

Winter, Joachim; Crossley, Thomas; de Bresser, Jochem; Delaney, Liam

Conference Paper

Can Survey Participation Alter Household Saving Behavior?

Beiträge zur Jahrestagung des Vereins für Socialpolitik 2014: Evidenzbasierte Wirtschaftspolitik
- Session: Survey Methods, No. C19-V3

Provided in Cooperation with:

Verein für Socialpolitik / German Economic Association

Suggested Citation: Winter, Joachim; Crossley, Thomas; de Bresser, Jochem; Delaney, Liam (2014) : Can Survey Participation Alter Household Saving Behavior?, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2014: Evidenzbasierte Wirtschaftspolitik - Session: Survey Methods, No. C19-V3, ZBW - Deutsche Zentralbibliothek für Wirtschaftswissenschaften, Leibniz-Informationszentrum Wirtschaft, Kiel und Hamburg

This Version is available at:

<https://hdl.handle.net/10419/100379>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Can Survey Participation Alter Household Saving Behavior?*

Thomas F. Crossley[†] Jochem de Bresser[‡] Liam Delaney[§] Joachim Winter[¶]

February 28, 2014

Abstract

Much empirical research in economics is based on data from household surveys. Panel surveys are particularly valuable for understanding dynamics and heterogeneity. A possible concern with panel surveys is that survey participation itself may alter subsequent behavior. We provide novel evidence of survey effects on household financial behavior in a developed country. We exploit randomized assignment to survey modules within the LISS-panel, an internet panel survey which is representative of the Dutch population. Our saving measure is based on linked administrative wealth data, allowing us to distinguish changes in saving behavior from changes in reporting behavior. We find that households that respond to detailed questions on expenditures and needs in retirement reduced their non-housing saving rate by 3 percentage points. The size of the effect is increasing in the education level of the household. One interpretation is that the survey acted as a salience shock.

Key words: Survey effects, savings, administrative data, retirement

1 Introduction

Much empirical research in economics analyzes data from surveys of individuals and households. The development of panel surveys has allowed researchers to assess and account for heterogeneity and dynamics in economic behavior. However, repeated data collection from the same individuals or households brings a risk of survey effects: the possibility that questioning individuals about the actions or attitudes in a particular domain can alter their later behavior.

The extent to which the act of measurement alters the very behaviors being measured is a core question in many disciplines but not one that has been confronted in depth in economics. Finding

*The authors thank the participants at the 2012 MESS-workshop for helpful comments on an early version. We also thank Arthur van Soest, Marcel Das, Frederic Vermeulen, Pierre-Car Michaud, Rob Alessie, Martin Salm; and seminar and conference participants at Tilburg University, the University of Groningen, the University of Stirling, Mannheim University, Ludwig Maximilian University of Munich, Essex University, The 2013 Canadian Research Data Centre Network Conference and the 2014 Netspar international pension workshop. Financial support for data access was provided by Netspar and CentERdata. TC acknowledges support from the ESRC through the ESRC-funded Centre for Microeconomic Analysis of Public Policy at the Institute for Fiscal Studies (CPP, reference RES-544-28-5001).

[†]Essex University & Institute for Fiscal Studies, London. tcross@essex.ac.uk

[‡]University of Groningen; email: J.de.Bresser@rug.nl

[§]University of Stirling. liam.delaney@stir.ac.uk

[¶]University of Munich. joachim.winter@lrz.uni-muenchen.de

significant survey effects in important areas of economic research would require a rethinking of data collection strategies. More positively, finding such effects might also provide insight into the cognitive processes underlying broader economic behavior.

In this paper we test for survey effects in a central domain of economic research: household saving behavior. In particular, we ask whether being asked apparently uninformative questions about retirement income needs leads to changes in household saving behavior. Recent work in behavioral finance suggests possible mechanisms for survey effects in saving behavior. Limited attention means that individuals tend to overlook some of the consequences of their decisions (DellaVigna, 2009). If those unnoticed consequences materialize in the future, as do the benefits of saving today, this results in biases that are similar to those induced by limited self-control (Karlan et al., 2012). However, in contrast to self-control problems, limited attention suggests that behavior can be corrected by focusing individuals' attention on the aspects they are missing. For instance, in a developing country context Karlan et al. (2012) show that reminders are an effective means to increase saving among subjects enrolled in a goal-specific saving program. To the extent that surveys on retirement planning can direct participants' attention, they may have similar effects even though it is not a priori clear whether this leads to an increase or a decrease in saving.

Our research design has two critical features. First, the randomized allocation of members of an internet panel to survey modules provides for clean identification of the causal effect of a module of questions. Second, we measure household saving with linked administrative data – not with the survey. This allows us to distinguish changes in economic behavior from changes in reporting behavior. In an environment in which public and occupational pension schemes implied high income replacement rates in retirement (the Netherlands in 2008), we find robust evidence that exposure to the retirement needs survey module subsequently led to *lower* household saving.

The survey methods literature has long been concerned with “panel conditioning”: the way in which experience in the panel survey affects subjects' responses. Several studies have examined panel conditioning in domains such as subjective well-being (Van Landeghem, 2012); marital satisfaction (Glenn, 1998) and a number of other areas. However, as pointed out by Das et al. (2011), disentangling panel conditioning from unobserved factors influencing attrition is a complex task. Das et al. (2011) conclude that, after controlling for unobserved attrition factors, there are significant panel conditioning effects in knowledge questions but not in other types of questions. In a recent survey, Warren and Halpern-Manners (2012) note that the survey research literature on panel conditioning generally failed to employ experimental or quasi-experimental methods. Also, they point out that so far, there is little systematic evidence on panel conditioning in large-scale longitudinal social science surveys. Our paper address both these concerns.

The psychology and marketing literatures have documented a number of related behavioral phenomena. The “question-behavior” effect refers to the observation that asking respondents to predict future behavior results in more of the behavior under consideration (Dholakia, 2010). For example, Spangenberg (1997) shows that asking people to predict their workout behavior induces

them to visit the gym more often. Where the behavior in question is normatively desirable this is sometimes called the “self-prophecy” effect (Sherman, 1980), and where it is normatively ambiguous, the “mere measurement effect” (Morwitz et al., 1993). Several papers have also shown that surveying people about risky behaviors can increase the propensity to engage in those behaviors (Fitzsimmons and Moore, 2008; Fitzsimmons and Shiv, 2001; Dholakia, 2010). In addition, emerging evidence in the public health literature supports the use of self-recording of behavior as a behavioral change intervention (Michie et al., 2011a,b; Burke et al., 2011). The effects documented in these literatures connect questions about the intention to or likelihood of engaging in specific behaviors, or the recording of specific behaviors, to the future propensity to engage in those behaviors. The survey effects that we study are less direct. We investigate the effect of being surveyed about topics related to the behavior of interest, rather than direct questions about that behavior. The question module that is our treatment has no specific questions about saving behavior.

Our analysis is most closely related to studies of Zwane et al. (2011) and Stango and Zinman (2013). Like us they consider survey effects that result from being surveyed on a topic related to the behavior of interest, rather than from direct questions about that behavior, and they use administrative data to measure outcomes, in order to distinguish genuine changes in behavior from changes in reporting style.

Zwane et al. (2011) study the effect of randomly assigning subjects to extra survey monitoring in the context of five randomized controlled trials of interventions in developing countries. They find that survey monitoring significantly increase the probability of water treatment product usage and medical insurance usage. They do not find effects for micro-lending take-up or renewal.

Stango and Zinman (2013) study participants in a marketing research panel. Panel members are invited to complete periodic surveys, which vary in content. While participants self-select into individual surveys, they do so without knowledge of the content. Stango and Zinman (2013) show that participants that enter a survey with general questions about overdrafts and overdrafts fees are less likely to incur such fees in the same month, and in subsequent months. Their interpretation is that the surveys with overdraft questions act as “salience shocks” which help respondents avoid costs arising from limited attention.

Our analysis differs from Zwane et al. (2011) in its developed economy context and in finding effects in a domain of financial behavior. It differs from Stango and Zinman (2013) in having a randomized design, and in that the survey effects we study concern a central life-cycle choice: the level of saving. In addition, we can estimate treatment effects (not just intention to treat) and the sample we study is representative of a national population – for the Netherlands.

Our analysis is based on the Dutch LISS panel, a population-representative internet panel survey of households that has taken place in the Netherlands since 2007. Members of the LISS panel complete online surveys on a regular basis. These surveys collect data on range of financial, health, and social topics. Crucially for this study, respondents are randomly assigned to eligibility for some of the modules. The treatment we study is a module of questions on expected needs in

retirement and preferences for current versus retirement consumption. The module was fielded in January 2008 and it was the first randomized content module in the LISS Panel. The module neither provides any information on the Dutch pension system in general nor on the pension entitlements of respondents. Nor does it contain direct questions on saving behavior.

We measure household saving through linkage to records in the Dutch national tax record system between 2007 and 2009. This system records detailed information on assets and debt across different asset classes at the beginning of the calendar year. We are thus able to construct a very accurate measure of saving (changes in wealth) for 2008 and 2007: the years immediately after and before treatment. As noted above, an independent measure of saving is necessary to distinguish genuine behavioral change from changes in survey reporting style.

Our experiment occurred just prior to the onset of the financial crisis. At this time, the combination of the first and second pillars of the Dutch pension system (the state pension and occupational pensions) offered a very high ratio of expected retirement income to peak working-life income to most Dutch workers. Bovenberg and Meijdam (2001) report an average after-tax replacement rate of 80 per cent.

We report both intention-to-treat (ITT) and instrumental variables (IV) estimates of the treatment effect. The latter use the survey invitation (which was random) as an instrument for survey completion. Survey take-up is very high, so that there is little difference between ITT and IV estimates.

The IV estimates indicate that survey participation *lowered* mean savings rate by 3 percentage points. Quantile-IV estimates indicate that the effects of similar magnitude are present from across a wide range of the distribution. A falsification test on pre-survey savings behavior finds no effect.

Our results extend the evidence base for the importance of limited attention in household financial decision making (Karlan et al., 2012; Stango and Zinman, 2013). We show that survey questions can alter choices with respect to a key life-cycle variable, the overall level of saving. Our findings also demonstrate that salience shocks can shift behavior in different directions, depending on the context. While Karlan et al. (2012) find that reminders raise contributions to a goal-specific saving program in Bolivia, Peru and the Philippines, we find that exposure to a module of questions about retirement income needs lowered overall saving in the Netherlands. We also find quite different patterns of effect heterogeneity. While Stango and Zinman (2013) find larger effects of being surveyed among lower-educated subjects, we find the largest effects among the high-education group. We discuss this further below.

Our findings also present a significant challenge to survey designers and the collection of longitudinal data on saving behavior and retirement preparation. Measurement is not innocuous. More positively, this study demonstrates the value of randomized survey content in allowing for the exploration of such effects.

The rest of the paper is organized as follows. Section 2 outlines our research design. Section ?? introduces the LISS survey data and the linked administrative data on financial assets. In section

3, we present the results as well as a number of robustness checks and falsification tests. Section 4 concludes.

2 Research design and data

2.1 The LISS panel

The LISS panel is a representative random sample from the Dutch population that was initiated during the autumn of 2007. The LISS panel is administered by CentERdata, a survey research institute affiliated with Tilburg University, and follows close to 8,000 individuals from 5,000 households. Scherpenzeel (2011) provides details on the design and sample. Surveys are distributed over the internet every week. Though the Netherlands has a high rate of internet access (more than 80% of Dutch households are connected), CentERdata safeguards representativeness by providing selected households with an internet subscription and a simple computer when necessary.

All members of the LISS panel respond to core surveys on subjects such as demographics, income, and assets, on a regular basis. In addition, researchers have the possibility to design and field survey modules. Those customized modules are usually distributed only once and, to keep costs down, are often limited to a random subsample of the eligible sample. This distribution of modules to random subsets of the LISS panel generates exogenous variation in survey participation, which we exploit to estimate the effect of participation on household behavior.

2.2 Treatment and eligibility

We define treatment as participation in the survey module entitled “What is an adequate old age income?” This module was fielded in January 2008, and it was the first randomized module in the LISS panel. The results of this study were published in Binswanger and Schunk (2012) who also designed the module. It consists of around 60 items that concern desired expenditure levels in retirement, the tradeoff between current and future consumption, and risk attitudes with respect to income after retirement. Moreover, the module elicits how much respondents have thought about retirement and whether they would be willing to cut down on housing expenditures in retirement. Importantly, the module did neither provide respondents with any information about the Dutch system of retirement income provision in general nor respondents’ personal entitlements in particular. Thus, the randomized survey module did not provide respondents with an information shock. Since the survey did not include any questions on predicted or intended savings, the randomized survey module could neither induce a question-behavior effect; respondents were not asked to predict their own behavior.

Eligibility for the survey was limited to all LISS members that were 25 years or older, who had a net household income of at least 800 euro per month and who were either the head of the household or his/her partner (children or other household members were excluded from participation).

This lead to a total eligible sample of 5,435 individuals, 2,755 of which were selected at random and were offered the survey. The take-up rate among those that received the offer was 74% at the individual level and 79% at the household level (at which our analysis is performed).

2.3 Outcome measures

We investigate the effect of survey participation on household savings. Though the LISS data include elaborate biannual surveys on assets and debt, we prefer to use administrative wealth records for two reasons. First, we want to rule out the confounding influence of differences in reporting behavior between those who were and were not offered the retirement expenditures module. If we found an effect of survey participation on self-reported savings, one could argue that the survey changed reporting styles rather than behavior. Deriving our outcome measures from administrative data eliminates that concern. Second, there is the general concern about the quality of self-reported survey data on assets (Bound et al., 2001). Exploratory analysis reveals that the self-reports of wealth in the LISS data are no exception. Therefore, measurement error is an important reason to prefer tax-derived administrative records over self-reports when it comes to savings.

An important concern when using combined survey and administrative data is incomplete linkage. Respondent refusal to consent is one reason for incomplete linkage. In our study, a second reason is panel attrition. Informed consent for the match of LISS data with administrative records was elicited only in September of 2011, almost four years after the survey module was fielded. Out of the 3,125 households that contain at least 1 member that was eligible for the retirement expenditures module, we could match only 1,602. De Bresser and Knoef (2013) show that this loss of data is mostly due to panel attrition rather than refusals: only 10% of the respondents to the retirement expenditures module that were still in the LISS panel in 2011 did not provide consent to the administrative data linkage.

The administrative assets data are taken from the Complete Asset Data of the Netherlands dataset (Integraal Vermogensbestand, CAD), which was constructed by Statistics Netherlands. The CAD contains a detailed decomposition of household-level wealth for the entire Dutch population. The categories of assets that we observe are checking and saving accounts, bonds, stocks, property, other real estate, business capital and other tangibles. For debt the CAD distinguishes between mortgage and other debt. It measures assets on the first of January for the years 2007, 2008 and 2009 (data for more recent years are not yet released at the time of writing). Available records thus allow us to compute yearly savings during 2007 and 2008 as the differences between wealth stocks in consecutive years. We compute these wealth stocks net of the value of the primary residence, because we want to focus on pure savings and housing has an important consumption component.

The CAD is based on tax records, which are supplemented with information from banks. Though the records provide a measure of assets that is likely to be more accurate than survey

data, the fact that they are mostly derived from taxes means they are not complete. We miss savings held in small accounts, because banks are not obliged to report accounts with a balance less than 500 euro or 15 euros in interest payments. We also do not observe debt for households without capital income, which means that we miss most short-term debt. Finally, we miss savings held in tax-exempt private retirement (“third pillar”) pensions; we discuss the four pillars of the Dutch pension system below. Such accounts are taxed only during the payout phase and are therefore invisible in tax records up to retirement. Given that the treatment-survey concerns expenditures after retirement, one may expect the effect of participation on savings to be particularly strong for those savings vehicles aimed at generating additional income after retirement. We cover these third pillar accounts by means of specific items from the LISS assets module that ask for ownership and balances of such accounts. Because of the high likelihood of substantial measurement error, we do not add the self-reported private pensions to the tax records. Instead, we analyze them separately and report the findings below.

In our analysis of savings we look both at *levels*, in euro per year, and *rates*, which are levels divided by yearly disposable income. The data on the yearly disposable income of households are also taken from tax records. We use the Complete Household Income Data of the Netherlands-dataset (Integraal Huishoudens Inkomstenbestand, CHID), assembled by Statistics Netherlands. The measure for primary income in the CHID is quite complete: in addition to labor income it includes income from entrepreneurship and from assets (interest payments and imputed rent for homeowners). Disposable income is defined as primary income plus government transfers that the household received minus the transfers and taxes paid by the household. The administrative income measure that we use is likely to be more accurate than survey measures of income, since information about the various income streams is provided electronically by employers and financial institutions to the tax authority.

2.4 Institutional context

The Dutch system of income provision during retirement is easily understood in terms of four categories or “pillars”. The first pillar is that of the public pension, which provides everybody who lived in the Netherlands between the ages of 15 and 65 with a subsistence income. Coverage of the public pension is close to universal, since uninterrupted residence in the country is the only criterion (benefits are cut by two percent for each year spent abroad).¹ The level of the public pension is set in reference to the minimum wage. Since public pensions only provide a minimum income, almost all employees accumulate additional entitlements in occupational pensions (the second pillar). Such arrangements cover 90% of all employees and are usually organized at the level of the sector or of the company (Bovenberg and Meijdam, 2001). Participation in the first two pillars

¹Technically, one is covered by the public pension if one’s income is subject to Dutch income taxes. Residence abroad does not affect the accumulation of entitlements as long as your income is taxed within the Dutch system.

is mandatory and together they replace 70 percent of gross last earned income on average, which translates to replacement rates net of taxes above 80 percent (Kapteyn and De Vos, 2008; Bovenberg and Meijdam, 2001). The third pillar contains all private savings vehicles that are aimed specifically at retirement, such as life annuities. Such voluntary arrangements are especially important for individuals that cannot rely on occupational pensions, such as the self employed. Finally, all other forms of wealth that can be drawn down to generate additional income after retirement, such as savings accounts, investments and real estate, make up the fourth pillar.

It is important to stress that the Dutch pension system in 2008 was characterized by arrangements that were almost universal and provided extremely generous income replacement. In this institutional environment it is not surprising that the first two pillars, public old age pensions and occupational pensions, together provided 95% of income in retirement (Kapteyn and De Vos, 2008). The final 5% was accounted for by private pension products and other savings.

Another important aspect of the environment is that starting in 2008, individuals can find detailed information on their own pension entitlement in occupational and private funds in their Uniform Pension Overview (UPO). These UPOs provide all members of pension funds, both in the second and third pillar, with yearly updates on their current entitlements and projected future entitlements at age 65. UPOs are mandatory for all financial institutions in the Netherlands since January 1st of 2008.

2.5 Threats to validity

Our analysis faces two threats to internal validity. The first problem is the issue of incomplete compliance with the treatment: not everybody who was offered the survey responded. We apply two remedies. First, we do an intention-to-treat (ITT) analysis that compares those who did receive the offer with those who did not (instead of comparing those who were treated with those who were not). Second, we perform instrumental variables (IV) analyses in which we use the random offer of treatment as an instrument for being treated. Both methods allow us to obtain estimates of treatment effects that are not affected by endogenous sample selection as a result of non-response, since they rely on exogenous variation in the survey-offer. In addition to IV regressions for the conditional mean of the savings distribution, we also estimate unconditional decile treatment effects in order to establish the robustness of our results. We use the estimator proposed in Frölich and Melly (2013).² The research design we use is characterized by one-sided non-compliance: Those respondents who were not randomly selected could not complete the module. Thus, the monotonicity requirement for the identification of local average treatment effects (Angrist et al., 1996) is satisfied.

The second threat to internal validity is that of selection into outcome measurement due to the substantial loss of observations when we match LISS observations with administrative records. As

²The Stata code we used for implementing this estimator is discussed in Frölich and Melly, 2010.

mentioned above, we could only link 1,602 out of 3,125 eligible households in the LISS panel to administrative records because of attrition in the period between the survey (January 2008) and the match (September 2011). Therefore, it is important that we verify that sample selection is not related to the offer of the retirement expenditures survey. We check this by testing for mean independence of sample selection with respect to the instrument.

2.6 Matching LISS and administrative data

Our basic unit of analysis is the household since we measure both wealth and income at the household level. We classify a household as being offered the survey if at least one household member that was eligible for the survey received the offer of participation. Likewise, we classify all households as treated in which at least one member that was offered the survey actually filled it out. The construction of our estimation sample starts with 3,125 households that contain at least one member that was eligible for the retirement expenditures survey according to the criteria mentioned in section 2.2. After matching the LISS respondents to administrative data, we obtain wealth records for 1,429, 1,437 and 1,449 households in the years 2007–2009 respectively. We drop those households for which all eligible members were retired in 2008, reducing the sample to 1,275 households. Even with accurately measured administrative data, there can be large outliers in ratio variables. We trim all households for which 2008 savings rates relative to after-tax household income were larger than 50% in absolute value, leaving us with an estimation sample of 999 households. We also tried trimming the sample at savings rates equal to 75% and 100% of net household income and found that our results are quantitatively similar. Estimates are available on request.

Appendix A presents descriptive statistics for the full sample and for the estimation sample, separately for couples and singles. The full sample consists of 2,816 rather than 3,125 households, because we exclude households for which all eligible members were retired in 2008. For couples, defined as households in which two partners live together irrespective of their marital status, individual-specific attributes are reported for the head of the household. Appendix A allows one to compare the characteristics of all eligible LISS households with those of the final estimation sample. Both for couples and singles, these samples are found to be similar.

One difference between the full sample and the estimation sample that is not in Appendix A is that of compliance to the survey offer. As mentioned in Section 2.2, 74% of the individuals in the full sample who were offered the survey participated. Household-level compliance is around 5 percentage points higher: among those households for which at least one eligible member was offered the survey, at least one member filled it out in 79% of the cases. In the estimation sample, the corresponding compliance rates are 82% for individuals and 87% for households, which is 8 percentage points higher than in the full sample. It is not surprising that compliance is related to being observed in our final dataset, since non-compliers to the survey offer are more likely to leave the LISS panel altogether. As a result, non-compliers were less likely to give their permission for

the match to administrative records and are lost from our estimation sample. However, this does not compromise our research design, so long as the instrument is orthogonal to this selection process. In Section 3.1, we show that the instrument is not correlated with selection into the estimation sample. Hence, the comparison of households based on the random survey offer is as valid there as it would be in the full sample.

2.7 Descriptive statistics

Appendix B describes our administrative assets records for the years 2007, 2008 and 2009 (all in 2008 euros). The single most important category of assets is that of the primary residence, with an average value of around 200,000 euro. Savings accounts follow at great distance as the second most important type both in terms of mean (27,000 euro) and median (13,000 euro) value. Real estate other than the primary residence is also important, but only for a small minority: the mean value is around 7,000 euro though only 8% of the sample has any non-residential real estate. The mean value of risky assets, stocks and bonds, drops from 7,210 euro in 2007 to 4,857 euro in 2009 (median holdings are zero in all years). Business wealth and other wealth are the least important categories of assets with a mean value below 1,500 euro in all years.

On average, households have about 105,000-110,000 euro in mortgage debt and around 2,000-2,500 euro of non-mortgage debt. Non-mortgage debt is concentrated in a small minority of 6% of the sample, among which the mean non-mortgage debt is around 20,000 euro.

Taking assets and debt together, the mean net worth of the households in the sample is around 135,000 euro. Unsurprisingly, net worth is concentrated in the primary residence, which has a mean value net of mortgage of around 95,000 euro. Because of the consumption value of housing, we compute savings based on the remaining 40,000 euro of non-housing savings.

Table 1 presents summary statistics for net income and for the outcome variables (savings and saving rates) used in the analysis. Mean household income is 38,165 euro in 2008 and the median is 35,699 euro, both of which are slightly higher than the average of 33,100 euro for the Dutch population at large (Centraal Bureau voor de Statistiek (CBS), 2012). 2008 non-housing savings, computed as the difference between the non-housing wealth stocks of January 1st 2008 and 2009, has a sample mean of 154 euro and a median of 2 euro, showing that the distribution of savings is centered around zero. There is, however, considerable variation in savings: the standard deviation is 9,411 euro. We compute savings rates as the level of savings divided by after-tax income. The distribution of savings rates is centered around zero, but there is considerable variation: the standard deviation of the savings rate is 19 percentage points.

[table 1 about here]

3 Results

3.1 Validity and relevance of the instrument

As explained in section 2.6, we lose about half of our sample when we match LISS records with administrative data. This loss of data would compromise the internal validity of our empirical strategy if attrition from the LISS panel were related to the instrument, the offer of the retirement expenditures survey. Table 2 shows estimates of a linear model that uses our instrument, called “offer”, to explain an indicator of inclusion in the estimation sample. We find that sample selection is not correlated with the offer of the retirement expenditures module, so the loss of data that associated with matching survey participants to administrative data does not invalidate our instrument.

[table 2 about here]

In the regression in Table 2 and in all other models reported below we control for the presence of multiple household members. This is necessary because the randomization of the offer was done at the level of the individual while the outcome variables we analyze are measured at the household level. We classify a household as being offered the survey if at least one eligible member received the offer, so by construction households with multiple eligible members are more likely to receive the offer. Conditional on the number of eligible household members, however, randomization across individuals ensures that the offer is random at the household level. We checked whether the randomization was successful by regressing our instrument, an indicator for being offered the survey, on all socio-demographic variables listed in Appendix A (controlling for the presence of multiple household members). The covariates are jointly insignificant ($p = 0.901$). Hence, the covariates are balanced, as one would expect given the randomization.

Turning to instrument relevance, the first-stage regression shows that the instrument is highly significant: the F -statistic for the coefficient of the instrument in a model that controls for the presence of multiple eligibles is 4,818.37. Complete estimation results for the first stage are given in Table 3.

[table 3 about here]

3.2 Effects of survey participation on household saving

Table 4 presents ITT estimates of the effect of survey participation on household saving, both for the mean and at various quantiles. The top panel uses 2008 non-housing savings as outcome measure, while the bottom panel explains the 2008 savings rate (non-housing savings divided by household income). In the mean regression, we find a 3 percentage points reduction in the savings rate, or about 1500 euros of 2008 non-housing savings. The estimated saving rates effects at various deciles are of the same order of magnitude as the mean effect, between 2 and 5 percentage points.

[table 4 about here]

Table 5 presents our main results from IV regression of participation in the retirement expenditures modules on the two savings measures, levels and rates. The leftmost column shows the estimated coefficients and accompanying standard errors for the treatment dummy in 2SLS models where we instrument survey participation with the random offer of the survey. Participation in the survey caused households to save 1,683 euro less on average during 2008. This is a large effect, especially considering the sample average of 154 euro and the standard deviation of 9,411. When we express savings relative to household income, we also find a significant and negative effect. Survey participation caused households to save 3.5 percentage points less on average, compared with a sample average of -1% and standard deviation of 19 percentage points.

[table 5 about here]

The remaining columns report quantile IV effects. We report estimates for the second up to the eighth decile in the remaining columns of Table 5. For the level of savings, we find significant and large effects for the third, sixth and seventh deciles. The estimated coefficients for the other deciles are also all negative. For the non-housing savings rate, we find strongly significant effects at the third and sixth decile as well as marginally significant effects at the second and fourth deciles. These estimates show that large parts of the savings distribution were shifted by the survey, with similar effect sizes below and above the median. Alternative trimming rules yield quantitatively similar results, as did narrower definitions of wealth that include only risky assets and bank accounts.³

Our identification is based on the randomized offer of the retirement expenditures module to a subset of the eligible panel members. Randomization allows us to cleanly measure the causal effect of interest. Nevertheless, we added to the models reported in Table 5 all the covariates listed in Appendix A and found that all effects are robust to including these additional controls.⁴

3.3 Falsification tests

As a falsification test, we run the same models on 2007 savings, that is on behavior realized before exposure to the retirement expenditures module. The results are shown in Table 6. We find no evidence of any systematic differences in savings behavior, neither in terms of the average level of savings nor any of the deciles. Note that, in contrast to Table 5, the coefficients of the various deciles are not even of the same sign.

[table 6 about here]

³Estimates available on request. Assets held in bank accounts and risky investments are provided directly by banks to the tax authority and as a result they are probably measured most accurately. Therefore, we also tried yearly savings in those categories as alternative outcome variables.

⁴Estimates available on request.

3.4 Effect heterogeneity

So far we have shown that participation in the retirement expenditures survey reduced average household non-housing saving, both in absolute terms and relative to household income. Next we investigate effect heterogeneity. One approach would be to run IV analyses on subsamples, but many variables that could be used for interesting splits of the sample are correlated. Examples are income and education and income and age. Therefore, we prefer a regression approach, where we regress savings on an indicator equal to one if a household was offered the survey (our instrument in the preceding analysis); all covariates from Appendix A; and interaction terms of covariates with the offer. Hence, we can interpret the results in this section as (heterogeneous) intention-to-treat effects. We investigate heterogeneity along the lines of income, education and age. Note that, for reasons of sample size, the specification does not contain dummies for all cells defined by those variables, but only interactions of the separate variables with the offer indicator. The upper panel of Table 7 displays coefficient estimates for the main effect and interaction terms of the model with the savings *rate* as the dependent variable. According to these estimates, the offer of the survey did not affect the average savings rate of young, income-poor households that are poorly educated. We find strong evidence for differential effects along the lines of age and education: households with more highly educated or older heads reduced their savings more after filling out the survey.

The lower panel of Table 7 shows the differences in savings between offered and non-offered households for subsamples defined along age, education and income categories; these estimates are functions of the estimates parameter values reported in the top panel. Offered households with poorly educated heads who are younger than 40 and have a disposable income above the sample median actually saved close to 7 percentage points more than non-offered households with the same education and age. Households in the highest education category saved less regardless of the age of the head and their household income, but we find the strongest effects for older households: the intention-to-treat-effect is -3/-7 percentage points for households below age 40; -5/-10 percentage points for age 40-54; and -10/-15 for households aged 55 or older.

[table 7 about here]

We find similar heterogeneity in the survey effect on the *level* of savings; these are available on request.

Table 7 shows that the size of the effect is much large for the highly educated (college and university graduates). Indeed the coefficients imply that there is no statistically significant effect for low and middle education levels, while point estimates for the high education level is 8 percentage points or more, and very statistically significant.

The higher education group have higher rates of home ownership. They also have higher pension wealth both in terms of a standardized annuity and in terms in terms of expected replacement rate of current income. However, neither home ownership nor either measure of pension wealth

significantly interacts with treatment effect. This suggests that differences in financial circumstances do not drive the heterogeneity in treatment effects across education groups.

An alternative hypothesis is that the education groups differ in the way the survey affects attention to retirement saving and needs. However there is no significant difference in their reported rates of thinking about retirement prior to the survey. The high education group answer the survey quickly than the low education group (median difference 2 minutes, $p = 0.058$), but the difference between the high education and middle income groups is not statistically significant.

A question in the retirement needs module elicited subjects expectations of housing costs in retirement. Interestingly, relative to low education households, high educations are more likely to expect a decrease in housing costs after retirement and less like to expect increase. These differences are economically and statistical significant ($p < 0.001$). Unfortunately, as this question only asked of the treatment, we cannot test for treatment effect heterogeneity in this specific dimension.

In addition to the effect heterogeneity reported in Table 7, we also checked whether the intention-to-treat effect of the survey offer differs depending on whether the individual(-s) who received the offer is a husband; a wife; both husband and wife; a single male; or a single female. However, we find no evidence for different intention-to-treat effects depending on whether men or women were offered the survey. Moreover, we investigated whether the intensity of the effect was different for respondents that took longer than the median time to answer the questions, but found no evidence to suggest that completion time affected the effect size. Estimates are available on request.

3.5 Evidence on tax-favored savings accounts

As explained in section 2.3, we do not observe investments in tax-favored savings accounts in the administrative data. One possible interpretation of our findings so far is that the negative effect of survey participation on non-housing savings results from a re-allocation of assets to those accounts. We used survey data from the LISS assets module to look at investments in private pensions, specifically for effects of participation in the module on ownership, changes in ownership, balance conditional on ownership, and unconditional balances. We carried out the analysis at the level of the individual and of the household. However, we do not find any evidence for a change in savings in voluntary pension accounts.⁵ Unfortunately, we cannot rule out the possibility of measurement error obscuring an actual effect.

4 Conclusion

In this paper we show that participating in a non-informative survey on expenditures after retirement led Dutch households to save significantly less. Our analysis uses administrative wealth data to calculate clean measures of savings that are not contaminated by the reporting styles of survey

⁵Estimates available on request.

respondents. Participation in the survey is instrumented by randomized assignment to treatment conditions, so our estimates are unaffected by endogenous compliance. Estimated effects are large: the survey caused households to save 1,700 euro, or 3.5 percentage points relative to disposable income, less during 2008 (sample means: 154 euro and -1%). These effects are robust to various trimming rules and decile IV models show that they occur on both sides of the median of the distribution of savings. Furthermore, falsification checks reveal no effect on savings prior to the survey, supporting the validity of our identification strategy. We find evidence for heterogeneous effects, with the strongest impact among highly educated and older households.

The finding that participation in a survey about retirement expenditures reduced savings may seem counterintuitive. One might expect that such survey induces respondents to think about their consumption needs during retirement, which would underline the importance of building a nest egg. However, we believe that the finding makes sense in the context of the Dutch pension system. Remember that in the Netherlands 95% of the entitlement to income during retirement is accumulated in public and occupational pension accounts. Savings in those accounts are mandatory for those who are covered and together they provide generous income replacement relative to the final earned wage. Hence, it is reasonable that respondents give the issue more thought and conclude that there is really no need for the accumulation of additional resources to finance their desired expenditures. De Bresser and Knoef (2013) show that around over 60% of the Dutch accumulate enough resources in non-voluntary pensions alone to meet their minimal level of expenditures (the median household exceeds their consumption floor by 23%). In such an institutional context it is not surprising that thinking more about one's consumption needs later in life causes a decline in savings.

The decline in savings after participating in a survey on consumption during retirement makes sense in the particular institutional context of the Netherlands in 2008, which was characterized by mandatory pensions that were generous, replacing 70% of final income on average, and covered nearly the entire population. Furthermore, De Bresser and Knoef (2013) show that older and more educated households can look forward to higher (occupational) pensions, which fits with the effect heterogeneity we document.

In terms of the mechanism behind these survey effects, limited attention seems to provide a coherent interpretation for our results. The heterogeneity in treatment effects we document is consistent with the survey acting as an attention shock. After being reminded of the tradeoff between current and future consumption, most individuals conclude that their pensions alone will provide them with an adequate income in retirement, resulting in a negative effect on average non-pension savings. However, some groups, namely the young and poorly educated, foresee that they may not be so lucky and do not cut back on savings (or even save a little more). In that sense, our results are comparable to those of Stango and Zinman (2013), who show that financial behavior can be influenced even by questions that do not directly concern that behavior.

Though we do not expect to find similar effects in different institutional contexts, the general

point of household financial behavior being affected by participation in household surveys is of considerable importance to empirical economists. It may lead panels that are representative in terms of demographics to behave in non-representative ways. As a result the external validity of any study based on that data would be compromised. If future research confirms that financial decisions of households are susceptible to survey effects, that would be an important reason for researchers to prefer administrative data whenever available. Also, as noted by Zwane et al. (2011), the presence of survey effects suggests that large panels that participate in surveys infrequently may be a better way to get statistical power than small samples that fill out questionnaires on a weekly basis.

Our use of a randomized survey module to identify a causal effect highlights the potential of household panels as laboratories for economic experiments. Randomized questionnaires can be used to investigate how individuals update their expectations and how financial education affects perceptions and behavior. Researchers can use those surveys to identify causal effects of interventions and test economic theories.

One limitation of the present study is the fact that our wealth data only allow us to compute savings during 2007 and 2008. Therefore, we do not know how long-lived the survey effect is. Naturally we will investigate the durability of the effects when more data becomes available. Documenting the range of behaviors that are affected by surveys and the subpopulations that are most sensitive are fruitful areas for future research. Our evidence suggests that economists should follow psychologists and other social scientists and acknowledge the relevance of survey effects, even if that means that our favorite method of data collection is simultaneously more versatile and less innocent than we would like to believe.

References

- Angrist, J., Imbens, G., and Rubin, D. (1996). Identification of causal effects using instrumental variables. *Journal of the American Statistical Association*, 91(434):444–455.
- Binswanger, J. and Schunk, D. (2012). What is an adequate standard of living during retirement? *Journal of Pension Economics and Finance*, 11(2):203–222.
- Bound, J., Brown, C., and Mathiowetz, N. (2001). Measurement error in survey data. In Heckman, J. and Leamer, E., editors, *Handbook of Econometrics*, volume 5, pages 3705–3843. Elsevier, North-Holland, Amsterdam.
- Bovenberg, A. and Meijdam, L. (2001). The Dutch pension system. In Borsch-Supan, A. H. and Miegel, M., editors, *Pension Reform in Six Countries*. Springer, New York.
- Burke, L. E., Wang, J., and Sevvick, M. A. (2011). Self-monitoring in weight loss: a systematic review of the literature. *Journal of the American Dietetic Association*, 111(1):92–102.

- Centraal Bureau voor de Statistiek (CBS), D. (2012). Welvaart in nederland - inkomen, vermogen en bestedingen van huishoudens en personen.
- Das, M., Toepoel, V., and Van Soest, A. (2011). Nonparametric tests of panel conditioning and attrition bias in panel surveys. *Sociological Methods & Research*, 40(1):32–56.
- De Bresser, J. and Knoef, M. (2013). Can the Dutch meet their own retirement expenditure goals? Mimeo, Tilburg University.
- DellaVigna, S. (2009). Psychology and economics: evidence from the field. *Journal of Economic Literature*, 47(2):315–372.
- Dholakia, U. (2010). A critical review of question-behavior effect research. *Review of Marketing Research*, 7(7):145–197.
- Fitzsimmons, G. and Moore, S. (2008). Should we ask our children about sex, drugs and rock & roll? potentially harmful effects of asking questions about risky behaviors. *Journal of Consumer Psychology*, 18(2):82–95.
- Fitzsimmons, G. and Shiv, B. (2001). Nonconscious and contaminative effects of hypothetical questions on subsequent decision making. *Journal of Consumer Research*, 28(2):224–238.
- Frölich, M. and Melly, B. (2010). Estimation of quantile treatment effects with stata. *The Stata Journal*, 10(3):423–457.
- Frölich, M. and Melly, B. (2013). Unconditional quantile treatment effects under endogeneity. *Journal of Business and Economic Statistics*, just-accepted.
- Glenn, N. (1998). The course of marital success and failure in five American 10-year marriage cohorts. *Journal of Marriage and Family*, 60(3):569–576.
- Kapteyn, A. and De Vos, K. (2008). Social security and retirement in the Netherlands. In Gruber, J. and Wise, D. A., editors, *Social Security and Retirement Around the World*, pages 269–304. University of Chicago Press.
- Karlan, D., McConnell, M., Mullainathan, S., and Zinman, J. (2012). Getting to the top of mind: How reminders increase saving. No. w16205. National Bureau of Economic Research.
- Michie, S., Churchill, S., and West, R. (2011a). Identifying evidence-based competences required to deliver behavioural support for smoking cessation. *Annals of Behavioral Medicine*, 41(1):59–70.
- Michie, S., Hyder, N., Walia, A., and West, R. (2011b). Development of a taxonomy of behaviour change techniques used in individual behavioural support for smoking cessation. *Addictive Behaviors*, 36(4):315–319.

- Morwitz, V. G., Johnson, E., and Schmittlein, D. (1993). Does measuring intent change behavior? *Journal of Consumer Research*, 20(1):46–61.
- Scherpenzeel, A. (2011). Data collection in a probability-based internet panel: how the LISS panel was built and how it can be used. *Bulletin of Sociological Methodology*, 109(1):56–61.
- Sherman, S. (1980). On the self-erasing nature of errors of prediction. *Journal of Personality and Social Psychology*, 39(2):211–222.
- Spangenberg, E. (1997). Increasing health club attendance through self-prophecy. *Marketing Letters*, 8(1):23–31.
- Stango, V. and Zinman, J. (2013). Limited and varying consumer attention: Evidence from shocks to the salience of bank overdraft fees. Forthcoming in *Review of Financial Studies*.
- Van Landeghem, B. (2012). Panel conditioning and self-reported satisfaction: evidence from international panel data and repeated cross-sections. SOEPpapers on Multidisciplinary Panel Data Research.
- Zwane, A., Zinman, J., Dusen, E. V., Pariente, W., Null, C., Miguel, E., Kremer, M., Karlan, D., Hornbeck, R., Gine, X., Duflo, E., Devoto, F., Crepon, B., and Banerjee, A. (2011). Being surveyed can change later behavior and related parameter estimates. *Proceedings of the National Academy of Sciences*, 108(1):1821–1826.

Tables

Table 1: Descriptive statistics of outcomes

	Mean	Std. dev.	Percentiles				
			0.05	0.25	0.5	0.75	0.95
HH income ^a	38,165	17,649	16,289	27,056	35,699	46,107	67,474
Non-housing savings ^b							
Levels (2008 euros)	154	9,411	-13,632	-3,221	2	3,084	14,860
Savings rates	-0.01	0.19	-0.40	-0.09	0.00	0.09	0.33
N			999				

^a HH income net of taxes.

^b Savings corrected for inflation and net of property value and mortgages.

Table 2: Exogeneity of the instrument w.r.t. data linkage

Dependent variable: indicator for estimation sample	
Offered	-0.0209 (0.0201)
Multiple eligibles	0.00700 (0.0215)
Constant	0.364*** (0.0207)
Number of selected HHs	999
N	2,816

Robust standard errors in parentheses.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 3: First stage

Dependent variable: HH treated	
HH offered	0.879*** (0.0127)
Multiple eligibles	-0.0376** (0.0159)
Constant	0.0231** (0.00988)
R squared	0.688
F(1, n-(k+1))	4,818.37***
N	999

Robust standard errors in parentheses.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 4: The effect of survey participation on savings
(ITT effects)

		Deciles ^a						
	Mean	0.20	0.30	0.40	0.50	0.60	0.70	0.80
	Dependent variable: 2008 non-housing savings (thousands of euros)							
Offered	-1.478** (0.672)	-1.502 (1.011)	-0.956** (0.475)	-0.581 (0.373)	-0.499 (0.357)	-0.697* (0.382)	-0.968** (0.459)	-0.791 (0.685)
Sample statistics	0.154	-4.935	-2.061	-0.583	0.002	1.060	2.245	4.393
N				999				
	Dependent variable: 2008 non-housing savings rate (1 = 100%)							
Offered	-0.0308** (0.0127)	-0.0495* (0.0258)	-0.0256* (0.0148)	-0.0197* (0.0112)	-0.0113 (0.0107)	-0.0233** (0.0113)	-0.0243* (0.0131)	-0.0312 (0.0196)
Sample statistics	-0.01	-0.14	-0.07	-0.02	0.00	0.03	0.07	0.13
N				999				

^a For decile models we report unconditional treatment effects.

We control for the presence of multiple eligibles.

Standard errors in parentheses.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 5: The effect of survey participation on savings
(IV treatment effects)

		Deciles ^a						
	Mean	0.20	0.30	0.40	0.50	0.60	0.70	0.80
Dependent variable: 2008 non-housing savings (thousands of euros)								
Treated	-1.683** (0.764)	-1.792 (1.119)	-1.193** (0.552)	-0.644 (0.458)	-0.474 (0.438)	-0.955** (0.461)	-1.085** (0.530)	-0.784 (0.709)
Sample statistics	0.154	-4.935	-2.061	-0.583	0.002	1.060	2.245	4.393
Proportion compliers				0.875				
N				999				
Dependent variable: 2008 non-housing savings rate (1 = 100%)								
Treated	-0.0351** (0.0144)	-0.0519* (0.0286)	-0.0337** (0.0166)	-0.0224* (0.0135)	-0.00922 (0.0130)	-0.0352** (0.0141)	-0.0247 (0.0153)	-0.0317 (0.0208)
Sample statistics	-0.01	-0.14	-0.07	-0.02	0.00	0.03	0.07	0.13
Proportion compliers				0.875				
N				999				

^a For decile models we report unconditional treatment effects.
We control for the presence of multiple eligibles.
Standard errors in parentheses.
*significant at 10%; **significant at 5%; ***significant at 1%

Table 6: Falsification tests
(IV treatment effects)

		Deciles ^a						
	Mean	0.20	0.30	0.40	0.50	0.60	0.70	0.80
Dependent variable: 2007 non-housing savings (thousands of euros)								
Treated	-0.406 (0.749)	-0.582 (0.889)	0.130 (0.460)	0.158 (0.414)	-0.090 (0.417)	-0.393 (0.478)	-0.684 (0.832)	-1.792 (1.251)
Proportion compliers				0.866				
N				1,014				
Dependent variable: 2007 non-housing savings rate (1 = 100%)								
Treated	-0.0136 (0.0147)	-0.00961 (0.0249)	-0.00279 (0.0133)	0.00197 (0.0121)	-0.00198 (0.0122)	-0.00296 (0.0136)	-0.0211 (0.0218)	-0.0257 (0.0298)
Proportion compliers				0.866				
N				1,014				

^a For decile models we report unconditional treatment effects.
We control for the presence of multiple eligibles.
Standard errors in parentheses.
*significant at 10%; **significant at 5%; ***significant at 1%

Table 7: Heterogeneous intention-to-treat effects – savings rate

Dependent variable: 2008 savings rate (1 = 100%)						
Offered	0.0213 (0.0300)					
Offered × HH inc. high	0.0482* (0.0268)					
Offered × educ. middle	-0.0150 (0.0319)					
Offered × educ. high	-0.100*** (0.0319)					
Offered × age 40-54	-0.0200 (0.0294)					
Offered × age 55+	-0.0706** (0.0330)					
Controls	Yes					
R-squared	0.0574					
N	999					
	Heterogeneous effects					
	Income below median			Income above median		
	Education			Education		
	Low	Middle	High	Low	Middle	High
Age <40	0.0213 (0.0300)	0.00626 (0.0260)	-0.0789*** (0.0290)	0.0695* (0.0377)	0.0545* (0.0304)	-0.0307 (0.0298)
Age 40-54	0.00129 (0.0300)	-0.0138 (0.0308)	-0.0989*** (0.0330)	0.0495 (0.0319)	0.0344 (0.0280)	-0.0507* (0.0270)
Age 55+	-0.0493* (0.0280)	-0.0643* (0.0348)	-0.150*** (0.0342)	-0.00111 (0.0349)	-0.0162 (0.0371)	-0.101*** (0.0336)

Standard errors in parentheses.

*significant at 10%; **significant at 5%; ***significant at 1%

Appendix A Descriptive Statistics

Table 8: Descriptive statistics

	Couples				Singles			
	Full sample (LISS)		Estimation sample		Full sample (LISS)		Estimation sample	
	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)
Female	0.12	(0.32)	0.11	(0.32)	0.58	(0.49)	0.62	(0.49)
Age	47.4	(11.8)	46.9	(11.5)	45.4	(12.1)	44.6	(11.6)
Children	1.16	(1.15)	1.15	(1.16)	0.41	(0.81)	0.36	(0.78)
Homeowner	0.83	(0.38)	0.81	(0.40)	0.49	(0.50)	0.42	(0.50)
Marital status								
Married	0.81	(0.39)	0.83	(0.38)	0.06	(0.23)	0.04	(0.19)
Separated/divorced	0.05	(0.23)	0.04	(0.20)	0.33	(0.47)	0.38	(0.49)
Widowed	0.002	(0.04)	0.001	(0.04)	0.10	(0.30)	0.07	(0.25)
Never married	0.13	(0.34)	0.11	(0.32)	0.51	(0.50)	0.52	(0.50)
Education								
Primary	0.08	(0.28)	0.08	(0.28)	0.09	(0.28)	0.09	(0.29)
Int. Secondary	0.23	(0.42)	0.25	(0.43)	0.24	(0.43)	0.26	(0.44)
Higher secondary	0.07	(0.26)	0.08	(0.28)	0.08	(0.27)	0.07	(0.25)
Int. vocational	0.25	(0.43)	0.25	(0.43)	0.23	(0.42)	0.24	(0.43)
Higher vocational	0.25	(0.43)	0.25	(0.43)	0.25	(0.43)	0.28	(0.45)
University	0.11	(0.32)	0.09	(0.29)	0.10	(0.30)	0.06	(0.25)
Most imporant activity								
Employed	0.72	(0.45)	0.75	(0.43)	0.69	(0.46)	0.70	(0.46)
Self employed	0.11	(0.31)	0.07	(0.26)	0.09	(0.29)	0.08	(0.28)
HH work	0.01	(0.11)	0.01	(0.11)	0.05	(0.21)	0.06	(0.23)
Retired	0.11	(0.31)	0.11	(0.32)	0.00	(0.00)	0.00	(0.00)
Disabled	0.03	(0.16)	0.03	(0.17)	0.07	(0.26)	0.07	(0.25)
Other	0.03	(0.16)	0.02	(0.15)	0.10	(0.30)	0.09	(0.29)
N	2167 (77.0%)		768 (76.9%)		649 (23.0%)		231 (23.1%)	

For couples all individual-specific variables refer to the head of the household.

Appendix B Descriptives of Assets and Debt

Table 9: Descriptives of assets and debt

	2007			2008			2009		
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
Assets									
Saving accounts	25,551	12,799	40,870	26,728	12,728	42,568	28,008	13,196	44,963
Risky assets	7,210	0	23,974	6,627	0	22,560	4,857	0	17,468
Property	196,713	205,571	162,020	201,325	212,589	161,463	199,616	212,181	155,063
Real estate	9,808	0	53,750	6,906	0	41,722	7,689	0	44,442
Business	1,202	0	12,588	1,212	0	13,842	1,459	0	15,485
Other	861	0	9,914	959	0	10,871	1,014	0	10,779
Debt									
Mortgage	105,119	86,091	105,553	104,079	84,827	103,787	108,243	91,412	106,892
Non-mortgage debt	2,375	0	13,796	1,992	0	12,621	2,432	0	18,823
Net worth	133,852	78,760	172,072	137,687	81,551	170,305	131,968	77,021	167,938
Net housing wealth	91,594	40,984	131,904	97,246	47,128	128,481	91,374	43,949	124,627
Net worth excl. housing	42,258	16,292	82,970	40,440	15,643	76,797	40,594	15,232	77,325
N		983			999			999	

All assets are reported in 2008 euros.