

## ASSESSING THE QUALITY OF SELF-REPORTED FINANCIAL INFORMATION

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It is well known that self-reported financial information differs from administrative records. This article advances previous studies by using a unique matched representative sample of individual borrowers from the Chilean Household Finance Survey with administrative banking loan records. Our linked dataset allows us to test whether the differences between the two sources are due to the number of nonreported loans or to differences in the reported loan amounts. We show that discrepancies in debt ownership are larger when respondents have a mortgage and are not the highest income member of the family, when respondents have weak financial literacy and do not have confidence in the study, and when respondents live in complex or multigenerational households. However, borrowers report the maturity of their mortgage and installment loans quite accurately. Concerning loan amounts, differences between the two sources decrease with the financial literacy of the respondent. Finally, a simple form of rounding can go a long way towards explaining differences in the middle part of the debt amount and maturity distributions.

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### Statement of Significance

Our paper examines how self-reported loan information differs from the administrative loan records at a microlevel. It is one of the most granular validation analyses of the financial self-reported information in survey datasets. Thanks to our rich dataset, we analyze how the quality of survey information differs for households according to their education and financial literacy, age profile, and household complexity (i.e., households in which several financial decision-makers coexist in the same household).

## 1. INTRODUCTION

Survey organizations face many challenges in their efforts to produce high-quality survey data given the level of funding and the specific research questions the survey data would like to answer (Yang, Fricker, and Eltinge 2019). According to the Total Survey Error framework, there are five main potential sources of error that can influence survey-based estimates: (1) sampling error, (2) sample coverage error, (3) nonresponse error, including both unit and item nonresponse, (4) measurement error, and (5) processing error (Groves and Lyberg 2010; Davern, Meyer, and Mittag 2019). This paper focuses on one of these sources, which is measurement error. Specifically, our objective is to improve our understanding of measurement error in household finance surveys by providing microdata evidence on how households' self-reported financial information differs from their administrative records. Our study takes advantage of a novel matched dataset between the Chilean Household Finance Survey and the complete banking administrative records.

This linked dataset considerably improves upon previous studies because it is not simply a comparison between aggregate debt amounts in survey data and administrative records (Zinman 2009; Brown, Haughwout, Lee, and van der Klaauw 2015); instead, it enables us to conduct a granular validation analysis of the self-reported financial information in survey datasets (Maynes 1968; Bucks and Pence 2008). The matched database allows us to investigate whether the differences between the two sources are due to the number of non-reported loans or to differences in the reported loan amounts; how the quality of survey information differs for households according to their education and financial literacy, age profile, and willingness to participate in the survey. Also, we examine the effect of rounding on the accuracy of the survey reports.

Our linked dataset provides the respondents' entire banking loan contracts (including mortgages, consumer installment loans, credit cards, and credit lines) for the period 2003 until 2018, as well as the respondents' self-reported information on loans (with either banking or nonbanking institutions) for each survey year (the survey interviews took place in 2011, 2014, and 2017). The matched dataset also includes survey-reported measures of income, assets, age, and education for both the interviewee and other household members (partner/spouse, parents, children, or siblings).

This paper is organized as follows. Section 2 reviews the literature and details the hypotheses explaining the differences between the survey and the administrative records, whereas section 3 describes the data and methods. Section 4 analyses the discrepancies in debt ownership, while section 5 focuses on the discrepancies in loan amounts. Section 6 concludes with a discussion of the methodological implications of our results. The appendix presents additional details about the survey methodology and questionnaire. [Supplementary material](#) detailing public access to the survey is available.

## 2. LITERATURE REVIEW AND HYPOTHESES

### 2.1 Literature Review

Household finance surveys, such as the Household Finance Consumption Survey in Europe or the Survey of Consumer Finances in the United States, are one of the most important sources of information for policymakers and researchers ([Meyer and Mittag 2019](#)). As some illustrations, surveys are increasingly being used to study families' decisions on savings, investments, and borrowing ([Christelis, Georgarakos, and Haliassos 2013](#); [Bover, Casado, Costa, Caju, McCarthy, Sierminska, et al. 2016](#)) and to assess households' over-indebtedness and the financial risks of the household sector ([Zinman 2009](#); [Beck et al. 2015](#); [Cifuentes, Margaretic, and Saavedra 2020](#); [Meriküll and Room 2020](#)). However, comparative studies of the aggregate amounts of household liabilities in surveys and national accounts ([Zinman 2009](#); [Brown et al. 2015](#); [Dettling, Devlin-Foltz, Krimmel, Pack, and Thompson 2015](#)) find a significant degree of underreporting of loans in household surveys, especially for student loans ([Brown et al. 2015](#)) and short-term debt ([Karlán and Zinman 2008](#); [Zinman 2009](#)). Our paper adds to this literature by investigating differences in debt ownership and loan amounts between self-reported financial information and administrative records at the microlevel.

This paper is also related to studies that link survey and administrative records at the individual level. [Maynes \(1968\)](#) focus on savings and loans; [Bound and Krueger \(1991\)](#), [Kapteyn and Ypma \(2007\)](#), [Pedace and Bates \(2000\)](#) examine earnings; [Alvarez, Guiso, and Lippi \(2012\)](#) investigate risk preferences, demographics, and financial asset management; [Ameriks, Caplin,](#)

Lee, Shapiro, and Tonetti (2015) and Neri and Monteduro (2013) focus on wealth; Eggleston and Reeder (2018) investigate asset income, among others. In particular, Maynes (1968) is, to our knowledge, the first and one of the few previous papers working with debt data. Our work adds to this literature by testing possible hypotheses explaining the differences between the survey and the administrative records at individual debt type level. Testing these hypotheses is important as it allows us to extract conclusions on the ways we can improve the quality of self-reported financial information.

## 2.2 Hypotheses

This paper's objective is to provide microdata evidence on how households' self-reported financial information differs from their administrative records. We hypothesize that there are four main reasons behind these discrepancies.

First, differences between the two sources may arise due to weak financial literacy of the respondents (Disney and Gathergood 2013; Lusardi and Tufano 2015; Crossley, Schmidt, Tzamourani, and Winter 2021). Financial literacy refers to the ability to manage personal financial matters in an efficient manner, which includes the knowledge of making appropriate decisions about personal finance; the understanding of various financial concepts; and the ability to use available services and tools (Hung, Parker, and Yoong 2009). From these three elements, we would expect the understanding of various financial concepts to be the most relevant aspect to explain discrepancies between the survey and the administrative records. This is probably because respondents without a good understanding of financial concepts like consumer debt or types of financial service providers (all else equal) are more likely to incorrectly report the debt type, debt provider, owner of the debt, or loan amount, which in turn would create a discrepancy between the two sources (Disney and Gathergood 2013). For instance, a financially illiterate respondent might mistake a consumer loan from a retail store for a consumer loan from the corresponding retail bank. There are three companies in Chile that own both a retail store and a bank specialized in consumer credit. Therefore, it is possible that some borrowers confuse loans from a bank with a retail store of the same name. Our first hypothesis hence becomes:

*Hypothesis 1: Differences in debt ownership and/or in loan amounts between the survey and the administrative records are likely to be smaller if the respondent is financially literate.*

Second, we conjecture that differences between the two sources may occur if respondents do not have enough confidence in the study. If respondents are concerned about being identified by their responses, or if they have a general aversion to sharing financial information, even anonymously, these respondents may not want to provide accurate responses (Barceló 2006; Wenemark, Persson, Noorlind Brage, Svensson, and Kristenson 2011; Gideon, Helppie-

McFall, and Hsu 2017; Eggleston and Reeder 2018). For instance, respondents with privacy concerns may *deliberately* abstain from reporting a certain loan or they may choose to incorrectly report the debt provider, the debt owner or they may misreport the loan amount. The second hypothesis becomes:

*Hypothesis 2: Differences in debt ownership and/or in loan amounts between the survey and the administrative records are likely to be smaller if the respondent has good confidence in the survey.*

Third, differences in debt ownership between the two sources may arise if the respondent does not correctly report the household member who is legally responsible for the loan. For instance, the respondent might be reporting as owning a loan that actually belongs to another household member who signed the loan contract, or conversely, the respondent may not report a loan as their own, wrongly believing that its owner is another household member.

The likelihood of a respondent misreporting the debt owner is probably larger if the respondent is not the highest income member of the household (Brown et al. 2015). This is because banks give loans based on individuals' credit scores and income. Therefore, members of the household without the largest income are less likely to ask for a loan in the name of the household. Also, we hypothesize that this effect should be more prevalent for mortgages, since the probability of getting a mortgage is strongly tied to the individual's income.

Furthermore, the likelihood of a respondent misreporting a loan should be larger if the respondent lives in a multigenerational or complex household, that is, a household that contains members that are not part of the nuclear family, such as senior members living with the nuclear family of one of their offspring (Kim and Waite 2016) or a multigenerational family living in the same home (Browning, Crossley, and Winter 2014). This is because there are in principle more members that could potentially be the owners of the debt. As a result of the above, we make the following two testable hypotheses

*Hypothesis 3a: The likelihood of differences in debt ownership is larger if the respondent is not the highest income member of the household.*

*Hypothesis 3b: The likelihood of differences in debt ownership is larger if the respondent lives in a complex household.*

Last, differences in loan amounts may arise if respondents round their answers. Survey responses to open-ended quantitative questions frequently display strong patterns of heaping on rounded numbers, with heaping occurring when respondents show a preference for these rounded numbers (often those divisible by 5 or 10, Gideon et al. (2017); Holbrook, Anand, Johnson, Cho, Shavitt, and Chávez et al. (2014)). Response heaping may be the result of taking cognitive shortcuts to make question answering easier, and as such, it may be a form of survey satisficing (Krosnick 1991, 1999; Narayan and Krosnick 1996;

Tourangeau, Rips, and Rasinski 2000; Schaeffer and Presser 2003; Holbrook et al. 2014). While investigating the reasons for heaping is out of the scope of this paper, in this hypothesis we aim at investigating the evidence for rounding behavior in households' self-reported financial information. Our fourth testable hypothesis then becomes:

*Hypothesis 4: Differences in loan amounts between the survey and the administrative records are likely to be smaller if we round the administrative records.*

There is one additional point to make regarding our expectations about the various outcome variables. In this paper, we do not make any *ex ante* conjecture on the outcomes. Precisely, hypotheses 1, 2, and 3 are valid both for the possibility that a debt appears only in the survey or only in the administrative records. Similarly, hypotheses 1, 2, and 4 hold both for the respondents under- or overreporting their financial information, relative to the administrative records. It is hence an empirical question to determine whether a given hypothesis explains differences in debt ownership and/or in loan amounts and to what extent. Our empirical design will address these points.

### 3. DATA AND METHODS

#### 3.1 The Matched Survey-Administrative Dataset

We use the 2011, 2014, and 2017 national waves of the Chilean Household Finance Survey. Each sampled household has one member who is selected as the interviewee, with this member being the person with the greatest knowledge of the family finances or the highest income. The survey elicits demographic, net wealth, asset, debt, and income information for the household or all the household members when corresponding. The survey is representative of the national urban population after expansion weights are applied to each unit (Banco Central de Chile 2018a). The survey is a cross-sectional interviewer-administered survey. It contains a mix of open-ended and closed-ended questions. As regards the debt questions, the respondent answers questions about the household and/or all household members. [Appendix A.1](#) details the methodology and the structure of the survey questionnaire, whereas [appendix A.2](#) provides details of the debt questions. The American Association for Public Opinion Research response rate for the 2011, 2014, and 2017 survey waves are 40 percent, 64 percent, and 65 percent, respectively.

To obtain a more accurate view of household's indebtedness, the Banking Authority linked each survey response to the administrative records. The administrative credit information includes all the people who have ever used a banking credit product; it is available from January 2003 to December 2018. The match between the survey responses and the administrative records is

possible thanks to the Chilean national identity number that survey respondents are asked to provide. Precisely, the link between the survey and the administrative records is done at respondent (thanks to the national identity number) and type of credit (mortgages, installment, credit card, and credit line loans) level. Hence, if, for a given respondent, the survey and/or the administrative records register more than one loan per type of credit, loans are consolidated as if they were one. This way, we sum all the loans in each debt category reported by a respondent and then compare it to the sum of all loans in those categories for the administrative register.

Finally, to avoid the influence of disparities between the survey responses and the administrative records, for the match, we look for the closest administrative record to each survey response over a time window of two months around the interview date. Precisely, let  $t^S$  be the date of the survey interview and  $t^A$ , the adjusted administrative record date. Hence,  $t^A = t^S + k$ , with  $k = -2, -1, 0, 1, 2$ . The reason for including such a time window is to account for situations where, for instance, borrowers ask for a new loan at the end of a month, say month  $t_1$ , but because the respondents do not remember the exact date the loan was granted, they report it in the survey as a loan at  $t^S = t_1 + 1$ , whereas the bank registers it at  $t_1$ .

There are five remarks to make about the Survey-Administrative matched dataset. First, the universe is limited to individuals who have used a banking product; therefore, it does not include loans from retail stores, unions, or other lenders. Second, the matched dataset provides information on the original loan amount at the time the contract was made and its maturity. However, it does not include information on loan renegotiation and interest rates. Note that the administrative banking loan dataset is not a panel dataset of loans. It lists all the loans for each individual in a given month, but it is not possible to connect each loan with loans in other periods. Third, to eliminate unusually low loan amounts, we exclude all loans in the administrative records which were lower than 2,000 Chilean pesos (around 3 US dollars). Fourth, since the match between the survey and the administrative records is at national identity number and debt type level, matching mistakes (a common issue with linked data, [Abowd and Stinson 2013](#)) should not be present.

Last, in this paper, we argue that the quality of the administrative records is high. There are three main reasons for that. First, the administrative dataset is a loan register that is used for several supervision purposes, such as for interest rate ceiling regulation. Furthermore, banks use it to check the total banking loans of prospective borrowers. Consequently, each bank delivering information would have incentives to report mistakes by other banking competitors. Moreover, if a bank failed to accurately report a loan, it would be a serious legal violation, implying large fines and reputational losses. Therefore, there are supervisory incentives to keep a clean register, report mistakes, and correct the dataset. Second, our analysis focuses on three simple concepts that banks regularly report to the regulator: debt ownership, total debt amount, and maturity.



**Table 1. Number of Total Respondents in the Survey and Matched Survey-Administrative Datasets**

Wave	Survey	Survey with ID	Matched data: with debt in Admin
2011	4,059	2,329	933
2014	4,502	2,362	1,132
2017	4,549	3,356	1,790
<b>Total</b>	<b>13,110</b>	<b>8,047</b>	<b>3,855</b>

Third, while the Banking Authority requires all banks to update their information every month, we do not have high-frequency data. Therefore, it seems unlikely that possible errors would go unnoticed one year after the last survey wave (in 2017) when the matching process was made.

Table 1 reports, by survey wave, the number of households being surveyed (second column), the number of respondents having provided a correct national ID number (third column), and finally, the number of respondents with a correct ID number that have been matched in the administrative banking loan records, conditional on registering a nonzero loan amount in the administrative records over the period where each survey wave took place (fourth column).

Table 1 shows 13,110 households in total in the survey dataset, with 8,047 of them having provided a correct national ID number. Furthermore, out of those 8,047, there are 3,855 with positive amounts of debt in the administrative records over the period where each survey wave took place. Table A.1 in the appendix shows that the subsample of households having provided their ID number correctly during the survey interview is representative of the total survey sample.

## 3.2 Methods

*3.2.1 Testing hypotheses 1, 2, and 3.* To study differences in debt ownership between the survey and the administrative records for hypotheses 1, 2 and 3, we estimate multinomial logit regressions (Train 2009). The dependent variable has three categories. The first category corresponds to the cases where the respondent reports a loan in the survey and the same loan appears in the administrative records. This is the base category. The second category occurs when the loan is available only in the survey, whereas the third one indicates the situations where the loan appears only in the administrative records. The multinomial logit model specification becomes



$$\ln\left(\frac{\text{Prob}(D_{i,t,d,k} = k)}{\text{Prob}(D_{i,t,d,0} = 0)}\right) = \alpha + X_{i,t,d}\beta + \epsilon_{i,t,d}, \tag{1}$$

where  $k$  is one of the following two categories: When the loan is available only in the survey or when the loan appears only in the administrative records; 0 refers to the base category.  $D_{i,t,d,k} = k(D_{i,t,d,0} = 0)$  is an indicator variable if the category  $k(0)$  is present for respondent  $i$ , debt type  $d$ , and year wave  $t$ .  $X_{i,t,d}$  is a matrix of respondent-, household-, and debt type-characteristics (to be detailed in section 3.4);  $\alpha$  is the intercept;  $\beta$  is the vector of regression coefficients; and  $\epsilon_{i,t,d}$  is the error term. All regressions have clustered standard errors at the household level to account for households who own several debt types. As it is standard with multinomial logit models, all parameters related to the base category take zero value to ensure model identification. Estimations do not include survey weights (see [appendix A.1](#) for details).

3.2.2 *Testing hypotheses 1, 2, and 4.* To investigate differences in loan amounts between the survey and the administrative records, in hypotheses 1, 2, and 4, we estimate ordinary least squares (OLS) regressions. The dependent variable is the absolute value of the relative discrepancy for loan amounts between the survey and the administrative records. Specifically, let  $y_{i,t,d,A-S}$  be the ratio of the difference between the loan amount in each dataset as a proportion of its mean value:

$$y_{i,t,d,A-S} := \frac{Y_{i,t,d,Admin} - Y_{i,t,d,Survey}}{(Y_{i,t,d,Admin} + Y_{i,t,d,Survey})/2},$$

with  $Y_{i,t,d,Admin}$  and  $Y_{i,t,d,Survey}$  representing the loan amount of borrower  $i$  at time  $t$  and debt type  $d$ , for the respective survey and administrative reports, divided by the mean value. Since there can be disparities between the month at which the administrative dataset is recorded and the date reported in the survey (see section 3.1), we take the closest value of  $Y_{i,t,d,Admin}$  in a two-month window, that is,

$$Y_{i,t,d,Admin} \equiv \underset{\tilde{Y}_{i,t+k,d,Admin}, k \in \{-2, -1, 0, 1, 2\}}{\text{argmin}} \left| \tilde{Y}_{i,t+k,d,Admin} - Y_{i,t,d,Survey} \right|.$$

The ratio statistic  $y_{i,t,d,A-S}$  has been used to measure differences between two datasets (Törnqvist, Vartia, and Vartia 1985; Davis, Faberman, and Haltiwanger 2006). Also, note that  $y_{i,t,d,A-S}$  is by definition bounded between  $-2$  and  $2$  (or equivalently,  $-200$  to  $200$  percent) and it can include cases in which one of the data sources records a zero outcome. However, in the analysis for differences in loan amounts, only respondents who report loans of a certain debt type in the survey and who also appear in the administrative records with the same debt type are included, that is, we exclude the values  $-2$  and  $2$ .

The model specification to study differences in loan amounts between the survey and the administrative records is as follows:

$$y_{i,t,d,A-S} = \alpha + X_{i,t,d}\beta + \epsilon_{i,t,d}. \quad (2)$$

As before, regressions include clustered standard errors at the household level.

### 3.3 Proxies for the Hypotheses

To proxy for financial literacy (hypothesis 1), we rely on the information for whether the respondents know their individual pension account type conditional on being part of the individual capitalization pension system. For robustness, we also consider a categorical variable for the respondent (or someone in the household) using automatic means of payments (such as automatic bill payments with credit card or current account). Note that knowing their capitalization account type is capturing the dimension of financial literacy that we are interested in, that is, the knowledge of the respondent's personal finances. In turn, using automatic means of payments relates to the household's ability to use available services and tools. While it is less related to the hypothesis we want to test, we consider it for robustness. Unfortunately, the survey does not contain additional and/or alternative proxies for financial literacy.

To proxy for the respondent's confidence in the survey (hypothesis 2), we rely on the questions that each interviewer has to respond about the respondent at the end of the interview. Specifically, we use the information provided by the interviewers about the perceived willingness of the respondent to answer the survey questionnaire. The alternative response categories are *bad*, *neutral*, and *good*. We then create an indicator variable that takes the value of one if the interviewer perceives the respondent as having a good readiness.

To examine the possibility that the respondent might be misreporting the owner of the loan (hypothesis 3), we add to the multinomial logit regressions an indicator variable for the respondent not being the highest income member of the household, interaction terms between this indicator variable and debt types, and a categorical variable measuring the number of generations present in the household (one, two, three or more). Table 2 details the proxies we use for hypotheses 1–3.

To study the impact of rounding (hypothesis 4), we propose a simple way to quantify the extent of bias that rounded responses might imply. In our context, errors due to rounding in survey self-reports would imply, for instance, a respondent declaring a loan amount of 1.5 million Chilean pesos (representing USD2,000 approximately), instead of answering the correct 1,647,150 pesos. To examine this possibility, we compare the survey self-reports with a “rounded” version of the administrative records  $y_{i,t,d,A-S}^R$ , which we estimate as

**Table 2. Summary of the Proxies for Hypotheses 1 to 3**

Variable	Description	Categories
Financial literacy	Respondent’s knowledge about personal finances, in particular, pension funds.	0. The respondent does not have a pension fund 1. The respondent knows his or her pension fund 2. The respondent does not know his or her pension fund
	Household uses automatic bill payments.	0. No 1. Yes
Confidence on the survey	Respondent’s willingness to participate in the survey.	0. Not good 1. Good
“Not the owner” of the debt	The respondent is not the highest income member of the household	0. Respondent has the highest income in the household 1. Respondent does not have the highest income in the household
	Multigenerational households	0. One generation 1. Two generations 2. Three or more generations

NOTE.—The total number of respondents involved in the calculations is 3,434.

$$y_{i,t,d,A-S}^R := \frac{Y_{i,t,d,Admin}^R - Y_{i,t,d,Survey}}{(Y_{i,t,d,Admin}^R + Y_{i,t,d,Survey})/2}, \tag{3}$$

with

$$Y_{i,t,d,Admin}^R \equiv \underset{Y_{i,t,d,Admin} \in \{10 \times \mathbb{Z}\}}{\operatorname{argmin}} |\widehat{Y}_{i,t,d,Admin} - Y_{i,t,d,Survey}|$$

$$\text{s.t. } |\widehat{Y}_{i,t,d,Admin} - Y_{i,t,d,Survey}| \leq \frac{1}{3} Y_{i,t,d,Admin}.$$

The rounding function that we assume takes the closest rounded value in terms of a number that is an integer multiple of 10 (with  $\mathbb{Z}$  being the set of integer numbers; therefore,  $10 \times \mathbb{Z}$  denotes the set of integers multiples of 10), but with a rounding error less than one third of the original value. It thus means that 651,000 can be rounded to 500,000 but not to 1,000,000 and that 800,000 can be rounded to 1,000,000 but not to 500,000. To investigate the importance

of the rounding hypothesis, we will examine the empirical distribution of the rounded discrepancy ratio  $y_{i,t,d,A-S}^R$ .

### 3.4 Additional Controls

As additional controls, we include demographic variables, namely, age, sex, a dummy variable for the respondent being married, the respondent's level of education and occupation; an indicator variable for the household being interviewed for the first time; whether the interview took place during the weekend; fixed effects for the regions where the household is situated, for the survey waves and for debt types (mortgages and non-mortgages). Furthermore, following [Gideon et al. \(2017\)](#), we incorporate the following: (i) the average item nonresponse rate in questions regarding income amounts and investments in financial assets; (ii) the mean household's rounding intensity in income and asset questions. In particular, [Gideon et al. \(2017\)](#) define the measure of rounding as the number of trailing zeros in an answer divided by the number of potential trailing zeros. We add these two variables to control for unobserved characteristics that may affect measurement errors. [Table A.2](#), in the appendix, describes the distribution of these additional controls. The table shows that 51 percent of respondents are female, 39 percent have a school diploma, 73 percent are workers, and the average age is 49.1. Furthermore, mortgage and credit card or credit line holding is 38 percent, and the metropolitan area concentrates 46 percent of respondents.

## 4. DIFFERENCES IN DEBT OWNERSHIP

### 4.1 Comparing Discrepancies in Debt Ownership

[Table 3](#) reports, by debt category, the frequency of the four possible sources of (mis)matches between the survey and the administrative data, namely, when both the survey and the administrative records register no debt at the time of the interview (which we denote as Neither); the situations where the individual has a positive loan amount in one of the datasets but not in the other one (which correspond to the discrepancies Survey-only or Admin-only), and finally, the observations where the individual registers a positive loan amount of a given debt category in both datasets (Both).

[Table 3](#) shows that the Admin-only errors are more prevalent for installment loans and credit card and credit line loans, whereas the Survey-only errors are more frequent for mortgages. It is important to notice that, for credit cards and credit lines, the survey only asks households to report debts that last more than one month; therefore, the survey does not include revolving loans that are paid

**Table 3. Debt Ownership in the Survey and the Administrative Records, Distinguishing by Debt Category**

(Mis)matches between the two sources (%)	Neither	Admin-only	Survey-only	Both
Mortgages	78.3	1.9	9.6	10.2
Installment loans	75.8	9.6	2.6	11.9
Credit cards and credit lines	61.2	19.0	2.9	16.9

at the end of the month. This feature should then explain a bulk of the 19.0 percent of Admin-only errors for credit cards and credit lines exhibited in [table 3](#).

#### 4.2 Explaining Discrepancies: Financial Literacy and Confidence in the Survey Hypotheses

To test hypotheses one to three for differences in debt ownership (equation (1)), [table 4](#) reports the multinomial logit model estimates. Specifically, the first three columns of results examine one hypothesis at a time, that is, the confidence in the survey (first column, *SurvConf*), the financial literacy (column *SurvConf+FinLit*), and “not the debt owner” hypotheses (*SurvConf+FinLit+NotOwn*). This last model specification is our baseline model. Column 4 estimates the baseline specification but without the 2011 wave (*No2011*). Last, column *No2011+InterFE* adds fixed effects for the survey interviewers. The reason for having columns 4 and 5 is that the information from the interviewers is only available for the waves 2014 and 2017. [Table A.3](#) reports the multinomial logit average marginal effects for the above specifications.

[Tables A.3](#) and [A.4](#) provide support to the financial literacy, the lack of confidence and “not the owner” hypotheses for the discrepancies in debt ownership between the survey and the administrative records. First, we find that knowing the respondent’s pension account type decreases the probability of Survey-only and Admin-only errors between 3 percent and 4 percent, depending on the model specification ([table A.3](#)). Also interestingly, being financially literate increases the probability of a (consolidated) loan correctly being in the two data sources between 6 percent and 7 percent. These results are robust to alternative proxies for financial literacy (e.g., the use of automatic means of payments). Results are available from the authors upon request.

Concerning hypothesis 2, we find that being perceived as having a good willingness to participate in the survey reduces the probability of Survey-only or Admin-only errors by 5 percent. Also, results show that having a good willingness to participate in the survey increases the probability of a correct report

**Table 4. Coefficient Estimates in the Multinomial Logit Regressions with the Respondent Reporting a Loan in Both Sources as the Base Category**

	SurvConf	SurvConf +FinLit	SurvConf+FinLit +NotOwn	No2011	No2011 +IntervFE
<b>Loan only in the survey</b>					
Gender of the resp. (ref. female)	-0.238** (0.091)	-0.198* (0.092)	-0.027 (0.096)	-0.077 (0.105)	-0.093 (0.112)
Resp.'s marital status (ref. not married)	0.147 (0.094)	0.156 (0.094)	0.064 (0.098)	0.062 (0.107)	0.089 (0.116)
Resp.'s educational level (ref. primary or secondary school only)	-0.293* (0.136)	-0.259 (0.138)	-0.241 (0.140)	-0.173 (0.155)	-0.204 (0.165)
Undergrad studies	-0.720*** (0.105)	-0.660*** (0.107)	-0.642*** (0.109)	-0.596*** (0.122)	-0.678*** (0.137)
Postgraduate studies	-1.258*** (0.161)	-1.201*** (0.162)	-1.177*** (0.164)	-1.128*** (0.180)	-1.303*** (0.197)
Type of loan (ref. installment loans)					
Mortgages	1.561*** (0.103)	1.567*** (0.104)	1.239*** (0.115)	1.235*** (0.125)	1.273*** (0.134)
Credit cards and credit lines	0.014 (0.107)	0.009 (0.107)	0.020 (0.117)	-0.005 (0.127)	-0.054 (0.133)
Resp. willingness (ref. willingness is not good)	-0.551** (0.169)	-0.534** (0.168)	-0.556** (0.175)	-0.547** (0.190)	-0.149 (0.232)

Resp.'s knowledge about his or her pension fund (ref. respondent does not have pension fund)					
Knows his or her pension fund	-0.404**	-0.375*	-0.401**	-0.333*	
	(0.126)	(0.146)	(0.130)	(0.160)	
Does not know his or her pension fund	0.049	0.138	0.048	0.159	
	(0.128)	(0.150)	(0.132)	(0.164)	
Number of generations in the household (ref. one generation)					
Two generations		0.096	0.115	0.156	
		(0.107)	(0.098)	(0.113)	
Three or more generations		0.316*	0.398**	0.300	
		(0.157)	(0.135)	(0.169)	
<b>Loan only in the survey</b>					
Whether the resp. has the highest income (ref. highest income)					
Not highest income		-0.182	-0.050	-0.216	
		(0.287)	(0.225)	(0.306)	
Not highest income × mortgage		1.735***	1.542***	1.839***	
		(0.322)	(0.265)	(0.344)	
Not highest income × credit cards and lines		-0.027	-0.001	-0.144	
		(0.341)	(0.272)	(0.364)	
<b>Loan only in the adm. data</b>					
Gender of the resp. (ref. female)		-0.235**	-0.165*	-0.230**	
		(0.079)	(0.072)	(0.081)	
Resp.'s marital status (ref. not married)		0.046	0.090	0.039	
		(0.078)	(0.071)	(0.080)	

*Continued*



Table 4. Continued

	SurvConf	SurvConf +FinLit	SurvConf+FinLit +NotOwn	No2011	No2011 +IntervFE
Resp.'s educational level (ref. primary or secondary school only)					
Technical studies	-0.429*** (0.108)	-0.405*** (0.108)	-0.404*** (0.108)	-0.432*** (0.119)	-0.408** (0.126)
Undergrad studies	-0.478*** (0.083)	-0.437*** (0.083)	-0.440*** (0.084)	-0.538*** (0.094)	-0.529*** (0.098)
Postgraduate studies	-0.618*** (0.109)	-0.578*** (0.110)	-0.589*** (0.111)	-0.613*** (0.121)	-0.554*** (0.128)
Type of loan (ref. installment loans)					
Mortgages	-1.354*** (0.115)	-1.351*** (0.115)	-1.414*** (0.127)	-1.520*** (0.142)	-1.611*** (0.154)
Credit cards and credit lines	0.507*** (0.066)	0.505*** (0.066)	0.555*** (0.073)	0.585*** (0.080)	0.610*** (0.087)
Resp. willingness (ref. willingness is not good)	-0.401** (0.126)	-0.386** (0.125)	-0.390** (0.126)	-0.330* (0.137)	-0.286 (0.160)
Resp.'s knowledge on his or her pension fund (ref. respondent does not have a pension fund)					
Knows his or her pension fund		-0.264** (0.097)	-0.267** (0.097)	-0.285** (0.109)	-0.255* (0.118)
Does not know his or her pension fund		0.052 (0.100)	0.050 (0.100)	0.086 (0.111)	0.136 (0.119)

**Loan only in the adm. Data**

Number of generations in the household (ref. one generation)

Two generations	-0.071 (0.074)	-0.083 (0.080)	-0.025 (0.083)
Three or more generations	0.025 (0.106)	-0.031 (0.120)	0.006 (0.124)

Whether the resp. has the highest income (ref. highest income)

Not highest income	0.105 (0.152)	0.099 (0.184)	0.193 (0.193)
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Not highest income × mortgage

	0.514 (0.290)	0.383 (0.342)	0.443 (0.360)
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Not highest income × credit cards and lines

	-0.277 (0.171)	-0.321 (0.204)	-0.360 (0.219)
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Number of observations

	6,412	6,412	5,416
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Loan in both sources

	2,844	2,844	2,441
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Loan only in the survey

	1,084	1,084	920
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Loan only in the adm. data

	2,484	2,484	2,055
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Number of respondents

	3,434	3,434	3,375
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Pseudo R-squared

	0.135	0.138	0.162
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NOTE.—Resp. stands for respondent and ref. denotes the reference category. Resp. willingness stands for respondent's willingness to participate in the survey is either not good or good, based on interviewer's perception. Control variables also include the age and age squared of the respondent, the average rate of missing values in income and asset amounts, the rounding intensity in income and asset amounts, as well as indicator variables for the labor classification, for the respondent being interviewed during the weekend, for first time interviewed, year and region. Constant is not reported. For details about the proxies for the hypotheses and the control variables, refer to tables 2 and A.2. Clustered standard errors at household level are in parentheses. \*, \*\*, \*\*\*: statistical significance at the 5%, 1%, and 0.1%, respectively.

(Both) by 10 percent (a finding which is in line with [Kirchner, Olson, and Smyth 2017](#)). However, note that when we include the interviewers' fixed effects, the proxy for the confidence in the survey appears as insignificant. The reason may be that as the willingness proxy is based on the interviewer's perception, the two pieces of information (the covariate and the interviewers' fixed effects) may be too correlated between each other.

Furthermore, [table 4](#) shows that Survey-only errors are more likely if the respondent reports a mortgage and does not earn the highest income of the household. To support the latter, [table 5](#) exhibits, by debt category, the percentage of respondents that are not the highest income member of the household, distinguishing between the cases where a loan is reported in the survey only versus the situations where there is a positive loan in both datasets. Interestingly, [table 5](#) shows that respondents with reported loans in the survey only are more likely not to be the member of the household with the highest income, relative to those respondents that appear in the two datasets. The latter is most evident in the case of mortgages where the difference between the two groups is the largest. Therefore, both findings are consistent with the interpretation of the respondents not being the owner of the mortgage debt contract that they report.

In addition, households whose members belong to three or more different generations increase the likelihood of Survey-only errors between 3 percent and 4 percent, which is also consistent with the "not the owner hypothesis." On top of that, we find that the likelihood of Admin-only errors is 30 percent smaller ([table A.3](#)) if the loan is a mortgage, presumably because these loans are larger; hence, it is less likely for a respondent to forget to report such a loan. Interestingly, [table A.3](#) also shows that the likelihood of Survey-only and Admin-only errors is smaller if the respondent has undergraduate or postgraduate studies.

Last, some robustness checks show that results are robust to (i) the interaction terms between the indicator variable for the respondent not being the highest income member of the household and the proxies for financial literacy; (ii) the estimation of the model by debt type; and (iii) the exclusion of the covariates for the education level. Concerning the last check, we conclude that our proxies for financial literacy have additional informational content relative to respondents' level of education and that this additional information contributes to explaining the discrepancies between the survey and the administrative records.

**Table 5. Frequency of Respondents in the Survey that Are Not the Highest Income Member, Distinguishing between the Status Survey-Only and Both**

Loan ownership (Survey, Admin)	Percent of borrowers who are not the highest income member	
	Survey-Only	Both
Mortgages loan	57.3	17.8
Installment loans	26.6	22.1
Credit cards and credit lines	27.1	25.0

## 5. DISCREPANCIES IN LOAN AMOUNTS

We now focus on the survey respondents who have matched in the administrative records, conditional on individuals registering nonzero loan amounts of a certain debt category in both datasets.

### 5.1 Comparing Discrepancies in Loan Amounts

The top panel of [table 6](#) reports, by debt category, the 10, 25, 50, 75, and 90 percentiles and the mean of the discrepancy ratio  $y_{i,t,d,A-S}$  computed for loan amounts, total, and residual maturity. In turn, the bottom panel of the same table exhibits the same statistics for the rounded version of the discrepancy ratio,  $y_{i,t,d,A-S}^R$  (see (3)). Note that if a borrower has more than one loan in a certain category, we report the total maturity weighted by the debt amount of each loan.

The top panel of [table 6](#) shows that the median borrower reports debt amounts fairly well in the case of mortgages, installment loans, and credit lines and credit cards. It is important to add that part of these discrepancies may occur because banks register in their records some administrative costs whereas respondents do not include them in their reports. These costs are notary fees, stamp tax, and banking fees. In contrast, the survey only asks households for the requested amount to the bank (see [appendix A.2](#)). Also, the median borrower provides fairly accurate information for both mortgage and installment loan total and residual maturities. To illustrate the latter, the percentiles 25 to 75 of the ratios  $y_{i,t,d,A-S}$  for total and residual maturity for mortgages are in the ranges of  $[-0.4\%, 4.5\%]$  and  $[-12.0\%, 21.0\%]$ , respectively. However, the differences between the survey and the administrative records in the tails of the empirical distributions of  $y_{i,t,d,A-S}$  can be substantial.

While the median borrower tends to report loan amounts and maturities fairly well, for those respondents who do not provide accurate responses, do they tend to under or overreport relative to the administrative records? To answer this question, we simply compare the mean and the median values exhibited in the top panel of [table 6](#). Interestingly, we find that with only two exceptions (credit card and credit line loan amounts and mortgage total

**Table 6. Empirical Distributions of the Discrepancy Ratio  $y_{i,t,d,A-S}$  (Top Panel) and of the Rounded Discrepancy Ratio  $y_{i,t,d,A-S}^R$  (Bottom Panel)**

Variable	Debt Category	P10	P25	P50	P75	P90	Mean
<b>Top panel: Distribution of the discrepancy ratio <math>y_{i,t,d,A-S}</math></b>							
<b>Loans amounts</b>	Mortgages	-65.9	-32.0	-4.6	15.3	65.0	-2.7
	Installment loans	-58.4	-9.3	7.3	42.6	98.6	13.6
	Credit cards and credit lines	-93.9	-30.9	2.6	40.9	82.7	0.1
<b>Total maturity (months)</b>	Mortgages	-25.6	-0.4	1.7	4.5	24.4	1.1
	Installment loans	-32.3	0.0	4.1	30.3	70.3	9.1
<b>Residual maturity (months)</b>	Mortgages	-36.5	-12.0	0.6	21.0	53.8	5.0
	Installment loans	-66.7	-28.6	-1.8	33.0	82.9	2.3
<b>Bottom panel: Distribution of the rounded discrepancy ratio <math>y_{i,t,d,A-S}^R</math></b>							
<b>Loans amounts</b>	Mortgages	-58.4	-22.6	0.0	3.7	55.3	-1.3
	Installment loans	-46.2	0.0	0.0	28.6	94.7	10.7
	Credit cards and credit lines	-85.7	-20.6	0.0	28.6	66.7	-1.9
<b>Total maturity (months)</b>	Mortgages	-22.2	0.0	0.0	0.4	22.2	0.7
	Installment loans	-18.2	0.0	0.0	28.6	66.7	7.8
<b>Residual maturity (months)</b>	Mortgages	-28.6	-5.7	0.0	18.2	47.6	5.3
	Installment loans	-66.7	-16.8	0.0	23.6	66.7	1.5

NOTE.—The table excludes the 5% and the 95% most extreme observations. Loan amounts are in logarithm of Chilean pesos. Maturities are in months. Credit cards and lines stands for credit cards and credit lines.

maturity), the mean values are larger than the median ones. The latter hence indicates that the discrepancy ratio tends to be right-skewed and that survey respondents are more likely to under-report their loan amounts and maturities relative to the administrative records. These findings are consistent with [Brown et al. \(2015\)](#), [Karlán and Zinman \(2008\)](#), [Maynes \(1968\)](#), and [Zinman \(2009\)](#). In the next section, we investigate the reasons that might explain these differences.

## 5.2 Explaining Discrepancies in Loan Amounts: Rounding Errors and Hypotheses

Section 5.2 begins by examining whether “rounding” the administrative records results in smaller differences between the survey and the administrative records. Next, we test hypotheses 1 (financial literacy) and 2 (lack of confidence) to explain differences in loan amounts.

*5.2.1 Accounting for rounding errors.* To examine the rounding hypothesis, we need to focus on the bottom panel of [table 6](#). The main finding is that a simple form of rounding in which we round the administrative records to a multiple of 10 can go a long way towards explaining the differences between the survey and the administrative records, especially in the center of the distribution. One way to see it is that the rounded discrepancy ratio  $y_{i,t,d,A-S}^R$  for the median borrower now becomes 0 both for loan amounts and total and residual maturities, regardless of the debt type. However, the discrepancies in the tails of the distributions continue to be substantial.

To quantify the effect of rounding on survey reports' accuracy, we look at the reduction in the 25–75 percentile difference of the rounded discrepancy ratio  $y_{i,t,d,A-S}^R$  relative to that difference in the not rounded version  $y_{i,t,d,A-S}$  (as exhibited in the top panel of [table 6](#)). Interestingly, results show that the impact of rounding is considerable: The 25 – 75 percentile intervals of the rounded discrepancy ratio  $y_{i,t,d,A-S}^R$  for mortgage, installment loan, and credit card and credit line debt amounts are 44 percent, 45 percent, and 31 percent smaller than their counterparts in the top panel of [table 6](#), respectively.

*5.2.2 Hypotheses on financial literacy and confidence in the survey.* To examine the financial literacy and the willingness to respond hypotheses, we rely on the absolute value of the discrepancy ratio  $y_{i,t,d,A-S}$  for loan amounts as the dependent variable (equation (2)). Also, we consider the same variables as in the analysis for differences in debt ownership. [Table 7](#) reports the OLS model estimates. Specifically, column *SurvConf* in [table 7](#) examines the confidence in the survey hypothesis, while the *SurvConf+FinLit* column adds the financial literacy hypothesis. Both analyses include all survey waves. The third and fourth columns are robustness checks which exclude the 2011 wave: Column *No2011+IntervFE* adds to column 3 (*No2011*) interviewers' fixed effects.

[Table 7](#) provides support to the financial literacy, whereas it does not confirm the confidence in the survey hypothesis. Specifically, in the case of the financial literacy hypothesis, we find that the respondents having a good understanding of their personal finances (as proxied by the respondents' knowledge of their pension account type) decreases the discrepancies between the survey and the administrative records. This effect is statistically significant in all model specifications in [table 7](#).

On the other extreme, the coefficient estimates for the willingness to participate hypothesis are negative (which is consistent with our hypothesis), but statistically insignificant. Therefore, we conclude that while being perceived as having a high confidence in the survey is a strong explanation for differences in debt ownership, conditional on the households having correctly reported their debt, this hypothesis does not appear to be relevant to explain the differences in loan amounts. Despite their willingness, respondents may not exactly remember or may not know the required information ([Groves, Fowler, Couper,](#)

Table 7. Linear Regressions of the Ratio  $|y_{it,DA} - s|$  as Dependent Variable

	SurvConf	SurvConf +FinLit	No2011	No2011 +IntervFE
Resp.'s marital status (ref. not married)	11.083*** (2.469)	11.079*** (2.461)	12.106*** (2.646)	12.312*** (2.846)
Resp.'s educational level (ref. primary or secondary school only)				
Technical studies	0.198 (3.977)	1.313 (3.981)	2.522 (4.399)	3.026 (4.566)
Undergrad studies	0.604 (3.000)	2.382 (3.047)	2.367 (3.310)	2.678 (3.582)
Postgraduate studies	-2.976 (3.617)	-1.555 (3.632)	-2.393 (3.953)	-4.018 (4.183)
Age of the resp.	-0.507 (0.689)	-0.093 (0.689)	-0.423 (0.739)	-0.468 (0.803)
Age squared of the resp.	0.004 (0.007)	-0.000 (0.007)	0.003 (0.008)	0.004 (0.008)
Resp.'s labor status (ref. self-employed)				
Civil servant	-4.964 (3.665)	-3.223 (3.646)	-5.570 (3.999)	-5.519 (4.286)
Private sector	-4.135 (3.102)	-1.520 (3.115)	-2.607 (3.444)	-3.676 (3.884)
Inactive or unemployed	-6.260 (4.761)	-7.300 (4.790)	-6.417 (5.477)	-6.733 (5.844)
Pensioner	1.015 (6.208)	-1.231 (6.265)	-0.919 (6.924)	-2.795 (7.559)
Gender of the resp. (ref. female)	-6.231* (2.445)	-5.598* (2.441)	-6.149* (2.677)	-7.670** (2.770)



Type of loan (ref. installment loans)					
Mortgages	-5.134 (2.936)	-4.976 (2.936)	-3.543 (3.078)	-5.438 (3.186)	
Credit cards and credit lines	8.352*** (2.515)	8.095** (2.509)	10.142*** (2.688)	9.251*** (2.799)	
Resp. willingness (ref. willingness is not good)	-7.127 (5.226)	-6.885 (5.222)	-7.937 (5.500)	-7.665 (6.519)	
Resp.'s knowledge about his or her pension fund (ref. respondent does not have a pension fund)					
Knows his or her pension fund		-11.426** (3.840)	-10.357* (4.157)	-9.355* (4.531)	
Does not know his or her pension fund		-3.201 (4.206)	-2.409 (4.596)	-3.242 (4.847)	
Number of observations	2,473	2,473	2,109	2,109	
Number of respondents	1,603	1,603	1,342	1,342	
R-squared	0.032	0.038	0.045	0.115	

NOTE.—Resp. stands for respondent and ref. denotes the reference category. Resp. willingness stands for respondent's willingness to participate in the survey is either not good or good, based on interviewer's perception. Control variables also include the average rate of missing values in income and asset amounts, the rounding intensity in income and asset amounts, as well as indicator variables for the respondent being interviewed during weekend, first time interviewed, year and region. Constant is not reported. For details about the proxies for the hypotheses and the control variables, refer to Tables 2 and A.2. Clustered standard errors at household level are in parentheses. The average variance inflation factor of the control variables from column "SurvConf+FinLit" is 1.53. For the estimates in column "SurvConf+FinLit," we apply the Ramsey regression equation specification error test; we do not reject the null hypothesis of no misspecification at 95% of confidence ( $p$ -value of 0.239). \*, \*\*, \*\*\*: statistical significance at the 5%, 1%, and 0.1%, respectively.

Lepkowski, Singer, et al. 2009). Consequently, encouraging respondents to consult bank statements should improve the quality of the information provided by those respondents who are willing to participate (Couper, Ofstedal, and Lee 2013; Eggleston and Reeder 2018).

Table 7 also shows that differences in loan amounts between the survey and the administrative records are larger if the respondent is married, is a woman or has credit card and credit line loans. Importantly, our results are robust to: (i) alternative dependent variables, such as the inverse hyperbolic sine of the absolute difference, the difference between the logarithm of loan amounts in each dataset, and the rounded discrepancy ratio  $y_{i,t,A-S}^R$ ; (ii) alternative proxies for financial literacy; (iii) interactions terms between debt type and the occurrence of over-reports; (iv) excluding from the sample credit cards and credit lines; and (v) estimating the model specifications in table 7 by debt type.

## 6. DISCUSSION AND CONCLUSIONS

It is well known that self-reported financial information differs from administrative records. In this article, we advance this literature by looking at a novel match of individual borrowers from the Chilean Household Finance Survey with their administrative records. We show that discrepancies in debt ownership are larger when respondents have a mortgage and are not the highest income member of the family, when respondents have weak financial literacy and do not have confidence in the study, and when respondents live in multigenerational households. In turn, differences in loan amounts are larger if the respondent is financially illiterate. Table 8 summarizes our main findings.

From a methodological standpoint, there are several lessons that we can derive from our results. First, while improving financial literacy is a systemic, long-term issue, in the interim, there are some specific actions that could improve households' self-reports. For example, more than one member of the household may be allowed to respond to the survey (Fisher, Reimer, and Carr 2010). Another action is to encourage respondents to use documents (e.g., credit card statements, bank statements). This could improve the quality of the information provided by respondents who are willing to participate (Couper et al. 2013; Eggleston and Reeder 2018). These two actions should mitigate the recalling and lack of financial knowledge problems that households face when answering financial questions. Second, field experiments in a randomized controlled setting or cognitive interviews could be used to test different ways of asking certain questions before the survey. The objective would then be to implement those questions that were found to be more effective.

Third, to lower respondents' aversion to sharing financial information and help with cooperating in interviews, some possible actions could include media campaigns before the survey fieldwork; increasing households' awareness about the survey; the dissemination of the survey results in different media so

Table 8. Summary of Findings

Hypothesis	Description of the proxy	Differences in Debt Ownership				Discrepancies in	
		Survey-only		Admin-only		Loan Amounts	
		Expected sign	Our result	Expected sign	Our result	Expected sign	Our result
Financial literacy	The respondent knows their pension fund	-	-**	-	-**	-	-***
Confidence on the survey	Respondent's willingness to participate in the survey is good	-	-**	-	-**	-	Insig
„Not the owner of the debt“	The respondent is not the highest income of the household	+	+***	+	Insig	N/A	N/A
	Three or more generations in a household	+	+***	+	Insig	N/A	N/A

NOTE.—The column Expected sign corresponds to the expected sign according to the theory as discussed in section 2. The column Our results for differences in debt ownership (in loan amounts) corresponds to the marginal effects' (estimated coefficients) signs and statistical significance of the model SurvConf + FinLit + NotOwn (SurvConf + FinLit), Table A.3 (Table 7). N/A stands for not applicable, whereas Insig stands for non-significant effects. \*, \*\*, \*\*\*, \*\*\*: statistical significance at the 10%, 5%, and 1%, respectively.

that households understand the relevance of the information they have provided; and training interviewers, so that they can (i) explain to households the importance of the survey before starting the survey; (ii) gain the confidence of the respondents and elicit accurate information from them; and (iii) explain specific financial concepts necessary to answer the survey.

Our analysis is subject to some drawbacks. First, our matched sample is limited to bank loans. Therefore, it is silent about other types of loans like informal credits which are under less harsh supervision rules. Second, our analysis is not based on a randomized controlled experiment. Consequently, some results like the respondents' willingness to participate may be confounded with unobserved factors. Third, the survey interview is limited to respondents who self-report being the household member with the most knowledge of the household's finances. Last, we cannot test the quality of the administrative records. While we argue that the quality of the administrative data is good, we acknowledge that there may be potential problems such as errors in the generation of the administrative data, definitional differences between the survey and the administrative records, and errors in the matching process. Also, our access to the administrative records is only for the survey sample; therefore, we cannot say anything about the administrative data outside of our sample.

To conclude, due to the importance of household finance surveys for policy analysis, further research on the understanding of the strengths and shortcomings of survey financial data is needed. From a methodological standpoint, one venue of future research could be to further investigate the role of interviewers' characteristics and behaviors on measurement errors, relying on linked datasets. In particular, investigating the interplay between interviewers and the use of documents seems a promising area of research. Finally, studying the reasons for heaping responses, for instance, whether heaping may be linked to higher or lower data quality depending on the type of questions (subjective or objective) could be another avenue of forthcoming research.

## SUPPLEMENTARY MATERIALS

Supplementary materials are available online at [academic.oup.com/jssam](https://academic.oup.com/jssam).

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