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

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Abstract

We construct a new consumption measure as a residual from the budget constraint. Consumption is that part of income from labor, transfers and taxes, and from financial assets, housing, and debt and of assets that is not reinvested. Our measurement relies on extremely detailed data of income and of the composition of households' asset portfolio from Swedish registries, collected by the government as part of the tax assessment process. As such, the registries are essentially free from measurement error. The richness of the data allow us to impute a household-specific portfolio return, which is important to arrive at an accurate consumption measure with our method. The data allow us to match households that are surveyed with a standard European-wide Household Budget Survey with our data set, allowing a detailed comparison of the two consumption measures. We find that the survey-based measures understate consumption for home-owners, for high-income, and for high-wealth households. They appear unbiased for renters and slightly understate consumption for the youngest and poorest in our sample. Taken together, the survey understates consumption inequality. Separately, Swedish car registry data on car transactions clearly indicate reporting biases in the survey.

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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 with limited asset market participation. The answer to the questions of interest, and therefore our understanding of consumption behavior, may well depend on such accurate measurement. 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). The pattern observed in the data changes depending on the exact source of consumption data that is used.

But 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 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, for example of consumer durables, over the past year in a separate interview in addition to the diary.

In the U.S, 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.[1] In the U.K., 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.

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

[1] 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.

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.[2] Measurement error is often found to be non-classical (Bound, Brown, and Mathiowetz, 2001; Pudney, 2008). The measurement error in household-level consumption data, and the difficulty of estimating non-linear 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 paper, 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 provide 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 home ownership and households' permanent address. Finally, the Swedish data also contains information on labor, transfer, and financial income. The resulting series is a measure of total consumption (including durables), measured at annual frequency.

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 allows us to compare registry-imputed and survey-based measures of consumption between 2003 and 2007 for thousands of households. We study that comparison for different income and wealth levels. We are particularly interested in the question of whether surveys accurately measure consumption for

[2]See Battistin, Miniaci, and Weber (2003); Browning, Crossley, and Weber (2003); Battistin (2004); Gibson (2002) among others.

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 particularly relevant in light of the fact that most household budget surveys under-sample the rich. Our registry-based approach does not suffer from this under-sampling. We uncover discrepancies between registry- and survey-based consumption measures that increase with wealth. We study how sensitive registry-based consumption is to an accurate imputation of returns that households are earning on their assets. We find that incorrectly applying a broad total return measure to a households' financial asset holdings leads to substantial deviations from the properly imputed registry measure. 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.

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, Johannesson, Lichtenstein, Sandewall, and Wallace, 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, Heyman, Nilsson, Svaleryd, and Vlachos, 2009), we are the first to compute a measure of consumption based on Swedish income and asset data.

The rest of this paper is organized as follows. Section 1 describes our Swedish data set. Section 2 describes how we construct registry-based consumption. It discusses the measurement components, as well as a few sources of measurement error. Section 3 describes the properties of our new registry-based measure of consumption in detail. It reports consumption by home ownership status, age, wealth, and income. 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. It presents a model of measurement error for survey- and

registry-based consumption. Section 4 study car transactions as an external validation tool for the survey data. Section 5 concludes with lessons for survey-based consumption measurement.

1 Data

Our analysis compares registry-based and survey-based consumption measures between 2003 and 2007. The foundation of the registry-based data is LINDA (Longitudinal INdividual DAta for Sweden). We added detailed registry-based data on individuals' asset holdings. Our survey-based measure is from the Swedish Household Budget Survey (HBS). 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. Sections 1.1 to 1.5 describe the three 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 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 so-called capital insurance accounts, for which we observe the account balance but not the asset composition. 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.

1.1 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.[3] It is a panel data set that covers slightly more than three percent of the Swedish population annually. There are approximately 300,000 core individuals of 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 under-sampling 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.

[3] Edin and Fredriksson (2000) provide a detailed account of the data collection process for LINDA. More information on LINDA is also available from the web sites of the Department of Economics, Uppsala University (http://nek.uu.se/), and Statistics Sweden (http://www.scb.se/).

1.2 Registry-based Financial Asset Data

Sweden had a wealth tax in place up until 2007. The Swedish tax authority had therefore the mandate to collect detailed information about each tax payer's holdings of financial assets, such as bond, 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).[4] 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 U.S., such private pension accounts are used to defer labor income taxes between contribution and withdrawal dates. Every year the tax payer can deduct approximately 12 kSEK, or about $1,800.[5] 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.

[4] 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.

[5] 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.

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).

1.3 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.[6]

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

[6] 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% of apartment owners or 1.9% of the population.

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 which 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 section 2.3.3.

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 remain 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.

1.4 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 second-hand 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 U.S. 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.

1.5 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. To the best of our knowledge, a similar match has only been made before by Browning and Leth-Petersen (2003) on Danish data for 1994 to 1996. 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.

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%, 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 52 weeks, marked by the first week of the diary, and the same procedure is used for each subsample. Table I describes the data collection procedure for the subsample of households

Table I: Data Collection Procedure for the Household Budget Survey

The table reports all the steps in the data collection procedure for the households who have been alloted to week 1 and 2 of the year. It is a reproduction from page 5 in the documentation of survey wave 2007, published on Statistics Sweden's website.

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 12 months.
Week 52	Instructions
	Detailed instructions on the diary are given over phone.
Week 1-2	Consumption diary
	Either the household performs the diary over 14 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 12 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

who keep a diary during the first two weeks in a year (week 1 and 2 of the calender year).

Table II 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 7 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 home owners. 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 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, also furnishings can 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, etc. 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 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).

2 Constructing Registry-based Consumption

This section describes our approach to impute consumption expenses. We focus on the households that appear at least in the LINDA data set between 1999 and 2007 for at least two consecutive years. We explain how the information on income, asset holdings, and asset returns is combined to arrive at imputed consumption expenditure. Section 3 then compares the imputed consumption expenses to reported total consumption expenditure in the Household Budget Survey.

The main idea to impute consumption using Swedish registry data is to use the household budget constraint. Consumption of household i in year t is given by:

$$c_{it} = y_{it} + d_{it} - (1 + r^d_{it})d_{it-1} - a_{it} + a_{it-1}(1 + r^a_{it}) \tag{1}$$

Table II: Summary Statistics for the 2005 Wave of the Household Budget Survey

Note: 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 and fees to unemployment insurance and taxes that are paid for benefits received from the employer. Some households report expense items which 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).

	Source	Mean	Std	Min	Max
01. Food and non-alcoholic 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. Miscellanous 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

where y_{it} denotes household i's labor income after taxes and transfers 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. Each component in (2) is defined below. All amounts are denoted in real terms (with base year 2005), where the deflator is Swedish consumer price index.

2.1 Labor Income after Taxes and Transfers

The term y_t captures labor income minus taxes on labor income plus transfers from the government. 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 III provides the details of this computation, which changes in 2004 due to a change in the definition of disposable income in 2004.[7] The variable y includes rental income from renting out (primary or secondary) owned houses.

2.2 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

[7] Using the the 1991 definition of disposable income for 2004 and beyond would not change the results much.

Table III: 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 IV: Interest expenses from tax records and the HBS (kSEK)

Note: 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.

	Mean	Sd	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

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 IV 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.[8]

[8]The alternative treatment of (i) defining housing consumption as the sum of maintenance and mortgage expenses, as in a standard user cost approach, in the survey and (ii) not subtracting mortgage expenses in the registry-based imputation is not possible because we do not separately observe mortgage interest expenses in the registry data.

2.3 Financial Assets

2.3.1 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.

2.3.2 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 consumption 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.[9] Financial income adds to consumption, ceteris paribus.

[9]Total income from all financial assets is given by the variable 'kiranta' minus four tax variables. The tax variables are skubank from tax form KU20 and kkuvpi, kkuvpr, and skkuvp from tax form KU21.

2.3.3 Housing wealth

Changes in housing wealth are given by Δh_t, which capture changes in primary residencies (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 receive 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 which 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 tradeoff between maximizing sample size and minimizing measurement error.[10]

[10]We 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 which 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 loose about half the sample, and decided therefore not to report results for

As the sampling restrictions, discussed below, will clarify, our main results are for the strictest subsample.

2.3.4 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% tax rate on the account balance, rather than to the standard 30% capital gain and dividend income taxes.[11] 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.[12] A decrease in account balances leads to an increase in consumption, all else equal.

2.3.5 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.

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

this subsample.

[11] To be precise, the tax rate fluctuates somewhat from year to year. It is equal to 27% of the average government bond yield during the year. This yield is reported every week by the Swedish National Debt Office.

[12] We use the index SIXRX.

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%. The resulting number of matched household-year observations in our sample is 10,734. 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.

3.1 Sampling Restrictions

We start from a sample of 10,734 households. Recalling that each household is only present in one HBS wave, the number of household-year observations equals the number of households. We impose the following nine sampling restrictions on this set of matched households. Table V lists the impact of each to the overall size of the sample.

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 21 years, otherwise the oldest female if there is a female who is at least 21, otherwise the oldest person in the household. The household head must remain the same in two consecutive waves and the number of adults (aged 21 or older) must remain the same. This restricts the sample to 9,780 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 9,017 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 7,395 households. Most of 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 V implements the strictest criterion on changes in housing wealth, discussed in section 2.3.3. In particular, we exclude households who change official address or who transact a house or cabin according to the registries. This restricts the sample to 7,276 households. We explore below how our consumption measurement changes if we only exclude those who change official address or if we exclude neither category. Fifth, we exclude 119 households where a household member owns any derivative product (including own-company stock options), which are hard to value correctly. Sixth, we require that exact identification of the entire financial asset portfolio (a perfect match on ISIN code for all stocks and mutual funds the household owns). Although we are able to match more nearly 95% of all asset positions, the restriction that all of a household's positions must be identified implies that we loose an additional 1,372 households. Seventh, we drop households for which the calculated financial asset return (the portfolio of stocks, bonds, and mutual funds) is extreme. The lower truncation point is a net return of -100%, which corresponds to about the bottom 1% of the return distribution, while the upper truncation point is year-specific and corresponds to the top 1% of the return distribution.[13] The remaining sample has 5,813 observations. Eighth, 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 bequests or inter-vivos 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.[14] This eliminates 117 observations. Ninth, we delete one household for which the surveyed consumption is negative. The final sample consists of 5,695 households, or about 1,140 households per survey year on average. Of these, 1,531 are renters (27%) and 4,164 are owners (73%).[15]

[13]Specifically, the top restrictions are 111% (2003), 64% (2004), 98% (2005), 57% (2006), and 36% (2007).

[14]At percentile 2.5, the change in net worth in thousands of SEK is as follows: -953 (2003), -740 (2004), -755 (2005), -751 (2006), -568 (2007). At percentile 97.5, the change in net worth is 1,061 (2003), 1,175 (2004), 1,549 (2005), 1,487 (2006), and 1,702 (2007).

[15]The home ownership rate in our sample matches the rate in the Swedish population at large.

Table V: Sample Exclusions

Type of restriction	Observations
0. Full sample	10,734
1. Excl. instable households over time (in terms of household head, number of adults)	9,780
2. Excl. farmers and entrepreneurs	9,017
3. Excl. households with inconsistent home ownership status in registry and survey	8,301
4. Excl. households who change official address or transact real estate	7,395
5. Excl. households who hold derivatives	7,276
6. Excl. households who hold unidentified mutual funds or stocks	5,904
7. Excl. households who have extreme portfolio returns (top and bottom 1 percent)	5,813
8. Excl. households who have big changes in net worth (top and bottom 2.5 percent)	5,696
9. Excl. households with negative surveyed consumption	5,695

3.2 Timing of Survey

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. As Table I details, 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, etc. Thus, survey consumption conceptually refers to the 52 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 calendar (registry) year. Thus, 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 as late in the calendar year as possible. Our main comparison therefore focuses on households surveyed in December. It contains 597 households, of which 173 renters and 424 home owners.

3.3 Summary Statistics

Tables VI and VII 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 three reports the moments of the distribution of the difference between registry- and survey-based measures (not the difference of the moments). Column four scales that difference by median registry-based consumption. Column 5-8 are analogous to Columns 1-4 but focus on the subset of households interviewed in December, a group for which the individual measurement comparison ought to be much closer (though not necessarily the moments of the distribution).

Renters Starting with the 1,531 renters, we find average consumption of 217 kSEK imputed consumption (about $32,600), and basically identical to the survey mean of 216 kSEK. The standard deviation is slightly higher in the registry- than in the survey-based measure (132 versus 120 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 $25^{th}$and 50^{th}percentiles, and higher consumption form the 75^{th}percentiles of the consumption distribution onwards. For example, the 75^{th}percentile of imputed consumption is 286 kSEK compared to 267 kSEK in the survey while the ninety-fifth percentile is 441 for the registry- versus 420 kSEK for the survey-based measure. Despite these differences, the two consumption distributions line up remarkably well for renters. Even the 99^{th}percentiles differ by less than $5,000 on a consumption of $89,200. Columns 5 and 6 report the same statistics but for the subset of 173 renters surveyed in December. While the December sample is obviously much smaller (the first and 99^{th}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 4,164 homeowners in Table VII, we find average consumption of 337 kSEK imputed consumption (about $50,475), and noticeably above the survey mean of 302 kSEK, about a $5,250 difference. The log difference is 10.8%. The average consumption of home

Table VI: Summary Statistics for Renters

Note: Column 3 and 7 report the distribution of the difference between survey-based and registry-based consumption measures. Column 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.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variable	Registry	Survey	Diff.	Rel. Diff.	Registry	Survey	Diff.	Rel. Diff.
Mean	217.4	216.2	-1.22	-0.01	218.4	219.7	1.37	0.01
Std	132.4	119.9	135.8	0.70	130.6	111.1	125.9	0.64
Percentile 1	-32.2	15.2	-323.3	-1.66	-175.1	16.6	-288.8	-1.79
Percentile 5	77.6	58.5	-186.3	-0.96	59.8	75.5	-176.6	-1.47
Percentile 25	134.3	134.8	-64.0	-0.33	130.0	133.2	-63.7	-0.90
Percentile 50	189.1	194.8	-11.6	-0.06	192.4	196.4	-12.3	-0.32
Percentile 75	286.3	267.0	49.9	0.26	313.2	264.5	58.8	0.30
Percentile 95	441.3	420.0	199.7	1.03	438.4	457.7	242.9	1.24
Percentile 99	594.5	561.2	407.3	2.09	615.8	525.4	478.0	2.43
Survey Month	1-12	1-12	1-12	1-12	12	12	12	12
Observations	1,531	1,531	1,531	1,531	173	173	173	173

owners is 43.7% higher than that of renters in the imputation, compared to 33.5% 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 200 kSEK in the registry- versus 156 kSEK in the survey-based measure. The fifth percentile of the consumption distribution is lower in the registry-based measure (92 versus 108 kSEK), the median is higher (321 kSEK versus 275 kSEK) but the 95^{th} percentile is considerably higher (651 versus 575 kSEK). The 99^{th} percentiles of the two consumption distributions differs by 19.3% (965 versus 795), the equivalent of $25,000. Columns 5 and 6 report the same statistics but for the subset of 424 owners 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.

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

Table VII: Summary Statistics for Homeowners

Note: Column 3 and 7 report the distribution of the difference between survey-based and registry-based consumption measures. Column 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.

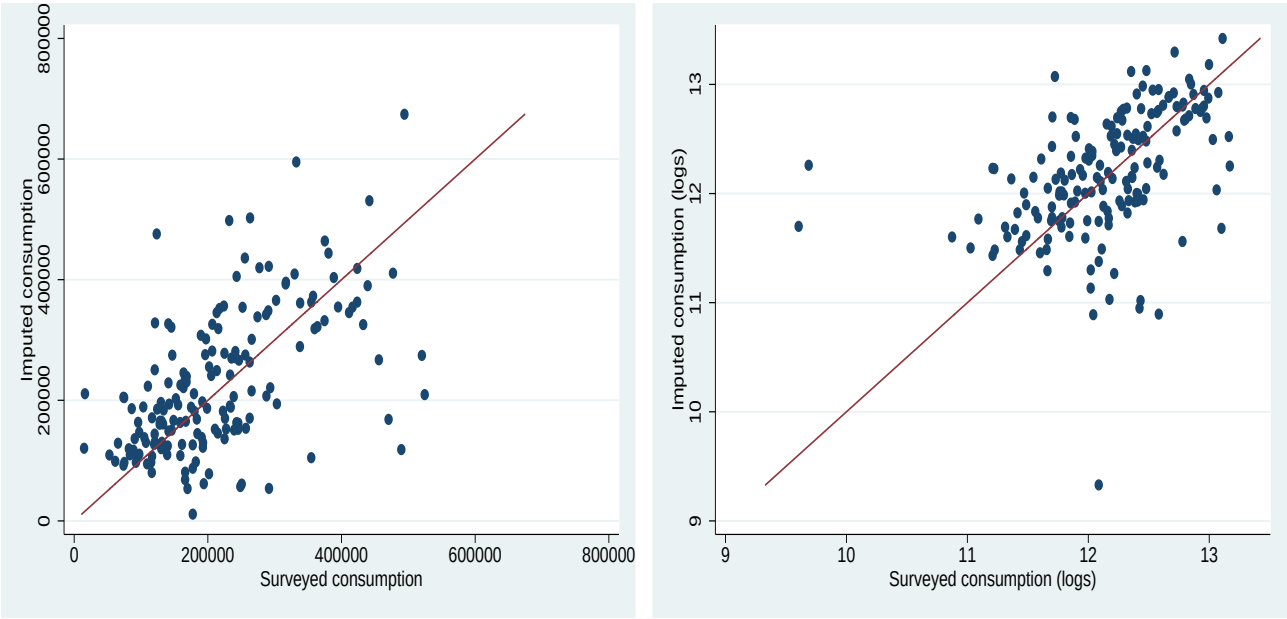
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variable	Registry	Survey	Diff.	Rel. Diff.	Registry	Survey	Diff.	Rel. Diff.
Mean	336.5	302.1	-34.4	-0.125	344.6	329.8	-14.8	-0.049
Std	199.9	156.1	196.9	0.717	192.5	173.5	171.2	0.572
Percentile 1	-105.7	71.4	-599.6	-2.18	-117.9	83.1	-549.0	-1.84
Percentile 5	91.9	108.0	-306.5	-1.12	105.7	126.8	-280.8	-0.94
Percentile 25	210.7	198.8	-118.8	-0.43	230.3	215.5	-97.3	-0.33
Percentile 50	320.6	274.7	-37.4	-0.14	331.4	299.1	-17.3	-0.06
Percentile 75	430.7	377.0	49.2	0.18	445.6	397.9	69.3	0.23
Percentile 95	651.3	574.6	253.4	0.92	671.5	634.1	257.7	0.86
Percentile 99	964.7	795.5	502.3	1.83	1041.3	878.7	455.0	1.52
Months	1-12	1-12	1-12	1-12	12	12	12	12
Observations	4,164	4,164	4,164	4,164	424	424	424	424

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 truth, then the relative differences are a direct measure of the bias. 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 VI show that while the average difference is essentially zero, its standard deviation is substantial at 126 kSEK or about sixty percent of median consumption. The difference ranges from -177 kSEK at the $5^{th}$to 243 kSEK at the 95^{th}percentiles, or between -0.9 and 1.2 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 (-0.33) for the 25^{th}percentile, -0.01 (-0.07) at the median, 0.28 (0.27) at the 75^{th}percentile, and 6.66 (4.07) 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 that the distribution of deviations looks quite similar for the full sample and the December sub-sample. In part, of course, this is because the full sample is much bigger and less sensitive to outliers.

Figure 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 173 December home owners (including those with negative consumption) is 46.9%. Extending the sample to all 1,531 home owners reduces the correlation to 42.3%, most likely due to the timing misalignment issue alluded to above.

Figure 1: Survey- versus Registry-based Consumption for Renters

The left panel plots survey-based consumption in levels (horizontal axis) against registry-based consumption in levels (vertical axis) for the group of 173 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.



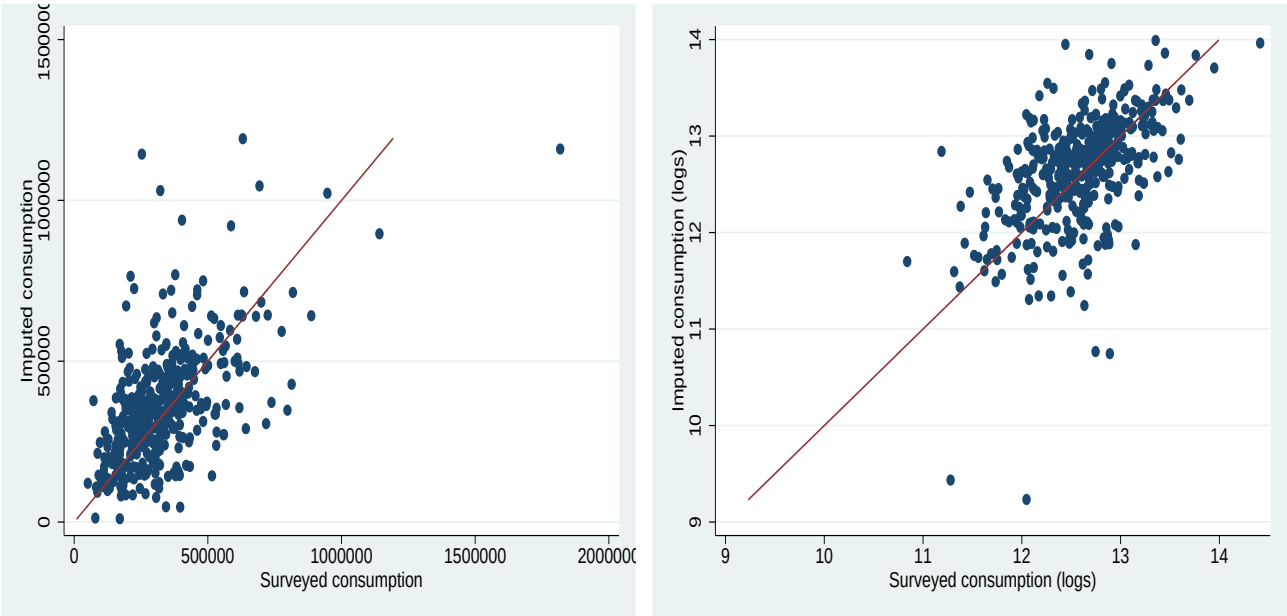
For homeowners, the standard deviation of the deviation between survey- and registry-based measures is 171 kSEK or 57% of median survey-based consumption. The difference ranges from -281 kSEK at the $5^{th}$to 258 kSEK at the 95^{th}percentiles, or between -0.9 and 0.9 times median consumption, similar 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 (-3.0) for the minimum, -0.29 (-0.33) for the

25^{th}percentile, -0.02 (-0.06) at the median, 0.26 (0.23) at the 75^{th}percentile, and 10.7 (2.6) at the maximum. We conclude that our Swedish registry-based measure appear 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 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 plot excludes seven owners with negative imputed consumption. The correlation between the consumption-measures in levels for all 424 December home owners (including those with negative consumption) is 57.3%. Extending the sample to all 4,164 home owners reduces the correlation to 45.3%, most likely due to the timing misalignment issue alluded to above. Combining all renters and owners surveyed in December leads to correlation between the survey- and registry-based consumption levels of 60%, while the full sample of 5,695 households results in a correlation of 45.3%.

Figure 2: Survey- versus Registry-based Consumption for Homeowners

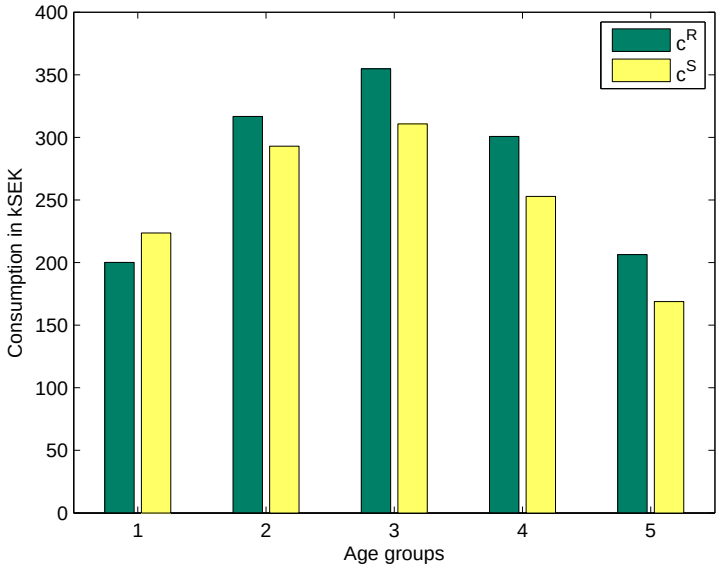
The left panel plots survey-based consumption in levels (horizontal axis) against registry-based consumption in levels (vertical axis) for the group of 424 homeowners surveyed in December. The right panel plots survey-based consumption in logs (horizontal axis) against registry-based consumption in logs (vertical axis) for the same group of households. For the purpose of this figure, we eliminated seven observations with negative consumption since their log consumption is not defined. The solid line is the 45-degree line.



Consumption by Age Figure 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. The percentage difference between the two consumption measures increases monotonically in age. For the 25-year olds, registry-based consumption is 4.8% below survey-based consumption. For the 26-40 year olds, it is 3.4% above that in the survey. That positive difference further rises with age and peaks at 8.7% for the 71 and older group. To the extent that older households are also wealthier, this is consistent with the consumption-by-wealth discussion we turn to next.

Figure 3: Survey- versus Registry-based Consumption by Age

The figure plots survey-based consumption in levels and registry-based consumption in levels for different age groups. Group 1 has households whose head is less than 25 years old (200 observations), group 2 is aged 26-40 (1,709 obs.), group 3 is aged 41-55 (1,929 obs.), group 4 is aged 56-70 (1,271 obs.), and group 5 is aged 71 and older (497 obs.). The total sample is 5,606 observations (5695 households minus 89 households with negative registry-based consumption).



3.4 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.

Wealth Distribution Table VIII reports summary statistics of the wealth distribution by year. The sample is all 5,695 households in our sample. In 2007, the $10^{th}$of net worth is negative indicating debt outstripping assets, the $90^{th}$is almost 3,000 kSEK, the equivalent of $445,000, and the $95^{th}$is more than 3,800 kSEK or $575,000.

Table VIII: Wealth Distribution

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 5,695 households in our sample. All numbers are expressed in thousands of Swedish krona (kSEK).

	2003	2004	2005	2006	2007
Percentile 5	-264.8	-280.1	-308.0	-256.1	-249.2
Percentile 10	-139.7	-134.3	-158.1	-121.3	-112.9
Percentile 20	-37.2	-25.1	-34.9	-1.44	3.13
Percentile 50	290.4	405.4	458.2	535.2	600.3
Percentile 80	1,151.4	1,393.9	1,554.3	1,661.1	1,860.3
Percentile 90	1,832.6	2,092.3	2,304.6	2,591.5	2,960.8
Percentile 95	2,601.5	2,744.7	3,038.4	3,735.8	3,837.1
Observations	1,115	1,236	1,125	1,039	1,1180

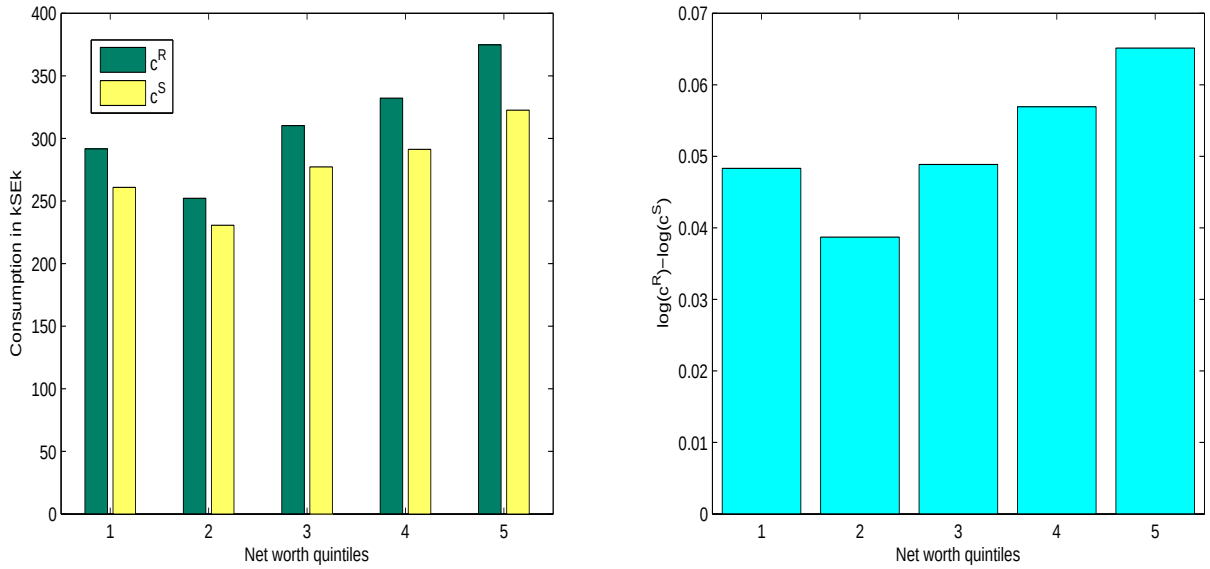
Consumption by Wealth We sort all 5,606 households with positive registry-based consumption into wealth quintiles, ranked from lowest to highest. The left panel of Figure 4 is a bar chart of average survey- and registry-based consumption for each of these wealth deciles. It shows that, other than a decline from wealth quintile 1 to 2, consumption increases in wealth, but that registry-based consumption is steeper in wealth. The gap between the two consumption measures increases from 21 kSEK in quintile 2 to 52 kSEK in quintile 5 ($3,200 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 3.9% for quintile 2 to 6.5% for quintile 5.[16] In other words, the survey understates consumption, and the understatement is substantially larger for the wealthy.

As an aside, the slight non-monotonicity in wealth we observe between quintiles 1 and 2 is

[16]Median, as opposed to average, consumption is 50 kSEK or 7% higher in the registry- than in the survey-based measure for quintile 5.

Figure 4: Survey- versus Registry-based Consumption by Wealth

The left panel plots average survey-based consumption in levels (blue bars) and registry-based consumption in levels (red bars) for ten groups of households surveyed in December 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 nine observations with negative consumption since their log consumption is not defined. The sample for this figure contains 5,606 households (5695 households minus 89 households with negative registry-based consumption).



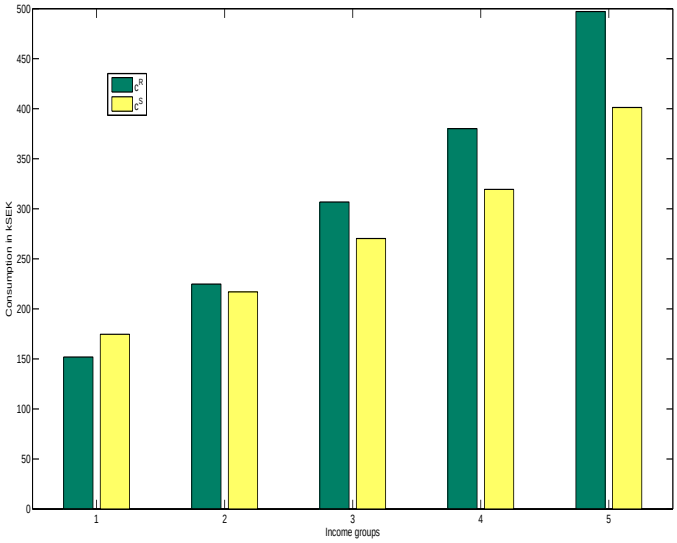
mostly due to student loans. If we exclude student loans from net worth, the non-monotonicity all but disappears for survey- and registry-based consumption measures. The log difference between the two measures retains its non-monotonicity.

Consumption by Income We obtain a similar picture when we study consumption by income. Figure 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 by age. 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 (1.5%) and increases further with income to 9.3% 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, and are omitted for brevity.

Figure 5: Survey- versus Registry-based Consumption by Income

The figure 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 total sample is 5,606 observations (5695 households minus 89 households with negative registry-based consumption).



3.5 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 emulate our approach for other countries, 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). 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.[17]

Table IX reports survey- and registry-based consumption measures for all 597 households, owners 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 99^{th}percentile of the wealth distribution. The median consumption is too high by 9 kSEK, the average by 7 kSEK. Using a 50-50 mix of stocks and bonds to proxy for the household-specific return leads to understatement of consumption for the top half of the consumption distribution. The bias in the median (mean) is -8 kSEK (-5.5 kSEK). Finally, using the bond return as a proxy leads to a severe understatement across the board, with median and mean consumption too low by 20 kSEK. Consumption at the $95^{th}$and 99^{th}percentiles is off by 48 kSEK and 235.4 kSEK; the latter is almost $35,000. Using the all-bond return also leads one to underestimate the true dispersion in consumption, while using the all-stock return leads one to overstate the consumption dispersion.

3.6 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.

$$c_{it}^R = \alpha + \beta c_{it}^S + \varepsilon_{it} \tag{3}$$

The regressions fit the best straight line through the cloud of points reported in the left panels of Figures 1 and 2. Table X reports the results. Column 1 is for the December sample of 169 renters

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

Table IX: Effect of Portfolio Returns on Consumption

Note: The table reports survey- and registry-based consumption measures for all 574 households, owners 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).

	(1)	(2)	(3)	(4)	(5)
Variable	Survey	HH portf	Stocks	Stock-Bond	Bonds
Mean	292.8	308.0	314.9	302.5	288.5
Std	161.7	185.8	195.9	183.4	181.0
Percentile 1	65.5	-133.3	-121.9	-149.8	-262.3
Percentile 5	104.6	87.1	91.4	84.3	69.0
Percentile 25	181.6	178.5	182.9	178.2	166.6
Percentile 50	262.6	290.3	299.3	282.7	270.9
Percentile 75	367.8	397.9	403.4	396.9	388.2
Percentile 95	583.7	640.2	656.3	626.1	592.1
Percentile 99	813.4	1,022.4	1,008.7	950.1	787.0
Survey Month	12	12	12	12	12
Observations	597	597	597	597	597

with positive consumption, Column 2 is for the December sample of 417 owners with positive consumption, and Column 3 is for the combined December sample of 586 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 0.63 and an R^2 statistic of 33% for renters. For owners, the slope and R^2 are nearly identical at 0.62 and 34%, respectively. The R^2 is the highest for the full 586 sample with a value of 39%.

If there is 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 119 kSEK, or about $17,800. Panel B runs the same regressions but between consumption measured in logs. The slope coefficient for renters drops significantly to 0.46, but that for owners remains largely unaffected. The R^2 statistics drop to 21% for renters, 29% for owners, and 33% for the full sample. 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 non-trivial measurement error in one or both measures. We return to measurement error below.

Table X: Regression Diagnostic

Note: The table reports results from 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 sample are the households surveyed in December. We delete eleven observations with negative registry-based consumption, four renters and seven owners. The last two columns of the table report regression results if the sampling restrictions on housing transactions are relaxed.

	(1)	(2)	(3)	(4)	(5)
	Panel A: Consumption in Levels				
	Renters	Owners	All	Owners	Owners
Constant	90.5	150.2	118.9	150.8	153.2
	(16.8)	(15.8)	(11.8)	(15.9)	(16.7)
c^S	0.631	0.622	0.673	0.622	0.630
	(0.07)	(0.043)	(0.035)	(0.043)	(0.045)
R-squared	0.331	0.337	0.386	0.331	0.309
	Panel B: Consumption in Logs				
	Renters	Owners	All	Owners	Owners
Constant	6.54	4.59	4.56	4.69	4.60
	(0.853)	(0.617)	(0.472)	(0.620)	(0.622)
$\log(c^S)$	0.465	0.639	0.638	0.632	0.640
	(0.070)	(0.049)	(0.038)	(0.049)	(0.049)
R-squared	0.209	0.291	0.326	0.281	0.280
Observations	169	417	586	423	434
Change in official address	N	N	N	N	Y
Transaction of house or cabin	N	N	N	Y	Y

The last two columns of Table X enlarges the sample by including households who bought or sold 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 owners sample shows that the correspondence between survey- and registry-based consumption does not materially deteriorate once we include house purchasers or sellers or movers.

Table XI explores the effect on the regression diagnostics of wealth and of the use of household-specific portfolio returns. Panel A of Table XI studies regression results of equation (3) 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 (20^{th}-80^{th}percentiles), and Columns 5 and 6 for the top of the wealth distribution. Moving from Column 3 to 5, we notice

that while the slope coefficients are stable, the R^2 statistic increases noticeably from 29.6% to 41.8%. It increases further to 50.3% for the richest 60 households.

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 registry-based consumption measures that have the same relationship to surveyed consumption, with one exception. For the very wealthy (top 10% of the wealth distribution), the R^2 is substantially lower when we incorrectly use the broad stock return measure. Panel C shows that using the bond return leads to uniformly worse associations between survey- and registry-based consumption measures.

Table XI: Regression Diagnostic - Effect of Wealth and Portfolio Return

Note: For home owners, the most restrictive sample restrictions were used (no change in official address, no transaction of house or cabin). The ranges of net worth are reported in SEK in table VIII. Panel A uses the framework of equation (2) to impute consumption. Panel B uses a modified version of the framework which sets $y_t^v = 0$ and replaces the household-specific return R_t by SIXRX, the gross index of the Stockholm Stock Exchange. In panel C the term $y_t^v = 0$ and the household-specific return is assumed to equal a one-year government bond yield.

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Household-specific return			
Constant	118.6	105.6	127.0	122.2	143.1	114.6
	(11.8)	(38.4)	(26.5)	(15.0)	(34.7)	(42.1)
c^S	0.673	0.816	0.668	0.620	0.677	0.693
	(0.035)	(0.127)	(0.093)	(0.049)	(0.076)	(0.085)
R-squared	0.386	0.393	0.292	0.319	0.419	0.540
			Panel B: Stock return			
Constant	112.9	104.6	126.1	127.4	140.8	117.6
	(12.5)	(38.3)	(26.4)	(14.7)	(40.2)	(56.1)
c^S	0.716	0.823	0.675	0.611	0.753	0.796
	(0.037)	(0.126)	(0.092)	(0.048)	(0.088)	(0.113)
R-squared	0.390	0.399	0.297	0.322	0.399	0.466
			Panel C: Bond return			
Constant	136.1	109.8	129.7	122.8	137.4	70.1
	(12.5)	(38.7)	(26.5)	(14.9)	(40.6)	(48.1)
c^S	0.547	0.802	0.656	0.581	0.511	0.526
	(0.037)	(0.128)	(0.093)	(0.048)	(0.089)	(0.097)
R-squared	0.271	0.382	0.284	0.294	0.231	0.342
Observations	586	66	128	347	111	59
Range for net worth	P0-P100	P0-P10	P0-P20	P20-P80	P80-P100	P90-P100

3.7 Model of Measurement Error

To make sense of these regression results, as well as to understand the properties of survey- and registry-based consumption better more broadly, we now formulate and estimate a model of measurement error. The previous results, where we sort consumption by income or wealth, suggest that the survey-based consumption understates true consumption for the wealthy. We write down a model for measurement error that allows for such a bias in survey-based consumption.

[TBC]

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.[18] 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 we discussed above contains data on every purchase and sale of cars. The Household Budget Survey asks households about its *net purchases* of vehicles (V), further broken down into cars (C), motorcycles, bikes, and other vehicles. Net purchases are the difference between purchases and sales as measured over the past 12 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 or sold a car *in the month*

[18] Battistin (2004) investigate the accuracy between the Diary and Interview samples in the U.S. 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).

before they took the survey. This results in a sample of 754 purchases and 617 sales among the 5,569 households. We then ask what those same households report in the survey about these car transactions.

Table XII breaks this sample into car purchasers according to the registry (truth) in Panel A and car sellers in Panel B. In case of multiple purchases or sales in the registry, we require that the first purchase occurred before the month of the survey. The table reports net purchase expenditures on vehicles (V) and on cars (C), as reported in the survey. Although there is a separate category for cars in the registry, we choose to report results also 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 sub-item "cars".[19]

Implicit in our analysis is the assumption that, if at least one transaction has occurred, then V and C should not be equal to zero. The first three columns of Panel A show that only 75% of survey respondents report a vehicle purchase if indeed a car purchase occurred, while 22.7% report a zero purchase value. For the sub-question that asks about net car purchases, we only find 68.7% positive responses and 30.1% zero responses (Columns 5 and 6).[20] Likewise, in Panel B, we find that 67.9% of the sellers report a negative net purchase in the survey, but that fully 27.4% report a zero net sale. That number increases to 37.8% for net car purchases in Column 6. We conclude that there is substantial underreporting to the tune of at least 30% of respondents. This is a disturbingly high number, especially for such a salient item as car transactions.

Characteristics of Under-reporters Next, we ask what household-level characteristics are related to this under-reporting problem. We run a probit regression of the event $V = 0$ on the age of the head of household, a dummy for high school and one for college education, disposable income, and net worth. For the latter two continuous variables we include a set of dummy variables which indicate the quintile position in the cross-sectional distribution. The sample is the same as in Table

[19]In the COICOP standard, transactions of vehicles is defined by item U071 and transactions of cars by its sub-item U0711.

[20]The results are similar when we confine attention to a group of households that bought one car and sold no car. For this group of 213 households, 24.4% report a zero value for V and 31.4% report a zero value for C. 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 XII: Car Transactions in Survey versus Registry

Note: The table reports the number of observations and the mean value of survey item net purchase of vehicles (V) and net purchase of cars (C) for different subsamples. Panel A considers the households for which at least one car purchase has been recorded in the car registry in the twelve months prior to the survey month of the household. Panel B considers the households for which at least one car sale has been recorded in the car registry. 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). Note that most households both buy and sell a car and hence appear in both panels. In sum, there are 928 unique households.

	Panel A: Car buyers according to the car registry					
	(1)	(2)	(3)	(4)	(5)	(6)
	$V<0$	$V>0$	$V=0$	$C<0$	$C>0$	$C=0$
Mean	-44.4	83.4	0	-54.6	90.2	0
Observations	17	566	171	9	518	227
Fraction of obs.	2.3%	75.0%	22.7%	1.2%	68.7%	30.1%
	Panel B: Car sellers according to the car registry					
	(1)	(2)	(3)	(4)	(5)	(6)
	$V>0$	$V<0$	$V=0$	$C>0$	$C<0$	$C=0$
Mean	-42.2	84.9	0	-38.4	97.3	0
Observations	29	419	169	20	364	233
Fraction of obs.	4.7%	67.9%	27.4%	3.2%	59.0%	37.8%

XII. The resulting sample is 928 households who buy or sell a car according to the registry. We find that older households are more likely to under-report. A 65-year old is 16% less likely to report a car transaction than a 25-year old. Higher education levels reduce under-reporting compared to the omitted category of less-than-high-school. College-educated households have a 13% lower incidence of underreporting. Higher income also leads to lower under-reporting. In the top two quintiles of income, the incidence of under-reporting is 8.8% to 10.7% lower. Notably, wealth has a different effect than income. Households in quintile 3 and 4 are most diligent in reporting, while households in quintile 1 and 5, the tails of the distribution, are the most likely to not report. So whereas the incidence was linear in income, it is U-shaped in wealth. When combined, age and wealth turn out to be the most significant explanatory variables. The pseudo R^2 is 3.4% in Column 5. These effects are in line with intuition and indicate that the misreporting problem is more severe for wealth-poor and -rich households but also for low-education, low-income, and older households. There remains substantial residual uncertainty, as indicated by the low pseudo-R^2.

Table XIII: Which Households Under-report?

Note: Probit regressions of the form $Pr(V = 0) = \alpha + \beta X_i + \epsilon_i$. The sample of households in the regressions is the total of panel A and B in table XII (notice that since most households buy and sell a car the 754 buyers in panel A and the 617 sellers in panel B sum to 928 households). The table report marginal effects. Disposable income and net worth have been standardize to mean zero and unit variance. (*) indicates significance at the 10%-level, (**) indicates significance at the 5%-level and (***) indicates significance at the 1%-level.

	(1)	(2)	(3)	(4)	(5)
Age	0.004***	-	-	-	0.0040***
	(0.001)	-	-	-	(0.0013)
D(High school)	-	-0.119**	-	-	-0.053
	-	(0.050)	-	-	(0.053)
D(College)	-	-0.131***	-	-	-0.042
	-	(0.049)	-	-	(0.055)
Disp. income, 2nd quintile	-	-	0.025	-	0.040
	-	-	(0.057)	-	(0.059)
Disp. income, 3rd quintile	-	-	-0.037	-	-0.011
	-	-	(0.051)	-	(0.054)
Disp. income, 4th quintile	-	-	-0.088*	-	-0.052
	-	-	(0.047)	-	(0.051)
Disp. income, 5th quintile	-	-	-0.107**	-	-0.070
	-	-	(0.046)	-	(0.052)
Net worth, 2nd quintile	-	-	-	-0.068	-0.065
	-	-	-	(0.041)	(0.042)
Net worth, 3rd quintile	-	-	-	-0.100**	-0.093**
	-	-	-	(0.039)	(0.040)
Net worth, 4th quintile	-	-	-	-0.098**	-0.099**
	-	-	-	(0.039)	(0.041)
Net worth, 5th quintile	-	-	-	-0.030	-0.044
	-	-	-	(0.045)	(0.048)
Year effects	Yes	Yes	Yes	Yes	Yes
Observations	928	928	928	928	928
Pseudo R-squared	0.016	0.012	0.017	0.014	0.034

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 XIV. It reports the same regression as in equation (3), 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 2-4). The first observation is that the fit between survey- and registry-based consumption deteriorates substantially for the sub-sample that does transact a car relative to the sub-sample that does not. The R^2 falls dramatically from 45.7% in Column 1 to 24.2% in Column 2. Second, if we look at the households that under-report a car transaction

Table XIV: Regression Diagnostic - Car Transactors

Note: The table reports results from OLS regressions of registry-based consumption on a constant and on survey-based consumption. The sample are the households surveyed in December. The last two rows indicate sampling restrictions. The sample contains 406 households with no car transactions in the registry and 180 households who bought or sold a car in the month before they were surveyed. Of those, 50 reported a zero value on the survey question on car purchases (V), while 130 reported a positive or negative value.

	(1)	(2)	(3)	(4)
Constant	105.7	167.9	195.0	150.1
	(11.8)	(29.7)	(47.8)	(38.6)
Survey (c^S)	0.667	0.623	0.559	0.663
	(0.036)	(0.083)	(0.159)	(0.102)
R-squared	0.457	0.242	0.204	0.249
Observations	406	180	50	130
Transaction in car registry	N	Y	Y	Y
Restriction on V in survey	N	N	$= 0$	< 0 or > 0

in the survey -by answering zero to the question on vehicle purchases-, the fit deteriorates further (Column 3), and is worse than for the households who do report a non-zero car transaction in the survey (Column 4). We conclude that the under-reporting of even salient items such as car purchases poses important problems for survey-based measures of consumption.

Imputation of car prices In addition to the incidence of underreporting established sofar (extensive margin), we also investigate the extent of reporting bias (intensive margin). In order to increase the sample size for the imputation, we do not apply restriction 6 to 8 of Table V. We focus attention on those households who buy just one car and sell no car. We use price statistics of used cars, obtained from the Swedish car dealers' association to impute prices for a few car brands (Volvo, Saab, Audi, BMW, and Mercedes).[21] The imputed prices are a function of brand, model, manufacturing year, and mileage. In the car registry, mileage is recorded at every annual inspection at the Swedish Motor Vehicle Inspection Company (SWVIC). Because of the stringent data requirements, we end up with a small sample of 37 matches of used car transactions for the brands we selected. Table XV compares the imputed price, based on the official value according to SMVIC, and the reported value in the survey. If we look at all 37 survey responses, we obtain

[21] *Motorbranschens riksförbund*, see http://www.mrf.se/. These data come in the form of magazine pages in hard copy and were imputed in machine-readable form by hand.

Table XV: Intensive margin: Surveyed versus Imputed Car Expenditures

Note: The table reports the number of observations and the mean value of survey item net purchase of vehicles (V) and net purchase of cars (C) for different subsamples. Panel A considers the households for which at least one car purchase has been recorded in the car registry in the twelve months prior to the survey month of the household.

	(1)	(2)	(3)
Mean of V	70.9	87.5	92.8
Mean of C	67.1	82.7	88.6
Mean of imputed price	97.0	93.8	97.3
Observations	37	30	28
Sample restriction	N	$V>0$	$C>0$

a large bias of 30 kSEK or $4,500 in surveyed car expenditures. This is the combination of an extensive and intensive margin effect. If we focus on the 28 households who also report a positive car purchase, the bias goes down to 9 kSEK or $1,300. While these results could be due to households transacting below the official used-car price or due to unobserved characteristics of the car, not captured by brand, make, vintage, and mileage, it is nevertheless notable that the survey again understates the registry-based value. Unreported results show that a regression of the net purchase of vehicles as measured in the survey (V) on our imputed car value result in a slope of 0.50 and an R^2 of 17% for the sample of 37 households. For the 28 households who indicated a positive car purchase in the survey ($C > 0$), the regression of the net car purchase value on our imputed car value results in a slope estimate of 0.62 and an R^2 of 42%. We conclude that even for a salient consumption expenditure, like a car purchase, survey-based and registry-based measures of expenditures differ widely. A large part of this discrepancy is due to non-reporting of car purchases, but even for those who do report, the surveyed and official used car values differ substantially.

5 Conclusion

Faced with potentially severe measurement error problems in survey-based consumption, this paper studies an alternative consumption measure, based on tax registries for Sweden. We use detailed data on income, financial assets and housing, and debt to back out consumption as a residual

from the budget constraint. This measure refers to annual, total consumption expenditures (but excluding imputed rent for homeowners). The unique feature of our Swedish data is that we observe the financial portfolio of a household and that we can therefore use the correct 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 5,700 households, surveyed between 2003 and 2007. A close comparison of both measure shows that registry- and survey-based 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 7% lower consumption, on average, while in the highest income quintile, the gap is 9%. We show that incorrectly approximating the portfolio return with a safe bond return leads to downward-biased consumption, especially for the wealthy, and too little consumption dispersion. We obtain a correlation between the survey- and registry-based consumption levels of 60% for our sample that combining all renters and owners surveyed in December. Similarly, a regression on registry-based on survey-based consumption illustrates that the two measures (for a given household) are far from perfectly correlated. We formulate and estimate a model of measurement error to make sense of these results [TBD]. Finally, we take a closer look at car purchases, a salient consumer item. We find that about 30% 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. 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 U.S. 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 household somewhat, while understating consumption of the rich substantially.

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