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Chapter Author(s): Ralph Koijen, Stijn Van Nieuwerburgh, Roine Vestman

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Judging the Quality of Survey Data by Comparison with "Truth" as Measured by Administrative Records
Evidence from Sweden

Ralph Koijen, Stijn Van Nieuwerburgh, and Roine Vestman

Having accurate measures of consumption is crucial for research on the optimality of household decision making, on consumption and saving behavior, on inequality, poverty, and standards of living, and for research on consumption-based asset pricing models. Our understanding of consumption behavior may well depend on how accurate the measurement of consumption really is.[1] Accurate consumption data are difficult to collect. In practice, it is infeasible to ask large numbers of households to keep track of their expenditures in great detail and over a long enough period of time. Consumption surveys instead use paper or phone interviews to ask stylized questions on spending in a few broad consumption good categories over a particular recall period. Other times, households are asked to keep track of

Ralph Koijen is professor of finance at London Business School and a faculty research fellow of the National Bureau of Economic Research. Stijn Van Nieuwerburgh is professor of finance at Stern School of Business, New York University, and a research associate of the National Bureau of Economic Research. Roine Vestman is assistant professor of economics at Stockholm University and a visiting researcher at the Institute for Financial Research (SIFR) in Stockholm.

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1. For example, there is debate on whether consumption inequality has gone up along with income inequality during the 1980s and 1990s, and therefore on the question of whether households' insurance opportunities have improved (Krueger and Perri 2006; Attanasio, Battistin, and Ichimura 2005; Aguiar and Bils 2011). The pattern observed in the data changes depending on the exact source of consumption data that is used.

recurrent expenditures, such as groceries, for a short period of time (a few weeks usually) in a diary. Sometimes, they are asked about large and infrequent purchases (e.g., consumer durables) over the past year in a separate interview in addition to the diary.[2]

An existing literature has found basic problems with survey-based measures of consumption, and this volume contributes to the analysis. In prior work, Ahmed, Brzozowski, and Crossley (2006) compare two measurements for the same set of households and find that recall food consumption data, which is the basis of a great deal of empirical work, suffers from considerable measurement error while diaries records are found to be more accurate. Other work has compared consumption measures across different surveys or across different waves of the same survey.[3] Measurement error is often found to be nonclassical (Bound, Brown, and Mathiowetz 2001; Pudney 2008). The measurement error in household-level consumption data, and the difficulty of estimating nonlinear models in the presence of such error, have led some to call for abandoning Euler equation estimation altogether (Carroll 2001). Bound, Brown, and Mathiowetz (2001) emphasize the usefulness of validation data in characterizing the joint distribution of error-ridden measures and their true values. It seems fair to conclude that the measurement errors are sufficiently severe to warrant exploration of alternatives.

In this chapter we develop such an alternative measure of consumption, which avoids many of the problems with standard survey-based data. The basic idea is to measure consumption as a residual from the household's budget constraint: consumption is the part of total income that was not invested. This approach imposes heavy data requirements on the measurement exercise because one needs comprehensive measures of income as well as comprehensive asset holdings and asset price data. While most countries currently do not have such data, Sweden (and a few other Scandinavian countries) collects that information as part of its tax registry. The tax registry data contain information on every stock, bond, mutual fund, and bank account each household owns at the end of the year. Housing registry data also keep track of homeownership and households' permanent address. Finally, the Swedish data also contains information on labor, transfer, and financial

2. In the United States, the Consumption Expenditure Survey (CEX) is the standard data set for consumption measurement, while the Panel Study for Income Dynamics (PSID) contains a measure of food consumption. Blundell, Pistaferri, and Preston (2008) and Guvenen and Smith (2010) impute total consumption in the PSID based on the relationship between food consumption and total consumption in the CEX. In the United Kingdom, the corresponding data sets are the Family Expenditure Survey, now called the Living Cost and Food Survey, and the British Household Panel Survey (BHPS) for food consumption. In Continental Europe, the Household Budget Surveys were recently harmonized across countries. A special issue of the Review of Economic Dynamics (January 2010) provides an excellent overview of consumption measurement in various countries.

3. See Battistin, Miniaci, and Weber (2003), Browning, Crossley, and Weber (2003), Battistin (2004), and Gibson (2002) among others.

income. The resulting series is a measure of total consumption (including durables) measured at annual frequency.[4] A final necessary condition for our exercise is that Sweden runs a standard Household Budget Survey and that we can *match* up the households in the survey to the registry data.

This setup allows us to compare registry-imputed and survey-based measures of consumption between 2003 and 2007 for thousands of households. Our first set of results study that comparison by homeownership status, age, income, and wealth. We are particularly interested in the question of whether surveys accurately measure consumption for the wealthy. To the extent that consumption of the wealthy is understated, the registry data would be useful to gauge the size of the bias. This seems relevant in light of the fact that most household budget surveys undersample the rich. Our registry-based approach does not suffer from this undersampling. We uncover discrepancies between registry- and survey-based consumption measures that increase with income and wealth. While the mean and median of the consumption distribution are similar, the survey understates the consumption of wealthy and high-income households, while slightly overstating consumption of the poorest quintile of households.

Second, we study how sensitive registry-based consumption is to an accurate imputation of returns that households are earning on their assets. The ability to calculate a household-specific portfolio return is unique to our chapter; the otherwise similar study with Danish data by Kreiner, Lassen, and Leth-Petersen (chapter 10, this volume) assumes a common, zero capital gains return. We find that incorrectly applying a broad total return measure to a household's financial asset holdings leads to substantial deviations from the properly imputed registry measure. These discrepancies are increasing in wealth. This finding is of independent interest to researchers who need to make assumptions on household portfolio returns because they lack the detailed security-level data available in Sweden (e.g., Maki and Palumbo 2001; Hurd and Rohwedder, chapter 14, this volume).

Third, we look at a subsample of households who purchased a car and find that a surprisingly large fraction of households fails to report the car purchase in the survey. The likelihood of not reporting is particularly large in the two tails of the wealth distribution. The car purchases provide validation data that establish basic problems with the survey-based measure. Finally, we study a simple measurement error model that allows for both error in survey and in registry-based imputation and we compare the relative magnitudes of the error.

4. While others have exploited the richness of Swedish data to study households portfolio choices (e.g., Massa and Simonov 2006; Calvet, Campbell, and Sodini 2007, 2009; Cesarini et al. 2010; Vestman 2011), or to study various topics within labor economics and inequality (e.g., Björklund, Lindahl, and Plug 2006; Domeij and Floden 2010; Lindqvist and Vestman 2011), or corporate finance (Cronqvist et al. 2009), we are the first to compute a measure of consumption based on Swedish income and asset data.

The rest of this chapter is organized as follows. Section 11.1 describes our Swedish data set. Section 11.2 describes how we construct registry-based consumption. The details of the various data sources and consumption measurement components are relegated to the appendix. Section 11.3 describes the properties of our new registry-based measure of consumption. It also compares it to the properties of survey-based consumption and discusses the correlation between the two measures for the set of households for which we observe both measures. Section 11.4 studies car transactions as an external validation tool for the survey data. Section 11.5 concludes with lessons for survey-based consumption measurement.

11.1 Data

Our analysis compares registry-based and survey-based consumption measures between 2003 and 2007. The foundation of the registry-based data is a representative panel data set LINDA (Longitudinal INdividual DAta for Sweden) of 300,000 households and their members. We add detailed registry-based data on individuals' asset holdings from LINDA's wealth supplements. Our survey-based measure is the Swedish Household Budget Survey (HBS), which tracks about 2,000 *different* households each year. Since 2003, Statistics Sweden uses LINDA as the sample frame for this survey. Therefore, it is possible to perfectly match the survey-based information with the registry-based information.[5] Appendix A describes the data sets in more detail. Along the way, we point to some measurement issues in the registry data.

It is possible to obtain detailed administrative records of Swedish tax payers for two reasons. First, each tax payer has a unique social security number and this number is used as an identifier in every administrative database. Second, the Swedish tax authority shares records with the national statistical agency, Statistics Sweden. Thus, it is possible to use all information generated in tax filings and match it with other administrative databases, such as the real estate registry or the car registry. Of particular importance is the fact that, up until 2007, Sweden levied a wealth tax on those individuals who were sufficiently rich. To establish who qualified, authorities gathered comprehensive information on all asset holdings for all households. For instance, each household reports each and every listed stock or mutual fund she holds in her tax filings. Two exceptions to this are the holdings of financial assets within private pension accounts, for which we only observe additions and withdrawals, and "capital insurance accounts," for which we

5. To the best of our knowledge, a similar match has only been made on Danish data by Browning and Leth-Petersen (2003) and Kreiner, Lassen, and Leth-Petersen (chapter 10, this volume).

observe the account balance but not the asset composition.[6] The reason is that tax rates on those two types of accounts depend merely on the account balances and not on actual capital gains. There is also a tax on real estate, which allows for an accurate measurement of the value of owner-occupied single-family houses and second homes (cabins). Apartment (co-op) values are less accurately measured.

11.2 Constructing Registry-Based Consumption

This section describes our approach to impute consumption expenses. We combine information from Swedish registry data on income, asset holdings, and asset returns to arrive at imputed consumption expenditure from the household budget constraint. Consumption of household i in year t is given by:

$$c_{it} = y_{it} + d_{it} - (1 + r_{it}^{d})d_{it-1} - a_{it} + a_{it-1}(1 + r_{it}^{a}), \tag{1}$$

where y_{it} denotes household i's labor income minus taxes plus transfers plus rental income from renting out owned houses in year t, d_{it} denotes the value of total debt at the end of year t, r_{it}^{d} the *household-specific* interest rate on debt between $t-1$ and t, a_{it} denotes the total value of the asset portfolio at the end of year t, and r_{it}^{a} the *household-specific* holding period return on the asset portfolio held between $t-1$ and t. Income that is not invested or used to reduce debt, declines in net asset values, and net increases in debt all translate into higher consumption. The richness of the Swedish data makes all terms on the right-hand side of equation (1) observable. When adapted to the Swedish registries, equation (1) can be spelled out in more detail as follows:

$$c_t = y_t + \Delta d_t - y_t^{d} - \Delta b_t - \Delta v_t + y_t^{v} - \Delta h_t - \Delta \psi_t - \omega_t, \tag{2}$$

where the subscript i has been omitted for brevity. The variable y_t^{d} measures the interest service on debt, Δb_t are changes in bank accounts, $\Delta v_t = v_t - v_{t-1}R_t$ measures a household's active rebalancing of mutual funds, stocks, and bonds,[7] y_t^{v} is after-tax financial asset income (interest on bank accounts, coupons from bonds, dividends from stocks, and income from stock option contracts), Δh_t are changes in housing wealth due to active rebalancing (sales or purchases, not valuation effects), $\Delta \psi_t$ is the net change in capital insurance

6. Capital insurance accounts are savings vehicles that are not subject to the regular capital gain and dividend income taxes, but instead are taxed at a flat rate on the account balance. Hence, we do not know the exact composition of these accounts, only the year-end balance.

7. The household-specific return on this portfolio excludes any distributions (dividends, coupons): $R_t = P_t / P_{t-1}$ where P_t is the end-of-year, ex-dividend price. When the household does not change its position in a given asset but passively earns an unrealized capital gain or takes a capital loss, that asset's contribution to Δv is zero.

accounts, while ω_t are contributions to private pension accounts. Each component in equation (2) is detailed in appendix B. All amounts are denoted in real terms (with base year 2005), where the deflator is Swedish consumer price index.

11.3 Properties of Registry-Based Consumption

We now study the properties of the consumption expenditure variable, constructed from the registry data, and compare it to the corresponding consumption measure from the Household Budget Survey. This comparison is possible for the *same set* of households for the five survey years between 2003 and 2007. We recall that each household enters once in the HBS, each HBS wave is about 2,000 households, and the match rate with LINDA is 100 percent. The resulting number of matched household-year observations in our sample is 10,705. In what follows, consumption measured from the survey is denoted by c^S and consumption imputed from registry data via equation (2) is denoted by c^R.

We impose several sampling restrictions on this set of matched households to ensure stable household composition, proper identification of owners and renters, complete data on financial asset portfolios, and to eliminate outliers in terms of year-on-year wealth changes, which may be due to errors in the raw data. Appendix C describes the restrictions in detail. The final sample consists of 5,134 households, or about one thousand households per survey year on average. Of these, 1,487 are renters (29 percent) and 3,647 are homeowners (71 percent).

One important issue when comparing the HBS and the registry-based consumption measures is that they pertain to a consumption flow measured over the same time frame. Because the registry-based imputation is based on tax data, it always refers to an annual consumption measure over the period January 1 until December 31. The survey is done during a two-week period when recurrent expenditure items are recorded in a diary and when households are interviewed about big ticket purchases of cars, boats, furniture, and so forth. Thus, survey consumption conceptually refers to the fifty-two-week period ending with the last interview. This implies that survey- and registry-based measures pertain to a different one-year measurement period. In the most extreme case, households interviewed in the first two weeks of January essentially report consumption that refers to the previous registry (calendar) year. When comparing the registry-based consumption measure for a given calendar year to the survey measure, the best comparison is for households who were surveyed late in the calendar year. Our main comparison, therefore, focuses on households surveyed in December. The December sample contains 529 households, of which 159 are renters and 370 homeowners.

11.3.1 Summary Statistics

Tables 11.1 and 11.2 report our imputed consumption series for renters and homeowners, respectively. In each table, the first column shows summary statistics for the distribution of registry-based consumption. The second column reports the survey-based consumption measure for the same sample of households. Column (3) reports the moments of the distribution of the difference between registry- and survey-based measures (not the difference of the moments). Column (4) scales that difference by median registry-based consumption. Columns (5)–(8) are analogous to columns (1)–(4), but focus on the subset of households interviewed in December, a group for which the timing of consumption measurement in survey and registry is in closer alignment.

Renters. Starting with the 1,487 renters, we find average consumption of 214 kSEK (in thousands of Swedish krona) imputed consumption (about $32,300), and basically identical to the survey mean of 212 kSEK. The standard deviation is slightly higher in the registry than in the survey-based measure (130 versus 116 kSEK). In terms of the percentiles of the distribution, our imputed measure indicates lower consumption in the very bottom of the consumption distribution, equal consumption at the 25th and 50th percentiles, and higher consumption from the 75th percentiles of the consumption distribution onward. For example, the 75th percentile of imputed consumption is 283 kSEK compared to 262 kSEK in the survey, while the 95th percentile is 578 for the registry versus 525 kSEK for the survey-based measure. Despite these differences, the two consumption distributions line up remarkably well for renters. Even the 99th percentiles differ by only $8,000 on a consumption of $88,000. Columns (5) and (6) report the same statistics, but for the subset of 159 renters surveyed in December. While the December sample is obviously much smaller (the first and 99th percentiles contain only one person), the consumption distribution is similar and lines up about as well with the survey-based distribution as the full sample.

Homeowners. Turning to the 3,647 homeowners in table 11.2, we find average consumption of 328 kSEK imputed consumption (about $49,700), and noticeably above the survey mean of 292 kSEK, about a $5,500 difference. The log difference is 12 percent. The average consumption of homeowners is 53 percent higher than that of renters in the imputation, compared to 38 percent in the survey. Since homeowners are on average substantially wealthier than renters, higher consumption is to be expected. It is also a first indicator that the survey may be understating consumption of the wealthy. In addition, there is substantially more consumption inequality among owners in the registries than in the survey, and more between owners than between renters. The standard deviation of consumption is 191 kSEK in the registry versus 147 kSEK in the survey-based measure. The 5th percentile of the consumption distribution is lower in the registry-based measure (87 versus 107 kSEK), the median is higher (315 kSEK versus 270 kSEK), and the 95th percentile is

Table 11.1 **Summary statistics for renters**

Variable	Registry (1)	Survey (2)	Diff. (3)	Rel. diff. (4)	Registry (5)	Survey (6)	Diff. (7)	Rel. diff. (8)
Mean	214.4	211.6	–2.81	–0.015	216.1	217.6	1.52	0.008
Std.	129.8	116.2	135.9	0.71	132.7	112.4	135.1	0.69
Percentile 1	–25.8	57.6	–347.8	–1.81	–189.2	34.3	–300.8	–1.55
Percentile 5	76.4	84.7	–187.1	–0.97	56.6	75.4	–177.3	–0.91
Percentile 25	130.4	133.1	–63.7	–0.33	126.0	131.1	–61.3	–0.32
Percentile 50	185.2	192.1	–9.48	–0.05	188.6	194.7	–11.2	–0.06
Percentile 75	282.6	261.9	48.2	0.25	318.9	262.3	51.7	0.266
Percentile 95	438.5	407.3	197.6	1.03	444.0	469.9	249.9	1.28
Percentile 99	577.5	524.8	374.5	1.95	627.3	526.0	657.5	3.38
Survey month	1–12	1–12	1–12	1–12	12	12	12	12
Observations	1,487	1,487	1,487	1,487	159	159	159	159

Note: Columns (3) and (7) report the distribution of the difference between survey-based and registry-based consumption measures. Columns (4) and (8) use the median of survey-based consumption as the denominator to compute a measure of the relative difference between the two measures.

Table 11.2 **Summary statistics for homeowners**

Variable	Registry (1)	Survey (2)	Diff. (3)	Rel. diff. (4)	Registry (5)	Survey (6)	Diff. (7)	Rel. diff. (8)
Mean	328.4	291.9	–36.4	–0.135	344.2	314.4	–29.8	–0.102
Std.	191.3	147.0	184.0	0.682	185.7	146.1	165.4	0.565
Percentile 1	–93.5	75.1	–528.9	–1.96	–25.0	78.4	–526.5	–1.80
Percentile 5	86.8	107.0	–302.7	–1.12	105.6	115.4	–328.9	–1.12
Percentile 25	203.1	192.4	–123.4	–0.457	217.4	208.8	–114.9	–0.39
Percentile 50	314.8	269.9	–39.7	–0.147	323.0	292.8	23.9	–0.082
Percentile 75	426.9	364.3	49.2	0.182	444.5	389.2	60.6	0.207
Percentile 95	633.7	553.3	239.7	0.888	693.5	621.6	235.6	0.805
Percentile 99	877.3	753.0	454.2	1.68	957.9	763.7	387.6	1.32
Survey month	1–12	1–12	1–12	1–12	12	12	12	12
Observations	3,647	3,647	3,647	3,647	370	370	370	370

Note: Columns (3) and (7) report the distribution of the difference between survey-based and registry-based consumption measures. Columns (4) and (8) use the median of survey-based consumption as the denominator to compute a measure of the relative difference between the two measures.

considerably higher (634 versus 553 kSEK). The 99th percentiles of the two consumption distributions differ by 15 percent (877 versus 753), the equivalent of $18,800. Columns (5) and (6) report the same statistics, but for the subset of 370 homeowners surveyed in December. The consumption distribution is shifted up slightly (probably a Christmas-shopping effect), but the conclusions from comparing the two distributions are the same for this subset.

The understatement of consumption in the survey at the top of the distribution is consistent with Aguiar and Bils (2011), who find that consumption inequality closely tracks income inequality between 1980 and 2007 once the *relative* undermeasurement of luxury good expenditures in the CEX is corrected. The (smaller) overstatement of survey-based consumption of the poorest is a new finding. In contrast, Meyer and Sullivan (2003, 2007) and Meyer, Mok, and Sullivan (2009) argue that income transfers from welfare programs and participation in the food stamp program is understated in surveys, particularly among the poorest. This underreporting, as always, may be due to recall problems and a desire to minimize reporting burden, but in this instance, also due to confusion about the exact name of the programs and social stigma associated with participation. We speculate that, by the same token, overreporting consumption expenses among the poorest could arise from a desire to conform to the average consumption pattern (see also Bertrand and Morse 2012). In addition, it might result from an (asymmetric) inability to adjust consumption downward in the short run when faced with a negative income shock around the time of the survey.

Comparing Survey and Registries. What this comparison of consumption distributions ignores is the identity of the respondent. Next, we compute the difference, for each household, between the survey- and the registry-based consumption measures. Columns (3) and (7) report the moments of that distribution for the full sample and for the December subsample. Columns (4) and (8) express this difference relative to the median survey-based consumption. If the registry-based consumption measures are true, then the relative differences are a direct measure of the bias in the survey. We argued above that the December comparison is most meaningful because of the timing misalignment for households surveyed too early in the year. For renters, columns (7) and (8) of table 11.1 show that while the average difference is essentially zero, its standard deviation is substantial at 135 kSEK or 69 percent of median survey consumption. The difference ranges from –177 kSEK at the 5th to 250 kSEK at the 95th percentiles, or between –1 and +1 times median consumption. The statistics in column (8) can be compared to the numbers reported in table 1 of Browning and Leth-Petersen (2003), for a sample of Danish renters. Their (our) numbers are: –5.79 (–1.81) for the minimum, –0.24 (–0.32) for the 25th percentile, –0.01 (–0.06) at the median, 0.28 (0.27) at the 75th percentile, and 6.66 (4.03) at the maximum. We conclude that the two sets of deviations for Swedish and Danish renters are close. Despite the timing issues, a comparison of columns (8) and (4) shows

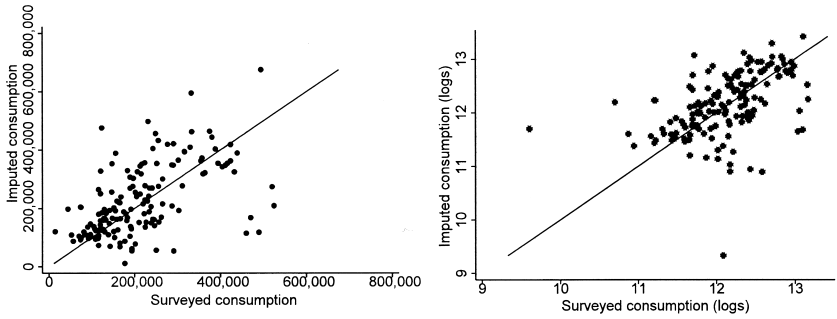

Fig. 11.1 Survey- versus registry-based consumption for renters

Notes: The left panel plots survey-based consumption in levels (horizontal axis) against registry-based consumption in levels (vertical axis) for the group of 159 renters surveyed in December. The right panel plots survey-based consumption in logs (horizontal axis) against registry-based consumption in logs (vertical axis) for the same group of households. For the purpose of this figure, we eliminated four observations with negative consumption since their log consumption is not defined. The solid line is the 45-degree line.

that the distribution of deviations looks quite similar for the full sample and the December subsample. In part, of course, this is because the full sample is much bigger and less sensitive to outliers.

Figure 11.1 shows a scatter plot of survey- versus registry-based consumption for the December sample of renters. The left plot measures consumption in levels, the right plot in logs. The figure also draws in the 45-degree line. The plot excludes four renters with negative imputed consumption. The correlation between the consumption measures in levels for all 159 December renters is 40.7 percent. Extending the sample to all 1,487 renters reduces the correlation slightly to 39.5 percent, most likely due to the timing misalignment issue alluded to above.

For homeowners, the standard deviation of the individual survey- minus registry-based differences is 165 kSEK or 56 percent of median survey-based consumption. The difference ranges from –329 kSEK at the 5th to 236 kSEK at the 95th percentiles, or between –1.12 and 0.80 times median consumption, similar to the numbers for renters. The statistics in column (8) can be compared to the numbers reported in table 2 of Browning and Leth-Petersen (2003), for a sample of Danish homeowners. Their (our) numbers are: –5.79 (–3.04) for the minimum, –0.29 (–0.39) for the 25th percentile, –0.02 (–0.08) at the median, 0.26 (0.21) at the 75th percentile, and 10.7 (1.55) at the maximum. We conclude that our Swedish registry-based measure appears somewhat closer to the survey-based measure than the Danish one, in that it seems to imply fewer large differences in the extremes of the difference distribution. Nevertheless, the two sets of deviations are close.

Figure 11.2 shows a scatter plot of survey- versus registry-based consumption for the December sample of owners. The left plot measures consumption in levels, the right plot in logs. The correlation between the consumption

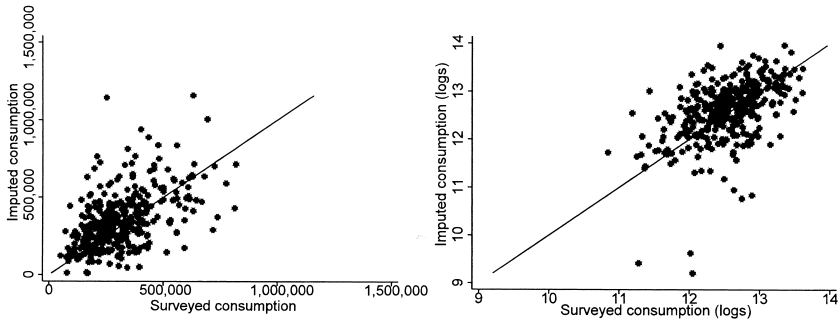

Fig. 11.2 Survey- versus registry-based consumption for homeowners

Notes: The left panel plots survey-based consumption in levels (horizontal axis) against registry-based consumption in levels (vertical axis) for the group of 370 homeowners surveyed in December. The right panel plots survey-based consumption in logs (horizontal axis) against registry-based consumption in logs (vertical axis) for the same group of households. For the purpose of this figure, we eliminated four observations with negative consumption since their log consumption is not defined. The solid line is the 45-degree line.

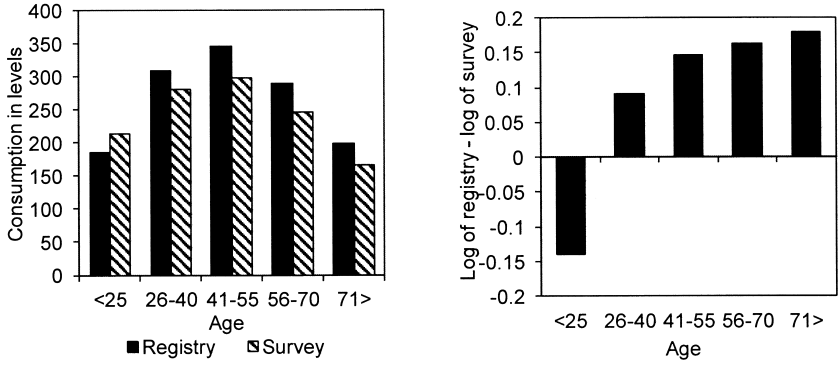

Fig. 11.3 Survey- versus registry-based consumption by age

Notes: The figure plots survey-based consumption in levels and registry-based consumption in levels for different age groups on the left panel and the percentage difference between the two measures on the right panel. Group 1 is made up of households whose head is less than twenty-five years old (180 observations), group 2 is age twenty-six to forty (1,511 obs.), group 3 is age forty-one to fifty-five (1,752 obs.), group 4 is age fifty-six to seventy (1,150 obs.), and group 5 is age seventy-one and older (456 obs.). The total sample is 5,049 observations (5,134 households minus 85 households with negative registry-based consumption).

measures in levels for all 370 December homeowners is 52.4 percent. Extending the sample to all 3,647 homeowners reduces the correlation to 43.4 percent. Combining all renters and owners surveyed in December leads to correlation between the survey- and registry-based consumption levels of 55.1 percent, while the full sample of 5,134 households results in a correlation of 46.7 percent.

Consumption by Age. Figure 11.3 plots registry- and survey-based consumption for five age groups, listed in the caption of the figure. Both measures of consumption display the well-known hump shape over the life cycle.

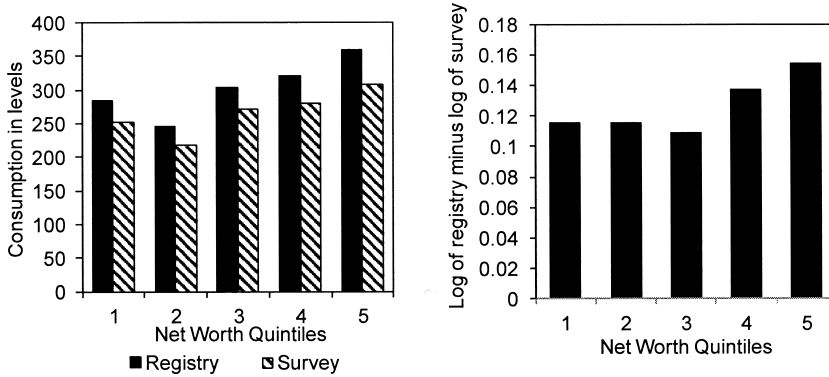

Fig. 11.4 Survey- versus registry-based consumption by wealth

Notes: The left panel plots average survey-based consumption in levels (striped bars) and registry-based consumption in levels (solid bars) for five groups of households that are ranked by wealth. Wealth is household net worth, measured as financial assets plus (primary and secondary) houses minus all debt. The right panel plots the percentage deviation (log difference) between registry-based and survey-based consumption for the same wealth groups. For the purpose of this figure, we eliminated eighty-five observations with negative consumption since their log consumption is not defined. The sample for this figure contains 5,049 households (5,134 households minus 85 households with negative registry-based consumption).

The percentage difference between the two consumption measures follows the hump-shaped profile. For the twenty-five-year-olds, registry-based consumption is minus 14 percent below survey-based consumption. For the twenty-six to forty-year-olds, it is 9.1 percent above that in the survey. That positive difference further rises with age to 14.7 percent for ages forty-one to fifty-five, and then further to 16 percent and 18 percent for the two oldest quintiles. To the extent that wealth is hump shaped over the life cycle, this is consistent with the consumption-by-wealth discussion we turn to next.

11.3.2 Role of Net Worth and Income

We now turn to the relationship between our two consumption measures and wealth. Our measure of wealth is household net worth, measured as financial assets plus (primary and secondary) houses minus all debt. Another advantage of our Swedish data is that there is no top-coding of wealth (or income). In 2007, the 10th percentile of net worth is negative, indicating debt outstripping assets (–112 kSEK), the median is 613 kSEK, and the 90th is almost 2,907 kSEK (the equivalent of $440,000), and the 95th is 3,995 kSEK (or $605,000). Table 11D.1 in appendix D reports the wealth distribution by year.

Consumption by Wealth. We sort all households with positive registry-based consumption into wealth quintiles, ranked from lowest to highest. The left panel of figure 11.4 is a bar chart of average survey- and registry-based consumption for each of these wealth quintiles. It shows that, other than a decline from wealth quintile 1 to 2, consumption increases in wealth, but

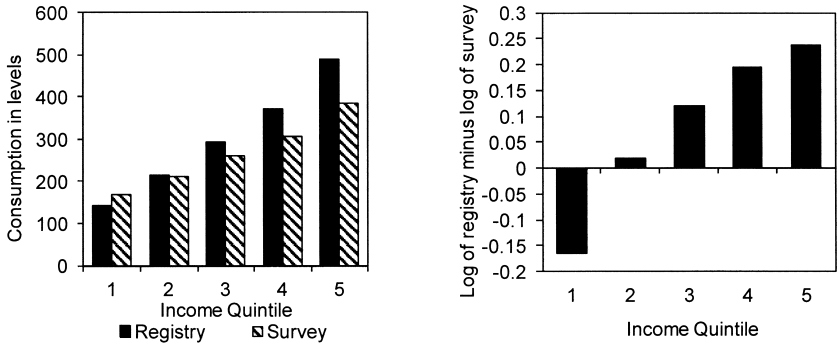

Fig. 11.5 Survey- versus registry-based consumption by income

Notes: The left panel plots survey-based consumption in levels and registry-based consumption in levels for different income quintiles. Income, y, is measured as labor income after taxes and transfers. It excludes financial income and interest payments on loans. The right panel plots the percentage deviation (log difference) between registry-based and survey-based consumption for the same income groups. The total sample is 5,049 households (5,134 households minus 85 households with negative registry-based consumption).

that registry-based consumption is steeper in wealth. The gap between the two consumption measures increases from 27 kSEK in quintile 2 to 51 kSEK in quintile 5 ($4,090 versus $7,800). The right panel plots the average *percentage* deviations between individual registry- and survey-based measures for each wealth group. This percentage deviation also increases in wealth, increasing from 11 percent for quintiles 1 to 3 to 14 percent and 15 percent for quintiles 4 and 5. In other words, the survey understates consumption, and the understatement is larger for the wealthy.

Consumption by Income. We obtain a similar picture when we study consumption by income. Figure 11.5 plots the two consumption measures for income quintiles. We use labor income after taxes and transfers, earlier defined as y_t, to group households. Registry-based consumption is lower than survey-based consumption for the lowest income quintile, similar to our results for the youngest age group. Because of the increasing life cycle profile in income, those two results reflect the same group of households to a large extent. The percentage difference between registry- and survey-based consumption turns positive for quintile 2 (2 percent) and increases further with income to 24 percent for the highest income group. This finding reinforces our conclusion that the survey may be understating consumption for the rich, as measured by either wealth or income. Results are nearly identical if we include financial income y^v and subtract interest payments on debt y^d, which are omitted for brevity.

11.3.3 Household-Specific Portfolio Returns

One major advantage of the Swedish data set, and the feature that makes it truly unique worldwide, is that it allows us to impute a highly accurate

financial portfolio return for each household because we observe all holdings of financial assets at the individual security level. It is natural to ask how sensitive our registry-based consumption measure is to our ability to do this imputation correctly. Put differently, how far off would we be if we had used a different return assumption? The answer to this question seems relevant for researchers that want to follow our method for other countries (such as the United States) where such individual-specific portfolio holdings data are not available.

We explore three natural variations on the individual portfolio-return calculation. We assume that every security the individual holds earns the rate of return on a well-diversified Swedish stock portfolio (the SIXRX Stockholm stock index return). In that case, we set financial income $y_y^v = 0$ to zero but use a cum-dividend stock return in equation (2).[8] We also consider a return equal to a 50-50 weighted average of a Swedish one-year Treasury note and the SIXRX. Third, we simply consider a one-year Treasury bond yield (and $y_y^v = 0$) as the portfolio return.

Table 11.3 reports survey- and registry-based consumption measures for all 529 households, homeowners and renters, surveyed in December. Column (1) repeats the summary statistics for survey-based consumption. Column (2) is our benchmark registry-based imputation where we use the correct household-specific return. Column (3) reports using the Swedish stock index, column (4) the 50-50 stock-bond return, and column (5) uses the bond return. Comparing column (3) to column (2) makes clear that assuming that household portfolio returns equal the Stockholm Stock Exchange index return leads to an overstatement of consumption for all but the 99th percentile of the benchmark registry-based consumption distribution. The median consumption is too high by 12 kSEK, the average by 8 kSEK, and the dispersion by 7 kSEK. Using a 50-50 mix of stocks and bonds to proxy for the household-specific return leads to both an understatement and overstatement of consumption at different points in the consumption distribution. The bias in the median (mean) is –2.5 kSEK (–3.9 kSEK). Finally, using the bond return as a proxy leads to a severe understatement across the board, with median too low by 11.4 kSEK and mean consumption too low by 16.2 kSEK ($1,700 and $2,450, respectively). Using the all-bond return or the all-stock returns also leads one to overestimate the true dispersion in consumption. This fact may suggest that households may choose portfolio allocations so that they can use them to self-insure. While the sign of the bias on consumption may depend on the exact period of study (presumably, the survey bias from using an imputation benchmark based on stocks could turn positive for a sample with unusually low stock returns), the conclusions on the volatility of consumption seem always applicable.

8. We also explored the MSCI world index return, but it gave similar answers to using the SIXRX.

Table 11.3 **Effect of portfolio returns on consumption**

Variable	Survey (1)	HH portf. (2)	Stocks (3)	Stock-bond (4)	Bonds (5)	Survey (6)	HH portf. (7)	Alternative (8)
Mean	281.0	305.7	313.5	301.8	289.5	272.1	293.8	280.7
Std.	141.8	181.1	188.1	183.4	183.6	143.1	181.7	189.1
Percentile 1	60.0	–45.3	–45.4	–94.0	–141.0	43.2	–123.6	–238.5
Percentile 5	96.8	82.7	85.9	80.3	68.1	82.5	55.4	12.3
Percentile 25	176.7	172.7	175.3	170.2	160.5	162.5	163.2	154.0
Percentile 50	257.2	280.6	292.1	275.2	269.2	250.7	262.2	249.3
Percentile 75	363.1	403.9	410.8	403.3	396.6	358.9	399.3	381.6
Percentile 95	560.8	636.0	669.5	659.2	625.8	560.3	632.4	616.9
Percentile 99	722.2	872.2	925.3	880.6	838.5	741.0	839.0	841.9
Survey month	12	12	12	12	12	12	12	12
Survey year	03–07	03–07	03–07	03–07	03–07	06–07	06–07	06–07
Observations	529	529	529	529	529	195	195	195

Note: The table reports survey- and registry-based consumption measures for all 529 households, homeowners and renters, surveyed in December. Column (1) repeats the summary statistics for survey-based consumption. Column (2) is our benchmark registry-based imputation where we use the correct household-specific return. Column (3) reports using the Swedish stock index, column (4) the 50-50 stock-bond return, and column (5) uses the bond return. The bond return is a one-year government bond yield. All amounts are in thousands of Swedish krona (kSEK). Columns (6) and (7) report the same statistics as in columns (1) and (2), but only for years 2006 and 2007. Column (8) reports the summary statistics for the alternative imputation framework given by equation (3), also for the years 2006 and 2007.

We conduct a final exercise that studies data limitations that exist in other contexts. This exercise compares our approach, spelled out in equation (2), to an alternative approach that ignores the asset composition of the household portfolio and the return earned on each component. Instead, it uses the change in financial wealth between tax years, denoted by Δa_t, as a proxy. This emulates the approach taken, for example, in the Danish exercise by Browning and Leth-Petersen (2003) and Kreiner, Lassen, and Leth-Petersen (chapter 10, this volume).

$$c_t^* = y_t + \Delta d_t - y_t^d + y_t^v - \Delta h_t - \omega_t - \Delta a_t \,. \tag{3}$$

Thus, instead of our "bottom-up" aggregation of security holdings to household asset balances, the alternative method relies on the aggregated asset holdings reported in the wealth supplement of LINDA. Since these data are only available for the waves 2005 to 2007, two changes can be computed in 2006 and 2007 (195 households in the December sample). Note also that the alternative measure still contains information on capital income, which consists of interest on bank accounts, bond coupons, and dividend distributions from owned stocks. But, it assumes a zero capital gain on all asset holdings. The lack of household-specific asset return information introduces measurement error in c_t^*, the latter is offset to some extent by a reduction in the type of measurement error that our approach suffers from, for example, because of incomplete or incorrect identification of securities' positions and prices.

Columns (6), (7), and (8) of table 11.3 report the results for this exercise. As can be seen in columns (6) and (7), there is substantial underreporting (21.7 kSEK) in the survey on average in 2006 and 2007, but it is confined to the top half of the consumption distribution. The average underreporting is much smaller when using the alternative registry-based measure in column (8) (8.6 kSEK). The consumption distribution in column (8) is a considerable downward shift from our preferred distribution. Even at the 5th percentile of the alternative measure, imputed consumption is just 12.3 kSEK, a difference of more than $6,530 to our measure that allows for household-specific returns. The standard deviation of the alternative measure is higher than the standard deviation of the baseline measure, implying that the utilization of the household-specific ex-dividend returns reduces the cross-sectional dispersion of consumption somewhat. This finding is in line with the reported dispersions in columns (2) to (5). Finally, the correlation between individual survey- and registry-based consumption measures is 50.1 percent in the years 2006 and 2007 for our measure, but drops substantially to 38.6 percent for the alternative measure. In sum, this comparison highlights the usefulness of our bottom-up approach of identifying individual securities, aggregation of households' asset balances, and the use of household-specific capital gain returns.

Table 11.4 Regression diagnostic

	Renters (1)	Owners (2)	All (3)	Owners (4)	Owners (5)
	A. Consumption in levels				
Constant	91.0	147.0	112.5	147.5	149.8
	(18.0)	(19.4)	(14.0)	(19.4)	(20.0)
c^S	0.630	0.649	0.708	0.649	0.656
	(0.076)	(0.056)	(0.044)	(0.056)	(0.058)
R-squared	0.312	0.266	0.328	0.264	0.252
	B. Consumption in logs				
Constant	5.76	4.60	4.28	4.71	4.63
	(0.077)	(0.719)	(0.542)	(0.718)	(0.711)
$\log(c^S)$	0.528	0.639	0.660	0.630	0.637
	(0.077)	(0.057)	(0.044)	(0.057)	(0.057)
R-squared	0.235	0.255	0.307	0.248	0.249
Observations	155	366	521	370	384
Change in official address	N	N	N	N	Y
Transaction of house or cabin	N	N	N	Y	Y

Notes: The table reports results from ordinary least squares (OLS) regressions of registry-based consumption on a constant and on survey-based consumption. The top panel expresses both consumption measures in levels while the bottom panel measures both in logs. The samples are the households surveyed in December. We delete eight observations with negative registry-based consumption, four renters and four homeowners. The last two columns of the table report regression results if the sampling restrictions on housing transactions are relaxed.

11.3.4 Regression Analysis

Besides the scatter plots and tables discussed above, we now turn to a more formal comparison of the two measures of consumption. We study cross-sectional regressions of registry-based consumption on survey-based consumption as an additional diagnostic of the closeness of fit.

$$c_{it}^R = \alpha + \lambda c_{it}^S + \varepsilon_{it} . \tag{4}$$

The regressions fit the best straight line through the cloud of points reported in the left panels of figures 11.1 and 11.2. Table 11.4 reports the results. Column (1) is for the December sample of 155 renters with positive consumption, column (2) is for the December sample of 366 owners with positive consumption, and column (3) is for the combined December sample of 521 renters and owners with positive consumption. We confirm a robust positive association between the two measures for both the level measures (top panel) and the log measured (bottom panel). The top panel shows an estimated slope coefficient of 0.630 and an R^2 statistic of 31.2 percent for renters. For owners, the slope is nearly identical at 0.649, but the R^2 is lower at 26.6 percent. The R^2 for the full sample of owners and renters is 32.8 percent.

If there is (independent) measurement error in survey-based consumption, this would bias the slope down from one. Given that the two measures have about equal mean, this would result in the need for a positive intercept. This is indeed what we find. In column (3), the positive intercept is 112.5 kSEK, or about $17,000. Panel B runs the same regressions but between consumption measured in logs. The regressions in logs give a similar picture with a full-sample slope of 0.660 and R^2 of 30.7 percent. The overall conclusion from the comparison of registry-based and survey-based consumption measures is that there is a robust positive correlation among them, but that they contain either substantially different information or that there is nontrivial measurement error in one or both measures.

Under the (somewhat restrictive) assumptions of Kreiner, Lassen, and Leth-Petersen (chapter 10, this volume) that (a) both log registry and log survey consumption are noisy measures of unobserved, true log consumption; (b) the errors in survey and registry consumption are uncorrelated; and (c) that true log consumption is uncorrelated with the measurement in log registry consumption, we can say more. The bias due to measurement error in the log survey consumption is $1 - \lambda$, where $\hat{\lambda}$ is the estimated slope coefficient in equation (4). Our estimated bias is 34 percent, compared to 21 percent in Kreiner, Lassen, and Leth-Petersen (chapter 10, this volume), which shows a fair amount of noise in the survey measure. Following the Danish paper, we also look at a regression of log survey- on log registry-based consumption for the subset of households for whom the individual difference $\log(c^S) - \log(c^R)$ is between –2 and +2. This reduces the December sample from 521 to 516 households and the full sample from 5,049 to 5,000 households. In unreported results, we find that the slope λ remains constant at 0.666 while the R^2 increases from 30.7 percent to 34.7 percent. For the full sample, the slope increases from 0.617 to 0.644 and the R^2 increases from 25.1 percent to 32.6 percent. Hence, eliminating outliers increases the association between survey- and registry-based consumption measures, and under the measurement error assumptions above, reduces the bias in the survey measure only modestly (at most 2.7 percentage points).

Our analysis of the previous section shows that using household-specific returns brings survey and registry measures closer, suggesting that the lower association between the two measures in the Swedish compared to the Danish data must be due to other reasons. For example, the household budget survey itself could be noisier in Sweden. Alternatively, other features of the Swedish registry data may be noisier than the Danish registry data. For example, other elements of the budget constraint such as housing or debt could have some measurement error or there the timing of tax payments may lead to measurement error.

Effect of Sampling Restrictions Based on Housing. The last two columns of table 11.4 enlarges the sample by including households who bought or sold

Table 11.5 **Regression diagnostic—Effect of wealth and portfolio return**

	(1)	(2)	(3)	(4)	(5)	(6)
			A. Household-specific return			
Constant	112.5	110.2	121.2	112.5	131.5	84.8
	(14.0)	(38.4)	(30.0)	(16.7)	(44.1)	(54.6)
c^S	0.708	0.797	0.679	0.683	0.710	0.800
	(0.044)	(0.128)	(0.104)	(0.057)	(0.113)	(0.138)
R-squared	0.328	0.432	0.289	0.319	0.286	0.385
			B. Stock return			
Constant	114.3	110.2	120.7	116.3	146.0	97.6
	(14.7)	(38.5)	(30.1)	(17.1)	(48.3)	(66.1)
c^S	0.730	0.804	0.687	0.691	0.727	0.849
	(0.047)	(0.128)	(0.104)	(0.058)	(0.124)	(0.166)
R-squared	0.322	0.435	0.291	0.316	0.259	0.326
			C. Bond return			
Constant	125.4	114.0	123.7	110.7	138.2	93.7
	(15.2)	(38.7)	(30.1)	(17.5)	(51.6)	(66.6)
c^S	0.604	0.777	0.665	0.665	0.515	0.513
	(0.048)	(0.129)	(0.104)	(0.059)	(0.132)	(0.168)
R-squared	0.233	0.417	0.279	0.288	0.134	0.148
Observations	521	53	107	313	101	56
Range for net worth	P0–P100	P0–P10	P0–P20	P20–P80	P80–P100	P90–P100

Note: For homeowners, the most restrictive sample restrictions were used (no change in official address, no transaction of house or cabin). The ranges of net worth are specific for each year and are reported in table 11D.1. Panel A uses the framework of equation (2) to impute consumption. Panel B uses a modified version of the framework, which sets $y_t^y = 0$ and replaces the household-specific return R_t by SIXRX, the gross index of the Stockholm Stock Exchange. In panel C the term $y_t^y = 0$ and the household-specific return is assumed to equal a one-year government bond yield. As in the previous regressions, we exclude observations with negative imputed consumption (a total of eight for the full sample corresponding to the first column).

a house or cabin (column [4]) and by additionally including households who changed their official address (column [5]). The latter additionally picks up apartment purchases and sales. Comparing the results to the more restricted homeowners sample shows that the correspondence between survey- and registry-based consumption does not materially deteriorate once we include house purchasers or sellers or movers.

Effect of Wealth Distribution and Portfolio Returns. Table 11.5 explores the effect on the regression diagnostics of wealth and of the use of household-specific portfolio returns. Panel A of table 11.5 studies regression results of equation (4) for different wealth groups. Column (1) repeats the full sample result, columns (2) and (3) are for the bottom of the wealth distribution, column (4) for the middle of the distribution (20th–80th percentiles), and columns (5) and (6) for the top of the wealth distribution. Looking across columns (2) to (6), we notice that the R^2 statistics are highest for the bottom and top deciles. The R^2 is 6 percentage points higher at the top

than in the full sample and both slope coefficient and R^2 are lower closer to the middle of the net worth distribution. Under the measurement error assumptions described above, the bias in the survey is largest closer to the middle ($1 - \lambda = 32\%$). Panels B and C explore the effect of assuming different rates of return on the financial wealth portfolio. Panel B shows that using a broad stock return index results in essentially identical slope estimates for the wealthy but the R^2 statistic decreases by 6 percentage points for the wealthiest decile. Panel C shows that using the bond return leads to much worse associations between survey- and registry-based consumption measures, especially for the wealthy.

11.4 External Validation: Car Transactions

Since both survey- and registry-based consumption measures contain measurement error, many researchers have advocated finding external validation data to help understand the properties of measurement error.[9] Swedish registry data on car purchases offer an appealing source of validation data. Arguably, car purchases are one of the most salient purchase decisions households make. To the extent that recall errors plague survey data, we would expect those to be minimal for car transactions. Conversely, to the extent that there are discrepancies, they are revealing about substantial problems with survey-based data. The connection between the discrepancy and the characteristics of the household may be useful in correcting the survey, or for modeling measurement error in surveys.

Incidence of Underreporting. The Swedish car registry (discussed in the appendix) contains data on every purchase and sale of cars. The Household Budget Survey asks households about *net purchases* of vehicles (*Veh*), further broken down into cars,(*Car*), motorcycles, bikes, and other vehicles.[10] Net purchases are the difference between purchases and sales as measured over the past twelve months since the survey. To make the recall issue particularly stark, we focus on our sample of households that are both in the HBS and in the registries, and who purchased at least one car in the year they were surveyed *though at least one month before the beginning of the survey period*.[11] This results in a sample of 640 car-purchasing households (among

9. Battistin (2004) investigates the accuracy between the diary and interview samples in the US. CEX Ahmed, Brzozowski, and Crossley (2006) use two different Canadian surveys to compare recall food consumption responses. For a suggestion on how to set up a measurement error model using validation data, see section 3 in Bound, Brown, and Mathiowetz (2001).

10. In the COICOP standard, transactions of vehicles is defined by item U071 and transactions of cars by its subitem U0711.

11. As a robustness check, we tried a two-month lag as well. Our results were essentially the same as with a one-month lag. We are careful to exclude twenty-two car transactions between household members.

Table 11.6 **Car transactions in survey versus registry**

	Veh < 0 (1)	*Veh* > 0 (2)	*Veh* = 0 (3)	*Car* < 0 (4)	*Car* > 0 (5)	*Car* = 0 (6)
Mean	–40.8	78.8	0	+54.0	88.2	0
Observations	12	452	176	7	397	236
Fraction of obs. (%)	1.9	70.6	27.5	1.1	62.0	36.9

Note: The table reports the number of observations and the mean value of survey item net purchase of vehicles (*Veh*) and net purchase of cars (*Car*) for different subsamples. The sample consists of households for which at least one car purchase has been recorded in the car registry during the year of the survey, but at least one month prior to the survey month of the household. With multiple transactions, we require that at least one of the transactions occurred before the month of the survey. The amounts reported are in thousands of Swedish krona (kSEK). In sum, there are 640 households (22 transactions and gifts between different members of the same household are excluded).

the 5,134 households).[12] We then ask what those same households report in the survey about these car transactions.

Table 11.6 reports the distribution of interview responses among the car purchasers. In case of multiple purchases, we require that the first purchase occurred before the month of the survey. The table reports net purchase expenditures on vehicles (*Veh*) and on cars (*Car*), as reported in the survey. Although there is a separate category for cars in the registry, we choose to also report results for vehicles broadly defined to be able to rule out that the interviewer for convenience assigns a car transaction value only to the "vehicle item" but not to the appropriate subitem "cars." Implicit in our analysis is the assumption that, if at least one transaction has occurred, then *Veh* and *Car* should not be equal to zero.[13] The first three columns of table 11.6 show that only 72.5 percent of survey respondents report a vehicle purchase, if indeed a car purchase occurred, while 27.5 percent report a zero purchase value. For the subquestion that asks about net car purchases, we only find 62.0 percent positive responses and 36.9 percent zero responses (columns [5] and [6]).[14] We conclude that there is underreporting to the tune of 30 percent among respondents. This is a disturbingly high number, especially for such a salient item as car transactions.

12. Notice that since we require that households made their car purchase before they were surveyed, we only analyze half of the car purchasers in our sample (assuming that car purchases are distributed evenly over the year). Thus an approximation of the car purchaser fraction in our sample equals (2 * 640)/5134 = 24.9 percent. This is roughly equal to the aggregate statistics that state that in Sweden there are 1.1 million transactions of used cars every year and, in addition, 280,000 purchases of new cars. Given a population of five million households, this results in a car purchaser fraction of 27.6 percent.

13. In e-mail conversations, Statistics Sweden confirmed that this is the correct interpretation.

14. The results are similar when we confine attention to a group of households that bought one car and sold no car. Hence, our main results are not driven by a sale and purchase that exactly cancel each other out and lead to a zero net expenditure.

Table 11.7 Which households underreport?

	(1)	(2)	(3)	(4)	(5)
Age	0.0028*	–	–	–	0.0025
	(0.0015)	–	–	–	(0.002)
D(High school)	–	–0.213***	–	–	–0.178***
	–	(0.061)	–	–	(0.064)
D(College)	–	–0.161***	–	–	–0.118*
	–	(0.058)	–	–	(0.064)
D(Disp. income, 2nd quintile)	–	–	–0.057	–	–0.050
	–	–	(0.064)	–	(0.065)
D(Disp. income, 3rd quintile)	–	–	–0.067	–	–0.041
	–	–	(0.060)	–	(0.063)
D(Disp. income, 4th quintile)	–	–	–0.114*	–	–0.074
	–	–	(0.056)	–	(0.061)
D(Disp. income, 5th quintile)	–	–	–0.073	–	–0.043
	–	–	(0.058)	–	(0.065)
D(Net worth, 2nd quintile)	–	–	–	–0.094*	–0.096*
	–	–	–	(0.048)	(0.049)
D(Net worth, 3rd quintile)	–	–	–	–0.084*	–0.085*
	–	–	–	(0.047)	(0.048)
D(Net worth, 4th quintile)	–	–	–	–0.087*	–0.101*
	–	–	–	(0.048)	(0.050)
D(Net worth, 5th quintile)	–	–	–	–0.029	–0.055
	–	–	–	(0.054)	(0.057)
Year effects	Yes	Yes	Yes	Yes	Yes
Observations	640	640	640	640	640
Pseudo *R*-squared	0.077	0.088	0.077	0.079	0.100

Note: Probit regressions of the form $Pr(Veh = 0) = \alpha + \beta X_i + \epsilon_i$. The sample of households in the regressions is the same as in table 11.6. The table reports marginal effects.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Characteristics of Underreporters. Next, we ask what household-level characteristics are related to this underreporting problem. Table 11.7 estimates a probit regression of the event $Veh = 0$ on the age of the head of household, a dummy for high school and one for college education, and quintile dummies for disposable income and net worth. We find that older households are more likely to underreport. A sixty-five-year-old is 10 percent less likely to report a car transaction than a twenty-five-year-old. Higher education levels reduce underreporting compared to the omitted category of less-than-high school. As reported in column (3), higher income also reduces underreporting, but only the dummy for the middle income bracket is significant at conventional levels. Similarly, higher wealth also reduces underreporting, especially in the middle of the wealth distribution.

Table 11.8 Regression diagnostic—Car transactors

	(1)	(2)	(3)	(4)
Constant	103.8	175.1	158.9	155.4
	(14.1)	(35.6)	(48.8)	(48.7)
Survey (c^S)	0.672	0.660	0.868	0.678
	(0.047)	(0.100)	(0.175)	(0.128)
R-squared	0.347	0.253	0.429	0.231
Observations	386	130	35	95
Transact. in car reg.	N	Y	Y	Y
Restr. on vehicle in survey	N	N	= 0	< 0 or > 0

Note: The table reports results from OLS regressions of registry-based consumption on a constant and on survey-based consumption. The samples are the households surveyed in December. The last two rows indicate sampling restrictions. The sample contains 386 households with no car transactions in the registry and 130 households who bought (and possible also sold) a car in the month before they were surveyed (excluding five within-household transactions). Of those 130 households, 35 reported a zero value on the survey question on vehicle purchases (*Veh*), while 95 reported a positive or negative value.

A common feature for income and net worth is that the incidence of underreporting is U-shaped. When combined, education and wealth turn out to be the most significant explanatory variables. The pseudo R^2 is 10 percent in column (5). These effects are in line with intuition and indicate that the misreporting problem is more severe for wealth-poor, low-education, low-income, and older households. There remains substantial unexplained variation, as indicated by the low pseudo R^2.

Implications for Consumption. If a household fails to report an important purchase, such as a car, we would expect the match between survey- and registry-based consumption to deteriorate substantially. This is what we find in table 11.8. It reports the same regression as in equation (4), but splits the sample into those who did not transact a car according to the car registry (column [1]) with those who did buy or sell (columns [2], [3], and [4]). The first observation is that the fit between survey- and registry-based consumption deteriorates substantially for the subsample that does transact a car relative to the subsample that does not. The R^2 falls dramatically from 34.7 percent in column (1) to 25.3 percent in column (2).

Second, if we look at the households that do report a car transaction in the survey by answering a nonzero amount to the question on vehicle purchases, the fit deteriorates further to 23.1 percent (column [4]), and is much worse than for the households who do report a zero car transaction in the survey (column [3]). Third, the measure of survey bias $1 - \lambda$ increases from column (3) (13.2 percent) to column (4) (32.2 percent). In sum, even conditional on reporting of even salient items such as car purchases poses important problems for survey-based measures of consumption.

11.5 Conclusion

Faced with potentially severe measurement error problems in survey-based consumption, this chapter considers an alternative consumption measure derived from Swedish tax registries. Basically, we use detailed data on income, financial assets and housing, and debt to back out total annual consumption expenditures as a residual from the budget constraint. The unique feature of our data is that we observe the complete financial portfolio, which allows us to construct a household-specific portfolio return. The second important feature of the data is that we can match up the standard survey-based consumption measure and our registry-based measure for 5,134 households, surveyed between 2003 and 2007. A close comparison of both measures shows that registry- and survey-based consumption measures have the same hump-shaped life cycle profile, and that they have about the same average and median for renters. The survey-based measure understates consumption for homeowners, as well as for richer households, either measured by high net worth or high income. In the highest net worth quintile, the survey has 15 percent lower consumption, on average, while in the highest income quintile the gap is 24 percent. We also show that incorrectly approximating the portfolio return with a safe bond return leads to downward-biased consumption, especially for the wealthy. Further, approximating the portfolio return with either a stock market return or a safe bond return leads to too much consumption dispersion. We obtain a correlation between the survey- and registry-based consumption levels of 55.1 percent for our sample that combines all renters and owners surveyed in December. Similarly, a regression on registry-based and survey-based consumption illustrates that the two measures (for a given household) are far from perfectly correlated. Finally, we take a closer look at car purchases, a salient consumer item. We find that almost 30 percent of the car transactions go unreported in the survey, even though the car purchase or sale took place in the month before the survey. Reported purchase values in the survey also appear to understate the likely transaction value. Overall, the car evidence casts doubt on the quality of the interview component of the survey data.

While our exercise is hard to replicate in other countries for lack of sufficiently rich data, it nevertheless contains a number of important lessons for the measurement of consumption in the United States and elsewhere. First, surveyed consumption seems to suffer from substantial measurement error. Second, it understates consumption inequality. Third, it may be overstating consumption for low-wealth and low-income households somewhat, while substantially understating consumption of the rich. Fourth, using broad return measures instead of household-specific portfolio returns has substantial effects on the consumption distribution.

Appendix A

Registry Data: Details

LINDA

LINDA is a widely used data set in economic research. It is a joint endeavor between the Department of Economics at Uppsala University, The National Social Insurance Board (RFV), Statistics Sweden, and the Ministries of Finance and Labor. Edin and Fredriksson (2000) provide a detailed account of the data collection process for LINDA. More information on LINDA is also available from the websites of the Department of Economics, Uppsala University (http://nek.uu.se/), and Statistics Sweden (http://www.scb.se/).

LINDA is a panel data set that covers slightly more than 3 percent of the Swedish population annually. There are approximately 300,000 core individuals in the data set. The starting point for LINDA is a representative, random sample of the Swedish population in 1994, which has been tracked back to 1968 and forward to 2007. New individuals are added to the database each year to ensure that LINDA remains representative of the cross-section of Swedish individuals. In addition, the data set contains information on all family members of the sampled individual. Thus, LINDA covers all members of approximately 300,000 households in each year. The core of LINDA are the income registers (Inkomst- och Förmögenhetsstatistiken) and population census data (Folk- och Bostadsräkningen). Each wave of LINDA contains information on taxable income and social transfers (e.g., unemployment benefits) from the Income Registers in a given year. In addition, LINDA contains information on occupation, wages, and educational attainment from separate registers held at Statistics Sweden. We also use the wealth supplement of LINDA, which is available between 1999 and 2007. The wealth supplement contains information on the market value of houses, owned apartments (co-ops), cabins, plots of land, and other forms of real estate. It also reports the value of total debt and the value of student loans.

When Statistics Sweden compiles LINDA, it lacks the information to assign two people that belong to the same household but that are unmarried and without children. Such individuals are treated as two separate households. This leads to undersampling of this particular kind of household. Among the households that appear in the 2007 wave of the HBS, the number of adults reported in the HBS and the number of adults reported in LINDA agree for 85 percent of the observations.

Registry-Based Financial Asset Data

Sweden had a wealth tax in place until 2007. The Swedish tax authority, therefore, had the mandate to collect detailed information about each taxpayer's holdings of financial assets, such as bonds, stocks, and mutual

funds. The data collection took place through the financial institutions. The collected data also contains information on coupon income from bonds and interest income from bank accounts. Since 1999 these data have been delivered to Statistics Sweden, which uses it for constructing the wealth supplement of LINDA. In the raw data file, each financial security and fund is identified by its International Securities Identification Number (ISIN). In rare instances, the Swedish firm ID number is reported instead, requiring a careful matching procedure by hand. For an in-depth description of this component of the data, see Calvet, Campbell, and Sodini (2007, 2009) who used this data component for the period 1999 to 2002. After matching with LINDA, we have information on all asset holdings of the LINDA respondents.

We obtain separate data on the prices, dividends, and returns for each stock, coupons for each bond, and net asset values per share for each mutual fund in the database from Datastream and from MoneyMate. We match this price and cash flow information to the holdings in order to be able to compute total returns on each asset that each individual holds. This results in a close-to-complete picture of each household's wealth portfolio.

The data set contains limited information about two kinds of financial accounts. These accounts are private pension and "capital insurance" accounts. Both types are surrounded by special tax regulations. As a result, the detailed asset composition of these accounts (regular savings accounts, stocks, mutual funds, bonds, or some other kind of financial asset) is not known. For private pension accounts, we observe the annual withdrawal or contribution to the account. Like in the United States, such private pension accounts are used to defer labor income taxes between contribution and withdrawal dates. Every year the taxpayer can deduct approximately 12 kSEK, or about $1,800. One Swedish krona is $0.15 as of November 1, 2011. It fluctuates between $0.11 and $0.17 over our sample period. We use the abbreviation SEK to denote amounts in Swedish krona and kSEK to denote amounts in thousands of Swedish krona. For our purpose of constructing annual flows of consumption expenses, the pension account reporting does not pose a limitation. For capital insurance accounts, the account balance is reported, but it is impossible to accurately impute the rate of return since the holdings in this account are unobserved. For the purpose of imputing consumption, we have to make an assumption on that rate of return. According to Calvet, Campbell, and Sodini (2007), such savings made up 16 percent of the total financial savings in 2002, making this assumption neither crucial nor unimportant. We explore different assumptions below.

Data on the balances of households' bank accounts suffers from measurement error. Until 2004, positive balances are reported only if the interest income during that year was greater than 100 SEK (roughly $15). After 2004, the balance of a bank account is reported only if it is greater than 10 kSEK (roughly $1,500).

Housing Registry Data

Housing consists of (single-family) houses, tenant-owned apartments (co-ops), and second homes (cabins). We use the national real estate registry (Fastighetstaxeringsregistret) to gain information on real estate transactions. The information on ownership and valuation of houses and cabins is more accurate than that of apartments.

The real estate registry records every purchase or sale of a house or cabin, along with the transaction date. Transactions of co-ops, however, are not contained in the real estate registry. Co-ops are registered on the title deeds of the buildings as opposed to being assigned to the individual share owners, and there is no national registry for owners of shares in co-operations. Statistics Sweden therefore needs to infer co-op membership based on the official address of the household. This method causes mistakes when a household rents an apartment in a co-op and declares this as her primary address. Consequently, the true apartment owner will not get recorded as the owner of the co-op. A third type of misclassification would occur if an owner purchases or sells one of several co-op units. This transaction goes unrecorded unless the person also changes his or her official address. In 2004, the method used to identify owners of apartments was overhauled. The reform lead to a net change of 10,000 apartment owners in a total population of nine million Swedes and 900,000 apartment owners. (As part of the reclassification, 90,000 individuals were no longer classified as owners while 81,000 were newly classified as owners, a gross change of 19 percent of apartment owners, or 1.9 percent of the population.)

Houses and cabins are valued quite accurately in the registry because there is a real estate tax on them. The tax basis, that is, the registered property value used for tax purposes, is a function of a long list of characteristics of the property, and is updated frequently. Based on transactions during the year, Statistics Sweden computes the ratio of the tax value to the market value for each of Sweden's 290 municipalities and uses this value to assign market values for all houses and cabins. Average tax-to-market value ratios are around 0.5, but they vary over time and cross-sectionally. This method implies that the aggregate stock of houses and cabins is likely to be valued accurately. The registry data, however, do not include the actual transaction price of a property, only the market value (the market-value-adjusted, property-specific tax value). Thus, property-specific changes in market values that are not accurately reflected in the property-specific tax reassessments, as well as deviations of the transaction price from the market value, are sources of measurement error.

In contrast to the relatively accurate valuation of houses and cabins, there is no national effort to collect tax values on apartments that belong to a co-op. Statistics Sweden uses the average sale value of the apartments in a co-op in a given year to assign market values to all apartments in that

co-op, including to those apartments that were not transacted. However, if too few sales occurred at the co-op level, Statistics Sweden uses the average sale value in the parish instead for the imputation. This implies that there is too little variation in reported apartment values and that small apartments suffer from an upward bias in assigned values and large apartments suffer from a downward bias. Due to the inaccuracies that surround co-ops, we explore various alternative sampling restrictions described in appendix B.

From the registries, we also order a tailored dummy variable that registers whether an individual changes her official address. For the vast majority of people, the official address equals the primary residence. Some young people may rent a home on a short-term basis and may keep their official address at their parents' home. If a household member changes his or her address in the public registries, then the dummy variable takes on a value of one. The variable is helpful for identifying households that undergo a change in composition during the year (due to marriage, divorce, children moving away from home, etc.), but it is also helpful for identifying households who sell or purchase an apartment.

Car Registry Data

Finally, we add information from the car registry. Specifically, we obtain data on the characteristics of the cars that LINDA individuals purchased and sold between 1999 and 2007. Those characteristics are car brand, model (e.g., engine type, station wagon, etc.), manufacturing year, and reported mileage at the annual inspection of the car. Separately, we hand-collect data on prices of secondhand cars by brand, model, and mileage for a few common car brands (namely Audi, BMW, Mercedes, SAAB, and Volvo) from the Swedish equivalent of the Kelley Blue Book in the United States. Matching the pricing information to the LINDA data allows us to compare reported car purchases in the survey to imputed car purchases from the registry and car price data.

Household Budget Survey

Statistics Sweden produced the Household Budget Survey (Hush UTgifter) for the years 1999, 2000, and 2001. The data collection procedure was then overhauled and a new version of the survey started in 2003. The purpose of the revision was to better adhere to the guidelines of the European statistical agency, Eurostat. An important change in 2003 is that LINDA is used as the sample frame. Thus, in each LINDA wave after 2003, there is a subset of approximately 2,000 households for which we can match HBS and LINDA data. In contrast, it is not possible to identify the set of individuals and households that were surveyed in the years 1999 to 2001. Note that, in contrast to LINDA, the HBS is not a longitudinal database. Each household only appears once. As a result, the HBS does not allow for a construction of consumption growth for a household.

Table 11A.1 Data collection procedure for the Household Budget Survey

Week 50	A first letter with information is sent to subsample 1
Week 51	The first interview
	Household composition, occupation, type of home
	Purchased and sold furniture, refrigerators, microwave ovens, stoves, and other durable goods during the last twelve months
Week 52	Instructions
	Detailed instructions on the diary are given over phone
Weeks 1–2	Consumption diary
	Either the household performs the diary over fourteen days, or the household sends all the receipts to Statistics Sweden
Week 1	The second interview
	Expenses on primary residence and secondary residences such as cabins, phone, domestic services, child care, cars, insurances, and travels during the last twelve months
Week 1, 2	Follow-up phone calls
	The interviewer calls so that any issues concerning the diary can be solved
Week 3	The third interview
	Short questions about expenses. The questions are changed every quarter.
	The interviewer reminds the household to send the diary and any receipts
Week 3	Statistics Sweden receives the diary and any receipts

Notes: The table reports all the steps in the data collection procedure for the households who have been allotted to weeks 1 and 2 of the year. It is a reproduction from page five in the documentation of survey wave 2007, published on Statistics Sweden's website.

The HBS selects about 4,000 households, of which at least one member is between 0 and 79 years old. The response rate to the survey is about 50 percent, leaving it with a final sample of about 2,000 households each year. Data is collected via a consumption diary and a phone interview, and some auxiliary information is pulled from Statistics Sweden's registries. The sample is distributed equally over fifty-two weeks, marked by the first week of the diary, and the same procedure is used for each subsample. Table 11A.1 describes the data collection procedure for the subsample of households who keep a diary during the first two weeks in a year (weeks one and two of the calendar year).

Table 11A.2 reports summary statistics for the 2005 wave of the HBS by expense category (first column). The second column reports whether the data come from the consumption diary (D), the phone interview (I), or whether they are pulled from the registries (R). The 2005 wave consists of 2,079 households. All amounts are in current SEK (divide by seven to get approximate dollar values) and refer to annual expenditures. The first twelve rows denote the twelve (European-wide) consumption categories. Housing consumption (shelter, part of category 4) is measured as rent for renters and maintenance for homeowners. It *excludes* net mortgage interest expenses for owners because our measure of net capital income in the registry-based approach below also excludes this expense. Second homes (cabins) are treated

Table 11A.2 Summary statistics for the 2005 wave of the Household Budget Survey

	Source	Mean	Std.	Min.	Max.
01. Food and nonalcoholic beverages	D	38.9	22.0	0	348.0
02. Alcoholic beverages, tobacco, narcotics	D	6.1	8.8	0	65.0
03. Clothing and footwear	D	17.1	26.6	0	337.2
04. Housing, water, electricity, gas, etc.	I,D	51.0	33.6	0	662.2
05. Furnishings, household equipment, etc.	I,D	21.8	37.7	–55.0	690.6
06. Health	D	7.1	19.4	0	315.6
07. Transport	I,D	48.5	66.9	–155.3	699.7
08. Communications	I,D	9.8	7.4	0	156.3
09. Recreation and culture	I,D	43.3	49.7	–511.2	779.8
10. Education	D	68	923	0	27.0
11. Restaurants and hotels	D	12.3	17.8	0	231.4
12. Miscellaneous goods and services	I,D	21.8	43.5	0	1,827.0
13. Fees to unions, unempl. insurance, etc.	D,R	4.8	3.9	0	43.3
14. Taxes on vehicles	I,D	2.0	1.9	0	14.8
15. Donations	D	2.3	8.0	0	130.0
16. Cabins	I,R	2.5	9.2	0	195.4
17. Tax on benefits	R	1.9	6.5	0	63.7
18. Expenses outside of COICOP	I,D	0.5	7.5	–30.4	211.5
Total expenditure	I,D,R	295.9	164.2	–324.5	2,318.2

Notes: The expense categories follow the international COICOP standard. The number of households is 2,079. We define total expenditure as being equal to total expenditure as reported in the survey minus interest rate expenditure (COICOP category 22). As sources of the data, "D" indicates diary, "I" indicates interview, and "R" indicates registry. The registry-based expense items are: taxes on plots of land, houses and cabins, fees to labor unions, fees to unemployment insurance, and taxes that are paid for benefits received from the employer. Some households report expense items that do not fit into the COICOP standard. In such cases Statistics Sweden adds the expenses directly to total expenditure. These expenses are referred to as expenses outside of COICOP in the table. All amounts are in thousands of Swedish krona (kSEK).

analogously to primary residences and are reported separately (category 16). Transport (category 7) includes the net purchases of cars, which could be a negative number if the household sells a car but does not buy a new one in a given year. Likewise, recreation (category 9) includes the net purchases of boats—quite an important expenditure category in Sweden—which again can be negative. Finally, furnishings can also be negative if a household sells more furniture or equipment than it buys. As a result, survey-based consumption can be negative, and indeed it is for some households. Category 12 reports miscellaneous goods and services, such as hair dresser, parking tickets, funerals, bank fees, fees for ordering passports, and so forth. Categories 13, 14, 15, and 17 contain outlays on donations, vehicle taxes, taxes to unions, and taxes paid for benefits received, some of which are imputed from registries. Finally, row 18 measures other expenses that are outlays but that are not part of the harmonized European consumption expenditure standard (COICOP). Total consumption expenditure is the sum of all

these categories; it includes net outlays on consumer durables (which can be negative) and excludes mortgage payments for homeowners. It refers to the consumption flow over the twelve months prior to the week following the end of the interview. Total 2005 household consumption has a mean of 296 kSEK (or about $44,400), with a considerable standard deviation of about 165 kSEK or $24,600. The minimum value is –325 kSEK (–$48,700) and the maximum value is above 2.3 million SEK ($347,700).

Appendix B

Construction of Consumption in Registries: Details

Labor Income after Taxes and Transfers

The term y_t captures labor income minus taxes on labor income plus government transfers. We compute this variable by excluding capital income from all assets, net capital gains (gains minus losses) from financial assets, and net increases in student loans (increases minus decreases) from the disposable income variable. Table 11B.1 provides the details of this computation, which changes in 2004 due to a change in the definition of disposable income in 2004. Using the 1991 definition of disposable income for 2004 and beyond would not change the results much. The variable y includes rental income from renting out (primary or secondary) owned houses.

Net Change in Debt

The term $\Delta d_t = d_t - d_{t-1}$ equals the change in total debt from the end of year $t - 1$ to the end of year t. A positive value denotes an increase in the debt balance. Debt includes credit card debt, car loans, student loans, mortgages, and other kinds of debt. We do not have a breakdown of this debt in subcategories, except for student loans, which are reported separately. The total interest payment on all debt (the debt service), y_t^d, is directly reported in the tax registries. Interest expenses lower consumption. The registry-based debt service numbers are directly comparable to the corresponding debt service numbers in the household budget survey. Table 11B.2 reports summary statistics of these two variables for the same set of households, in thousands of SEK. The table shows that the survey tends to understate interest expenses. For high interest-expense households, the bias grows in absolute terms but attenuates in relative terms. Finally, note that we are subtracting mortgage expenses as part of subtracting total interest expenses. This is consistent with the budget survey where we also excluded mortgage expenses. The alternative treatment of (a) defining housing consumption as the sum of maintenance and mortgage expenses, as in a standard-user cost approach, in the survey and (b) not subtracting mortgage expenses in the

Table 11B.1 Computing labor income after taxes and transfers

1999–2003		2004–2007	
	y_t =		y_t =
Disposable income, 1991 def.	cdisp	Disposable income, 2004 def.	cdisp04
–total capital income	–kiranta	–total capital income	–kiranta
–increases in student loans	–ismlan	–increases in student loans	–ismlan
+decreases in student loans	+uater	+decreases in student loans	+uater
–net capital gains, if positive	–max((kv–kf),0)	–gross capital gains	–kvbrut
		+gross capital losses	+kfbrut

Table 11B.2 Interest expenses from tax records and the HBS (kSEK)

	Mean	Std.	P5	P25	P50	P75	P90	P95
Interest expenses in tax registry	27.7	38.8	0	1.1	15.6	40.0	71.0	95.2
Interest expenses in HBS	21.5	29.5	0	0	10.9	31.3	59.9	81.9

Notes: This table compares total debt service (interest expenses on all debt) from the tax registry and from the Household Budget Survey. The registry variable, y_t^d, is kakuru and comes from tax form KU25. The variable for total interest expenses in the HBS is u22. The comparison is for the same set of households. The numbers are in thousands of SEK.

registry-based imputation is not possible because we do not separately observe mortgage interest expenses in the registry data.

Bank Accounts

The term $\Delta b_t = b_t - b_{t-1}$ measures the change in bank accounts (checking, savings, certificates of deposit, etc). A decline in bank accounts increases consumption, ceteris paribus. Recall that in 2006 and 2007 the balance of every single bank account is reported if the balance is greater than 10,000 SEK. In prior years, the balance of a bank account is reported if the earned interest exceeds 100 SEK.

Stocks, Bonds, and Mutual Funds

The term $\Delta v_t = v_t - v_{t-1}R_t$ measures a household's active rebalancing of mutual funds, stocks, and bonds. The household-specific return on this portfolio excludes any distributions (dividends, coupons): $R_t = P_t / P_{t-1}$ where P_t is the end-of-year, ex-dividend price. The purchase of a new fund, stock, or bond reduces consumption while the sale of an existing one increases consumption, all else equal. When the household does not change its position in a given asset but passively earns an unrealized capital gain or takes a capital loss, that asset's contribution to Δv is zero. Realized capital gains and losses are reported for tax purposes as gains and losses relative to the original purchase price. Such gains or losses do not reflect consumption-relevant cash flows. Rather, what matters for the consump-

tion flow in a given period is the sale price of the asset rather than the difference between the sale price and the original purchase price. Our variable Δv captures the relevant capital gains and losses. Positive values for Δv reflect active increases in the financial asset position and translate in a reduction in consumption, unless they are offset elsewhere in the budget constraint. We compute income from financial assets, y_t^v, as the after-tax interest on bank accounts, coupons from bonds, dividends from stocks, and income from stock option contracts. (Total income from all financial assets is given by the variable kiranta minus four tax variables, skubank from tax form KU20 and kkuvpi, kkuvpr, and skkuvp from tax form KU21. Financial income adds to consumption, ceteris paribus.

Housing Wealth

Changes in housing wealth are given by Δh_t, which capture changes in primary residence (houses and apartments) and in second homes (cabins). Since the aim is to measure only cash flows, Δh_t differs from zero only if the household purchases or sells a house, apartment, or cabin. Parallel to the treatment of financial assets, Δh_t should reflect active rebalancing decisions and not unrealized capital gains or losses due to house price appreciation or depreciation. An increase in housing lowers consumption, unless offset elsewhere. Primary housing does not generate income. The shadow value of the housing services (rental equivalent) that the house provides is excluded both in registry- and survey-based consumption measures. If a household receives payments for renting out their second home, that rental income is measured as part of y_t. Note that, to the extent that households extract resources from their home equity through a second mortgage, cash out refinancing, or home equity line of credit, this is already captured in Δd_t .

To capture only active rebalancing on housing assets, as opposed to unrealized capital gains and losses, as well as to deal with the measurement issues in apartments described above, we set $\Delta h_t = 0$ unless at least one household member has purchased or sold a house or cabin according to the real estate registry, or unless the head of household changes her official address. A change in official address typically indicates a change of primary residence and allows us to capture active changes in ownership of co-ops that are used as primary residences. Because of measurement error in Δh_t, we also explore two sampling restrictions. In the first subsample, we exclude any household-year observations if the official address of any household member has changed in that year. Since the official address typically is equal to the address of the primary home, this set of restrictions is meant to allow households that have transacted secondary homes to remain in the sample. In a second stricter subsample, we additionally exclude household-year observations if any household member has purchased or sold any real estate according to the real estate registry that year. Effectively, the latter subsample only considers households with $\Delta h_t = 0$. These sampling restrictions offer a trade-off between maximizing sample size

and minimizing measurement error. (We also considered a third subsample where we included households who report a change in official address, but whose reported value of apartment holdings are zero in the two consecutive years. The intention was to allow households that had sold or purchased a house or cabin to remain in the sample. However, since co-ops are a common form of primary housing, we lose about half the sample, and decided therefore not to report results for this subsample.) As the sampling restrictions discussed below will clarify, our main results are for the strictest subsample.

Capital Insurance Accounts

The so-called *capital insurance accounts* are savings vehicles that receive special tax treatment. Assets held in such accounts are subject to a flat 1 percent tax rate on the account balance, rather than the standard 30 percent capital gain and dividend income taxes. (To be precise, the tax rate fluctuates somewhat from year to year. It is equal to 27 percent of the average government bond yield during the year. This yield is reported every week by the Swedish National Debt Office.) Households may change the portfolio allocation within such accounts and reinvest the financial income spun off by the assets in the account, but may not withdraw funds lest they incur penalties. In our data, the account balance is reported, but the allocation to regular savings accounts, stocks, mutual funds, bonds, or some other kind of financial asset is unknown. The net change to this kind of account is imputed by $\Delta\psi_t = \psi_t - \psi_{t-1}R_t^{\psi}$, where R_t^{ψ} is the cum-dividend return on the portfolio of assets. We assume that the return on these accounts, R_t^{ψ}, equals the cum-dividend return on the all-share Stockholm Stock Exchange.[15] A decrease in account balances leads to an increase in consumption, all else equal.

Pension Accounts

For private pension accounts, we observe new contributions and withdrawals. Since withdrawals from private pension accounts are taxed as labor income, they are already included in income, y_t. Contributions to private pension accounts, denoted by ω_t, are reported separately in the registries and enter equation (2) as reduction in consumption.

Appendix C

Sampling Restrictions

We impose the following ten sampling restrictions on this set of matched households. Table 11C.1 lists the impact of each to the overall size of the sample.

15. We use the index SIXRX.

Table 11C.1 Sample exclusions

Type of restriction	Observations
0. Full sample	10,705
1. Excl. instable households over time (in terms of household head, number of adults)	9,711
2. Excl. farmers and entrepreneurs	8,937
3. Excl. households with inconsistent homeownership status in registry and survey	8,052
4. Excl. households who change official address or transact real estate	7,207
5. Excl. households who hold derivatives	7,078
6. Excl. households who hold securities with missing ISINs	6,965
7. Excl. households who hold mutual funds or stocks with missing prices or returns	5,283
8. Excl. households who have extreme portfolio returns (top and bottom 1 percent)	5,253
9. Excl. households who have big changes in net worth (top and bottom 2.5 percent)	5,135
10. Excl. households with negative surveyed consumption	5,134

First, we remove households whose composition changes between year ends $t - 1$ and t, leaving us only with households with a stable composition. These restrictions concern the household head and the number of adults in the household. The household head is defined as the oldest male if this person is at least twenty-one years old, otherwise the oldest female if there is a female who is at least twenty-one years old, otherwise the oldest person in the household. The household head must remain the same in two consecutive waves and the number of adults (age twenty-one or older) must remain the same. This restricts the sample to 9,711 households.

Second, we exclude farmers as well as households who report more than 50 kSEK (around $7,500) in income from an own business in the registries. For self-employed households, personal and business expenditures are hard to separate, making a consumption imputation somewhat meaningless. This restricts the sample to 8,937 households.

Third, we require that households who are homeowners (renters) in the registries report to be homeowners (renters) in the survey. A homeowner (renter) in the registries is defined as a household who has positive (zero) housing wealth (i.e., apartment, house, or cabin) according to the wealth supplement of LINDA. This restriction reduces the sample to 8,052 households. These restrictions are also imposed in a similar exercise on Danish data by Browning and Leth-Petersen (2003).

In addition, we impose a set of restrictions that are aimed at mitigating potential measurement errors in households' asset changes. The fourth restriction in table 11C.1 implements the strictest criterion on changes in housing wealth, discussed in appendix B. In particular, we exclude households who change the official address or who transact a house or cabin according to the registries. This restricts the sample to 7,207 households. We explore below how our consumption measurement changes if we only exclude those who change official addresses or if we exclude neither category.

Fifth, we exclude 7,078 households where a household member owns any derivative product (including own-company stock options), which are hard to value correctly.

Sixth, we require exact identification of the entire financial asset portfolio, that is, no reported holding can have a missing ISIN in the raw data. This implies a drop of 113 households.

Seventh, we require that we carry both prices and returns for each holding of the household's portfolio. Although we are able to match nearly 95 percent of all asset positions, the restriction that all of a household's positions must be identified implies that we lose an additional 1,682 households. Approximately 600 of those are lost due to a particular harsh restriction—we require that in the case of multiple versions of a given mutual fund with the same ISIN (such as a retail version with one kind of fee structure and another version offered within the pension segment) we can establish which version of the fund that is the correct match or that the NAVs per share do not deviate more than by 15 percent from each other (in unreported results, we have verified that this restriction could in fact be relaxed).

Eighth, we drop households for which the calculated financial asset return (the portfolio of stocks, bonds, and mutual funds) is in the tails of the distribution. The lower truncation point is at the bottom 1 percent of the return distribution, while the upper truncation point corresponds to the top 1 percent of the return distribution. Specifically, the top restrictions are 111 percent (2003), 64.3 percent (2004), 67.6 percent (2005), 49.0 percent (2006), and 28.1 percent (2007). The bottom percentile restrictions on household returns are –99.9 percent (2003), –99.9 percent (2004), –99.9 percent (2005), –99.9 percent (2006), and –99.9 percent (2007). The remaining sample has 5,253 observations.

Ninth, a small number of households experience a dramatic change in net worth from one year to the next. This could happen for many reasons, among which are bequests or intervivos transfers from family members, which we do not observe. We choose to exclude households if the change in net worth is in the bottom 2.5 or in the top 2.5 percent of the corresponding year-specific distribution. At percentile 2.5, the change in net worth in thousands of SEK is as follows: –866 (2003), –663 (2004), –751 (2005), –616 (2006), and –719 (2007). At percentile 97.5, the change in net worth is 1,058 (2003), 1,116 (2004), 1,504 (2005), 1,468 (2006), and 1,397 (2007). This eliminates 118 observations.

Tenth, we delete one household for which the surveyed consumption is negative.

The final sample consists of 5,134 households, or about one thousand households per survey year on average. Of these, 1,487 are renters (29 percent) and 3,647 are homeowners (71 percent). The homeownership rate in our sample matches the rate in the Swedish population at large.

Appendix D

Wealth Distribution

Table 11D.1 reports summary statistics of the wealth distribution by year. The sample is all 5,134 households in our sample.

Table 11D.1 **Wealth distribution**

	2003	2004	2005	2006	2007
Percentile 5	–276	–280	–318	–257	–239
Percentile 10	–153	–136	–174	–124	–112
Percentile 20	–52	–31	–43	–7	0
Percentile 50	252	375	405	486	613
Percentile 80	1,061	1,274	1,412	1,551	1,903
Percentile 90	1,765	1,952	2,158	2,424	2,907
Percentile 95	2,437	2,582	2,940	3,528	3,995
Observations	1,053	1,143	1,035	936	967

Notes: The table reports summary statistics of the Swedish wealth distribution. Our measure of wealth is household net worth, measured as financial assets plus (primary and secondary) houses minus all debt. The sample is all 5,134 households in our sample. All numbers are expressed in thousands of Swedish krona (kSEK).

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