



Digging into the Pocketbook: Evidence on Economic Voting from Income Registry Data Matched to a Voter Survey

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Abstract

We combine fine-grained data on voters' personal financial records with a representative election survey to examine three central topics in the economic voting literature: pocketbook versus sociotropic voting, the effects of partisanship on economic views, and voter myopia. First, these data show that voters who appear in survey data to be voting based on the national economy are, in fact, voting equally on the basis of their personal financial conditions. Second, there is strong evidence of both partisan bias and economic information in economic evaluations, but fine-grained financial data is required to separate the two. Third, although in experiments, and aggregate historical data, voters appear focused on recent economic conditions when choosing how to vote, we find no evidence of myopia when examining actual personal economic data. Collectively, the results show our understanding of economic voting depends crucially on the quality of available data.

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1 Introduction

Economic performance is one of the best predictors of election outcomes (Duch and Stevenson, 2008; Lewis-Beck and Paldam, 2000). Yet, the mechanisms by which money flowing through people’s pockets and communities maps into votes is much less clear (Ansolabehere, Meredith and Snowberg, 2014). Indeed, some of the more robust findings from this literature, such as the *sociotropic* hypothesis—national economic conditions seem to matter more to voters than their own personal economic experiences—continue to inspire debate decades after they were first identified (Fiorina, 1981; Kinder and Kiewiet, 1979; Kiewiet and Lewis-Beck, 2011).

The continuing struggle to understand the mechanisms of economic voting is largely due to the coarse data available. Almost all of the evidence about the individual-level effects of economic circumstances comes from survey questions that depend on recollections. Moreover, these recollections are elicited at only a single point in time: right before or right after an election (Lewis-Beck and Paldam, 2000). This is potentially problematic as partisan preferences, limited human memory, and other factors might color subjective assessments, making such survey data less than ideal (Wlezien, Franklin and Twiggs, 1997).

Using improved data that links a nationally-representative election survey to comprehensive personal financial information, we examine three long-standing debates in the economic voting literature: pocketbook versus sociotropic voting, the effects of partisanship on economic views, and whether or not voters are myopic. The personal financial data provide an individual’s history of income and assets, as verified by their tax returns, over a complete four-year term of a government. Merging this data with a detailed national election survey allows us to directly analyze the impact that an individual’s financial history has on economic evaluations, vote choice, and political preferences. The results suggest that that previous conclusions in the debates mentioned above may need to be revised.

In particular, we demonstrate that *pocketbook* considerations—that is personal economic circumstances—are, in our data, at least as important as sociotropic ones. Traditional anal-

yses of the survey data we use support the prior conclusion in the literature that sociotropic motivations have the greatest influence on voters. But those same voters are shown to actually vote equally on the basis of their pocketbooks when personal financial data is added to the analysis. Further, we use our data to examine the sources of bias and inaccuracy in economic perceptions, and show that after personal economic conditions are taken into account, partisanship is still an important predictor of economic evaluations. Finally, when focusing on personal financial circumstances, vote choice does not exhibit an end-year bias—an over-weighting of economic information in the final year(s) of a government’s term—counter to prior research. In particular, we demonstrate that respondents put the most weight on income changes in the first year of the government’s term, which, in our study, coincides with the government’s implementation of a large tax cut.

Together, the results show that our understanding of economic voting depends crucially on the quality of available data. Fine-grained personal financial data show the hidden impact of pocketbook considerations, the nature of partisan bias in economic perceptions, and the way that personal economic experiences over time affect vote choice.

2 Background

Our data covers the 2010 election, and the previous four years of personal income, in Sweden.¹ While there are theoretical reasons to believe that economic voting in Sweden may not generalize to other places—in particular, Sweden has a small open economy (Duch and Stevenson, 2008)—our study encompasses two results that suggest otherwise. First, when examining survey data on economic perceptions, Swedish voters appear sociotropic, just like voters in other countries. Second, Swedes appear, in survey experiments, to suffer from end-year bias—just like voters in the U.S. (Healy and Lenz, 2014).

Ahead of the 2006 election, the four center-right parties (the Moderates, the Center Party,

¹Due to privacy concerns, after two years, the personal identifier is stripped from the Swedish National Election Survey (SNES) data. Therefore we could only merge respondents in the 2010 survey with register data. However, approximately half of the respondents in 2010 were also surveyed in 2006.

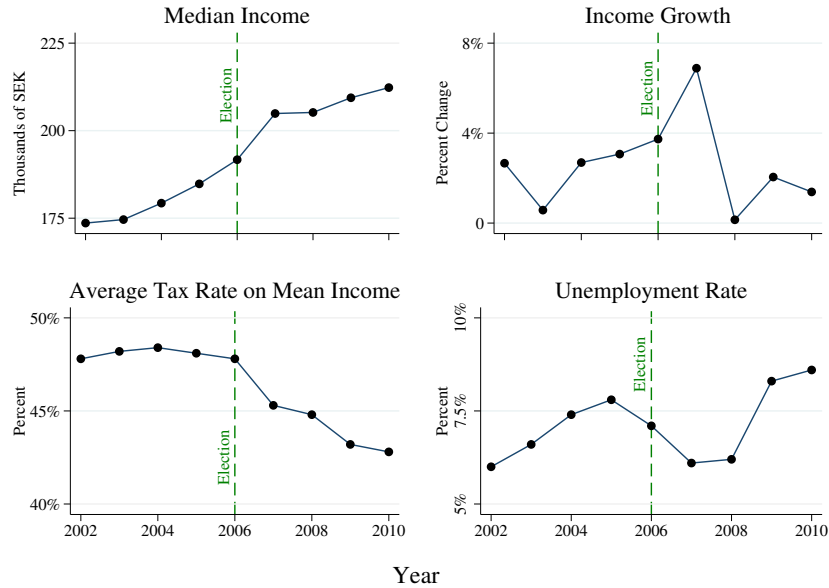
the Liberal People's Party, and the Christian Democrats) formed a coalition, the *Alliance for Sweden* (henceforth the *Alliance*). The Alliance won the 2006 election and formed a majority coalition government, ending 12 years of rule by the Social Democrats.

The campaign of 2006 focused heavily on the domestic economy, and employment was seen as the most important issue among voters (Widfeldt, 2007). Soon after the election, the Alliance enacted new economic policies. A central part of their program was large reductions in taxes for those with low and middle incomes. The first tax reduction was introduced in January 2007, and new tax reductions followed each year during the Alliance's term. However, in order to encourage work, these tax reductions only applied to labor income, and not to other forms of income such as disability benefits, unemployment insurance, pensions, and so on. Moreover, the Alliance increased fees for unemployment insurance, decreased the levels of unemployment benefits, and repealed wealth taxes.

Median income (in thousands of Swedish Kronor—SEK), income growth, tax on labor income, and the unemployment rate during the two government terms from 2002 to 2010 are shown in Figure 1. As can be seen, income growth was fairly constant across the period, with the tax cuts of 2006–2007 leading to a one year increase in the growth rate. The great recession hit Sweden in 2008, increasing unemployment and lowering the growth rate, although the growth rate remained positive.

In 2010, the Alliance successfully defended its economic record, convincing voters that Sweden had handled the external factors causing the great recession better than most other countries in Europe and elsewhere. Although the Alliance increased its vote share, it lost its majority position due to the rising popularity of the anti-immigrant Sweden Democrats (Widfeldt, 2011). The Alliance was, however, still the largest block, with 172 seats in parliament, followed by *Red-Greens*—a coalition consisting of the Social Democrats, the Green Party and the Left Party—which amassed 157 seats.

Figure 1: Income, Taxes, and Unemployment in Sweden, 2002–2010



Notes: Data on median disposable income (in constant 2012 prices) is from <http://www.statistikdatabasen.scb.se> (go to: Hushållens ekonomi / Hushållens ekonomi (HEK) / Inkomstfördelningsundersökningen). Data on taxes is from <http://www.ekonomifakta.se>, (go to: Skatter / Skatt påarbete / Skatt påarbete internationellt). Unemployment is for all people ages 15–74 and is from <http://prognos.konj.se/>, (go to: Arbetsmarknad / Arbetslöshet och syssel-sättningsgrad). All websites accessed August 2014.

2.1 Economic Voting in Sweden

As studies of economic voting are conducted in many different countries, and often focus on the U.S. case, the generalizability of our conclusions relies on whether Swedish voters are different than voters elsewhere. At least for the phenomena we examine, Swedish voters look remarkably like voters elsewhere in traditional, survey-based, studies.

Most recent studies have concluded that Swedish voting behavior, and the importance Swedes place on economic conditions, closely resembles economic voting in other countries (Martinsson, 2013). A number of papers based on the Swedish National Election Study (SNES) find that sociotropic considerations dominate pocketbook ones (Jordahl, 2006; Holmberg, 1984; Holmberg and Gilljam, 1987; Gilljam and Holmberg, 1993; Holmberg and Oscarsson, 2004). Thus, survey evidence from Sweden conforms to the conventional wisdom

established in the U.S. and several other countries: the economy matters, and sociotropic evaluations matter substantially more than pocketbook ones (Kinder and Kiewiet, 1979). We confirm these previous results from Sweden in Section 3. Moreover, we replicate the survey-based experiment of Healy and Lenz (2014) and show that the results match the U.S. results closely: Swedish voters appear myopic in the data from this experiment. While this cannot prove that the differences between our findings and the previous literature are solely due to superior data, it is strongly consistent with that hypothesis.

Despite these empirical facts, there remain theoretical concerns: in particular, Sweden is a multi-party parliamentary democracy with a small, open economy. The parliamentary system makes it easier for voters to assign credit or blame to the incumbent government for economic circumstances. However, the multi-party nature of that government, and the size of the economy, make this harder (Duch and Stevenson, 2008). Because the electoral coalitions in Sweden were known ahead of the 2010 election, we can mitigate concerns about coalition governments by coding a vote for any member of the incumbent coalition a vote for the incumbent (1), and a vote for any other party as a vote against (0). There is little to be done about the fact that Sweden's economy is small in global terms. However, it is worth noting that the results in Section 5 show voters rewarding the government much more for tax cuts in their first year in office than punishing them for changes in income due to the great recession in the second and third years.

2.2 Economic Voting

The economic voting literature consists of a large number of sub-literatures. Due to the nature of our data, we are able to examine three inter-related topics that are usually considered in isolation. As such, we consider the literature on each separately.

A large portion of the economic voting literature attempts to discern whether voters have pocketbook or sociotropic motivations, settling on the latter (Fiorina, 1981; Kinder and

Kiewiet, 1979; Kiewiet and Lewis-Beck, 2011).² A common interpretation of this conclusion is that voters are motivated by public interest (Lewin, 1991). Others argue that voters are self interested, and the apparent importance of sociotropic evaluations occurs because the national economy is a clearer signal of governmental performance than personal economic experiences (Ansolabehere, Meredith and Snowberg, 2014; Kramer, 1983; Peltzman, 1990). Our findings suggest that it is unlikely voters are primarily motivated by the public interest, and that it is researchers, not voters, who are hobbled by the noisiness of personal economic data. However, to our knowledge, no study has examined how pocketbook evaluations relate to verified, comprehensive, personal financial data.

The pocketbook and sociotropic evaluations that are the basis for studies in the preceding paragraph have come in for their fair share of criticism. In particular, several studies argue that these evaluations reflect political, rather than economic, considerations. That is, voters decide who they are going to vote for, and then report an economic evaluation that conforms with that choice (Chzhen, Evans and Pickup, 2014; Duch, Palmer and Anderson, 2000; Evans and Andersen, 2006; Evans and Pickup, 2010, 2013; van der Eijk et al., 2007; Wlezien, Franklin and Twiggs, 1997). Equally, perceptions might be colored by a “partisan lens” that leads voters to view the same economic events more favorably if their preferred party is in office (Zaller, 1992). We show that once real economic conditions are taken into account, partisanship still explains some of the voters’ economic evaluation, demonstrating that these contain both real economic information and partisan bias.³ However, to tease apart these two contributors to economic evaluations, fine-grained financial data is necessary.

Additionally, many studies argue that voters are poorly informed, and thus subjective evaluations will be noisy (Bartels, 1996; Conover, Feldman and Knight, 1986; Hellwig and

²The part of this literature most closely related to our study compares sociotropic evaluations to actual national and regional conditions (Ansolabehere, Meredith and Snowberg, 2014; Bisgaard, Dinesen and Sønderskov, 2016; Erikson, MacKuen and Stimson, 1992, 2002; Nadeau, Lewis-Beck and Bélanger, 2013).

³A final study of particular note is recent work by Alt and Lassen (2016) that, as with our paper, combines survey and registrar data in a Scandinavian country—in their case Denmark—to examine economic voting. The closest relationship is in our Section 4: there, we focus on the portion of economic evaluations driven by partisanship, while they focus on how providing information causes changes economic perceptions and reported vote choice.

Marinova, 2015; Kramer, 1983). Presumably, the level of noise should vary with political sophistication (Alt, Lassen and Marshall, 2016; Duch, Palmer and Anderson, 2000). Surprisingly, there is no general agreement in the direction of the relationship. Low-sophistication voters may require media cues to make economic evaluations, and thus, sociotropic evaluations may be more accurate for these voters than pocketbook evaluations (Mutz, 1992, 1994). On the other hand, the lack of sophistication may make it difficult for low-sophistication voters to incorporate external information, making pocketbook evaluations more accurate (Delli Carpini and Keeter, 1997). We find that increased political sophistication is associated with more accurate pocketbook assessments.

Concerns have also been raised about voters' abilities to retain and use economic information from early in a government's term.⁴ However, there is some disagreement over whether this reflects rational concerns—as it usually takes some time for economic policy to filter through to economic outcomes (Hibbs, 1987; Erikson, 1989)—or a myopic bias (Healy and Lenz, 2014; Huber, Hill and Lenz, 2012). The latter is of particular concern as it limits democratic accountability, and may lead to inefficient attempts at economic manipulation (Abrams, 2006; Achen and Bartels, 2004). However, the evidence in all cases come from surveys or aggregate data. By examining actual personal economic information, we reveal patterns that are incompatible with voter myopia. Namely, in our setting, voters place most weight on economic information from the beginning of the incumbent government's term, which corresponding to a large tax cut—the government's most important economic policy.

On the whole, then, voters seem decently well informed, at least about their own personal economic circumstances, and seem to use their information rationally. While much of the economic voting literature above disagrees with this conclusion, proponents of macropolitics have routinely argued that, on average, voters make very good projections about future economic conditions, and this influences both their partisanship and vote choice. (Erikson,

⁴Wlezien (2015) provides an elegant literature review that shows that U.S. aggregate data is consistent with voters basing their judgements only (and equally) on the final two years of a President's term.

MacKuen and Stimson, 1989, 1992, 2000, 2002).⁵ Our findings are broadly consistent with this perspective.

2.3 Data

Most prior studies of economic voting rely on voters' economic evaluations at a single point in time, or aggregate government statistics on the economy and elections. A number of factors have pushed researchers towards these measurement techniques. First, conventional wisdom holds that quantities are difficult to ask about on surveys, and thus, self-reported income measures have reliability problems (Micklewright and Schnepf, 2010; Moore and Welniak, 2000; Yan, Curtin and Jans, 2010).⁶ Second, recollections of income across time may exceed limited human memory (Withey, 1954).

By combining detailed and verified data on income with survey data, it possible to address topics that prior research, lacking such data, has been unable to address. Our survey data comes from the Swedish National Election Study (SNES), executed by Statistics Sweden in collaboration with the University of Gothenburg. The 2010 study was based on a random sample of 3,963 Swedish citizens aged 18 to 80; 2,736 interviews were conducted for a response rate of 69 percent. Approximately half of the sample was also interviewed for the 2006 SNES, the rest were re-interviewed in 2014. Additionally, half of the sample was interviewed before the election, and half afterwards, with a somewhat different survey instrument. As such, many of our results apply to about one-quarter of the overall sample.⁷ Most interviews were conducted face-to-face in respondents' homes or workplaces. The average time for a full interview was about one hour.

Detailed information about each citizen's income is collected in the Income and Taxation Register. After approval from the Swedish Research Ethical Review Board, Statistics Sweden

⁵See Green, Palmquist and Schickler (1998) for a critique of this literature.

⁶However, see Ansolabehere, Meredith and Snowberg (2013) for a discussion on how to best measure quantities in surveys.

⁷Voter turnout was validated with the official registers, and this information is used to correct reported vote choice. Note that turnout in Sweden is quite high: since 1960 it has been greater than 80%.

merged this data with the 2010 SNES.⁸ This data includes information on different kinds of income before and after taxes at the personal and household level from 2006 to 2010.⁹ This data is the exact value in Swedish Krona (SEK). At the end of 2010, \$1 USD \approx 6.75 SEK.

Our analyses focus on household disposable income as, in most cases, this best reflects the “true parameter of interest” (Kramer, 1983)—that is, economic conditions that can fairly be attributed to the government. An individual’s personal income will give an inaccurate picture when his or her partner has significantly higher or lower income. Total income, as opposed to disposable income, will give an inaccurate picture when tax rates and other obligatory expenses vary.

Throughout, we present results from both the full sample and a *stable sample*. The latter consists of households whose composition remained the same from 2006 to 2010. Household income is affected by the number of adults living in a household. This may change due to divorce, death, a retired parent moving in, or a grown child moving out. As such, our stable sample considers households that went through no such changes from 2006 to 2010. The stable sample also leaves out those that were retired, as they experience very little change in income.¹⁰ Of the full sample, 60.8% are in the stable sample, and a further 11.8% are retired. In general, results in both samples are quite similar.

3 Sociotropic and Pocketbook Voting

We begin our analysis of the data by re-examining a central finding of the economic voting literature: voters are sociotropic. Instead, we find that the portion of pocketbook evaluations that corresponds to real economic conditions is highly correlated with vote choice. This

⁸As mentioned above, half the respondents also took the 2006 SNES, so this data could be matched as well. The time from beginning of the application process to obtaining the data was about one year. The data never left Statistics Sweden’s servers, which could only be accessed from within Sweden.

⁹See Appendix A for detailed information on the register data.

¹⁰A more accurate label would be *stable, labor-market participating household sample*, but we abbreviate this to the *stable sample* for simplicity. The data contains information on the number of “consumption units”, which can be used to determine household composition change, and household pension income, which can be used to determine the time of retirement.

leads naturally into the next two sections, where we first examine what makes up the rest of pocketbook evaluations, finding that it is partisan bias and random noise. Finally, we examine how vote choice is affected by personal economic conditions across time.

A voluminous literature documents that voters seem to rely more on evaluations of the national economy (*sociotropic*) rather than personal economic circumstances (*pocketbook*) in choosing who to vote for (Kiewiet and Lewis-Beck, 2011; Kinder and Kiewiet, 1979; Lewis-Beck and Paldam, 2000). This finding is so ingrained that scholars often forget just how puzzling it is: personal economic experiences are salient and directly experienced, while national economic conditions need to be gleaned from news sources or, possibly, one’s surroundings (Ansolabehere, Meredith and Snowberg, 2014; Grafstein, 2009; Kinder and Kiewiet, 1981). As Fiorina (1981, p. 5) notes, pocketbook voting is theoretically more robust, because, “In order to ascertain whether the incumbents have performed poorly or well, citizens need *only* calculate the changes in their own welfare” [emphasis ours].

Here, we document that, taking account of voters’ actual economic circumstances, pocketbook and sociotropic considerations are equally important. To measure sociotropic and pocketbook considerations, we use the answers to two questions that have been extensively studied in the economic voting literature. Sociotropic evaluations come from answers to:

Would you say the the economic situation in Sweden has improved, stayed the same, or gotten worse, compared with the situation 12 months ago?

whereas the pocketbook evaluation comes from:

If you compare your economic situation with what it was 12 months ago, has it improved, stayed the same, or has it gotten worse?

Both questions allow responses on the same five-point scale, from “much worse”, which we code as 1, to “much better”, which we code as 5.

The role of raw sociotropic and pocketbook evaluations in vote choice are analyzed in Table 1. In all columns, the dependent variable is whether or not a respondent, whose turnout has been verified, reported a vote for the incumbent Alliance (coded 1), or a vote for a non-incumbent party (coded 0). This is regressed on sociotropic and pocketbook

evaluations, and, in some cases, controls, using ordinary least squares (OLS).¹¹ The first set of regressions uses the stable sample, as defined in Section 2.3, and the second set uses all respondents that answered these questions.

The first two columns report the standard analysis in this literature, using just survey data, and show the standard result: sociotropic evaluations appear to be much more important than pocketbook ones in determining vote choice. The addition of controls has little effect. This is reassuring as it matches the extant economic voting literature in both the U.S. and Sweden, emphasizing the general applicability of the results of our further analyses.

To understand the effect of personal economic circumstances on vote choice, and compare it with the effect of sociotropic evaluations, we first project pocketbook evaluations onto actual economic circumstances as observed in the register data. That is, we implement a two-staged-least-squares (2SLS) procedure to isolate the relationship between vote choice and the portion of the pocketbook evaluation that is correlated with actual economic conditions.

The results of this 2SLS procedure for the Stable Sample are displayed in the third and fourth column of Table 1. In particular, we regress pocketbook evaluations on economic circumstances—the change in household income in the year preceding the election (2009–2010), and baseline income in 2009. We use the change in the year prior to the election, as this matches the period of time in the evaluative question.¹² We represent the change in income with two variables to separate out the effects of small to moderate income shocks (below 20%) from very large income shocks, so that our specifications will not be overly influenced by outliers.¹³

¹¹We avoid discrete choice specifications, such as probit or logit, as these can produce biased and inconsistent coefficients when the dependent variable is measured with error (Hausman, 2001). As the dependent variable here comes from a self-report, we cannot be confident that it is exactly measured.

¹²Including changes in preceding years, as in Section 5, has little effect.

¹³In particular, income shocks of less than 20% are represented linearly, and those that are greater than 20% are indicated in a second, dummy, variable. The F-statistic on excluding these instruments is usually well above 10 (Angrist and Pischke, 2008; Stock, Wright and Yogo, 2012). The results of this first-stage specification can be found in Appendix B.

Table 1: Voting for the Incumbent and Retrospective Economic Evaluations

	Stable Sample		Full Sample					
	OLS	2SLS	OLS	2SLS				
Evaluation of the National Economy	0.18*** (.019)	0.17*** (.020)	0.15*** (.026)	0.15*** (0.027)	0.18*** (.015)	0.17*** (.016)	0.14*** (.024)	0.12*** (.029)
Evaluation of Personal Economic Situation	0.047** (.020)	0.051** (.022)	0.20** (.088)	0.18* (.10)	0.034** (.016)	0.043** (.017)	0.23*** (.078)	0.29*** (.11)
Constant	-0.35*** (.084)	-0.43* (.23)	-0.74 (.23)	-0.83** (.36)	-0.27*** (.068)	-0.40** (.15)	-0.74*** (0.20)	-1.12*** (0.36)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	514	513	512	511	858	856	856	854

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with heteroscedasticity-consistent standard errors in parentheses. The dependent variable equals 1 for those voting for the incumbent centre-right Alliance and 0 for those voting for opposition parties. For a description of the Stable Sample, see Section 2.3. Controls include: age, age squared, education, gender, marital status, immigration status, and education, which are the independent variables in Table 3.

When the projections from this first stage are entered in a second stage regression, the results are striking. Pocketbook considerations go from being a distant also-ran to as important as sociotropic ones. However, the fact that coefficients on sociotropic evaluations decrease only slightly implies that they contain additional information beyond personal economic circumstances.¹⁴ This is true whether one examines just the Stable Sample, or the full sample of respondents (Columns 5–8).

The 2SLS estimates provide strong evidence that pocketbook considerations are more important than previously appreciated, and call into question the common interpretation of sociotropic voting: that voters are primarily motivated by public interest rather than their own self-interest (Lewin, 1991). Statistically, the fact that the 2SLS estimates are so much larger than the OLS estimates suggests the presence of substantial noise in the pocketbook evaluations.¹⁵ This noise would attenuate regression coefficients towards zero, and would have prevented previous scholars from appreciating the importance of pocketbook evaluations. A natural next question arises: what are the drivers of that noise? This is the question we turn to in the next section.

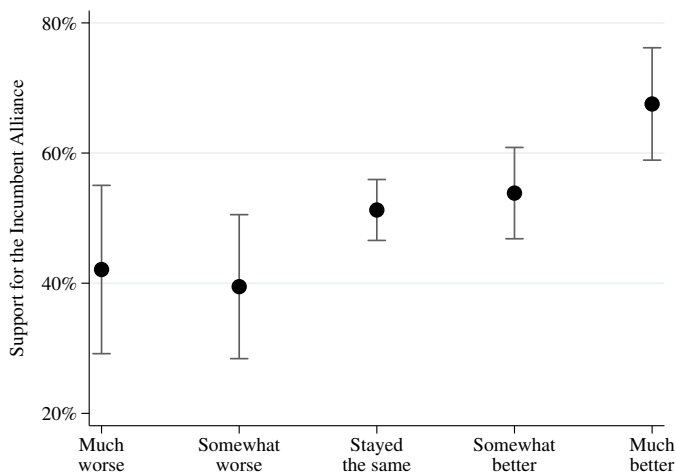
4 Bias and Accuracy in Pocketbook Evaluations

As noted in the previous section, there is substantial noise in pocketbook evaluations. Prior scholarship contends that this noise is driven by partisan bias. Figure 2 illustrates this concern: those who report that their personal economic circumstances are “much better” than a year ago are much more likely to vote for the incumbent than those that report it is “much worse”. However, this result leaves the direction of causality unclear. Are they voting for the incumbent because their economic circumstances have improved? Or do they report

¹⁴An obvious caveat, of course, is that we cannot project the sociotropic evaluation on national economic conditions as the national economy was the same everywhere in 2010. Results from similar models, but which include personal and national evaluations separately, are presented in Tables B.2 and B.3 of Appendix B. These specifications are also consistent with the general conclusion that pocketbook considerations are at least as important as sociotropic considerations.

¹⁵This pattern of noise could be referred to as measurement error, see Gillen, Snowberg and Yariv (2015) for a treatment of that subject in surveys and experiments.

Figure 2: Those who report improved economic circumstances are more likely to vote for the incumbent.



Notes: Figure shows average level of incumbent voting and 95% confidence intervals for respondents expressing a particular pocketbook evaluation, as indicated on the x-axis.

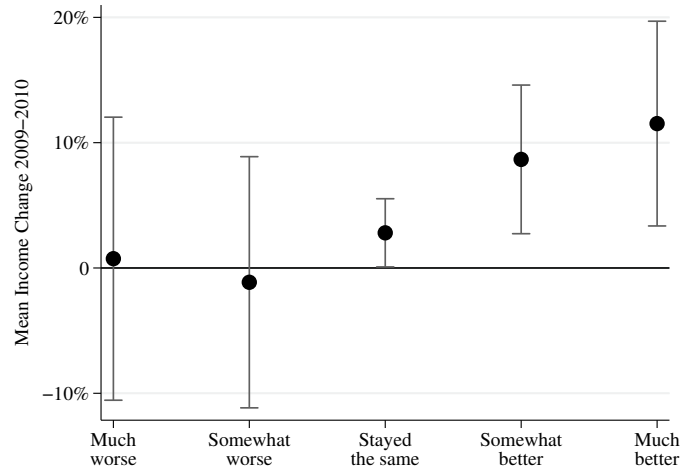
that their economic circumstances have improved because they are voting for the incumbent for non-economic, possibly partisan, reasons?

Additionally, as discussed in Section 2.2, the relationship between the level of noise in subjective evaluations and political sophistication is unclear, both theoretically and empirically. We can reject out of hand the idea that pocketbook evaluations are *only* noise: if that were the case then actual economic circumstances would not explain pocketbook evaluations, counter to the findings in the previous section. We can illustrate this another way: Figure 3 shows that those who reported improved economic circumstances over the previous year indeed experienced greater increases in income than those who reported their personal circumstances got worse.¹⁶

A deeper examination of our data can provide additional insights into the bias and accuracy of pocketbook evaluations. To analyze sources of bias in economic evaluations, we first regress the pocketbook evaluation on actual economic circumstances—as in the

¹⁶Although, as figures are adjusted for inflation, even those that reported personal economic circumstances were “much worse” on average experienced increasing incomes over the previous year.

Figure 3: Pocketbook evaluations are correlated with actual personal income growth.



Notes: Figure shows average and 95% confidence intervals of income change for respondents expressing a particular pocketbook evaluation, as indicated on the x-axis.

first stage specification underlying Table 1—and generate residuals. These residuals are the difference between evaluations of two people with the same economic circumstances, so predictors of these residuals are predictors of bias.¹⁷ We regress the residuals on measures of partisanship, political knowledge, and a host of demographic characteristics. In Table 2 we find clear evidence of partisan bias in reported economic evaluations. In particular, once actual economic circumstances are taken into account, those who plan to vote for the incumbent Alliance report more favorable economic evaluations.

To reduce concerns that unobserved factors may be driving both the residual of economic evaluations and voting for the incumbent we instead look at the role of vote choice in 2006 on residual economic evaluations. Recall that in 2006 the Alliance was not the incumbent, so if the same unobserved factors were driving both voting in 2006 and residual evaluations in 2010, we would expect to see a negative coefficient on vote choice. However, this is not what we observe. Instead, the coefficient is positive. It is smaller and insignificant when using the 2006 vote choice (reported concurrently) for the very small sample of people for whom we

¹⁷Note that this bias might be due to different ways the two individuals interpret a qualitative scale (Ansolabehere, Meredith and Snowberg, 2013).

Table 2: Correlates of Bias in Pocketbook Evaluations

Dependent Variable: Sample:	Residual from Regression of Pocketbook Evaluation on Income Variables					
	Stable	Full	Stable	Full	Stable	Full
Vote Choice, 2010	0.25** (.10)	0.21** (.082)				
Vote Choice, 2006 (2006 survey)			0.15 (.16)	0.049 (.13)		
Vote Choice, 2006 (recall, 2010)					0.18* (.10)	0.26*** (.085)
Political Knowledge	0.20 (.36)	0.31 (.28)	-0.64 (.55)	-0.071 (.40)	-0.20 (.36)	-0.00 (.29)
Secondary Education	0.028 (.20)	-0.17 (.13)	-0.35 (.27)	-0.35** (.16)	0.027 (.20)	-0.24* (.13)
College Education	0.24 (.21)	-0.094 (.14)	0.13 (.28)	-0.22 (.18)	0.28 (.21)	0.024 (.14)
Age	-0.015 (.027)	-0.0080 (.016)	-0.0047 (.036)	0.030 (.022)	-0.018 (.029)	0.0018 (.015)
Age ²	0.00 (.00)	-0.00 (.00)	0.00 (.00)	0.00** (.00)	0.00 (.00)	-0.00 (.00)
Gender	0.034 (.10)	0.088 (.082)	0.20 (.16)	0.086 (.12)	0.067 (.11)	0.16* (.086)
Married	-0.044 (.11)	-0.053 (.085)	-0.13 (.16)	-0.10 (.12)	-0.019 (.12)	-0.058 (.088)
Immigrant	-0.15 (.18)	-0.073 (.15)	0.16 (.25)	-0.39* (.21)	-0.17 (.18)	-0.033 (.16)
Constant	0.32 (.59)	0.36 (.41)	0.77 (.86)	-0.0021 (.60)	0.60 (.68)	0.30 (.45)
Observations	385	627	161	266	321	549

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with heteroscedasticity-consistent standard errors in parentheses. For a description of the Stable Sample, see Section 2.3.

have this information. However, the coefficient size and statistical significance is relatively similar to the coefficient on 2010 vote choice when we use 2006 vote choice as recalled in 2010. The differences appear to be due to sample size, as an overwhelming majority of

respondents (92%) reported the same 2006 vote choice in both 2006 and 2010.

However, the biases uncovered in Table 2 may just be reflective of general inaccuracies in economic evaluations among people of a given political persuasion. To test this, we regress the absolute value of the residual from economic evaluations on the same variables as in Table 2. The results are in Table 3. There is little evidence that anything other than general political knowledge (measured as the number of correct answers to 18 general political knowledge questions), and possibly immigration status, are related to accuracy of economic evaluations. However, these results need to be interpreted carefully as “accuracy” here is defined as the closeness of a respondent’s reported economic evaluations to the average of others with the same economic experiences. Nevertheless, it suggests that inaccuracies in economic evaluations do not drive the patterns of bias among Alliance or opposition voters.

It is important to note two things. First, the prior literature uses panel data to provide evidence of partisan bias in economic evaluations. This requires (reasonable) assumptions about the dynamics of partisanship, or that other economic events (such as a job loss) are correctly reported, while economic evaluations are subject to partisan bias. Personal financial data allows for the separate identification of economic and political contributions to economic evaluations, and shows they are both important. Second, our results only apply to pocketbook evaluations, as we cannot control for the relevant variation in macroeconomic information that would be needed to examine sociotropic evaluations.¹⁸ However, the macropolitics literature, discussed in Section 2.2, has shown that voter’s forecasts of aggregate economic outcomes are, on average, quite good, so there are other indications that sociotropic evaluations—at least of the prospective variety—contain important economic information as well.

Up until this point we have largely examined election-year income changes, as this is the quantity asked about in retrospective economic evaluations. The literature provides

¹⁸Conducting the same exercise as in Table 2 with sociotropic evaluations leads to a significant coefficient on partisanship, as implied by the results in Table 1. However, as this is only controlling for personal economic experiences, rather than variation in macroeconomic information, these results are, unfortunately, not very useful.

Table 3: Correlates of Accuracy in Pocketbook Evaluations

Dependent Variable: Sample:	Negative of absolute residual from regression of pocketbook evaluation on income variables					
	Stable	Full	Stable	Full	Stable	Full
Vote Choice, 2010	-0.071 (.068)	-0.043 (.054)				
Vote Choice, 2006 (2006 survey)			-0.010 (.11)	0.036 (.083)		
Vote Choice, 2006 (recall, 2010)					0.029 (.071)	0.022 (.056)
Political Knowledge	0.26 (.22)	0.36** (.17)	0.19 (.32)	0.38 (.25)	0.19 (.23)	0.36** (.18)
Secondary Education	0.12 (.14)	0.00 (.092)	-0.013 (.17)	-0.12 (.12)	0.017 (.15)	-0.074 (.093)
College Education	0.11 (.14)	0.027 (.096)	0.072 (.17)	-0.074 (.13)	0.021 (.16)	-0.051 (.097)
Age	0.023 (.017)	0.0077 (.011)	0.0028 (.025)	-0.013 (.016)	0.0089 (.020)	0.0058 (.012)
Age ²	-0.00 (.00)	-0.00 (.00)	-0.00 (.00)	0.00 (.00)	-0.00 (.00)	-0.00 (.00)
Gender	0.039 (.069)	0.057 (.054)	-0.0017 (.10)	-0.016 (.080)	0.024 (.076)	0.054 (.057)
Married	0.093 (.072)	0.073 (.056)	0.0044 (.11)	0.022 (.082)	0.087 (.077)	0.070 (.057)
Immigrant	-0.029 (.13)	-0.015 (.10)	0.37* (.20)	0.077 (.18)	0.095 (.13)	0.061 (.11)
Constant	-1.58*** (.37)	-1.34*** (.25)	-0.98* (.58)	-0.77* (.40)	-1.19** (.46)	-1.25*** (.29)
Observations	385	627	161	266	321	549

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with heteroscedasticity-consistent standard errors in parentheses.

another reason for focusing on this quantity: voters are thought to be myopic, primarily using election-year changes when making voting decisions. Our data allows us to examine this conclusion, and we do so in the next section.

5 Is the Economic Vote Myopic?

Much of the evidence for end-year myopia comes from surveys, survey experiments, and aggregate statistics. Each is potentially problematic. Surveys, administered at one point in time, only consider a single aggregate economy. Survey experiments assume that voters' reported responses to hypothetical economic conditions are representative of the way that they process real economic information. Aggregate statistics provide little detail on mechanisms mechanisms, making it difficult to distinguish a rational focus on the end of a government's term (Hibbs, 1987) from a myopic bias.

Our data presents a unique and novel opportunity to test whether such end-year myopia appears when we examine voters' income across a government's term. To argue that our results generalize, we first verify that Swedish voters respond in the same way as U.S. voters to hypothetical economic conditions in survey experiments. We do so by replicating the experiment of Healy and Lenz (2014) in two Swedish surveys.¹⁹ We then proceed to examine how vote choice responds to real changes in household income. We find, contrary to the existing survey-based evidence (including our own), that Swedish voters put the most weight on personal income changes early in a government's term. While this may seem puzzling, it is actually heartening: as shown in Figure 1 the incumbent government's largest impact on disposable income was felt in the first year of its term, when it sharply cut taxes.

The survey experiment we replicate asks respondents to rate graphs of the economic performance of a government. This is operationalized by showing respondents four bars illustrating the growth rate in each year of the government's term.²⁰ The graphical representations are hypothetical: we use the same 25 pictures used in Healy and Lenz (2014), in which each year was independently drawn from a normal distribution parameterized by the first and second moments of postwar yearly growth in the U.S. We supplement this with an-

¹⁹Results based on aggregate statistics, as in Wlezien (2015), also suggest that Swedish voters over-weight the last years of a government's terms, as in the U.S. However, interpretation is tricky given the parliamentary structure and unequal term lengths of the Swedish government. Results available from authors upon request.

²⁰Examples can be found in Appendix C.

other 100 hypothetical economic records that are drawn according to a normal distribution parameterized by the first and second moments of postwar yearly growth in Sweden. Thus, some economic records showed steady growth, others showed brief or long-lasting recessions, and some big changes in growth rates.

Our experiment ran on a probability-sampled representative survey of 584 Swedes. As Healy and Lenz (2014) largely conducted their experiment on a convenience sample—Mechanical Turk—we also ran our experiment on 1,374 Swedes in an opt-in (convenience) sample.²¹ Those in the opt-in sample were shown five hypothetical economic records—two with U.S. growth rates and three with Swedish growth rates, while those in the probability sample were shown seven hypothetical economies—three with U.S. growth rates, and four with Swedish growth rates. Participants were informed that the plots represented growth rates in national average levels of personal income from a government’s second term, and were asked to assess, on a four point scale, how good the economy was during the term.

Table 4 analyzes the results of this experiment. The analysis follows the structure in Healy and Lenz (2014). In particular, we regress each assessment of each hypothetical economy on the growth rates presented in those graphs. We include respondent-specific fixed effects so that the identifying variation is within person. Moreover, we cluster standard errors by hypothetical economy, as this is the unit of “treatment”.

The results for our Swedish sample replicate those in Healy and Lenz’s U.S. sample. Swedish voters heavily over-weight economic growth in the final years of the government’s term—especially the last year. Indeed, the coefficient on final-year economic growth is significantly larger than the coefficients on any other year. This is true regardless of whether the economic records are meant to mimic the U.S. or Sweden. Moreover, the result is the same whether one examines the representative sample or the probability sample.

Having established that Swedes show the same kind of end-year bias in national economic

²¹The survey experiment was conducted in the “Citizen Panel” at the Laboratory of Opinion Research (LORE) hosted by the Multidisciplinary Opinion and Democracy Research Group (MOD) at the University of Gothenburg, Sweden.

Table 4: Effects on Assessments of Hypothetical Income Growth. OLS estimates.

	Full experiment	U.S. economies	Swedish economies	Opt-in sample	Probability sample
Year 1 Growth	0.10*** (.022)	0.10** (.041)	0.083*** (.021)	0.12*** (.023)	0.068*** (.026)
Year 2 Growth	0.26*** (.024)	0.32*** (.05)	0.24*** (.022)	0.26*** (.024)	0.27*** (.028)
Year 3 Growth	0.34*** (.028)	0.36*** (.054)	0.32*** (.023)	0.34*** (.028)	0.33*** (.032)
Year 4 Growth	0.51*** (.032)	0.50*** (.041)	0.55*** (.033)	0.49*** (.032)	0.53*** (.036)
Constant	2.97*** (.17)	2.75*** (.25)	3.113*** (.15)	3.08*** (.18)	2.79*** (.18)
Observations	10,811	4,442	6,369	6,794	4,017
Economies	125	25	100	125	125
Individuals	1,958	1,953	1,953	1,374	584

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with standard errors clustered by economy (picture) in parentheses. All specifications include respondent fixed effects.

evaluations as U.S. voters on a survey experiment, we turn to the register data on personal income. In particular, in Table 5 we regress an indicator for whether the respondent voted for the incumbent center-right Alliance on changes in (logged) household disposable income over the Alliance’s term. In all cases, the specifications show that the most important yearly changes in disposable income were in the first years of the Alliance’s government. Baseline income—in 2006—also increases the probability that the respondent voted for the Alliance, reflecting the general finding that higher income is associated with right-leaning political views. While the emphasis on income growth in the early years of the Alliance’s term is unexpected, it is consistent with the fact that the most important economic changes—ones that impacted disposable income immediately—were implemented in the Alliance’s first year in office, as discussed in Section 2 and shown in Figure 1.

Table 5: Relationship between incumbent voting 2010 and yearly income changes.

Sample:	Stable	Full
Change in Disposable Household Income 2006–2007 (log)	0.21*** (.029)	0.20*** (.026)
Change in Disposable Household Income 2007–2008 (log)	0.076*** (.025)	0.11*** (.022)
Change in Disposable Household Income 2008–2009 (log)	0.088*** (.024)	0.11*** (.021)
Change in Disposable Household Income 2009–2010 (log)	0.051** (.024)	0.059*** (.018)
Disposable Household Income 2006 (log)	0.17*** (.020)	0.18*** (.019)
Vote choice 2006		
Constant	-1.68*** (.26)	-1.82*** (.24)
Controls	No	Yes
Observations	1,449	2,376

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with heteroscedasticity-consistent standard errors in parentheses. The dependent variable equals 1 for those voting for the incumbent centre-right Alliance and 0 for those voting for opposition parties.

The specifications vary according to the sample used—stable or full, as defined in Section 2.3, whether controls are included, and whether we include vote choice in 2006 as an independent variable. Both varying the sample and the inclusion of controls make very little difference in the results. However, including 2006 vote choice has two important effects: it reduces the sample size by more than one-half, and reduces the coefficients on all years. However, changes in disposable income in the early years of the government’s term are still the most important correlates of vote choice.

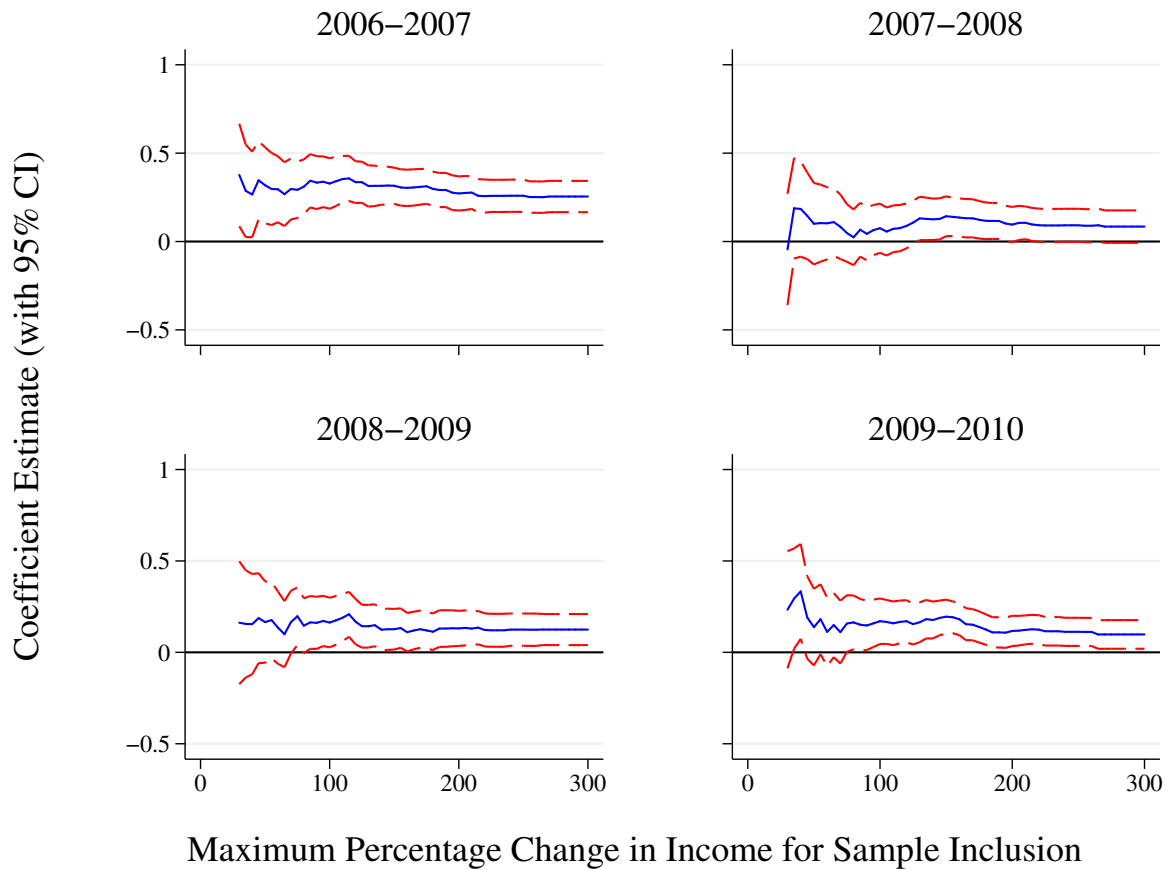
A concern with this analysis is that the results may be affected by outliers—respondents with particularly large changes in income. Therefore, we rerun the model in the first column of Table 5, varying the threshold of income change at which we drop an observation. We start by including those that had less than a 30% change in income in all years (932 persons), and then gradually expand the sample by increasing the cutoff 5 percentage points at a time.²² The four coefficients on income changes, and their 95% confidence intervals are displayed in Figure 4. The y-axis of each panel shows the magnitude of the coefficients, while the x-axis indicates the cutoff used in the regression. Thus, on the left side of each panel is the most restricted regression, including only those with relatively small income changes, and the sample increases as one moves right. All four coefficients are relatively stable over the range, although the standard errors shrink as the sample grows. Most importantly, this analysis shows no sign of end-year bias, and continues to show that income changes in the first years of the Alliance’s term had the largest impact on respondents’ vote choices.

A plausible, simple, view of these findings is that those who benefitted from a large tax cut in the first year of the Alliance’s term were most likely to vote to re-elect. This is inconsistent with a view of voters as suffering from a myopic bias. Perhaps more importantly, this is consistent with a view of democratic accountability with self-interested preferences.²³

²²We continue to do so up to the value of 300 percent. Households with larger income changes represent less than 0.25 percent of the sample.

²³There is a moderate negative correlation (-0.3) between changes in income in the first three years of the Alliance’s term—or the first two changes. This suggests that those who benefitted the most from the Alliance’s tax cut were also hardest hit by the recession. This is interesting as it suggests that voters correctly gave the government credit for the increase in disposable income in the first year, but did not incorrectly

Figure 4: Coefficients and 95% confidence intervals for effects of changes in disposable household income on vote choice 2010 over different windows of income changes.



We thus conclude in the next section by summarizing our results and discussing how they relate to democratic accountability.

6 Discussion

By augmenting survey data with detailed personal financial data from tax records and other government sources, we are able to revisit three findings from the economic voting literature. Taken together, our analyses show that survey and aggregate data can obscure, and

blame the government for the decrease in disposable income the next year—this decrease was due to events outside the government’s control, the worldwide great recession.

sometimes mislead, researchers about how voters behave.

To summarize: we find that pocketbook evaluations contain real information about economic experiences, and the evaluations based on those experiences are important in determining vote choice. Moreover, the personal financial data provide clear evidence on the effect of partisan bias in economic perceptions on voting decisions. Finally, in our data, economic evaluations are largely driven by changes in income during the first year of the government's term, when the biggest changes in policy occurred. This shows that voters are not myopic when it comes to their personal financial circumstances. In addition, their economic perceptions are plausibly driven by policy outcomes.

These findings have implications that extend beyond the economic voting literature to more general questions about democratic accountability. Indeed, findings from the economic voting literature have been used to suggest that voters may be easy to manipulate, and thus have limited ability to control politicians. For example, it has been suggested that the fact that voters seem to display end-year myopia can be exploited by incumbents artificially pumping up the economy in the final year of their term to mask otherwise poor economic records (Abrams, 2006; Achen and Bartels, 2004). Moreover, if voters ignore their own personal economic information they may be more reliant on the media for information used to evaluate governments (Hetherington, 1996), opening themselves to further manipulation. Yet our findings suggest that voters are not myopic and use their own economic information. This warrants a reduction in cynicism about voter's rationality, and ability to exercise democratic accountability.

An easy retort would be that our study considers Swedish citizens, who are different in a number of ways. However, previous scholars, and our own results, suggest that when it comes to economic voting, Swedes look remarkably like U.S. citizens—at least in conventional analyses. Thus, we find the explanation that the differences in findings between this work and the previous literature is largely due to better data more likely than vague hypothesis about cultural differences. However, we acknowledge that analyses of finer-grained data in

the U.S., or other countries, may produce far different results than what we find here.

We look forward to these analyses. For now, the only thing that is certain is that the increasing availability of personal income data promises to change our understanding of economic voting.

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Online Appendix—Not Intended for Publication

A Additional Information on the Register Data

Household disposable income is defined as gross wage and salary income, plus income from public sources, plus income from pensions and other annuities, minus taxes, summed to the household level. We also have data on the number of “consumption units” in a household, and pension income, which we use to determine the stable households, and those households with retired persons, respectively. Detailed information on the data in the Income and Taxation Register can be found at http://www.scb.se/sv_/Vara-tjanster/Bestalla-mikrodata/Vilka-mikrodata-finns/Registret-over-inkomster-och-taxeringar-IoT/.

Table A.1 shows the annual mean household disposable income levels in the register data (adjusted for inflation to the 2010 price level). The data corresponds quite well to the pattern shown among the entire population in Figure 1. The variability of incomes in our data reflects well the actual variation in the Swedish population, reflecting the fact that the sample is representative.

Our primary interest is in changes in income. Figure A.1 shows the distribution of annual changes disposable household income during 2006–2010 term. In the figure, changes are grouped into 25 percentage point bins, with the final bin including all respondents with income changes larger than 300%. The largest group is those with positive income changes from 0 to 25 percent, and almost three-quarters of respondents have income changes between –25 percent to 25 percent. However, a substantial proportion of the respondents have considerably larger changes in income.

Table A.1: Descriptive Income Statistics (in Swedish Kronor—SEK)

Personal disposable income		Household disposable income		
Year	Mean	SD	Mean	SD
2006	187,249	258,775	374,225	325,847
2007	201,340	246,390	401,964	492,833
2008	202,255	164,102	389,555	281,593
2009	211,778	225,553	401,559	353,183
2010	217,122	180,606	400,641	304,745

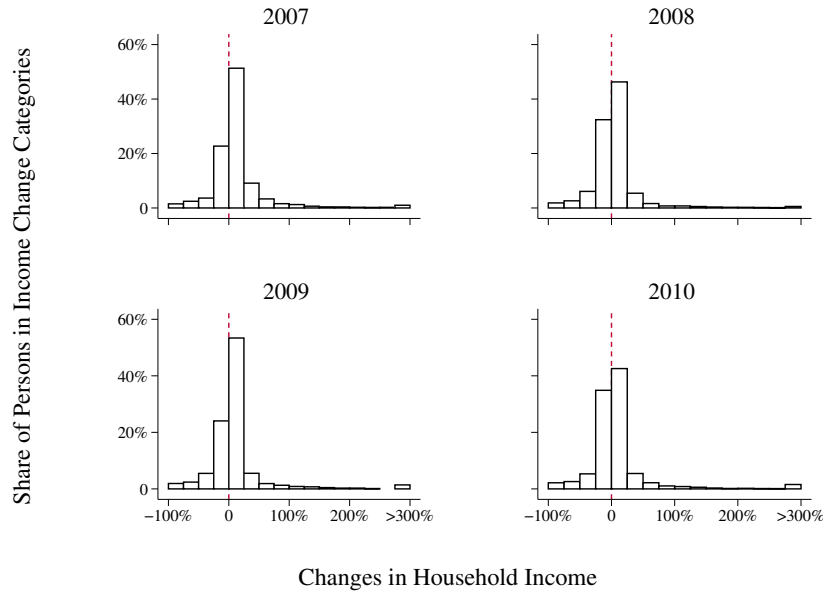


Figure A.1: Annual changes in household income 2007–2010.

B Further Analysis of Sociotropic and Pocketbook Evaluations

Table B.1 shows the first stage regression underlying Table 1. Importantly, the F-statistic on the excluded instruments is generally well above the rule-of-thumb level of 10 for excluding weak instruments. The only exception is in the stable sample, with controls, but the

coefficients in the second stage regression are not significantly different in this specification, so this does not seem to be a cause for concern.

Table B.1: First stage regression results. Effects on personal economic evaluations, 2010.

	Stable Sample		Full Sample	
National Economic Evaluation	0.19*** (.046)	0.18*** (.046)	0.20*** (.037)	0.19*** (.037)
Change in Disposable Household Income 2009-2010 (log)	1.12* (.65)	0.75 (.67)	1.80*** (.52)	1.404*** (.540)
Large Change in Disposable Household Income 2009-2010 (dummy)	0.52*** (.11)	0.46*** (.11)	0.34*** (.079)	0.268*** (.079)
Disposable Household Income 2009 (log)	0.22*** (.058)	0.23*** (.070)	0.20*** (.053)	0.169*** (.061)
Constant	-0.32 (.73)	0.064 (.84)	-0.059 (.67)	0.880 (.734)
Controls	No	Yes	No	Yes
Observations	512	511	856	854
R^2	0.097	0.15	0.092	0.15
F	12.8	8.3	20.4	15.2

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with heteroscedasticity-consistent standard errors in parentheses. The dependent variable is retrospective evaluations of the personal economy.

Next we analyze separately the effects of personal and national economic evaluations on vote choice. Tables B.2 and B.3 show these analyses, respectively. The tables show results that are quite similar to specifications where both personal and national economic evaluations are included simultaneously, suggesting that our results are due to uncovering the true, separate, information content in personal evaluations, rather than showing that national evaluations contain information about personal economic circumstances.

Table B.2: Voting for the Incumbent and Personal Economic Evaluations

	Stable Sample		Full Sample	
	OLS	2SLS	OLS	2SLS
Evaluation of Personal Economic Situation	0.072*** (.021)	0.25*** (.092)	0.066** (.016)	0.31*** (.078)
Constant	-0.26*** (.072)	-0.31 (.30)	-0.31*** (.056)	-0.48* (0.26)
Controls	No	No	No	No
Observations	533	531	881	879
			879	877

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with heteroscedasticity-consistent standard errors in parentheses. The dependent variable equals 1 for those voting for the incumbent centre-right Alliance and 0 for those voting for opposition parties. For a description of the Stable Sample, see Section 2.3. Controls include: age, age squared, education, gender, marital status, immigrant, and education, which are the independent variables in Table 3.

Table B.3: Relationship between incumbent voting and retrospective evaluations of the national economy, 2010. OLS Estimates.

	Stable Sample		Full Sample	
National Economic Evaluation	0.187*** (0.019)	0.173*** (0.020)	0.186*** (0.015)	0.177*** (0.016)
Constant	-0.217*** (0.074)	-0.306 (0.221)	-0.186*** (0.059)	-0.286* (0.148)
Controls	No	Yes	No	Yes
Observations	518	517	862	860

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with heteroscedasticity-consistent standard errors in parentheses.

C Example graphs from the survey experiment

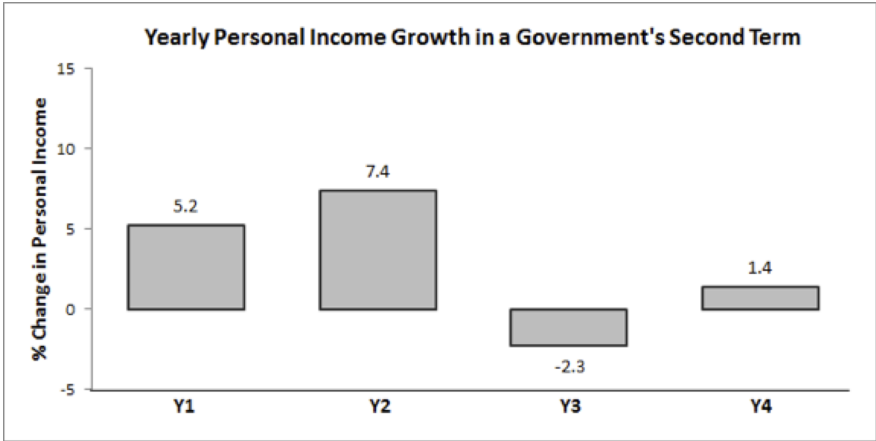


Figure C.2: Example of graphs shown in the experiment.

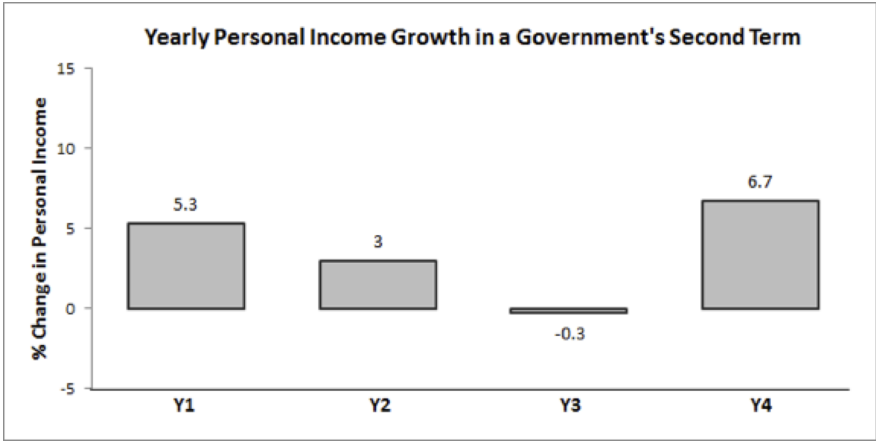


Figure C.3: Example of graphs shown in the experiment.