sense to use the regions as defined here, rather than the county level, in the regressions, responses to county level economic conditions have also been investigated. Using the county level instead of regional level growth and unemployment variables

yields somewhat smaller coefficients (growth: 0.48; unemployment: -0.74).[16] When both levels are included at the same time the coefficients of county level variables become much smaller (growth: 0.14: unemployment: -0.39) and statistically insignificant, while at the regional level the coefficients remain economically but not statistically significant (growth: 0.52; unemployment: -0.60). The correlations between the economic variables at the two levels are close to one, which makes it hard to safely say that responses are stronger to economic conditions at regional level than to the county level. The results point, however, to stronger responses to the regional level and for that reason the regional level will be used in the remainder of the analysis.

Although this dataset is not suited for estimating effects of national level economic conditions it is anyway interesting to relate the effects of local economic conditions to effects of macroeconomic variables. The year dummies in Table 2 suggest that changes in national level variables play an important role in explaining voting behavior. These dummies, however, capture not only effects of economic variables but all effects at the national level. This makes it difficult to infer that growth and unemployment at the national level are important from the estimates of the dummy variables. Many studies have, however, estimated effects of national level economic variables. In their survey of the economic voting literature Nannestad and Paldam (1994) conclude that the effect of changes in unemployment is found to be between -0.4 and -0.8 in most studies and that growth measures sometimes are more and sometimes less important than unemployment. Markus (1988) finds an unusually large effect of real disposable income per capita. A one percentage point increase is associated with 2.3 percentage points increase in the vote share of incumbent American presidents. He does, however not control for changes in unemployment which may explain why the effect of growth was unusually high. Jordahl (2006) analyzes Swedish data between 1985 and 1994 and finds that a one standard deviation decrease in unemployment is associated with 17 percentage points increase in the probability to vote for the governing bloc, but no or small effects of growth. The overall impression is, although with some exceptions, that effects of national level economic variables are of the same magnitude as the effects found at the regional level here. It is difficult to say if the effects of national level economic variables would change if measures of regional level variables would be included in such analyses, but it is plausible to believe that the effects of national level variables are biased upwards when regional level measures are excluded. Together this indicates that regional level economic conditions may be at least equally important for voting behavior as national level economic variables.

[16] Complete regression results for an analysis of county level responses are not reported here, but available upon request.

5.3 Support for government or political bloc?

An implicit assumption in the empirical investigation so far has been that voters hold the parties actually represented in the government responsible for the economic conditions. Typically, studies of economic voting in Sweden analyze how the support for the governing bloc depends on the economic conditions. The reason for treating the Swedish system as a two party system is that the parties within the two blocs tend to cooperate to a large extent. The centre-right bloc has formed coalition governments in which almost the entire bloc have been included.[17] The Social Democratic Party on the other hand has ruled as a minority government and therefore been forced to seek support from other parties. During the period covered in this study the Social Democratic Party has cooperated intensively with the Left Party and for extended periods also with the Green Party and the Centre Party. This makes it possible that voters hold not only the Social Democratic Party responsible for the economic conditions but also the cooperating parties. As a consequence it may be motivated to treat the Swedish system as a two party system and look at changes in the governing bloc's support.

To know how the results from this study depend on whether the governing parties or the entire bloc is used as dependent variable, estimates with the change in the governing bloc as dependent variable is presented as well.

Table 3 displays the estimates obtained with the change in the governing bloc as dependent variable. Column 1 is reproduced from Table 2, column 3, to facilitate the comparison. Column 2 shows the results when the whole bloc is used as dependent variable. It indicates that the effect of growth at the regional level is smaller and statistically insignificant when the bloc is used as dependent variable, while the coefficient for unemployment is somewhat larger. At the municipality level both estimates are close to zero and statistically insignificant. As is stands, it indicates that voters do not punish or reward the whole bloc for high or low economic growth at the regional level, but only the parties represented in the government. A deeper investigation reveals, however, that this conclusion may be misleading. During 1991 to 1994, the New Democrats, a party of discontent, supported the centre-right government. This party was the only centre-right party that was not a part of the coalition government. The party was founded in 1991 and received 6.7 percent of the votes in 1991. In 1994, however, they received only 1.4 percent of the votes and virtually disappeared from the national political arena. The drop in support for the centre-right bloc, in 1994, is therefore roughly 12 percentage points if the New Democrats are included and only about 6 percentage points if they are excluded. In column 3 the New Democrats has been excluded from the centre-right bloc, resulting

[17] In 1991 to 1994 the New Democrats (Ny Demokrati) was represented in the parliament and gave their support to the centre-right bloc, but was not included in the government.

in estimates that are similar to those in column 1 for the regional level. At the municipality level, the estimates are still close to zero. This shows that if the New Democrats are excluded from the centre-right bloc, both the parties in the government and the parties supporting it are rewarded and punished in similar ways in response to regional economic conditions.

Table 3: Effects of unemployment and economic growth on the governing bloc

	(1)	(2)	(3)
Dep. Var.	ΔGov	ΔBloc (alt 1)	ΔBloc (alt 2)
ΔG_R	0.640***	0.403	0.642**
	(0.234)	(0.284)	(0.283)
ΔU_R	-0.983***	-1.378***	-1.195***
	(0.196)	(0.223)	(0.251)
ΔG_M	-0.147**	0.039	-0.035
	(0.066)	(0.090)	(0.092)
ΔU_M	-0.377***	-0.099	-0.182
	(0.066)	(0.097)	(0.113)
Year Dummies	Yes	Yes	Yes
Constant	No	No	No
Elections	6	6	6
Observations	1675	1675	1675
Adj R^2	0.89	0.89	0.79

Notes: Alt 1 includes the New Democrats in the right-wing bloc. Alt 2 excludes the New Democrats from the centre-right bloc. Estimates based on weighted least squares estimations. Standard errors clustered on municipalities in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%. U and G refer to unemployment and growth, subscript M and R to municipality and region.

5.4 Do voters treat left and right-wing governments differently?

Is the negative effect of growth at the municipality level (from Table 2, column 3) a result of heterogeneity in how voters reward and punish the Social Democratic and the centre-right governments? Both the clientele hypothesis (Rattinger, 1980) and the salient goal hypothesis (Powell and Whitten, 1993) suggest that left- and right-wing governments are treated differently. More specifically, the clientele hypothesis suggests that left-wing parties are punished less in times of high unemployment, since they often put a higher priority at fighting unemployment than right-wing parties. According to the salient goal hypothesis, parties are held responsible for issues that they care much about. That means that left-wing parties may be more severely punished for high unemployment than right-wing governments. Table 4 shows results from two regressions. In column 1 the five periods with Social Democratic (left-wing) governments are used and in column 2 only the election in 1994, when a centre-right (right-wing) government was evaluated, is used. This makes the number of observations

differ much between the two regressions and as a consequence the results are not perfectly comparable but do anyway illustrate if some heterogeneity may be present in how citizens vote when left- and right-wing governments are evaluated.

At the regional level the size of the estimated coefficient for unemployment is roughly doubled for right-wing governments, which gives some support for the clientele hypothesis but contradicts the salient goal hypothesis. The coefficient for growth is roughly the same for both left- and right-wing governments. At the municipality level the effect of growth is negative for left-wing governments, but positive, although not statistically significant, for right-wing parties. This is consistent with the hypothesis discussed in section 5.2 that when municipalities get richer they tend to be more inclined to vote for right-wing parties. It is also in line with Leigh (2005), who finds that citizens living in richer neighborhoods are more likely to support right-wing parties. The effect of unemployment at the municipality level is more than doubled for right-wing governments. Together these results indicate that voters may treat left- and right-wing governments differently. To draw firm conclusions about the differences, one would ideally evaluate more than one right-wing government. Since the current Swedish government is formed by a centre-right government, such an analysis will be feasible at the earliest after 2010.

Table 4: Heterogeneity between left- and right-wing governments

	(1)	(2)
Dep.var.	ΔGov	ΔGov
	(left wing)	(right wing)
	Social Democrats	centre-right government
ΔG_R	0.692***	0.794
	(0.252)	(0.485)
ΔU_R	-0.851***	-1.662***
	(0.226)	(0.440)
ΔG_M	-0.199***	0.102
	(0.076)	(0.122)
ΔU_M	-0.240**	-0.595***
	(0.105)	(0.126)
Year Dummies	Yes	Yes
Constant	No	No
Elections	5	1
Observations	1393	282
R-squared	0.89	0.93

Notes: Estimates based on weighted least squares estimations. Standard errors clustered on municipalities in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. U and G refer to unemployment and growth, subscript M and R to municipality, county and region.

6. Concluding remarks

Empirical studies of economic voting have traditionally not included local economic conditions in their analyses. Johnston and Pattie (2001) questioned this methodology and showed that in the British General Election 1997 voters responded strongly to economic conditions in "their part" of Britain. Furthermore, empirical studies related to the literature on tactical redistribution have shown that citizens respond to central government spending in local districts and municipalities (Levitt and Snyder, 1997 and Jordahl, 2002).

Using data covering six elections, this paper has improved the understanding of how voters respond to local economic conditions by estimating responses to the most commonly used measures of economic conditions, viz. economic growth and unemployment. The empirical investigation points to a substantial response to changes in both regional level growth and unemployment. The preferred specification suggests that increasing regional growth or reducing regional unemployment by one percentage point is associated with an increase in the vote share for the government by about 0.6 and 1.0 percentage points from voters in that region. At the municipality level the effect of unemployment is lower than at the regional level but still statistically significant in most specifications, while the effect of growth is close to zero or negative. One possible, although speculative, explanation for the small responses to municipality level economic conditions could be that voters consider the local governments to be responsible for growth and unemployment in the municipalities.

The finding that voters respond to economic conditions at the regional level indicates that voters both know how their own region is doing relative to the country overall, but also that they use this information to reward the government if conditions have improved during its term in office.

Exposing the results to various robustness tests do not change the overall picture that voters seem to respond to changes in economic conditions at the regional level but not nearly as strongly to conditions at the municipality level.

Together the results indicate that regional economic conditions matter for how citizens vote and may be fruitfully included in empirical voting equations. If measures of regional economic conditions are omitted, then estimates of other variables may be biased and misleading. This paper also shows that voters respond not only to local spending by the central government, but also to traditional measures of economic outcomes. This further justifies the assumption in models of political behavior that voters are sensitive to local levels of economic conditions.

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Appendix A: Description of data

ΔGov: The change in the vote share of the parties forming the government. For example, at the election 1994 it is calculated as the total vote share of the four centre-right parties forming the coalition government 1991 to 1994 at the election 1994 minus the total vote share of the same parties at the election 1991. At the 1994 election this variable consists of the Moderate Party, the Centre Party, the Liberal Party, and the Christian Democrats (Kristdemokraterna) and at all other elections of the Social Democratic Party (Socialdemokraterna). Unit: percentage points. Source: www.scb.se.

ΔBloc: The change in the governing bloc's vote share. For example, at the election 1994 it is calculated as the total vote share of the five centre-right parties in 1994 minus the total vote share of the same parties at the election 1991. The left bloc is defined as the Social Democratic Party, the Left Party, and the Green Party and is being used at the elections 1985, 1988, 1991, 1998 an 2002. The centre-right bloc is defined as consisting of the Moderate Party, the Centre Party, the Liberal Party, and the Christian Democrats, and the New Democrats (Ny demokrati) and is being used at the elections 1982 and 1994. Unit: percentage points. Source: www.scb.se.

ΔU_M: The change in the mean unemployment rate between two elections. For example, at the election 2002 it is calculated as the arithmetic mean of the unemployment rate for 1999, 2000, 2001, and 2002, minus the mean unemployment for 1995, 1996, 1997, and 1998. The unemployment rate is calculated as the number of unemployed divided by the population aged 16–64. Unit: Percentage points. Source: www.ams.se.

ΔG_M: The change in mean of the growth rate of the tax base in the municipality, between two elections. For example, at the election 2002 it is calculated as the geometric mean of the tax base for 1999, 2000, 2001, and 2002, minus the tax base for 1995, 1996, 1997, and 1998. The tax base is deflated by CPI. Unit: Percentage points. Source: www.scb.se.

ΔU_C: The weighted mean of ΔU_M of all municipalities in each county. The weights are proportional to the municipalities' share of the population in the county. Unit: Percentage points.

ΔG_C: The weighted mean of ΔG_M of all municipalities in each county. The weights are proportional to the municipalities' share of the population in the county. Unit: Percentage points.

ΔU_R: The change in the weighted mean of ΔU_M of all municipalities in each region. The weights are proportional to the municipalities' share of the population in the region. Unit: Percentage points.

ΔG_R: The weighted mean of ΔU_M of all municipalities in each region. The weights are proportional to the municipalities' share of the population in the region. Unit: Percentage points.

Δyoung: The change in the fraction of people aged 18 or younger since the last election year in each municipality. Unit: Percentage points.

Δold: The change in the fraction of people aged 65 or older since the last election year in each municipality. Unit: Percentage points.

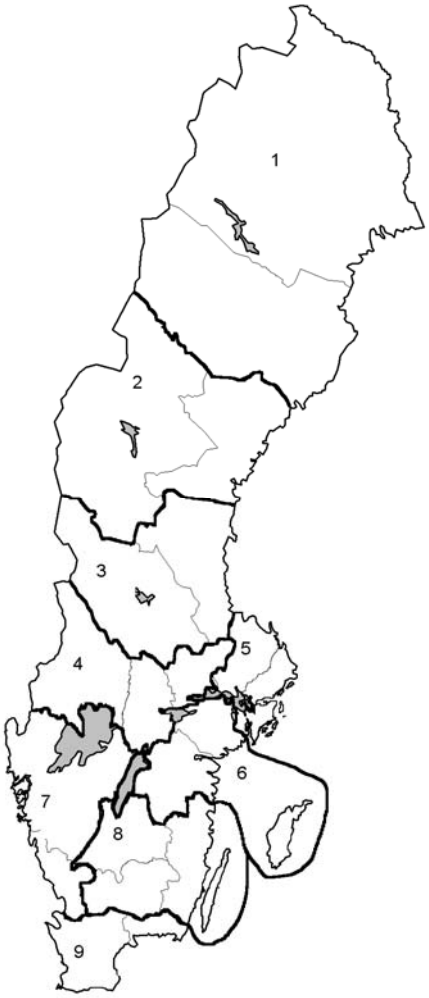
Δforeign: The change in the fraction of foreign citizens since the last election year in each municipality. Unit: Percentage points.

Appendix B: Description of regions

Table A2: Composition of regions

1. Nordnytt	Norrbottens län
	Västerbottens län
2. Mittnytt	Jämtlands län
	Västernorrlands län
3. Gävledala	Dalarnas län
	Gävleborgs län
4. Tvärsnytt	Värmlands län
	Örebro län
	Västmanlands län
5. ABC	Stockholms län
	Uppsala län
6. Östnytt	Södermanlands län
	Östergötlands län
	Gotlands län
7. Västnytt	Göteborgs & Bohus län
	Älvsborgs län
	Skaraborgs län
	Hallands län
8. Smålandsnytt	Jönköpings län
	Kronobergs län
	Kalmar län
9. Sydnytt	Malmöhus län
	Kristianstad län
	Blekinge län

Note: Län is the Swedish word for county.



Map of Regions

1. The Nordnytt region
2. The Mittnytt region
3. The Gävledala region
4. The Tvärsnytt region
5. The ABC region (The capital region)
6. The Östnytt region
7. The Västnytt region
8. The Smålandsnytt region
9. The Sydnytt region

Essay III

Correcting mistakes: Cognitive dissonance and political attitudes in Sweden and the United States

1. Introduction

Economists routinely assume that preferences are stable and determine people's behavior. In direct contrast, social psychologists suggest that behavior influences preferences and attitudes.[1] Cognitive dissonance theory has emerged as the most prominent development along these lines. The theory has been suggested to provide important insights to many areas involving human behavior.[2] The empirical support for the theory has, however, hitherto almost exclusively relied on psychological experiments, many of which have been heavily criticized by Chen (2008).

On prediction from cognitive dissonance theory is that the act of voting makes people more positive toward the party or candidate they have voted for, which suggests an effect also on how people vote in future elections. A few studies have found such an effect of voting on political attitudes (Beasley and Joslyn, 2001; Anderson et al., 2004; Mullainathan and Washington, 2008). Since these studies all measure effects on political attitudes long before an opportunity to vote, they provide limited information about the relevance of cognitive dissonance theory to voting and elections. When political attitudes are measured long before an opportunity to vote, there is no guarantee that the measured attitudes persist until the next election. If cognitive dissonance theory is of relevance to voting, the empirical evidence should at least support that political attitudes measured just before elections are biased toward the previously supported candidate or party.

[1] While preferences are inherently unobservable, attitudes are herein viewed as observable, and are thus akin to stated preferences.

[2] For reviews see: Harmon-Jones and Mills (1999) and Cooper (2007).

This paper contributes to this literature by testing if the act of voting influences political attitudes – measured just before elections – when they are highly predictive of party and candidate choices. As in Mullainathan and Washington (2008), the effect on political attitudes is identified by making use of exogenous variation in turnout provided by the voting age restriction. In contrast to earlier studies, I find no effect on political attitudes. This indicates that cognitive dissonance theory may not be as relevant for voting as previous studies have indicated. This result holds for a variety of political attitudes and for data from both Sweden and the United States.

Mullainathan and Washington argue that if cognitive dissonance theory has bearing on voting, then it provides a new explanation for the incumbency advantage and thus a motivation for term limits. They also argue that high turnout could then lead to inefficient electoral outcomes since a larger body of voters would then obtain biased attitudes toward candidates. These arguments rely on the assumption that the effect of voting on political attitudes has repercussions for future voting behavior. This assumption is not supported by data from Sweden or the United States. Instead, it seems as if any bias present after the first voting occasion has disappeared by the time of the subsequent election, hence casting doubt on the relevance of cognitive dissonance theory for voting and elections.

The theory of cognitive dissonance was first spelled out by Festinger (1957). Applied to the context of voting, the mechanism at work could be described as follows. Consider a young person being eligible to vote for the first time. She wants to vote for the party (or candidate) that best serves her political interests. She collects information about the different parties and votes for the preferred party. After the election, new information becomes available. This information may or may not support her prior that she voted for the "right" party. If she voted for the "wrong" party, her action was dissonant with her intention and a discomforting feeling arises. She can reduce the unpleasant feeling of dissonance either by changing her actions or by changing her political cognitions[3] so that the party she voted for still appears to be the best choice. But, when it comes to elections she typically has to wait several years before getting an opportunity to vote for another party. To relieve dissonance she may, therefore, instead change her political preferences or filter the available information in such a way that the chosen party appears to be better than it is.[4] Akerlof and Dickens (1982: p. 309)

[3]To quote Festinger (1957, p3) "By the term cognition, here and in the remainder of the book, I mean any knowledge, opinion, or belief about the environment, about oneself, or about one's behavior." The cognitions under study herein are political attitudes. Note also that the change of cognitions need not be conscious, but could just as well be a subconscious undertaking.

[4] It has also been suggested that to avoid the negative feelings caused by dissonant cognitions, people may change cognitions in advance of events that could question their choices. In the context of voting this would mean that voters become more

summarize the mechanism at work as: "… persons who have made decisions tend to discard information that would suggest such decisions are in error because the cognition that the decision might be in error is in conflict with the cognition that ego is a smart person."[5]

The relevant question is therefore if citizens tend to vote for the same party in subsequent elections, even when the circumstances have changed so that they otherwise had voted for another party, or if they behave more rationally and vote for a different party.[6] Because of data limitations it is, however, difficult to perform direct tests of whether the act of voting influences party choice in future elections. We therefore have to assess the relevance of cognitive dissonance theory, to voting and elections, by its impact on political attitudes. As a consequence it is important that we investigate political attitudes as close as possible to the elections in which we really would have liked to investigate voting.

2. Previous literature and methodological considerations

Empirical studies have suggested that cognitive dissonance theory provides fruitful insight to, for example, religious behavior (Festinger et al., 1964), protection against HIV (Stone et al., 1994), curing phobias (Cooper, 1980), effects of terror attacks (Masters, 2005), and to behavior among monkeys (Egan et al., 2008). In an influential article, Akerlof and Dickens (1982) argue that cognitive dissonance theory may have important implications also for a wide range of economic problems such as safety regulation, social security, innovation, marketing, and crime. Recently, cognitive dissonance theory has also been suggested to provide insights to the understanding of voting behavior and elections.

While providing interesting and appealing theoretical predictions – sometimes directly contrasting with predictions from rational choice theory – many empirical tests of cognitive dissonance theory suffer from severe methodological problems. As mentioned in the introduction, Chen (2008) criticizes much of the empirical tests of cognitive dissonance theory

positive to the chosen party, even if there is no information that questions their choice (Festinger, 1957).

[5] It should, however, be noted that the motivational mechanism for dissonance arousal is still under debate. For example, Aronson (1968, 1992) argue that dissonance arises when behavior conflict with one's view of oneself (typically but not necessarily as being smart, moral, honest, etc). Cooper and Fazio (1984) quite differently argue that dissonance arouses when behavior has aversive consequences for others.

[6] Although the original theory of cognitive dissonance acknowledge that dissonance can be reduced by changing behavior, this has been claimed to be less likely than changing attitudes (Cooper, 2007). Furthermore, it is when dissonance is reduced by changing attitudes that cognitive dissonance theory predicts interesting effects on voting.

in what has been labeled "The Free Choice Paradigm". This line of research started with Brehm's (1956) experiment, in which subjects were asked to rate objects and were then given a choice between two of the objects to take home. After the choice, they were asked to again rate all of the objects. The chosen object were then found to be rated higher and the rejected object lower than in the initial stage. This finding has been interpreted as evidence of choice induced attitude change and support for the theory of cognitive dissonance. Chen, however, points out that all studies in this tradition may be plagued by a problem of measurement error in ratings and fail to recognize that the choice may contain additional information about the respondents' preferences. This critique casts considerable doubts on the value of the empirical support for cognitive dissonance theory, although its factual consequence has not yet been investigated.

When it comes to voting, Beasley and Joslyn (2001) claim that the act of voting influences attitudes toward the presidential candidates in the United States. Their evidence is based on a comparison of attitudes in the pre-election survey and the post-election survey of the American National Election Studies. They find that those who report having voted tend to have more polarized attitudes toward the President than those who report that they abstained. Anderson et al. (2004) use data from the British Election Studies and compare perceptions of national economic conditions before and after the 1997 election. They find that after the election citizens that voted for the incumbent have a more positive view of past economic conditions than they had before the election.

The analyses by both Beasley and Joslyn (2001) and Anderson et al. (2004) suffer from several methodological problems. First, they are directly hit by the critique by Chen. Second, if information about the parties' achievements and the state of the economy becomes more cheaply available in the end of an election campaign, it would be no surprise if those who voted for the winner changed their attitudes in favor of the winner already before voting.[7] The same argument applies to those who voted for the loser. This mechanism would produce results similar to what Anderson et al. (2004) and Beasley and Joslyn (2001) interpret as support for cognitive dissonance. Third, Mullainathan and Washington (2008) criticize the approach in Beasley and Joslyn (2001) with the argument that turnout is likely to be correlated with attitudes. Consequentially, that approach almost

[7] To see this, suppose that there are two groups of voters A and B. Group A does not care about economic growth while group B does. Just before the election it becomes clear that the incumbent is responsible for low economic growth. Voters belonging to group A does not change their attitude toward the incumbent or the opposition, while members of group B become relatively more in favor of the opposition. As a consequence the probability that the opposition wins the election increases. When investigating political attitudes after the election, then if the opposition won, we will find that those who voted for the winner will have obtained a more negative view of past economic development.

automatically generates results that Beasley and Joslyn (2001) interpret as evidence of cognitive dissonance mechanisms being at work, even without any effect of voting on attitudes. Finally, even if none of these objections would be applicable, we would still not know if the attitude change would last until the next election and thus have consequences for voting.

Mullainathan and Washington avoid the problems associated with using pre- and post-election comparisons. Instead they identify the causal effect of voting on political attitudes with the exogenous variation in turnout provided by the voting age restriction. They find that those who were eligible to vote in the previous U.S. presidential election and report themselves as affiliated with the President's party rate the President almost 10 percentage points higher compared with those who were ineligible to vote but also consider themselves as affiliated with the President's party. As a result they find support for cognitive dissonance theory in the context of U.S. presidential elections.

The research design applied by Mullainathan and Washington clearly takes the literature forward by credibly identifying a causal effect of voting on political attitudes. Nevertheless, some problems and limitations remain. As mentioned in the introduction we are not sure whether the change in attitudes persists until the next election. This is a critical condition for voting behavior to be biased in favor of the previously supported candidate.

In an extension to their main analysis, Mullainathan and Washington test if their results hold if political attitudes are measured just before the subsequent presidential election. Interestingly, that analysis lends no support for cognitive dissonance mechanisms being at work. They claim, however, that the results from this extension are too weak, due to small point estimates and large standard errors, to draw firm conclusions and call for more research on this matter – a call that is answered in this paper.

Low turnout in American elections provides a further limitation, since cognitive dissonance theory predicts that only those who actually have voted should change their attitudes. The fact that less than half of the interviewed respondents actually voted in the elections could therefore confound the results.

In this paper, I make use of a similar research design as in Mullainathan and Washington, with the important difference that attitudes are measured just before elections, so as to more credibly investigate if any repercussion on future voting is likely. Furthermore, I extend the analysis to an investigation of data from both Sweden and the United States. The Swedish Election Studies contain similar questions as the American National Election Studies. This allows for a reexamination of the relevance of cognitive dissonance on a new data set, with a minimum of deviations in the empirical methodology. By using Swedish data, I also avoid the problem with low turnout in American elections. Turnout among first time voters in Sweden is very high in international comparisons and vary between 79 and 89 percent for the time period of this study.

2. The Swedish political system

A brief description of some key features of the Swedish political system follows. The Swedish Parliament (Riksdagen) is the country's legislative body and appoints the Prime Minister, who then selects ministers to form a government. Elections to the parliament were held every three years before 1994 and are held every four years since then. To be eligible to vote a citizen must be 18 years old on Election Day. Seats in parliament are allocated by proportional representation.

The Social Democratic Party has been the largest party in Sweden during the entire time-span for this investigation, receiving between 36.4 and 45.7 percent of the votes. The Social Democratic Party has ruled as a minority government for most of the years since 1982 (1982–1991 and 1994–2006). Between 1976 and 1982 a centre-right coalition[8] served two terms and between 1991 and 1994 another centre-right coalition[9] served one term.

3. Data

The empirical analysis uses data from the Swedish Election Studies and the American National Election Studies. Both data sets consist of individual-level survey data. In addition the Swedish data are supplemented with register data.[10] Below follows a description of the Swedish Election Studies.[11]

3.1 Swedish Election Studies

A separate wave of the Swedish Election Studies has been conducted in connection with all major elections in Sweden since 1956. The later surveys consist of a larger number of questions, which limits the time-span for this analysis back to 1979 for attitudes toward the parties and the Prime Minister.

[8]Between 1976 and 1979, the coalition consisted of the Centre Party, the Liberal Party, and the Moderate Party (Conservatives). In 1978 Prime Minister Torbjörn Fälldin (the Centre Party) left his post as Prime Minister, when both the Centre Party and the Liberal Party left the government. Ola Ullsten (The Liberal Party) became Prime Minister until the election in 1979. In 1979, Fälldin again became Prime Minister with the same three parties represented in the government. He kept that post until 1982. Excluding the 1982 election from the analysis does not change the qualitative results.

[9]This time it consisted of The Moderate Party (Conservatives), with Prime Minister Carl Bildt, the Centre Party, the Liberal Party, and the Christian Democrats.

[10] Appendix A and B report detailed descriptions of all variables as well as descriptive statistics for the different samples used in the analyses

[11] As the data from the American National Election Studies is publicly available and extensively discussed elsewhere, I refer to http://www.electionstudies.org, for references and description of that data.

In each wave about 3,000 respondents are interviewed, with oversampling of first time voters.

Turnout in Sweden is normally very high compared to other developed democracies with non-compulsory voting. From 1979 to 2002, turnout rates have varied between 81 and 94 percent. Table 1 displays turnout rates for first time, second time, and all eligible voters from 1979 to 2002. As can be seen, the turnout rates are marginally lower for first and second time voters than for the electorate at large. A t-test for different turnout rates between first and second time voters does not reject that they are equal. The data come from the electoral register for the sample contained in the Swedish Election Studies. The higher the turnout rates, the better the prospects for testing if voting influences the political attitudes, since those who did not vote in the previous election are predicted not to change their attitudes. Clearly, turnout rates among young Swedish citizens are much higher than the average of 48 percent that Mullainathan and Washington report for the United States in their study.

Table 1: Turnout

	1st-time eligible voters	2nd-time eligible voters	All eligible voters
1979	87	89	91
1982	86	89	91
1985	87	88	90
1988	81	78	86
1991	80	81	87
1994	89	85	87
1998	80	82	81
2002	79	77	80

Note: The numbers refer to the percentage of eligible voters that actually voted in the election, taken from the electoral register for the sample in the Swedish Election Studies, including non-respondents.

A key quality indicator of any survey is the response rate. The response rates in the Swedish Election Studies vary between 69 and 82 percent (1979-2002), which is almost 10 percentage points higher than in the American National Election Surveys.[12] Non-response in the Swedish Election Studies have been found to be somewhat more common among women, low income earners, the old (>70 years old)[13], and citizens born abroad. The biggest difference in response rates is found between voters and non-voters (Holmberg and Oscarsson, 2004). Table 2 displays response rates for first time, second time, and all eligible voters. The numbers in parenthesis shows

[12] Between 1978 and 2000 the response rates in the American National Election Surveys varied between 59.8 and 74.0 percent.

[13] Respondents over 80 years old are not included in the surveys.

the response rates conditional on voting. As can be seen the response rates are typically higher for first and second time voters than for the electorate at large. Although the response rates are somewhat higher for young voters than for the whole young electorate, the differences are small.

The variables used in the empirical analysis are discussed in section 4. Appendix A and B report detailed descriptions of all variables as well as descriptive statistics for the different samples used in the analyses.

Table 2: Response rates (in percent)

	1st-time voters	2nd-time voters	All voters
1979	83 (83)	80 (82)	77 (80)
1982	85 (88)	85 (85)	79 (82)
1985	81 (84)	83 (85)	75 (78)
1988	84 (87)	78 (83)	72 (76)
1991	83 (87)	84 (87)	74 (77)
1994	84 (85)	83 (84)	79 (82)
1998	93 (93)	82 (85)	80 (85)
2002	71 (72)	68 (75)	69 (74)

Note: Numbers in parenthesis are conditional on voting

4. Empirical investigation

The empirical investigation starts by discussing methodological issues and presenting the empirical model in subsection 4.1. 4.2 presents the main results, using Swedish data, with attitude toward the Prime Minister's party as the dependent variable. Results for alternative political attitudes are presented in 4.3, still using Swedish data. 4.4 reviews the results in Mullainathan and Washington and provides complimentary analyses of American data. 4.5 discusses implications for voting behavior.

4.1 Empirical strategy

To make the results from this analysis as comparable as possible to the results in Mullainathan and Washington I will follow the methodology applied by them as closely as possible. The key difference is that attitudes now are measured just before elections. This means that citizens who experience dissonance because they have previously voted for the "wrong" party now have the opportunity to relieve dissonance, by voting for another party, close at hand. As Table 3 shows, attitudes toward the Prime Minister's party, as measured in the weeks before elections, are highly predictive of party choice. Very few of those who rate the Prime Minister's party in the lower categories, report to vote for that party. In the top categories, most of the respondents report to have voted for the Prime Minister's party. This pattern suggests that if we find an effect of voting on political attitudes then it is reasonable assume an effect on party choice as well.

Table 3: The relationship between pre-election attitudes and vote choice.

Attitude toward the PM's party	Voted for the PM's party (All voters)	Voted for the PM's party (First-time voters)
0 (negative)	0.2	0
1	0.5	0
2	1.2	4.4
3	0.9	2.5
4	1.1	2.9
5	7.2	4.2
6	10.8	3.2
7	19.4	27.4
8	47.6	41.3
9	70.0	61.5
10 (positive)	87.1	65.4

Note: Column 2 and 3 display fraction of voters that voted for the PM's party within each attitude category.

Recall, that turnout among young citizens is much higher in Sweden than in the United States which takes us closer to the theory by not letting as many non-voters confound our estimates. The results are obtained by estimating variations of the following equation on pooled cross sections:

$$Attitude_{i,t} = \beta(Second)_{i,t} + \gamma(Party)_{i,t} + \delta(Second \times Party)_{i,t} + \boldsymbol{X}'_{i,t}\theta + \varepsilon_{i,t},$$

where subscript i and t denotes that the variable takes individual and election specific values. Attitude, represents different attitudes in different specifications. In the main specification, it captures the respondent's attitude toward the party to which the incumbent Prime Minister belongs, measured in the weeks just before the elections. The reason why attitudes toward a party is chosen as the dependent variable, and not attitudes toward the Prime Minister, is that – in Sweden and other countries with a multiparty system – general elections are typically framed as a choice between parties rather than between candidates. To ensure that the results obtained by this specification do not differ from the results obtained by Mullainathan and Washington just because of this particular choice of dependent variable, other measures of political attitudes will be examined in subsection 4.3. Second is a dummy that takes the value one if the respondent was eligible to vote in the previous election and therefore now is eligible to vote for the second time and zero if the first election in which the respondent is eligible to vote. Party, takes the value one if the respondent is affiliated with the party of the incumbent. Second × Party is an interaction term between Second and Party. X´ is a row vector of socioeconomic control variables, which include interactions with a dummy variable taking the value one if the incumbent Prime Minister is

from the Social Democratic Party to capture the fact that those variables could be predictive both of the support for the incumbent in general and of the support for different parties (for more details about the variables see Appendix A).[14]

The idea behind this set up is to test if voters that were eligible to vote in the previous election have more polarized attitudes toward the supported party just before the current election than those who were not eligible to vote in the previous election. The parameter of interest is δ, which if it is positive supports that voting affects political attitudes. Those who voted for the winner when they were first time voters should have obtained more positive attitudes toward the incumbent when interviewed just before the next election than those who are about to vote for the same party for the first time.[15]

There are two reasons to focus the analysis on young voters. The first is, as already discussed, that we want to use the exogenous variation in turnout provided by the voting age restriction. The second reason is that if the act of voting would influence political attitudes then it is reasonable to assume that the effect is decreasing in the number of voting occasions, making it difficult to find an effect by analyzing citizens of all ages.[16]

As the focus is on young voters and we want to rule out that aging is driving the results, I follow Mullainathan and Washington and restrict the sample to contain only first and second time voters that are close in age. This means that the respondents are between 19 and 23 years old.[17]

Mullainathan and Washington include a set of socioeconomic characteristics and interaction terms in the regressions as well as account for year and state fixed effects. In line with this, I include similar variables as well as year fixed effects.

4.2 Attitudes toward the party of the Prime Minister

Table 3 displays the results obtained with Attitude toward the party of the Prime Minister as the dependent variable. This variable takes values between 0 and 10, where a higher number means that the respondent likes that party

[14] This empirical strategy is applied also to the American data in section 4.4, and is adopted from Mullainathan and Washington.

[15] Note that if δ is positive, and β is negative and have a smaller absolute value than δ, then we have two-sided polarization. This means that second time voters that are not affiliated with the Prime Minister's party have changed their attitudes to be less positive toward the Prime Minister' party.

[16] If this was not true, almost all old people would have extremely polarized political attitudes.

[17] Before 1998, 19-21 year old first time voters are included and 21-22 year old second time voters. In 1998 and 2002 the corresponding ages are 20-22 and 22-23. This change is a result of having elections every four years instead of every three years since 1994.

more. Since this question has been asked since 1979, the time-period covered by this analysis is 1979 to 2002.[18]

Only respondents aged 19 to 23 are included in the analysis. This limits the sample but is likely to produce estimates that are less vulnerable to potential bias stemming from age differences between first and second time voters than if the age span was wider. The estimated coefficient for the interaction term Second × Party (-0.125) is negative, small and statistically insignificant. The coefficient for Second is slightly negative (-0.213) but statistically insignificant. Party is, as expected, statistically significant and positive (3.641). Most of the socioeconomic controls and interaction terms do not obtain statistically significant coefficients (reported in Appendix C, Table C1). These first results suggest that the act of voting does not have such long-lasting influence on political attitudes so as to still be of importance in the subsequent election. As a comparison, Mullainathan and Washington estimate δ to be 0.93 (rescaled to be comparable with the results here) when political attitudes are measured two years after each election.

Table 4: Basic results for Sweden (1979-2002)

Dependent variable: Attitude toward the party of the Prime Minister				
	(1)	(2)	(3)	(4)
Second x Party (δ)	-0.125	-0.115	-0.029	-0.018
	(0.345)	(0.279)	(0.350)	(0.285)
Second (β)	-0.213	-0.144	0.078	-0.107
	(0.250)	(0.221)	(0.348)	(0.296)
Party (γ)	3.641***	3.812***	3.590***	3.741***
	(0.237)	(0.197)	(0.240)	(0.201)
Control variables	Yes	Yes	Yes	Yes
Interaction terms	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Age controls	-	-	-	-
Age span	19-22	18-23	19-22	18-23
Observations	556	847	556	847

Notes: Robust standard errors clustered on individuals in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

To examine if this difference is a coincidence or a robust feature of the data, small variations in the estimations are reported in column 2 to 4. In column 2 the sample is extended to include all first and second time voters. In column 3 are a linear control for age and the corresponding interaction term included; again with the sample restricted to respondents aged 19 to 23. Column 4 presents a combination of the extensions in column 2 and 3. The

[18] This closely corresponds to the sample period in Mullainathan and Washington, 1978–2000.

results are rather stable. The coefficients of interest (δ) are never statistically significant and in all regressions rather close to zero.[19]

An obvious critique against measuring party identification close to an election is that it may have changed since the last election. Mullainathan and Washington address this potential endogeneity problem by analyzing if those who were eligible to vote in the previous election are more likely to have extreme attitudes toward the President, than those who were not eligible. That test does not make use of the party variable and show that their results are not driven by endogeneity of the party affiliation variable. This endogeneity problem could be more problematic in the present analysis because of the longer time span between the last election and the interview. As a robustness test, I have therefore performed an identical test. It indicates that the results presented in this section are not driven by endogeneity of the party identification variable, since second time voters are no more likely to have extreme attitudes than first time voters.[20]

4.3 Analyzing alternative measures of political attitudes

So far we have only analyzed attitudes toward the Prime Minister's party. Before we draw too strong conclusions, it would be illuminating too see if the same result is found when alternative political attitudes are analyzed. Mullainathan and Washington analyze several attitudes toward the President as a person. To show that the discrepancy between their findings and the findings herein is not stemming from a particular choice of political attitudes, four alternative attitudes are analyzed below. These are similar to attitudes analyzed by Mullainathan and Washington, see subsection 4.4. Table 4 presents the results from this exercise. In the first column attitudes toward the Prime Minister is used as dependent variable. This variable takes values between 0 and 10, were 10 means that the respondent likes the Prime Minister very much. Column 2 to 4 show the results obtained when specific characteristics of the Prime Minister are analyzed. The three variables measure how much the respondent agrees with the statement that the Prime Minister is Knowledgeable, Inspiring, and Likable. The variables can take values between 1 and 4, were 4 means that the respondent completely agrees, and 1 means that the respondent strongly disagrees, with the statement. Since these questions have been asked in connection with four and two elections only, the sample sizes are relatively small. For that reason

[19] Furthermore, excluding the election in 1979 when there had been a restructuring of the government does not change the qualitative results. The δ coefficient becomes even closer to zero.

[20] The test is performed by estimating a logit model of *Second* on having extreme attitudes. The dependent variable is a dummy variable taking the value one if the respondent has an attitudes toward the Prime Minister's party that lies outside one standard deviation of the mean and zero otherwise. The estimated coefficient of eligible is -0.09 and the cluster robust standard errors are 0.16.

all first and second time voters are included in the regressions when Likable is used as dependent variable.

As can be seen in Table 4, attitude change induced by voting is not found for any of the four alternative attitudes. In fact, the results are similar to the ones obtained in section 4.2. The parameter of interest is never significantly different from zero and two of the coefficients take negative values. As a consequence we can conclude that when attitudes are measured just before elections we do not find evidence of the kind of polarization we would expect if the act of voting by itself had long term effects on political attitudes. If we would see that attitudes were influenced by the act of voting, in the direction predicted by cognitive dissonance theory, then in line with the argument in Mullainathan and Washington it would be reasonable to assume that voters tended to stick with their vote. This seems, however, not to be the case – at least not for Swedish voters.

Table 5: Alternative attitudes

Dependent variable:	Prime Minister	PM Knowledgeable	PM Inspiring	PM Likable
Second x Party (δ)	-0.336	0.081	0.333	-0.071
	(0.446)	(0.150)	(0.203)	(0.189)
Second (β)	-0.272	-0.029	-0.215**	-0.244*
	(0.266)	(0.097)	(0.103)	(0.139)
Party (γ)	2.695***	0.212**	0.354***	0.554***
	(0.278)	(0.104)	(0.122)	(0.128)
Control variables	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time Span	1979-2002	1988, 91, 98, 2002	1988, 91, 98, 2002	1998, 2002
Age span	19-22	19-22	19-22	18-25
Observations	554	282	284	217
Adjusted R2	0.84	0.96	0.90	0.93

Notes: Robust standard errors clustered on individuals in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

4.4. A further investigation of American data

Mullainathan and Washington present evidence of attitude change two years after elections, but find either very small or non-existent attitude change at the time of the subsequent election. They perform an extensive robustness analysis for political attitudes two years after the election, including an analysis of a wide range of attitudes, but do no such robustness analysis for attitudes measured close to the subsequent election.

So far we have seen that the "no effect" results are a robust feature of the Swedish data. It would be illuminating to investigate if the "no effects" results obtained by Mullainathan and Washington, when attitudes are measured just before elections, are a robust feature of the U.S. data as well.

If so, it would further strengthen the evidence against long term attitude change arising from cognitive dissonance mechanisms. For descriptive statistics of the data in this section, see Appendix B.

Column 1 in Table 5 is a replication of the results in column 1 of Table 10 in Mullainathan and Washington.[21] The dependent variable is a "thermometer" measuring the respondents' feelings toward the President on a scale from 0 to 100, were 100 indicates that the respondent likes the President the most. In column 1, feelings toward the President are analyzed for all elections between 1980 and 2000. In column 2, the age span has been increased to include 19- to 25-year-olds. In column 3, controls for age have been added. Finally, in column 4, the age span has been increased and the controls for age have been added. The coefficient of interest is small in all of the specifications and not statistically significantly different from zero. In fact, the effect is reduced by about 80 percent, as compared with two years after elections.[22]

Table 6: Robustness analysis for the American case (1980-2000)

Dependent variable: Feelings toward the U.S. President (Thermometer: scale 0-100)				
	(1)	(2)	(3)	(4)
Second x Party (δ)	1.849	2.218	1.717	2.511
	(3.704)	(3.080)	(3.765)	(3.102)
Second (β)	-5.575**	-4.303*	-7.343	-7.491**
	(2.719)	(2.286)	(4.710)	(3.565)
Party (γ)	20.922***	20.965***	20.958***	20.774***
	(2.644)	(2.301)	(2.682)	(2.307)
Control variables	Yes	Yes	Yes	Yes
Interaction terms	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Age control	-	-	Yes	Yes
Age span	20-23	19-24	20-23	19-24
Observations	632	901	632	901

Notes: Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Detailed regression results are presented in Appendix C, Table C2.

As a further robustness check, Table 6 shows the results when the full set of alternative political attitudes, that Mullainathan and Washington analyze two years after elections, is instead analyzed close to the subsequent

[21] The results differ somewhat from what Mullainathan and Washington report. Their δ=0.144 (statistically insignificant). This difference is likely to be caused by a small difference in the number of observations. They report 630 observations. When I use their reported specification and a do-file provided by them, I obtain 632 observations.

[22] Their estimate of δ is 9.3 two years after elections.

election. While some of the coefficients of interest are positive, most of them are negative (all attitudes are rescaled so that a higher number corresponds to a more positive attitude). Furthermore, most of the coefficients are small and, except for Hopeful, none of them are statistically significantly larger than zero. Consequentially, we can conclude that there is no attitude change consistent with the prediction from cognitive dissonance theory – neither in Sweden nor in the United States – when we analyze attitudes close to elections.

Table 7: Alternative attitudes -The American Case (1980-2000)

Dep. Var.	Is the President…? (4 pt scale)					Approval
	Inspiring	Know-ledgeable	Moral	Good leader	Caring	2 pt scale
Second x Party (δ)	-0.05	-0.24*	-0.07	-0.19	-0.05	-0.02
	(0.15)	(0.12)	(0.14)	(0.14)	(0.15)	(0.07)
Second (β)	-0.11	0.04	0.05	0.15	-0.04	-0.03
	(0.10)	(0.09)	(0.10)	(0.10)	(0.11)	(0.06)
Party (γ)	0.60***	0.52***	0.44***	0.59***	0.67***	0.37***
	(0.11)	(0.09)	(0.10)	(0.11)	(0.12)	(0.05)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Interaction terms	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	559	611	603	612	491	613
Dep. Var.	Does the President make you…? (2 pt scale)				Approval 4 pt scale	Handling of the economy
	Angry	Afraid	Hopeful	Proud		
Second x Party (δ)	-0.02	0.06	0.14*	-0.06	-0.05	-0.05
	(0.08)	(0.08)	(0.08)	(0.08)	(0.16)	(0.26)
Second (β)	0.04	-0.06	-0.10*	0.01	-0.05	0.11
	(0.06)	(0.05)	(0.06)	(0.06)	(0.11)	(0.18)
Party (γ)	0.22***	0.15***	0.25***	0.30***	0.85***	1.07***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.12)	(0.19)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Interaction terms	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	615	616	614	615	612	491

Notes: The dependent variables are recoded so that higher number refers to more positive attitudes. Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

4.5. Direct effects on voting behavior

The investigation has so far implicitly assumed that the act of voting affects future voting only through political attitudes. Clearly, this assumption is questionable, as other channels may be missing. It could therefore be illuminating to test if the act of voting has a direct effect on future party choice.

Mullainathan and Washington perform such a test and finds an effect of previous voting of about two percentage points (δ=0.02). The effect is not statistically significant and the confidence interval indicates that both a large negative and a large positive effect are plausible. The power of this test is simply too low to be informative. This direct test of the effect of voting on subsequent party choice can therefore not be used as support for cognitive dissonance theory in the context of elections.

When this test is applied to Swedish data, low power turns out again to be a problem. The estimated effect is seven percentage points, but it is not even close to being statistically significant and the confidence intervals include an equally large negative effect.[23] As a result, this kind of test provides little information. The relevance of cognitive dissonance theory, in the context of elections, has, therefore, to be assessed by the indirect evidence provided by effects on political attitudes.

5. Concluding remarks

This paper has investigated whether the act of voting influences attitudes toward political parties and candidates, which is a prediction from cognitive dissonance theory. To be able to assess if cognitive dissonance theory has implications for our understanding of elections, attitudes are measured just before elections, when citizens have the possibility to vote in the near future. In contrast to earlier studies, I find no evidence of dissonance induced attitude change. This finding is confirmed by analyzing a variety of political attitudes with data from both Sweden and the United States.

There are at least two possible explanations for why no dissonance induced attitude change is found in this paper, while Mullainathan and Washington provide such evidence. First, it could be that citizens temporarily change their attitudes to be consistent with previous voting behavior. But, when the time to act upon these attitudes is coming, they admit to themselves that they were wrong (if they were) and regain unbiased attitudes and consequentially vote for another party. A second possibility is that dissonance induced attitude change is taking place right after voting, and then slowly converge back to an unbiased point as time elapses. The two explanations predict different dynamic adjustments of attitudes. The present analysis can, however, not discriminate between them.

[23] Detailed results are available upon request.

Regardless of which of the two explanations above that is correct, we have no reason to assume that voters would refrain from voting for another party if the circumstances motivate such a change. As a consequence, the argument that cognitive dissonance could provide a new explanation for the incumbency advantage, and a motivation for term limits, is lacking support in the data. Similarly, the analyses herein do not support the claim that high turnout is inefficient. Instead, the results are more in line with a rational formation of political attitudes–at least at times when citizens are about to act upon their attitudes.

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Appendix A: Detailed description of the Swedish data

A1. The Swedish Election Studies

The Swedish National Data Service (SND) has made the data available for this paper. The data in the Swedish Election Studies were originally collected in a research project at the Department of Political Science at Göteborg University, under the guidance of Sören Holmberg, Henrik Oscarsson, and Mikael Gilljam. Neither SND nor the primary researchers are responsible for the analyses presented in this paper. The sample of the Swedish Election Studies is drawn from a population of 18 to 80 year old Swedish citizens entitled to vote in the general election. Swedes living abroad are not included in the sample. The Swedish Election Studies are available for research given that the project has passed an ethical vetting of research involving humans.[24]

A2. Variables

Attitude about the party of the Prime Minister: Explained in the text (4.1).

Attitude about Prime Minister: Explained in the text (4.2).

Party: 1 if the respondent prefers the party of the Prime Minister to any other party, 0 otherwise.

Second: 1 if the respondent was eligible to vote in the previous election, 0 otherwise.

Knowledgeable, Inspiring, and Likable: Explained in the text (4.3).

High school: 1 if the respondent has attended high school, 0 otherwise.

University: 1 if the respondent has attended education at the university level, 0 otherwise.

Cohabit: 1 if the respondent is married or cohabiting, 0 otherwise.

Male: 1 if respondent is male, 0 if female.

Age: Years of age

Urban: 1 if the respondent lives in a city (town), 0 otherwise.

Employed: 1 if the respondent is employed (part or full time), 0 otherwise.

Income: Category variable between 1 and 5 (1=low income, 5=high income).

Student: 1 if the respondent is a student, 0 otherwise.

Union: 1 if the respondent is a member of a union, 0 otherwise.

Homeowner: 1 if the respondent own a home, 0 otherwise.

Race variables are included in Mullainathan and Washington but not in the analysis of Swedish data in the present paper. Information about in which

[24] For information on how to access the data contact either snd@snd.gu.se or mikael.elinder@nek.uu.se.

country the respondent was born has not been included in the Swedish Election Studies until recently.

A3. Summary statistics

Table A1: Descriptive statistics for the Swedish data (1979-2002)

Variable	First time voters	Second time	Sign.
Attitude toward the Prime	5.50	5.40	-
Minister's party	(5.21-5.76)	(5.09-5.69)	
Attitude toward the Prime	5.28	5.07	
Minister	(5.00-5.57)	(4.77-5.37)	
Knowledgeable	3.10	3.07	-
	(2.99-3.20)	(2.96-3.17)	
Prime Minister: Inspiring	2.03	1.95	-
	(1.92-2.14)	(1.82-2.08)	
Prime Minister: Likable	2.51	2.48	-
	(2.31-2.71)	(2.27-2.69)	
Party	0.19	0.20	
	(0.15-0.23)	(0.15-0.25)	
Age	19.8	21.8	***
	(19.77-19.92)	(22.70-21.84)	
Male	0.46	0.48	-
	(0.41-0.51)	(0.42-0.54)	
Cohabit	0.19	0.37	***
	(0.15-0.23)	(0.32-0.43)	
High school	0.71	0.61	***
	(0.66-0.75)	(0.55-0.66)	
University	0.14	0.21	**
	(0.11-0.18)	(0.17-0.26)	
Student	0.26	0.28	
	(0.22-0.31)	(0.23-0.33)	
Employed	0.51	0.62	***
	(0.46-0.56)	(0.56-0.67)	
Union	0.43	0.61	***
	(0.39-0.49)	(0.56-0.67)	
Income (5 grade scale)	1.41	1.98	***
	(1.33-1.48)	(1.87-2.09)	
Urban	0.60	0.71	***
	(0.55-0.65)	(0.66-0.76)	
Home owner	0.36	0.25	
	(0.31-0.41)	(0.20-0.30)	***
Number of observations	391	305	

Notes: Means and 95 % confidence intervals (in parentheses). * significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix B: Summary statistics for the American data

Table B1: Descriptive statistics for the American data (1980-2000)

Variable	First time	Second time	Sign. Diff.
Presidential thermometer	59.4	55.1	**
(Range 0-100)	(56.5-62.3)	(52.2-58.0)	
Party	0.45	0.40	
	(0.39-0.51)	(0.35-0.46)	
Age	20.5	22.5	***
	(20.4.-20.6)	(22.5-22.6)	
Male	0.41	0.48	*
	(0.35-0.47)	(0.43-0.54)	
Married	0.24	0.36	***
	(0.19-0.29)	(0.31-0.42)	
High school	0.88	0.88	
	(0.83-0.91)	(0.84-0.91)	
Income (log)	9.48	9.56	
	(9.36-9.59)	(9.46-9.65)	***
Employed	0.58	0.72	
	(0.52-0.64)	(0.67-0.77)	
Union	0.13	0.16	
	(0.09-0.17)	(0.12-0.20)	
Urban	0.34	0.25	**
	(0.29-0.40)	(0.21-0.30)	
Home owner	0.37	0.39	
	(0.32-0.43)	(0.33-0.44)	
Employed	0.58	0.72	
	(0.52-0.64)	(0.67-0.77)	
Black	0.17	0.15	
	(0.13-0.22)	(0.11-0.19)	
Hispanic	0.07	0.09	
	(0.05-0.11)	(0.07-0.13)	
Asian	0.02	0.006	*
	(0.008-0.045)	(0.00-0.02)	
Native	0.02	0.05	*
	(0.01-0.05)	(0.03-0.08)	
Democrat	0.49	0.45	
	(0.43-0.55)	(0.40-0.50)	
Republican	0.33	0.37	
	(0.28-0.39)	(0.32-0.43)	
Independent	0.18	0.18	
	(0.14-0.23)	(0.14-0.22)	
Number of observations	287	345	

Notes: Means and 95 % confidence intervals (in parentheses). * significant at 10%; ** significant at 5%; *** significant at 1%..

Appendix C: Detailed regression results

Table C1: Basic results for Sweden (detailed reports related to Table 3)

Dependent variable: Attitude toward the party of the Prime Minister 1979-2002.				
	(1)	(2)	(3)	(4)
Age span	19-22	18-23	19-22	18-23
Second × Party	-0.125	-0.115	-0.029	-0.018
	(0.345)	(0.279)	(0.350)	(0.285)
Second	-0.213	-0.144	0.078	-0.107
	(0.250)	(0.221)	(0.348)	(0.296)
Party	3.641***	3.812***	3.590***	3.741***
	(0.237)	(0.197)	(0.240)	(0.201)
Control variables				
Male	-0.071	0.154	-0.065	0.147
Cohabit	-0.578	-0.504	-0.603	-0.629*
High School	0.045	0.090	0.046	0.056
University	0.464	0.226	0.376	0.022
Student	-0.685	-0.610	-0.665	-0.512
Employed	0.576	0.101	0.546	0.037
Income	-0.486**	-0.323*	-0.523**	-0.384**
Union	-0.100	-0.198	-0.108	-0.317
Urban	0.726**	0.924***	0.693**	0.871***
Homeowner	0.745*	0.483	0.708*	0.441
Age			-0.024	0.156
Interactions				
Male	-0.082	-0.538	-0.086	-0.523
Cohabit	0.258	0.251	0.346	0.434
High School	0.045	0.035	0.049	0.085
University	-0.512	-0.452	-0.314	-0.119
Student	0.320	0.476	0.319	0.325
Employed	-1.040	-0.286	-1.017	-0.241
Income	0.619**	0.345	0.736**	0.478**
Union	0.882*	0.899**	0.930*	1.076**
Urban	-0.729*	-1.019***	-0.665	-0.925**
Homeowner	-1.044**	-0.611	-1.029**	-0.589
Age			-0.242	-0.249*
Year dummies				
1979	4.299***	4.541***	4.805	1.652
1982	4.636***	4.658***	5.179	1.830
1985	4.507***	4.359***	9.663***	6.085***
1988	4.899***	4.916***	10.060***	6.652***
1991	4.921***	4.729***	10.104***	6.455***
1994	3.494***	3.151***	4.031	0.227
1998	5.387***	5.194***	10.807***	6.988***
2002	5.788***	5.653***	11.227***	7.478***
Observations	556	847	556	847

Notes: Standard errors not reported. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C2: Robustness results for the American case (detailed reports related to Table 5)

Dependent variable: Feelings toward the U.S. President 1980-2000.				
	(1)	(2)	(3)	(4)
Second × Party	1.849	2.218	1.717	2.511
Second	-5.575**	-4.303*	-7.343	-7.491**
Party	20.922***	20.965***	20.958***	20.774***
Control variables				
High school	-0.332	-0.536	-0.047	-0.405
Income (log)	-3.623*	-2.661	-3.681*	-2.663
Employed	4.799	1.720	4.615	1.935
Married	10.850***	5.425*	10.692***	5.574*
Urban	0.759	-1.918	0.752	-2.063
Male	0.715	1.355	0.703	1.406
Black	18.963***	16.742***	18.868***	16.880***
Hispanic	10.108**	12.108***	10.194**	12.288***
Asian	12.183**	9.703**	12.404**	9.721**
Native	1.446	2.365	1.591	2.684
Union	-6.378	-5.290	-6.453	-5.384
Homeowner	7.674**	3.036	7.537**	3.003
Interactions (republic)				
High school	1.587	1.741	1.204	1.461
Employed	6.531***	5.222**	6.534***	5.203**
Married	-1.812	0.169	-1.570	-0.300
Urban	-5.884	-2.046	-5.775	-2.733
Male	-5.955	-3.304	-6.040	-3.339
Black	-2.739	-2.457	-2.712	-2.579
Hispanic	-30.355***	-27.954***	-30.268***	-28.144***
Asian	-12.392*	-13.874**	-12.354*	-13.800**
Native	-22.192***	-17.475***	-22.280***	-17.480***
Union	-1.326	-6.254	-1.355	-6.284
Homeowner	4.360	2.541	4.531	2.571
High school	-5.171	0.773	-4.970	0.909
State dummies	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Age			1.234	0.618
Age interaction			-0.526	0.742
Observations	632	901	632	901

Notes: Standard errors not reported. * significant at 10%; ** significant at 5%; *** significant at 1%.

Essay IV

Trust and growth: A shaky relationship[1]

(together with Niclas Berggren and Henrik Jordahl)

1. Introduction

Numerous studies indicate that generalized trust is beneficial for economic growth – see, e.g., Knack and Keefer (1997), La Porta et al. (1997), Dasgupta and Sergaldin (2000), Glaeser et al. (2000), Zak and Knack (2001) and Beugelsdijk et al. (2004). By generalized trust (henceforth referred to merely as trust) is meant trust in people in general, i.e., trust in people one knows nothing about. Zak and Knack (2001) develop a theoretical model where trust is defined as the time people spend in production rather than in verifying that others do not cheat or behave opportunistically. High-trusting societies are societies in which such transaction costs are low, which is thought to stimulate investment, production and trade, which in turn leads to economic growth.[2] Beugelsdijk et al. conclude that the relationship between trust and economic growth is highly robust in terms of statistical significance and reasonably robust in terms of the size of the estimated effect. In this article we examine the conclusions of the previous literature by taking the robustness analysis further.

We do this on the basis of the realization that many economic relationships are notoriously unstable. What once appeared true in one place can often look quite different in other places or at a later point in time. We find it important to investigate the stability of previous findings by exposing them to systematic empirical scrutiny.

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[2] For other results from the literature on the determinants of economic growth, see, e.g., Barro (1991, 1997), Sala-i-Martin (1997), Durlauf and Quah (1999), Temple (1999) and Sturm and de Haan (2005).

Here we investigate whether previous results on the trust-growth relationship for the period 1970–1992 hold also for the 1990s. In so doing, we use new data on trust from the fourth version of the World Values Survey (WVS) (Inglehart et al. 2004) and from the Latinobarómetro (2004), as well as new data on growth. This also increases our sample to 63 countries, compared to 29 countries in Knack and Keefer (1997) and 41 in Zak and Knack (2001) and Beugelsdijk et al.[3] To separate time and sample effects we compare results based on our sample for the period 1990–2000 with results based on the smaller sample, previously studied by Zak and Knack and by Beugelsdijk et al., for the periods 1970–1992 and 1990–2000.

We use extreme bounds analysis (EBA) to see how the statistical significance and the size distribution of the estimated coefficients of trust are affected by a systematic variation of the control variables. Furthermore, we provide a methodological extension of the trust-growth literature by using the robust estimation technique Least Trimmed Squares (LTS) to measure the impact of outliers (i.e., observations that deviate from the general pattern). This technique is also combined with EBA. These extensions of previous studies of the trust-growth relationship make it possible to offer a firmer conclusion about its robustness and to improve the prospects of gaining policy-oriented insights.

Our findings show that the trust-growth relationship is less robust than claimed earlier. When removing outliers, not only is the average size of the estimated trust coefficient distinctly lower – it also turns out that the trust coefficient is statistically significant at the 5 percent level in only ten percent of our 1,140 regressions! The lack of robustness is partly connected with the study of a later time period, but mostly depends on the removal of a few outlying observations, especially China.

2. Robustness, empirical strategy and data

2.1 Robustness and empirical strategy

There is no universally accepted definition of robustness – the concept is multifaceted and continuous rather than dichotomous – which is why most studies in this area incorporate a variety of robustness criteria.[4] The most basic and perhaps most important way to examine the robustness of a relationship is to see whether it is stable over time. Previous studies have analysed the relationship between trust and growth using data for 1970–1992. In this study we examine the same relationship for the 1990s.

Many results from cross-country growth regressions have been sensitive to changes in the model specification. It has therefore become common to

[3] Adding new countries is especially relevant since Beugelsdijk et al. report a distinct sensitivity of the results to the countries included in the sample.
[4] See Florax et al. (2002).

focus on EBA, which looks at the statistical significance and sign of the estimated coefficients. We incorporate these types of tests into our analysis.

However, there are other ways, often overlooked, in which results may be fragile. As pointed out in Rousseeuw and Leroy (1987), OLS estimates are quite sensitive to outliers, i.e., observations that deviate from the linear pattern formed by the majority of the data.[5] Outliers occur frequently in datasets because of measurement errors, because some observations may be drawn from a different population with a different type of relationship between the variables of interest or because of exceptional but irrelevant events (e.g., earthquakes).[6] Applying OLS on a dataset contaminated by outliers may result in severely biased estimates. In the extreme case, one single outlier can result in an infinite bias of OLS estimates, i.e., it has a breakdown point of 0 percent.[7] To deal with this problem, robust regression methods, i.e., methods that have a breakdown point greater than zero, can be applied. By comparing the OLS estimates with robust estimates it is possible to assess the relationship's sensitivity to outliers.

A related way in which results may be fragile concerns the size of the estimated coefficients and how they change as the control variables are varied. We conduct such a study by looking at the distribution of the estimated trust coefficients. The rationale for this type of test, following McCloskey (1985), McCloskey and Ziliak (1996), Florax et al. (2002) and Ziliak and McCloskey (2004), is that whereas the statistical significance of an estimated coefficient is used for establishing the existence of a relationship between two variables, the real-world relevance of a relationship depends on the size of the estimate. We investigate such matters thoroughly.

Furthermore, results may be fragile in other ways, e.g., with respect to different measures of relevant variables[8]. Hence, there are many dimensions in which results may or may not be robust. To make an overall judgement, all the dimensions must be assessed and weighed against each other, and the conclusions must be based on informed judgement rather than a simple check of whether a certain test is passed.

In line with this, our robustness analysis of the data for the 1990s consists of three main parts. *First,* we apply the robust estimation technique LTS, a

[5] Such points may have an unusual value for the dependent variable, for a regressor or for both.

[6] Outliers may also occur on legitimate grounds and contain variation that should be included in the estimations. The problem is that there is often no way of knowing when this is the case.

[7] For a technical definition of "breakdown point", see Rousseeuw and Leroy (1987, p. 9).

[8] We do not investigate robustness with respect to the measure of trust. Although our survey-based measure is obviously far from perfect, as pointed out by, e.g., Glaeser et al. (2000, pp. 811, 815), no alternative measure is available for a large number of countries.

novel approach to assess the robustness of the trust-growth relationship.[9] This technique was pioneered by Rousseeuw (1984) and is described and advocated by, e.g., Temple (1999), Zaman et al. (2001) and Sturm and de Haan (2005). The idea is to use a method that is "robust against the possibility that one or several unannounced outliers may occur anywhere in the data" (Hubert et al. 2004, p. 1515) by, in this case, fitting the majority of the data and identifying outliers as the cases with large residuals.

Outliers are defined on the basis of the following procedure, as outlined in Rousseeuw and Leroy (1987). First, the 75 percent of the observations that give the best fit (that minimize the sum of the squared residuals) are identified and used to calculate a regression line. Then the remaining 25 percent of the observations are added, and their residuals are computed from the fitted values of the first-stage regression. Countries with a standardized residual above a certain value, approximately 2.5, are identified as outliers.[10] This procedure concentrates on the observations that best approximate the model to be estimated. After this identification, Reweighted Least Squares (RLS) is used for inference by giving outliers the weight zero and other countries the weight one. The advantage of LTS compared with single-case diagnostics like Cook's distance and DFITS is that it can handle cases with several jointly influential outliers. As we use LTS with a breakdown point of 25 percent, the method can handle cases where up to 25 percent of the observations are jointly influential.[11]

We think that the conclusion in Beugelsdijk et al. (2004), that the size of the trust-growth relationship depends on which countries are included in the sample, makes the systematic LTS/RLS procedure very valuable. Also, it is quite unlikely that the additional countries are perfectly representative for the population of all countries.[12]

Second, we investigate robustness with respect to model specification. Following Leamer (1985), Levine and Renelt (1992), Sala-i-Martin (1997) and Sturm and de Haan (2001), who point out that results of cross-country growth regressions need to be tested in this fashion, we investigate the

[9] Zak and Knack (2001, p. 310) use some form of robust estimator to downweight cases with large residuals, but it is not clear how this is done.

[10] 2.5 is the critical value for the studentized residuals with a confidence interval of 99.5 percent.

[11] For practical-technical information about the LTS estimator and its application, see Verboven and Hubert (2005) and Rousseeuw and Van Driessen (2006).

[12] As data become available for more countries two problems arise. First, results from cross-country regressions are generally first found for a small but relatively homogeneous group of countries (such as the OECD). By adding countries, the heterogeneity between countries increases. When the heterogeneity becomes large it may be reasonable to treat different groups of countries as belonging to different populations. Second, the risk of measurement errors probably increases, since it is usually rich countries with high-quality statistical services that are included first.

sensitivity of the statistical significance of trust when the control variables are varied. We look at four tests:

(i) the strong extreme bounds test (indicating whether all of the estimated coefficients are statistically significant at the 5 percent level and of the same sign),
(ii) the weak extreme bounds test (indicating whether at least 95 % of the estimated coefficients are statistically significant at the 5 percent level and of the same sign),[13]
(iii) the strong sign test (indicating whether all of the estimated coefficients have the same sign), and
(iv) the weak sign test (indicating whether at least 95 percent of the estimated coefficients have the same sign).

The basic idea of EBA, following Leamer (1985), is to systematically vary certain control variables to see what happens to the statistical significance of the estimates of the variable of interest. A regression equation of the following kind is used (in country i):

$$\Delta Y_i = \alpha + \boldsymbol{\beta}\mathbf{F}_i + \gamma x_i + \boldsymbol{\delta}\mathbf{C}_i + u_i, \quad (1)$$

where ΔY_i refers to growth in GDP per capita, where $\mathbf{F}_i$ is a vector with the fixed variables that are always included in the regressions, where x_i refers to the variable of interest (trust in our case), where $\mathbf{C}_i$ is a vector with three variables from the set of switch variables, and where u_i is an error term. We investigate the effects on the statistical significance of γ when varying **C.** This is done by including three switch variables at a time in all possible combinations (which has become the standard way of conducting this kind of test) and using data for up to 63 countries for the 1990s.[14]

[13] We do not use the weighted weak extreme bounds test or the cumulative density function test, following a critique of the weighted EBA expressed by Sturm and de Haan (2002). As shown by them, the varying number of observations in the regressions due to missing observations is problematic. First, the goodness-of-fit measure that is obtained may not be a good indicator of the probability that a model is true. Second, the weights constructed in this way are not equivariant for linear transformations of the dependent variable.

[14] Recently, robustness tests based on Bayesian analysis have been proposed and used – see e.g. Fernandez et al. (2001) and Sala-i-Martin et al. (2004). However, we choose not to use such tests here in spite of their advantages (fixed variables are not used and the number of explanatory variables can be varied). As pointed out be Sturm and de Haan (2005), they also have drawbacks, such as the need for a balanced data set (which would severely limit the number of countries or variables that we could include) and not taking outliers or parameter heterogeneity into account.

We also investigate how the size of the estimated trust coefficient changes as the switch variables are varied. To enable a broad assessment, we provide histograms of the distributions of all estimated trust coefficients; and we report the mean and the median of the estimated coefficients, as well as standard deviations.

Third, we combine the LTS/RLS approach with EBA by identifying outliers in each individual regression and by removing them before conducting the model-uncertainty analysis.[15] This enables us to see if the extreme bounds tests are passed by the countries that are not identified as outliers. A disadvantage of applying LTS for each regression is that it changes the sample from one regression to another. However, due to missing observations for a few variables, the sample changes anyway when performing the EBA.

2.2 The data[16]

This study compares results based on the sample used by Zak and Knack (2001) and Beugelsdijk et al. with results based on our sample of 63 countries, which encompasses new data, not least from the fourth version of the WVS. Henceforth we refer to our sample as the "large sample". The robustness tests are carried out for the small sample of countries used in previous studies both for the period 1970–1992 and for the period 1990–2000, as well as for our large sample for the period 1990–2000. The most recent trust data that we use have not been included in the previous studies. The small and the large sample are briefly described in Table 1.

Table 1: The two samples

Name of sample	Small	Large
Countries	39	63
Time period	1970–1992 and 1990–2000	1990–2000
Source for Trust	Inglehart et al. (2000), Inglehart et al. (2004)	Inglehart et al. (2000), Inglehart et al. (2004), Latinobarómetro (2004)

Notes: Our small sample corresponds to that in Zak and Knack (2001) and Beugelsdijk et al., but there the number of countries is 41 (Luxembourg and Nigeria are not included in our small sample due to a lack of data on *Schooling*). The countries are specified in Table A2.

The variables are divided into four groups: the dependent variable, the variable of interest (*Trust*), the fixed variables, and the switch variables. The fixed variables are control variables that are included in all regressions,

[15] We wish to thank a referee for suggesting this way of combining LTS/RLS and EBA.

[16] Our dataset can be downloaded from any of our web sites and is also available upon request.

whereas the switch variables are included and varied when we investigate robustness with respect to model specification. We list the four groups below. Descriptive statistics and sources for all variables can be found in Table A1 in the Appendix. Values for *Trust* and *Growth* are listed in Table A2 in the Appendix.

(i) Dependent variable (1): *Growth*: annual growth of real GDP chain per capita, 1990–2000.[17]

(ii) Variable of interest (1): *Trust*: the percentage of respondents in each country agreeing with the statement "most people can be trusted" rather than with the alternative "you can't be too careful in dealing with people" (earlier versions of the WVS) or "you need to be very careful in dealing with people" (the latest, fourth version of the WVS). The WVS has been conducted in 1981, 1990–91, 1995–96, and 1999–2002. For each country, we use the first non-missing value in the three latest versions of the WVS. We include additional values for Greece from the Eurobarometer survey and for New Zealand from a government survey;[18] in addition, we add values from eight Latin American countries for 1995 from the Latinobarómetro (2004).[19,20]

(iii) Fixed variables (3): *Schooling:* the average number of years in school, 1990; *Investment-good price*, the price level of investment; *Real GDP per capita*, in thousands of USD, 1990.

(iv) Switch variables (20): Control variables that are included in all possible combinations of three.

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[17] We have also carried out the whole analysis using the estimated growth trend for the same period as the dependent variable. This variable is less sensitive to the choice of start and end years, but it does not measure actual growth rates, which is a drawback. The results are available upon request and without exception close to the ones we present.

[18] See Zak and Knack (2001, p. 307).

[19] The Latinobarómetro survey question is consistent with the one from Inglehart et al. (2004). It is formulated thus (in Spanish): "Hablando en general, ¿Diría Ud. que se puede confiar en la mayoría de las personas o que uno nunca es lo suficientemente cuidadoso en el trato con los demás?"

[20] The questions were identical in all these surveys. Whilst we cannot rule out a framing effect – i.e., that the replies to the identical questions differed because of differences between the surveys overall – we think this risk is small. To check this, we compared the *Trust* measures for 1995 for the nine countries in the Latinobarómetro that are also included in the World Values Survey. Although the mean of *Trust* in these countries is a bit higher in the Latinobarómetro (19.6) than in the WVS (15.2), the difference is not statistically significant. In the WVS itself there is a similar, small risk that the comparability between countries is not perfect, stemming from the fact that the questions are asked in different languages which may entail different interpretations of certain terms (such as "most people").

How were the fixed variables and the switch variables chosen? Generally, they have all been advanced as potential determinants of growth on theoretical grounds, as measures of a possible convergence effect and of human and physical capital. But so have other variables. To make our results as comparable as possible we choose not to deviate from Beugelsdijk et al. and therefore use these three fixed variables.[21]

As for the switch variables, we started with the full set of the Beugelsdijk et al. variables and then implemented some changes on the following grounds. We have removed a few variables for three reasons: poor data, moving forward the time period under study, and avoiding reducing the sample size too much. We have also exchanged some variables, as we believe we have found better data. In total, 68 potential switch variables are in our original dataset. Out of these the 20 listed in Table A1 in the Appendix were chosen, as they have a correlation coefficient with *Trust* of less than 0.25 in absolute value. This procedure limits the problem of multicollinearity and increases comparability (cf. Beugelsdijk et al., pp. 123–124).[22] To make sure that our results do not critically depend on this restriction, we also use all 68 switch variables in the EBA in section 3.3.

One thing that should be pointed out is that because the data we use for the countries not included in previous studies are relatively new, from 1995 and 2000, it stems from the end of the period for which our dependent variable is measured. As in previous studies, there may be a problem of reverse causality. However, we think that the risk of this being more problematic in our study is rather small, since we obtain similar results when

[21] These variables are also used by Knack and Keefer (1997) and Zak and Knack (2001). They have also been linked to economic growth in several empirical studies, but naturally, other variables could have been included as well. Cf. Barro (1991), Levine and Renelt (1992), Temple (1999, 2001) and Sturm and de Haan (2005). If one were to replace one of the fixed variables, the most obvious candidate in our view is *Investment-good price*, which would then be exchanged for *Investment share of GDP*. The latter measure was used by Zak and Knack (2001) in addition to *Investment-good price*. The argument against using it is that it can be expected to be endogenous with respect to growth. In any case, as a sensitivity analysis, we have replaced *Investment-good price* with *Investment share of GDP* in our analysis. We find that this replacement has a rather small effect on the results. They are a bit more robust, but far from meeting general robustness criteria in EBA or LTS/RLS. As an alternative measure of another of the fixed variables, *Real GDP per capita*, we have used its natural logarithm, in line with some previous studies. However, this measure is not statistically significant in our basic regression, R^2 falls sharply and the *Trust* estimate is decreased only marginally.

[22] Furthermore, looking at the correlation coefficients between the switch variables, these are everywhere quite low (only above 0.5 in one case and distinctly lower in almost all other cases).

only using the countries looked at in Beugelsdijk et al. as when using the large sample (see the following section).

3. Robustness results

This section presents the results of several robustness tests.[23] First, we present basic OLS regressions for our two samples (3.1). This is followed by regression results when outliers are deleted, through the application of the robust estimation technique LTS in conjunction with RLS (3.2). Then EBA, focusing on the sign of the estimated *Trust* coefficient and its statistical significance, is applied (3.3), and it is followed by a combination of LTS and EBA (3.4). Lastly, we investigate how the size and precision of the *Trust* coefficient respond to changes in the model specification and to changes in the sample (3.5).

3.1 Basic regressions for two samples

It is useful to first take a look at the results from basic OLS regressions for the two samples of countries, as reported in Table 2. For the small sample we present estimates both for the 1990s and for 1970–1992, the period studied by Zak and Knack (2001) and Beugelsdijk et al. (2004). The regressions all contain the variable of interest, *Trust*, as well as the three fixed control variables.

[23] The results presented here, as in other cross-country studies, must be interpreted with caution and should only be interpreted as suggesting *the possibility of* a *causal* relationship. The results have been obtained using Stata, GAUSS for EBA and MATLAB with the LIBRA package for LTS. All results and calculations in this section are available upon request.

Table 2: Basic regressions (OLS)

	Dependent variable: Growth		
	Small sample 1970–1992	Small sample 1990–2000	Large sample 1990–2000
Trust	0.064***	0.046*	0.062***
	(0.019)	(0.024)	(0.019)
Real GDP per capita	-0.114	-0.184*	-0.154**
	(0.105)	(0.074)	(0.064)
Investment-good price	-0.041***	-0.004	0.015
	(0.011)	(0.018)	(0.009)
Schooling	-0.018	0.282	0.134
	(0.154)	(0.176)	(0.155)
Number of observations	39	39	63

Notes: Standard errors in parentheses. All estimated equations include a constant term not reported here. Sources and variable definitions: see Table A1. Sample list: see Table A2. For the period 1970–1992 we use the earliest available observation of *Trust* from the WVS, and values from 1970 for the three control variables. *significant at 10%; ** significant at 5%; *** significant at 1%

The OLS results from the basic model specification point to both a statistically significant and economically important relationship between trust and growth in the 1990s for both samples.[24] For the small sample, this is the case both for 1970–1992 and for the 1990–2000 period. The estimated trust coefficient for the small sample and the period 1970–1992 is almost a perfect replication of the same estimate in Beugelsdijk et al, which is 0.061 (in their Table 2). For the later time period, the size and the statistical significance of the *Trust* coefficient are greater in the large than in the small sample. When the large sample is considered, an increase in the share of people who believe that most people can be trusted by 10 percentage units entails an increased annual growth rate of 0.62 percentage units.[25] Of the fixed variables, *Real GDP per capita* (1990–2000) and *Investment-good price* (1970–1992) exhibit statistically significant relationships with *Growth,* while *Schooling* never attains statistical significance.[26] Let us now see how these basic results stand when we expose them to different robustness tests.

[24] If we exclude the countries with data on *Trust* from the Latinobarómetro, the empirical results are very similar to what we obtain for the large sample. This is the case throughout this article.

[25] *Trust* appears quite stable in most countries, but it may still be affected by various factors. Among the proposed determinants are income inequality, ethnic fractionalization, the quality of the legal system, and education. See e.g. Zak and Knack (2001), Alesina and La Ferrara (2002), Knack and Zak (2002) and Berggren and Jordahl (2006).

[26] There seems to be no partial correlation between *Investment-good price* and *Growth* for 1990–2000. The same result is obtained if we instead use *Investment share of GDP* in the regressions. The choice of investment variable – and even its

3.2 Least trimmed squares

We begin by investigating how outliers influence the results. As pointed out above, the previous literature lacks a systematic usage of such robust estimation techniques, an omission which we consider quite serious. Hence, we apply LTS in conjunction with RLS for inference in order to examine the impact of outliers.

Table 3 shows the results for the basic model, with *Trust* and the three fixed variables as control variables. The first column is based on the small sample, and the ensuing columns are in each case based on a gradual elimination of outliers on the basis of the procedure outlined in section 2.1.

Table 3: LTS and RLS, small sample, 1990–2000

	Dependent variable: Growth							
Trust	0.046*	0.036	0.033	0.027	0.028	0.026	0.025	0.032**
	(0.024)	(0.022)	(0.021)	(0.020)	(0.019)	(0.017)	(0.015)	(0.014)
Number of observations	39	38	37	36	35	34	33	32
Sample	Small	Excl. Ireland	Excl. Ireland Taiwan	Excl. Ireland Taiwan Dom. Rep.	Excl. Ireland Taiwan Dom. Rep Chile	Excl. Ireland Taiwan Dom. Rep Chile Venezuela	Excl. Ireland Taiwan Dom. Rep Chile Venezuela S. Korea	Excl. Ireland Taiwan Dom. Rep Chile Venezuela S. Korea Argentina

Notes: Standard errors in parentheses. All estimated equations include a constant term and three fixed variables not reported here. We eliminate countries in the descending order of their standardized residuals computed from the fitted values of the first-stage regression. Countries with a standardized residual greater than 2.5 are eliminated in this procedure. *significant at 10%; ** significant at 5%; *** significant at 1%. Sources and variable definitions: see Table A1. Sample list: see Table A2.

As can be seen, Ireland is identified as the largest outlier, and its removal reduces the estimated *Trust* coefficient from 0.46 to 0.036 and renders it statistically insignificant. The gradual elimination of outliers identified by LTS leads to an estimated and statistically significant coefficient of 0.032, almost half the original size. We have also applied the LTS/RLS procedure on the small sample for the period 1970–1992 with a similar result. When removing five identified outliers, the *Trust* coefficient drops from 0.064 to 0.026 but remains statistically significant at the 10 percent level.

complete exclusion – only has a small impact on the *Trust* coefficient in any case. As for *Schooling*, Lorgelly and Owen (1999), Krueger and Lindahl (2001); Temple (2001) and de La Fuente and Domenech (2006) likewise find that the relationship between education, be it male or female, and growth is seldom statistically significant. There are indications that this lack of significance is likely driven by measurement error.

In Table 4, the corresponding results for the large sample are reported. There, four countries are removed, starting with China, the country with the largest standardized residual.

Table 4: LTS and RLS, large sample, 1990–2000

	Dependent variable: Growth				
Trust	0.062***	0.039*	0.033*	0.035*	0.032*
	(0.019)	(0.020)	(0.019)	(0.019)	(0.018)
Number of observations	63	62	61	60	59
Sample	Large	Excl China	Excl China Ireland	Excl China Ireland Nicaragua	Excl China Ireland Nicaragua Latvia

Notes: Standard errors in parentheses. All estimated equations include a constant term and three fixed variables not reported here. We eliminate countries in the descending order of their standardized residuals computed from the fitted values of the first-stage regression. Countries with a standardized residual greater than 2.5 are eliminated in this procedure. *significant at 10%; ** significant at 5%; *** significant at 1%. Sources and variable definitions: see Table A1. Sample list: see Table A2.

Table 4, like the previous one, clearly suggests that outliers do affect our results. Removing China, Ireland, Nicaragua and Latvia halves the size of the estimate and sharply reduces the degree of statistical significance, indicating that OLS results may be misleading or, at least, that they should be interpreted carefully.

China and Ireland have *Trust* scores well above the average, in China's case around 60 percent, and both countries have reported exceptional growth rates. Both countries reappear in section 3.4 as frequent outliers, with China clearly being the most distinct one. We do not know for certain why China's effect on the results is so large. It may be because of measurement error, because China belongs to a different population than the other countries or because some exceptional but irrelevant events have taken place there.[27] Bjørnskov (2007) finds that China is an outlier when it comes to *Trust*: in fact, it is found to have a *Trust* level that is about 35 percentage points higher than what is predicted by a baseline specification that explains about half of the cross-country variation.[28] This may point to measurement error as

[27] In any case, we think that an important benefit of the LTS/RLS method is its transparency: irrespective of the reason for there being outliers like China, it is clear that this particular country tilts the regression line quite a bit.

[28] The same point is made by Uslaner (2002, pp. 220, 226): "I eliminated China, since its trust score is suspiciously high. … I see the Chinese figure as a likely outlier that might reflect the hazards of conducting survey research in a country that

a plausible explanation. There are also some indications that China's growth figures are not entirely credible.[29] Another possible explanation is cultural. Inglehart (1999) points at Confucianism as a particular type of influence. *Renquin* is an honor code that entails a negative attitude towards being in debt, both socially and financially, which Buchan and Croson (2004) see as a special and important influence on cooperation and trust. *Guanxi* denotes personal networks that, according to King (1991), also fill an important function for cooperation and interpersonal trust.[30] As for Ireland, it shows exceptional growth figures without a corresponding foundation in its trust levels. Institutional reforms, rather than trust, probably explain large parts of the growth record.

However, even with all four outliers removed, the Trust estimate is still economically significant and retains statistical significance at the 10 percent level. For the great majority of the countries, this indicates that an increase in *Trust* with 10 percentage units is associated with an increased annual growth rate of 0.32 percentage units on average. Although this is certainly not negligible, it is considerably less compared to what previous studies find.[31]

3.3 Extreme bounds analysis

We continue the robustness analysis by looking at the sign and the statistical significance of *Trust* as the set of control variables is varied in a systematic way. The results are found in Table 5. They are based on the basic regressions in Table 2, with the addition of all possible combinations of three switch variables, which gives a total of 1,140 regressions. Again, the results are presented for two different samples of countries.

Freedom House places at the bottom of its rankings on both political and civil liberties."

[29] See Ren (1997), Maddison (1998), Rawski (2001). The former two point at shortcomings in the data-collection process as the explanation, whereas the latter argues that there has been a systematic falsification of growth figures where lower-level officials have exaggerated in order to impress superiors. Things seem to have improved in later years (Holz, 2003), but throughout the 1990s, there seems to have been problems.

[30] Previous studies on the trust-growth relationship, such as Knack and Keefer (1997), Zak and Knack (2001) and Beugelsdijk et al., do not include China in their samples.

[31] A referee suggested that the financial crises in South Korea, Indonesia, Brazil and Argentina might have affected the trust–growth relationship, and to test this we have included a "financial-crisis" dummy variable in the basic regression (reported in Table 2) for the large sample, taking the value 1 for these countries and 0 for all others. The results indicate that *Trust* is still statistically significant at the 1 percent level, that its estimated coefficient increases marginally to 0.065 and that the four countries on average grew 2.7 percentage points faster per year than the other countries in the period under study.

Table 5: Robustness results with respect to model specification for two samples, 1990–2000

	Small sample	Large sample
Share of regressions where *Trust* is statistically significant at the 5 percent level	29.3 %	49.3 %
Number of regressions where *Trust* takes a negative sign	0	0
Number of observations	36-39	45-63

Notes: Three switch variables out of 20 are included in each regression. Number of regressions in each column: 1,140. Sources and variable definitions: see Table A1. Sample list: see Table A2.

How robust, then, is the statistical significance of *Trust* with regard to the model specification? We look at the four robustness tests listed in section 2.1. First, the strong extreme bounds test is not passed for any of the samples: for neither of them is a 100 percent statistical significance share obtained at the 5 percent level. Second, the same is true for the weak extreme bounds test: for neither of the samples is a 95 percent statistical significance share obtained at the 5 percent level. Third, the strong sign test is passed for both samples, as all estimated coefficients have the same, positive sign. Fourth, and by necessity, so is the weak sign test.

Compared to Beugelsdijk et al., where the weak extreme bounds test was passed, our results again point at a distinctly less robust relationship between *Trust* and *Growth*. While Beugelsdijk et al. report a 99.9 percent significance share for *Trust* at the 5 percent level, we report a much lower figure, 29.3 percent for the same sample. We have replicated the Beugelsdijk et al. finding: for the small sample and the period 1970–1992, we find a 99.3 significance share for *Trust* at the 5 percent level. In this undertaking, we used our set of switch variables, with one exception: *Language fractionalization* and *Religious fractionalization* are replaced by the variable *Ethnolinguistic fractionalization*, as defined in Beugelsdijk et al.[32] This replication indicates that the lower robustness in this dimension is due to a later time period.

3.4 Extreme bounds analysis without outliers

Here, we combine LTS and EBA. The results are again based on 1,140 regressions. For each regression outliers are uniquely identified and removed. Table 6 contains the results of this undertaking.

[32] We have also conducted a corresponding test using all 68 switch variables, which resulted in 50,116 regressions. The significance share for the small sample is then 22.3 percent; and it is 32.3 percent for the large sample. As expected, these shares are lower when additional variables that are more highly correlated with *Trust* are included. Of the four tests, only the weak sign test is passed – and it is passed for both samples of countries.

Table 6: Robustness results with respect to model specification combined with LTS and RLS

	Small sample, 1970–1992	Small sample, 1990–2000	Large sample, 1990–2000
Share of regressions where *Trust* is statistically significant at the 5 percent level	10.5%	0.7 %	10.1 %
Mean value of *Trust* estimates	0.035	0.032	0.027
Negative *Trust* estimates	0	1	26
Average number of outliers	3.0	4.1	3.9
Minimum number of outliers	0	2	1
Maximum number of outliers	7	7	8
Number of observations	32-38	31-37	39-62
Most frequent outliers (percent of regressions where the country is outlier)	South Korea (56%) Taiwan (48%) Japan (39%) Peru (32%) Switzerland (28%)	Ireland (59%) Dominican Rep. (53%) Chile (47%) Uruguay (37%) India (36%)	China (43%) Ireland (27%) Chile (22%) Latvia (20%) Czech Rep (20%)
Number of countries that are never outliers	9	7	9

Notes: Three switch variables out of 20 are included in each regression. Number of regressions in each column: 1,140. Sources and variable definitions: see Table A1. Sample list: see Table A2.

As can be seen in Table 6, the robustness of the trust-growth relationship is dramatically reduced when outliers are removed – in fact, it disappears by any reasonable standard.[33] By removing outliers identified by the LTS procedure, both the share of statistically significant estimates at the 5 percent level as well as the size of the estimates are reduced. For the large sample, the share of statistically significant estimates is reduced to 10.1 percent. The corresponding figure for the small sample is as low as 0.7 percent. For the small sample and the period 1970–1992, this share of statistically significant estimates of *Trust* is 10.5 percent. Robustness with regard to empirical specification is heavily influenced by outliers also for this period. China is the most extreme outlier in the large sample (it is not included in the small sample), and by excluding China from all regressions the share of statistically significant estimates falls from 49.3% to 13.2% and the mean size of the estimates falls from 0.044 to 0.033.[34] It is only by including China

[33] Our findings for *Trust* are broadly in line with those of Sturm and de Haan (2005) for other, "classic" variables thought to be robustly related to growth. Only *Number of years open, Equipment investment, Latin-American dummy, Sub-Saharan dummy* and *Fraction Muslim* meet the criteria of robustness and economic significance.

[34] This result is obtained from an alternative way of conducting this analysis, namely by excluding the four outliers identified in section 3.2 (in Table 4) one by one in order of importance. Compared to our method of identifying outliers uniquely for each regression, this method makes use of a slightly more homogenous sample since the same outliers are excluded in all regressions. In our view it is clearly more valuable to be able to identify outliers in each specification. The results of the analysis are in any case not sensitive to the choice of method, neither in terms of the

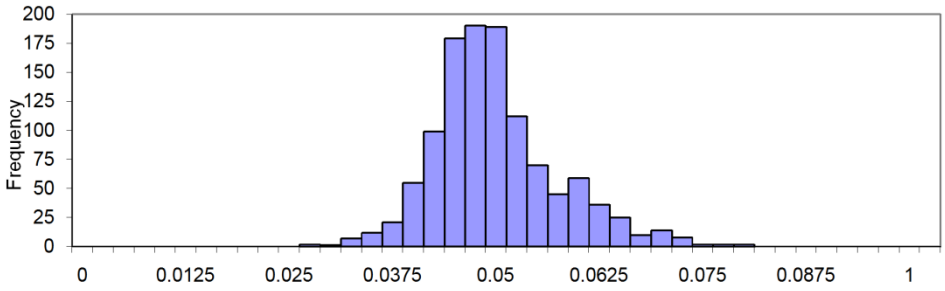
that it is possible to get results that indicate a reasonably robust relationship for the 1990s (but even then, the degree of robustness is low by conventional standards).

We conclude by noting that the lack of robustness is partly connected with the study of a later time period, but it mostly depends on the removal of a few outlying observations. Using LTS/RLS in combination with EBA clearly shows that EBA can be sensitive to outliers and that it is useful to use the techniques jointly.

3.5 Effect size

In this section we study the distribution and the mean of the estimated *Trust* coefficients as the 20 switch variables are varied in all possible combinations of three, both for the small and for the full sample during the period 1990–2000. Figures 1 and 2 display the distribution of the estimates for *Trust* in the 1,140 regressions carried out for the two samples of countries. Figure 1 shows the distribution for the small sample.

Figure 1: The distribution of estimates for *Trust:* the small sample



Notes: Min: 0.026. Max: 0.080. Mean: 0.049. Median: 0.048. Standard deviation: 0.008. Total number of estimates: 1140.

One might say that the relationship between *Trust* and *Growth* is fairly robust with respect to effect size for this sample. The spread around the mean is not excessive, and something like a bell shape can be observed. In comparison to what Beugelsdijk et al. find, we obtain a lower mean (0.049 instead of 0.061) but find only a small difference in the spread (a standard deviation of 0.008 instead of 0.011).

In Figure 2, the distribution for the large sample is shown. Now, a less robust relationship with respect to effect size can be detected. First, the spread is greater (the standard deviation is 0.018). Second, the shape of the distribution is much more uneven. However, the mean is quite similar (0.049 in the small and 0.044 in the large sample).

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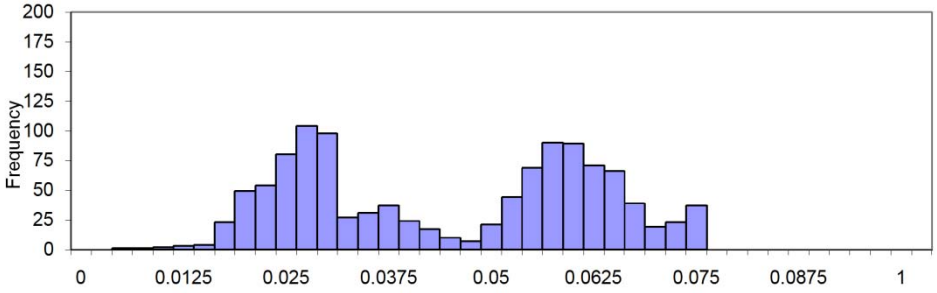
quantitative results nor in terms of identified outliers (e.g., China and Ireland are the most important outliers in the large sample using the other method as well).

Figure 2: The distribution of estimates for *Trust:* the large sample



Notes: Min: 0.004. Max: 0.075. Mean: 0.044. Median: 0.047. Standard deviation: 0.018. Total number of estimates: 1140.

How can the bimodality in Figure 2 be explained? We begin by taking a systematic look at how the mean of the *Trust* estimate changes when different switch variables are included in the regressions. Figure 3 shows the mean value of the *Trust* estimates from all regressions in which each specific switch variable is included. Some of the variables yield low trust estimates, most notably *Real exchange rate distortion*, *Political assassinations*, *Outward orientation* and *Frankrom,* whereas especially *European language* yields a high *Trust* estimate. Since China has missing values exactly for *Real exchange rate distortion*, *Political assassinations*, *Outward orientation* and *Frankrom* we suspect this country to be behind the peak with the larger estimates.

Figure 3: Conditional mean effect size for *Trust*

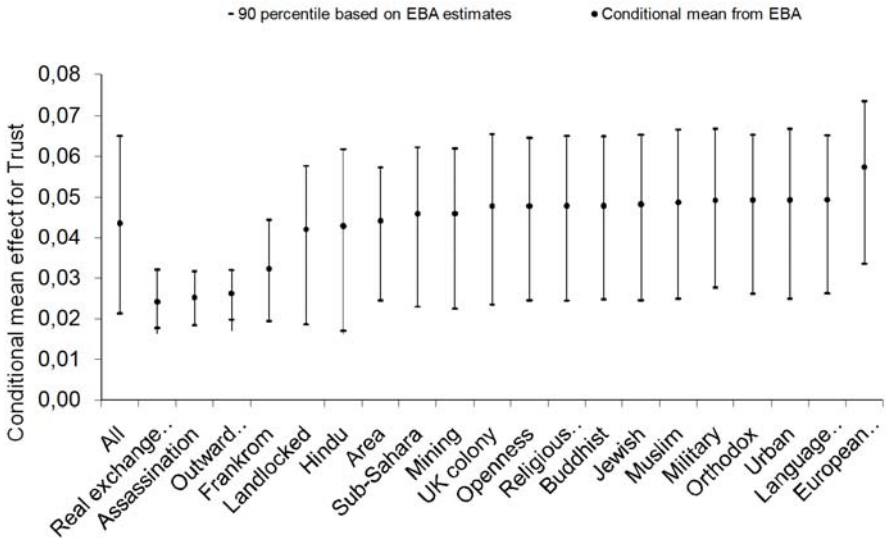
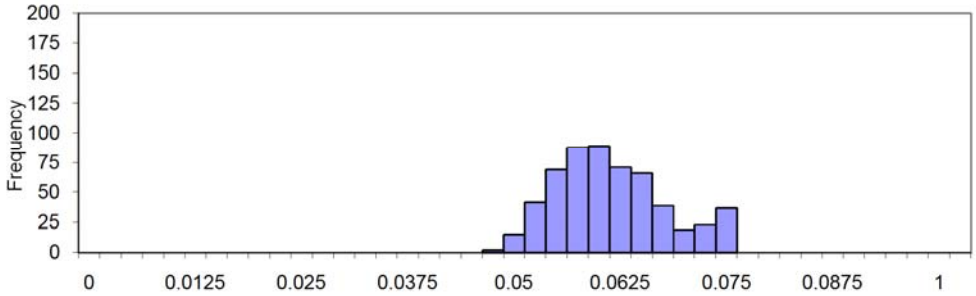


Figure 4 shows the distribution of *Trust* estimates from an EBA analysis which includes China but which excludes the four variables – *Real exchange rate distortion*, *Political assassinations*, *Outward orientation* and *Frankrom* – for which values are missing for China. The similarity to the right peak in Figure 2 is striking.

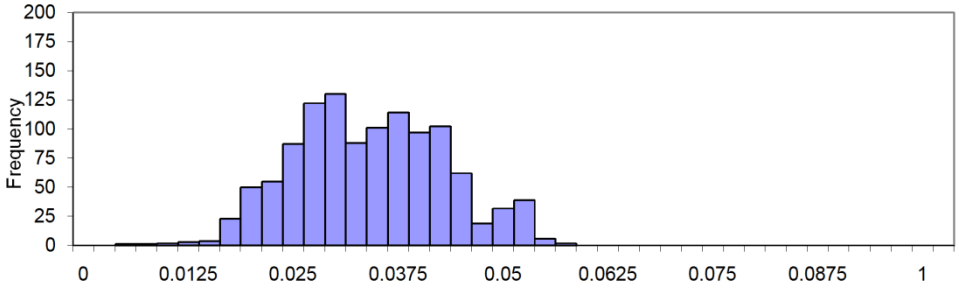
Figure 4: The distribution of estimates for Trust: the large sample when four switch variables with missing values for China have been excluded



Notes: Min: 0.046. Max: 0.075. Mean: 0.060. Median: 0.059. Standard deviation: 0.006.Total number of estimates: 560.

Figure 5 instead shows the distribution of estimates for an EBA where China has been excluded from the sample of countries. This time the distribution of estimates is spread over a range of lower values, roughly in the same segment as the left peak in Figure 2.

Figure 5: The distribution of estimates for *Trust:* the large sample, excluding China



Min: 0.004. Max: 0.057. Mean: 0.033. Median: 0.033. Standard deviation: 0.009. Total number of estimates: 1,140.

Together the results in this section underscore the impact of outliers on the estimates.[35] When excluding China from the large sample, the bimodality pattern disappears, the distribution shifts leftward and the mean of the *Trust* estimates is reduced considerably (to about 0.03).

4. Concluding remarks

We have explored the relationship between generalized trust and economic growth, taking previous investigations further in several respects. Most importantly we have analysed a later time period, utilizing the new World Values Survey with data for more countries than has been available before, in an attempt to re-examine and extend previous results. We have looked at two time periods and two samples to be able to separate time and sample effects. Furthermore we have applied a robust estimation technique, LTS, in combination with RLS for inference, in order to see how outliers affect our results. We have also looked at robustness with respect to model specification, both by examining statistical significance (through EBA) and effect sizes. Lastly, LTS/RLS has been combined with EBA.

What have we found? Our basic OLS regression indicated, as in previous studies, a positive and statistically significant relationship between trust and economic growth. However, our robustness analysis revealed that there is more to this story. In fact, we found that i) when four outliers (China, Ireland, Nicaragua and Latvia, with China being the most extreme) were removed, the estimated coefficient was almost halved; ii) EBA clarified that statistical significance at the 5 percent level was obtained in only 29.3 percent (the small sample) and 49.3 percent (the large sample) of the 1,140 regressions, which is much lower than what has been found before; iii) when conducting EBA without outliers, robustness all but disappears: the trust coefficient is statistically significant at the 5 percent level in only 0.7 percent

[35] The distribution of the estimates from combining LTS and EBA (in section 3.4) is very similar to the distribution in Figure 5.

(the small sample) and 10.1 percent (the large sample) of all regressions; and iv) the distribution of estimated trust coefficients is more widespread compared with the previously studied sample of countries, which is explained by the influence of China, and the mean estimate is substantially reduced (approximately halved) when outliers are removed. Overall, the lack of robustness is partly connected with the study of a later time period, but mostly depends on the removal of a few outlying observations.

Connecting this to broader issues, an important rationale for a study of this kind is that economic growth is at the top of most policy agendas around the world, which makes it essential to better disentangle its determinants. Even though trust may not be robustly related to growth, it could still be important to some degree – and at least as important as many other "classic" variables.

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Appendix

Table A1: Variable specifications and descriptive statistics (1990-2000)

Variable	Definition	# obs	Mean	Std dev	Min	Max	Source
Growth	Annual growth rate in percent of real GDP (chain) per capita, 1990-2000: $100*[(\text{Real GDP per capita}_{2000} / \text{Real GDP per capita}_{1990})^{1/10} - 1]$ Taiwan: 1990–1998	63	1.8	1.9	-2.6	7.7	Heston et al. (2002)
Trust	First value of trust 1990–2000, i.e., the share that agrees with the statement "most people can be trusted"	63	30.5	15.7	5.0	66.1	Inglehart et al. (2000); Zak and Knack (2001); Inglehart et al. (2004); Latinobarómetro (2004)
Schooling	Average years of schooling, 1990	63	6.7	2.6	2.2	12.0	Barro and Lee (2001)
Real GDP per capita	Real GDP (chain) per capita, thousands of USD in 1996 constant prices, 1990	63	10.2	7.6	0.7	26.5	Heston et al. (2002)
Investment-good price	The PPP of investment divided by the exchange rate times 100, 1990	63	79.0	33.5	12.5	177.7	Heston et al. (2002)
Investment share of GDP	Total investment as share of GDP, average for 1990-2000	63	18.0	6.5	3.2	36.3	Heston et al. (2002)
Openness	Exports plus imports divided by real GDP per capita, in current prices, 1990	63	57.4	29.0	15.0	154.6	Heston et al. (2002)
UK colony	Dummy with value 1 if former UK colony and 0 otherwise	63	0.2	0.4	0	1.0	Persson and Tabellini (2003); http://www.britishempire.co.uk; Encyclopaedia Britannica; *Nationalencyklopedin [Swedish National Encyclopedia]*

Table A1: continued

Language fractionalization	One minus the Herfindal index of linguistic group shares, 2001	62	0.3	0.3	0	0.9	Alesina et al. (2003)
Religious fractionalization	One minus the Herfindal index of religious group shares, 2001	63	0.4	0.2	0	0.9	Alesina et al. (2003)
Orthodox	Share of population that is Orthodox Christian, 2000	63	3.9	16.0	0	93.8	World Christian Database, http://www.worldchristiandatabase.org/wcd/; population from Heston et al. (2002), for Taiwan from http://www.census.gov/ipc/www/idbsum.html
Muslim	Share of population that is Muslim, 2000	63	11.5	28.0	0	98.1	Ditto
Buddhist	Share of population that is Buddhist, 2000	63	1.9	7.7	0	55.7	Ditto
Hindu	Share of population that is Hindu, 2000	63	1.7	10.1	0	79.8	Ditto
Jewish	Share of population that is Jewish, 2000	62	0.3	0.5	0	3.1	Ditto
Sub-Sahara	Dummy with value 1 if African country is located to the south of the Sahara and 0 otherwise	63	0.1	0.2	0	1.0	
Urban	Share of population in urban areas, 1990	62	60.7	19.1	11.2	96.4	United Nations (2003)
European language	Fraction of a country's population that speaks English, French, German, Portuguese or Spanish	63	0.4	0.4	0	1.0	Hall and Jones (1999); http://www.ethnologue.com
Area	Million square kilometres	63	1.2	2.4	0	10.0	Central Intelligence Agency (2004)
Mining	Fraction of GDP produced in the mining and quarrying sector, 1988	58	0	0.1	0	0.5	Hall and Jones (1999)

Table A1: continued

Outward orientation	Dummy with value 1 if outward orientation based and 0 otherwise, 1988	55	0.4	0.5	0	1.0	King-Levine Dataset at http://www.worldbank.org/research/growth/ddkile93.htm; primary source: Syrquin and Chenery (1988)
Assassination	Number of political assassinations per billion inhabitants, 1980s	54	0	0.2	0	1.3	King-Levine Dataset at http://www.worldbank.org/research/growth/ddkile93.htm
Frankrom	Natural log of the Frankel-Romer forecasted trade share, derived from a gravity model of international trade that takes into account only country population and geographical features	50	2.6	0.7	0.9	4.0	Persson and Tabellini (2003); primary source: Hall and Jones (1999)
Military	Military expenditure as a share of GNI	58	3.0	3.0	0	21.0	World Bank (2001)
Real exchange-rate distortion	Real exchange-rate distortion, index, 1991	54	114.6	33.7	70.0	248.0	Levine and Renelt (1992); primary source: Dollar (1992)
Landlocked	Dummy with value 1 if landlocked country, i.e., country without a coastline, and 0 otherwise	63	0.1	0.4	0	1	Central Intelligence Agency (2004)

Table A2: Values for *Trust* and *Growth* in the two samples (1990-2000)

Country	Trust	Growth
The small sample includes the following 39 countries:		
Argentina	23.3	4.3
Australia	39.9	2.5
Austria	31.8	1.8
Bangladesh	21.0	2.8
Belgium	33.2	1.8
Brazil	6.7	1.5
Canada	52.4	1.9
Chile	22.7	4.9
Colombia	10.0	0.9
Denmark	57.7	2.0
Dominican	26.4	5.2
Finland	62.7	1.6
France	22.8	1.1
Germany	37.8	1.6
Ghana	23.0	1.4
United Kingdom	43.6	1.9
Greece	50.0	2.0
Iceland	43.6	1.6
India	34.3	4.0
Ireland	47.4	6.4
Italy	34.0	1.2
Japan	41.7	1.1
Korea	34.2	4.8
Mexico	33.5	1.8
Netherlands	54.9	2.2
New Zealand	37.0	1.5
Norway	65.1	2.8
Peru	5.0	2.5
Philippines	6.0	1.3
Portugal	21.4	2.6
South Africa	28.3	-0.3
Spain	33.8	2.2
Sweden	66.1	1.3
Switzerland	43.2	0.1
Taiwan	42.0	5.7
Turkey	10.0	1.8
Uruguay	22.0	2.9
USA	52.0	2.3
Venezuela	14.0	-0.8

Table A2: continued

Country	Trust	Growth
The large sample includes the following 24 additional countries:		
Algeria	11.2	-0.1
Bolivia	17	1.1
China	60.3	7.7
Costa Rica	11	1.8
Czech Republic	28	0.1
Ecuador	20	-0.8
Egypt	37.9	2.6
El Salvador	14.6	2.3
Guatemala	28	0.8
Honduras	25	-0.8
Hungary	24.6	0.8
Indonesia	51.6	2.5
Jordan	27.7	1.2
Latvia	19	-2.6
Nicaragua	20	-2.4
Pakistan	30.8	1.4
Panama	25	2
Paraguay	23	-0.6
Poland	34.5	3.4
Romania	16.1	-1.1
Slovakia	23	-0.5
Slovenia	17	1.9
Uganda	7.6	3.2
Zimbabwe	11.9	-1.6

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