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# Financial Illiteracy and Pension Contributions: A Field Experiment on Compound Interest in China FREE

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## Abstract

I conduct a field experiment to study the relationship between peoples' misunderstanding of compound interest and their pension contributions in rural China. I find that explaining the concept of compound interest to subjects increased pension contributions by roughly 40%. The treatment effect is larger for those who underestimate compound interest than for those who overestimate compound interest. Moreover, financial education enables households to partially correct their misunderstanding of compound interest. I structurally estimate the level of misunderstanding of compound interest and conduct a counterfactual welfare analysis: lifetime utility increases by about 10% if subjects' misunderstanding of compound interest is eliminated.

Authors have furnished an Internet Appendix, which is available on the Oxford University Press Web site next to the link to the final published paper online.

The inadequacy of retirement savings in the United States is a common, and at times controversial, theme in the literature (Venti and Wise 1997; Lusardi 1999; Lusardi and Mitchell 2011; Scholz, Seshadri, and Khitatrakun 2006). Two strands of literature seek to explain this phenomenon. One, which focuses on the lack of information and financial sophistication, stresses the importance of financial literacy and financial education; the other attributes undersaving to self-control problems and procrastination.[1]

This paper follows the first strand. Financial illiteracy is widespread in the

Yoong, and Zinman 2017). I focus on one concrete definition of financial illiteracy: misunderstanding compound interest. Understanding compound interest is especially important to retirement savings decisions due to long investment horizons. Does such a misunderstanding influence pension contributions? Can pension contributions be improved by explaining the concept of compound interest to potential pension contributors?

I designed a field experiment to evaluate whether misunderstanding compound interest is partially responsible for low pension contributions in rural China. I randomly assigned more than 1,000 Chinese households to 1 of 3 groups: the *Control* group, the *Calculation* group, and the *Education* group. In the control group, I visited households, explained the pension contract, and conducted a survey about their socioeconomic background. In the calculation treatment, I also calculated respondents' expected pension benefit levels after age 60 if they contributed at various levels starting at age 30. In the education treatment, I asked respondents questions about compound interest, explained the correct answers, taught them the basic concept of compound interest, and performed the same calculations used in the calculation treatment. The education treatment tests whether I can correct individual bias about compound interest through consumer learning. Comparison of the calculation treatment and the education treatment tests whether providing information or improving understanding increases pension contributions. I collaborated with a local government to provide the interventions before participants chose their contributions for that year, and then collected administrative data on their actual pension contributions from the local government.

I find that 56% of rural households in the sample told us they do not know the answer to the simplest compound interest question (after repeated prompting), and 73% of those who answered the question underestimated compound interest. The experiment reveals that the education treatment increased the contribution for that year by 49–53 RMB, resulting in an increase of 40% relative to the average contribution of 133 RMB in the control group.

Why does teaching compound interest increase pension contributions? One possible channel is a better understanding of compound interest, so that subjects can apply the principle to their own choices. Another is learning the benefits of a pension in general, which suggests that providing retirement income projection should have equivalent effects. To distinguish these channels, I study the heterogeneous effect with respect to initial knowledge

I find that the treatment effect of the education group is larger for those who underestimate compound interest than for those who overestimate compound interest. Moreover, simply performing the calculations and explaining the benefits increased the contribution by 20–25 RMB, an increase of 15%–19% relative to the control group. This effect is significantly smaller than the effect of the education treatment. The treatment effects of the education and calculation groups are similar for those who are around age 30, but differ as age increases: the treatment effect of the calculation group is lower than that of the education group for those who are around ages 40 to 50. In addition, I measured financial literacy in a post-intervention survey, and found that financial education can help people improve their understanding of compound interest. These results suggest that learning the benefits of a pension in general is unlikely to explain the overall treatment effects. In contrast, explaining compound interest is likely to increase people' ability to link the calculation of benefits to their own choices, and thus increase pension contributions.

I further exploit the experimental variation and structurally estimate a life cycle model in which subjects underestimate compound interest. I combine both the survey data and actual pension contributions from the experimental data to estimate risk aversion parameters, time preferences parameters, and parameters of underestimating compound interest. Estimates show that households underestimate compound interest in both the control group and the education group. The estimated bias in the education group is much smaller than that in the control group, which is consistent with the reduced form results that the education treatment helps to reduce the underestimation of compound interest.

Finally, I use the estimated parameters to simulate the impact of several counterfactual policies on pension contributions. Simulation results show that, while both increasing the subsidy and financial education help to increase pension contributions, financial education about compound interest is more effective than increasing the existing subsidy by 100%. Based on the counterfactual simulation, I can calculate the lifetime utility for each individual and analyze the welfare effect. I show that the education treatment increases the lifetime utility by 8.6% compared to the control group, and the lifetime utility increases by 10.0% if the misunderstanding of compound interest is eliminated.

This paper makes several contributions. First, it contributes to the literature on financial literacy and financial education.[2] My main innovation is to study sending retirement-income projections in a printed brochure increases individual contributions to voluntary retirement plans in the United States. In particular, the calculation group in this paper is similar to Goda, Manchester, and Sojourner (2014) key interventions, and my results are similar; what differs is that I connect this response to a concrete aspect of financial literacy. I add the education group and show that teaching compound interest has a larger effect than projecting income. I also measure the understanding of compound interest so that I can identify consumer bias and the mechanisms by which my interventions can increase individual contributions; this design enables us to identify individual utility functions and, in turn, generate hypotheses for other household finance decisions, such as borrowing (Stango and Zinman 2009) and life cycle consumption (Levy and Tasoff 2016).

Second, I bridge the gap between my theoretical framework and experimental results by structural estimation of individual preference and welfare analysis of the financial education provided. Previous research on financial education focuses on identifying which interventions change financial decisions; few focus on whether the intervention affects underlying structural parameters or changes individual decisions toward the decisions implied by economic models, and thus improves welfare. In this paper, I combine a field experiment with structural estimation based on a benchmark life cycle model, and test whether the intervention changes individual decisions toward the decisions implied by the benchmark model. I exploit the experimental variation and quantify the treatment effects by estimating the parameters of underestimating compound interest in both the control group and the education group. The results are consistent with the reduced form results that the intervention reduces the misunderstanding of compound interest.

Third, the results contribute to the literature on consumer bias and pension savings. Prior research suggests that many people do not save enough voluntarily to maximize their lifetime utility (Barr and Diamond 2008). Low savings for retirement can be driven by behavioral factors, such as procrastination (Choi et al. 2002; Thaler and Benartzi 2004), defaults (Madrian and Shea 2001; Beshears et al. 2008), or how information and choices are framed (Choi et al. 2012; Beshears et al. 2013). Misunderstanding compound interest is another plausible explanation for low pension contributions, yet it has not drawn much attention in this literature. I build on previous studies that analyze the relationship between misunderstanding compound interest and saving decisions using laboratory experiments psychology and economics literatures have documented many individual biases, but whether these biases can be lessened is rarely explored. I show that I can improve consumers' financial decisions by correcting their erroneous understanding of compound interest through consumer education.

## 1. The New Rural Social Pension Insurance Program

In 2009, the Chinese government introduced the New Rural Social Pension Insurance Program (NRSPIP), which is a voluntary defined contribution plan that is highly subsidized by the central and local governments. Farmers aged 16 or older who are not students and are not enrolled in an urban pension plan are eligible for the program. An individual's pension fund account comprises the person's contributions and government subsidies. In the county I study, subjects can choose 1 of 5 annual contribution levels: 100, 200, 300, 400, or 500 RMB, which range from 2% to 8% of the county's 2009 rural annual per capita net income. Table 1 shows the match contributions made by the Chinese government as subsidies.

**Table 1** Pension contract

**A. Pension subsidy**

| Options | Contribution level (RMB/year) | Percentage of annual per capita income | Government subsidy (RMB/year) |
|---|---|---|---|
| 1 | 100 | 1.5% | 30 |
| 2 | 200 | 3.1% | 30 |
| 3 | 300 | 4.6% | 40 |
| 4 | 400 | 6.2% | 45 |
| 5 | 500 | 7.7% | 50 |

**B. Example of pension benefit**

| | | | | | |
|---|---|---|---|---|---|
| Age when you start to contribute | 30 | | | | |
| Annual contribution level (RMB/year) | 100 | 200 | 300 | 400 | 500 |
| Annual subsidy (RMB/year) | 30 | 30 | 40 | 45 | 50 |

| | | | | | |
|---|---|---|---|---|---|
| C=A+B: Amount received annually after age 60 (RMB/year) | 1,259 | 1,489 | 1,741 | 1,983 | 2,224 |

Panel A shows the corresponding government subsidy for each contribution level in the pension plans. Panel B provides an example to illustrate the explicit benefit of each contribution level for one who starts to contribute at age 30 and contributes for the next 15 years. The interest rate is assumed to be 2.5%, which was the 1-year interest rate in China at the time of this study.

All individual contributions and government subsidies are deposited in the individual's account. The interest rate is the 1-year base rate according to the People's Bank of China (China's central bank), which is 2.5% as of 2011. When the central bank changes the base rate, the pension plan's interest rate adjusts accordingly. Interest is compounded yearly.

Pensioners will receive their pension monthly after reaching age 60. The amount received consists of a basic pension from the government and a portion from the individual's account balance. For those who are already 60 or older, if all their eligible children living in the same village participate in the program, the parents can receive the basic pension every month without making any contributions. People between ages 45 and 60 are eligible to receive the basic pension after age 60 if they contribute each year until they reach 60. Those under age 45 are eligible to receive the basic pension after age 60 if they contribute each year for at least 15 years. The current basic pension is 80 RMB per month (960 RMB per year), which is about 18% of rural annual per capita net income in 2009 in my study county. The basic pension level of 960 RMB will be adjusted according to the price level of a given year.[3] The monthly pension benefit equals the individual account balance divided by 139 months. Thus, the monthly benefit is

$$monthly\ benefit = basic\ pension + \frac{individual\ account\ balance}{139}.$$

The new pension plans are highly subsidized by the central and local governments.[4] As an illustration, consider a farmer who is 30 years old and contributes the minimum amount (100 RMB) each year for 15 years. Assuming the interest rate is 2.5%, after age 60 the farmer is supposed to receive 1,259 RMB per year, of which about 82% comes from the government subsidy and its interest. If the farmer contributes 500 RMB, approximately 56% would come from the government subsidy. The new pension plan not only has high subsidies, but also has high returns compared to a standard

The pension program has several notable features. First, individual account balances are not forfeited at death. If pensioners die sooner than 139 months after age 60, their heirs will receive a lump sum payment that equals the remaining balance in the individual account minus the government subsidies. If pensioners live more than 139 months after age 60, they will still receive a monthly pension as an annuity until death. Second, participants may stop contributing for a few years and make up for the missed contributions later; they would only lose the subsidies for the years that they did not make contribution. Third, partial withdrawal is not allowed. They can withdraw all of their savings under the following conditions: migration, change from a rural *hukou* to an urban *hukou*, or enrollment in an urban pension plan.[5] In any case, withdrawing all one's money would result in the loss of all the subsidies one was receiving.

## 2. Theoretical Framework

To explain the pattern of pension contributions, I apply a basic discrete-time, life cycle model, augmented to incorporate uncertain lifetimes and uncertain incomes. Individuals maximize their expected lifetime utility

$$E[u(C_s) + \sum_{t=S+1}^{N} \beta^{t-S} u(C_t) \prod_{j=S}^{t-1} p_j], \tag{1}$$

subject to

$$X_{t+1} = R(X_t - C_t - Q_t) + Y_{t+1} + Z_{t+1} \text{ and } X_{t+1} \geq 0,$$

where $C_t$ represents total consumption at age $t$, $p_t$ is the probability that the individual at age $t$ survives to age $t+1$, $\beta$ is the discount factor, $X_t$ is the cash on hand (total liquid wealth), $Q_t$ is the contribution to the pension at age $t$, $Y_{t+1}$ is the income at age $t+1$ and $Z_{t+1}$ is the amount received from the pension fund after retirement. The utility function is assumed to exhibit constant relative risk aversion: $u(C) = \frac{C^{1-\rho}}{1-\rho}$.

To model the income uncertainty, I adopt Gourinchas and Parker (2002) formulation and decompose the labor income into a permanent component, $P_t$, and a transitory component, $U_t$: $Y_t = P_t U_t$ and $P_t = G_t P_{t-1} N_t$. The transitory shocks, $U_t$, are independently and identically lognormally

Following Gourinchas and Parker (2002), I write the optimal consumption rule as a function of age, $t$, and normalized cash on hand, $x_t \equiv X_t/P_t$. The Euler equation is

$$u'(c_t(x_t)) = \beta R p_t E[u'(c_{t+1}(x_{t+1}) G_{t+1} N_{t+1})], \tag{2}$$

where $c_t(x_t)$ represents the optimal consumption rule at age $t$ (normalized), and lowercase letters are normalized by the permanent component of income. I solve the dynamic programming problem by solving the Euler equation for each contribution level. I solve optimal consumption rules for each household based on age, time preferences, and risk attitude. Then I simulate optimal consumption (and therefore wealth) in each period for each household. Finally, given the optimal life cycle consumption path for each contribution level, I can calculate the lifetime utility for each contribution level and obtain the optimal contribution level (see Online Appendixes A.1 and A.2 for details).

Figure A2 compares distributions of actual contribution and calibrated contribution levels. The left-hand side shows that around 90% of rural households chose the lowest contribution level, and the right-hand side shows the benchmark model's prediction. The benchmark model captures some aspects well: most people participate in the program. However, it captures other aspects poorly: people save more in the calibration than what I observe in practice.

Next, I bootstrap the confidence interval of the calibrated contribution levels. To account for the correlation within each village, I use block bootstrap with each village as a block. I find that the mean of the contribution level is 225 RMB, with a 95% bootstrapped confidence interval [215 RMB, 236 RMB]. The average actual contribution level is 104 RMB. Therefore, these calibration results suggest that rural households should contribute more to their pension plans.

Given that the per capita balance of savings deposits in rural China in 2010 is about 8,765 RMB (Almanac of China's Finance and Banking 2011), and the return on pensions is much higher than that on savings deposits (Table B1), why do rural households decline to allocate more savings to their high-return pension plans? One possibility is that people underestimate compound interest. Evidence suggests that people tend to linearize exponential functions when assessing them intuitively (Eisenstein and Hoch 2007; Stango and Zinman 2009; McKenzie and Liersch 2019; Levy and Tasoff

I introduce a structural parameter in the utility function to measure the degree of underestimation. Consider the individual who underestimates compound interest with the following form: $f(i,t,\theta) = (1+r)^{(1-\theta)t}$. $\theta$ measures the degree of underestimation of compound interest: Unbiased consumers have $\theta = 0$ and correctly predict compound interest, while those with $0 < \theta < 1$ underestimate it. A higher $\theta$ indicates greater underestimation of compound interest. Perceived future values are then calculated using $FV = PV \cdot f(i,t,\theta)$. I incorporate underestimation of compound interest into the life cycle consumption model. Based on the experimental design and treatment effects, I structurally estimate the degree of underestimation and conduct counterfactual simulations in Section 6.

## 3. Experimental Design and Survey Data

The standard practice by which rural pensions are offered involves two steps: registration and payment. In step 1, county coordinators hire village coordinators to visit households in person to deliver information about the pension plan. Households then decide whether they would like to participate in the plan and, if so, at what level they will contribute. In step 2, households pay the given amount to village coordinators within 1 month. County coordinators then collect the list of participants and their contributions from village coordinators and administrate individual accounts for all participants with the banks.

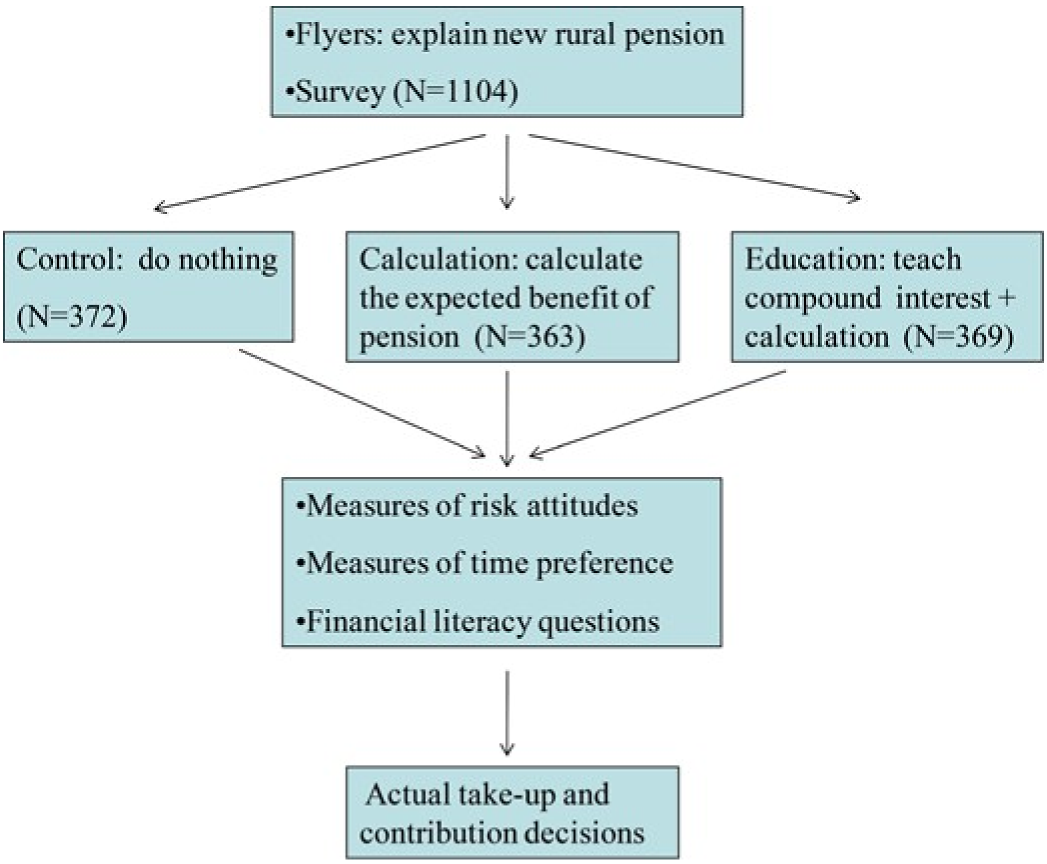
I collaborated with a county government in Shaanxi province during the registration period for the rural pension plan. The county launched the rural pension plan in 2010, and many residents made their first contributions in the same year. In 2011, 14 villages from the sample county were randomly selected as experimental sites. In these sample villages, instead of village coordinators calling on households, a research team visited them in step 1 and followed a protocol similar to that used by village coordinators in villages not included in the sample. I asked households to choose a contribution level, and provide their information to the village coordinators. In step 2, they pay the given amount to village coordinators within the next month. I collected administrative data from village coordinators after the households had made their contributions in 2011; the data include name, gender, age, village, and the contribution levels for both 2010 (baseline data) and 2011 (experimental data).

The procedures above have two advantages. First, this study takes place in a

Farmers are familiar with the logistics of separate registration and payment, because such procedures are typical in this area; also, local representatives had visited them the previous year to explain the pension program. Second, because I collected data about participants' actual contribution levels, I was able to test the impact of explaining compound interest on actual pension contributions.[6]

Figure 1 presents the experimental design. Randomization of intervention was conducted at the household level. Households were randomly assigned to 1 of 3 groups: the *Control* group, the *Calculation* group, or the *Education* group. Assignment of treatment was determined by the order of interviews for each enumerator, and the order of interviews was determined by the location of households along village roads. Enumerators were assigned to different roads in the villages and conducted door-to-door visits from the beginning of the road to the end under the guidance of village coordinators. For example, the first household on the road was assigned to the control group, the second to the calculation group, the third to the education group, and the fourth to the control group to begin the cycle again. The assignment rule, which was the same for all enumerators, helped to control for village fixed effects and enumerator fixed effects.

**Figure 1**



asked to fill out the short survey about their age, education, wealth, family members, risk attitudes, time preferences, and financial literacy. At the end of the visit, enumerators asked households to state their contribution decision. Households were informed that their decisions would be passed to local village coordinators, who would later collect the contributions based on registration in the program.

In the calculation group, enumerators followed the same procedure as the control group, but before administering the survey about preference, they calculated the expected pension benefits after age 60 if households contributed at various levels beginning at age 30. Table 1, panel B, provides the expected benefits. Enumerators went over the benefits at each contribution level with households and explained the range of differences; the purpose was to inform households of the explicit benefit amount at each contribution level without explaining the concept of compound interest. Comparing the control group and the calculation group will suggest whether explaining the benefits in general can increase participation and contribution levels in pension plans.

In the education group, the enumerators followed the same procedure for the control group, but then asked questions about compound interest, explained the concept, and provided the calculated benefit for each contribution level before administering the survey about preference. I asked three compound interest questions during the intervention, one of which is adapted from Eisenstein and Hoch (2007):

> You deposit 100 RMB as a certificate of deposit this year at a constant interest rate of 9% per year. Interest is compounded annually. How much money could you receive in 30 years? (1) Less than 300, (2) 300–500, (3) 500–1,000, (4) 1,000–1,500, or (5) more than 1,500?

Regardless of the participant's answers, the enumerator furnished the correct answer: 1,327 RMB (option 4). The enumerator then briefly explained the basic concept of compound interest in a manner similar to Eisenstein and Hoch (2007): "Compound interest means that when interest is earned, it is left in the account. In future years, interest accumulates on the full amount that is in the account, so you earn interest on the interest as well as on the original principal amount."

Table 3, panel A, provides the other two questions. Enumerators varied the interest rate (9% and 2.5%) and the number of years before receiving the debias them. Note that the questions asked are complex, and I am simply aiming to crudely measure subjects' understanding of compound interest. I also calculated expected benefits after age 60, which are the same for the calculation group.

**Table 3** Financial literacy

| | | Total | | Control | | Education Treat | |
|---|---|---|---|---|---|---|---|
| **Question** | | **%answer** | **%correct** | **%answer** | **%correct** | **%answer** | **%co** |
| | **A. Questions used during the education treatment** | | | | | | |
| a | You deposit 100 RMB as a Certificate of Deposit this year at a constant interest rate of 9% per year. Interest is compounded annually. How much money could you receive in 30 years? | | | | | 42.0 | 7.6 |
| | 1) Less than 300 2) 300-500 3) 500-1000 4) 1000-1500 5) More than 1500 | | | | | | |

| | | | |
|---|---|---|---|
| b | Suppose you were 45 years old and you deposit 100 RMB every year for 15 years at a constant interest rate of 2.5% per year. Interest is compounded annually. How much could you withdraw when you are 60 years old? | 30.6 | 14.1 |
| | 1) Less than 1800 2) 1800-2000 3) 2000-2500 4) 2500-3000 5) More than 3000 | | |
| c | Suppose you were 30 years old and you deposit 100 RMB every year for 15 years at a constant interest rate of 2.5% per year. Interest is compounded annually. How much could you withdraw when you are 60 years old? | 29.3 | 7.3 |
| | 1) Less than 1800 2) 1800-2000 3) 2000-2500 4) 2500-3000 5) More than 3000 | | |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 1 | A second hand car is selling at 60000 RMB, which is 2/3 of the new one. What is the price of a new car? | 58.4 | 34.7 | 56.5 | 33.9 | 58.8 | 35.8 |
| | 1) 90000 2) 40000 3) 80000 4) 120000 5) 180000 6) other | | | | | | |
| 2 | If you borrowed 100000 RMB from the bank, the interest rate is 2% per month and compounded monthly. How much do you owe the bank in three months? | 37.9 | 13.0 | 36.0 | 12.6 | 38.8 | 14.4 |
| | 1) Less than 102000 2) 102000 3) 102000-106000 4) 106000 5) More than 106000 | | | | | | |
| 3 | You deposit 100 RMB as a Certificate of Deposit this year at a constant interest rate of 6% per year. Interest is compounded | 33.4 | 5.6 | 29.8 | 3.5 | 35.0 | 7.0 |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | 1) Less than 300 2) 300-400 3) 400-500 4) 500-600 5) More than 600 | | | | | | |
| 4 | You deposited 10000 RMB in the bank and the interest rate is 2% per year. If the price level increases 3% per year, can you buy more than, less than, or the same amount of goods in 1 year as you could today? | 70.5 | 57.7 | 69.6 | 56.2 | 71.0 | 59.1 |
| 5 | You have two choices if you want to borrow 500000 RMB from the bank. Bank 1 requires you to pay back 600000 RMB in one month. Bank 2 requires you to pay back in one month 500000 RMB plus 15% interest. Which bank represents a better deal for you? | 52.5 | 22.8 | 49.7 | 22.0 | 53.1 | 24.1 |
| | Observations | 1,104 | 1,104 | 372 | 372 | 369 | 369 |

In summary, the calculation treatment provides households with information about the expected benefits at each contribution level. The education treatment asks households to estimate interest, explains compound interest, and provides information about its benefits.

Risk attitudes and time preferences were elicited using the questions outlined in Online Appendix Table A2.[7] These will help us to calibrate the life cycle model and conduct structural estimation. I also asked five questions to measure numeracy and financial literacy (Table 3, panel B, provides these questions, which were adopted from Banks, O'Dea, and Oldfiel 2010, Lusardi and Mitchell 2007a, Eisenstein and Hoch 2007, and Cole et al. 2012). As described previously, the survey about preference, which includes questions about financial literacy, was administered after the interventions in the education and calculation groups. I used a between-subject design based on random assignment. Comparison of the education group with the other groups allows us to test whether education changes financial literacy.[8]

Table 2 presents summary statistics for the different groups. I visited a total of 1,330 households; of these, 177 did not answer the door, 32 declined to participate in this study, and 17 households were not eligible but were mistakenly surveyed. Therefore, the sample contains 1,104 surveys. The overall nonresponse rate was 17.0%; differences in nonresponse rates between groups are not statistically significant. The sample county launched the rural pension plan in 2010 (baseline), before my interventions in 2011. The take-up rate in 2010 was 93%, and the average contribution was 104 RMB (which includes those who did not participate), so most households participated in the pension plans but chose the lowest contribution level. The last column shows the $p$-values for the Wald test of equal means of the three groups. Most control variables are balanced between different treatment groups.

**Table 2** Summary statistics

| | Total | Control | Calculation treatment | Education treatment | *p*-value |
|---|---|---|---|---|---|
| ***A. Baseline*** | | | | | |
| Male | 0.67 | 0.64 | 0.70 | 0.67 | .22 |
| | (0.47) | (0.48) | (0.46) | (0.47) | |
| Age | 44.90 | 44.87 | 44.40 | 45.42 | .30 |

| | | | | | |
|---|---|---|---|---|---|
| Household size | 4.78 | 4.80 | 4.82 | 4.73 | .66 |
| | (1.34) | (1.37) | (1.38) | (1.29) | |
| Land for production | 3.75 | 3.75 | 3.76 | 3.73 | .98 |
| | (1.61) | (1.66) | (1.59) | (1.57) | |
| Own business | 0.14 | 0.16 | 0.12 | 0.13 | .32 |
| | (0.34) | (0.36) | (0.32) | (0.34) | |
| Own a car | 0.10 | 0.13 | 0.11 | 0.06 | .00 |
| | (0.30) | (0.34) | (0.31) | (0.24) | |
| Own a motorcycle | 0.44 | 0.48 | 0.44 | 0.40 | .09 |
| | (0.50) | (0.50) | (0.50) | (0.49) | |
| Saving for children | 0.81 | 0.79 | 0.83 | 0.80 | .50 |
| | (0.39) | (0.40) | (0.38) | (0.40) | |
| Saving for future when she/he is old | 0.25 | 0.26 | 0.26 | 0.25 | .92 |
| | (0.44) | (0.44) | (0.44) | (0.43) | |
| Have a private pension plan | 0.13 | 0.14 | 0.13 | 0.13 | .83 |
| | (0.34) | (0.35) | (0.33) | (0.33) | |
| Receiving reimbursement from government health insurance | 0.91 | 0.93 | 0.90 | 0.91 | .53 |
| | (0.28) | (0.25) | (0.31) | (0.28) | |
| Baseline take-up | 0.93 | 0.92 | 0.93 | 0.94 | .34 |
| | (0.26) | (0.28) | (0.26) | (0.23) | |
| Baseline contribution level | 104.17 | 104.57 | 106.34 | 101.63 | .57 |
| | (65.28) | (71.23) | (70.03) | (53.14) | |
| *B. Post-intervention* | | | | | |
| Risk aversion | 4.04 | 4.11 | 3.98 | 4.03 | .56 |
| | (1.68) | (1.65) | (1.71) | (1.69) | |

| | | | | | |
|---|---|---|---|---|---|
| Absolute distance to the correct answer to question 1 | −1.08 | −1.13 | −1.06 | −1.07 | .58 |
| | (0.99) | (1.00) | (0.98) | (0.99) | |
| Absolute distance to the correct answer to question 2 | −1.72 | −1.73 | −1.74 | −1.68 | .72 |
| | (1.00) | (1.00) | (0.97) | (1.01) | |
| Absolute distance to the correct answer to question 3 | −2.95 | −3.05 | −2.92 | −2.89 | .06 |
| | (1.09) | (1.00) | (1.20) | (1.13) | |
| Absolute distance to the correct answer to question 4 | −0.80 | −0.82 | −0.790 | −0.78 | .78 |
| | (0.99) | (1.00) | (0.99) | (0.99) | |
| Absolute distance to the correct answer to question 5 | −1.55 | −1.59 | −1.53 | −1.53 | .59 |
| | (1.00) | (1.00) | (0.98) | (1.01) | |
| Take-up | 0.98 | 0.98 | 0.98 | 0.99 | .83 |
| | (0.13) | (0.13) | (0.14) | (0.12) | |
| Contribution level | 157.16 | 133.06 | 156.19 | 182.38 | .00 |
| | (123.72) | (96.62) | (125.19) | (140.80) | |
| Observations | 1,104 | 372 | 363 | 369 | |

Standard deviations are in parentheses. Baseline take-up and baseline contribution are the actual pension participation and contributions before my intervention in 2010. Absolute distance to the correct answer to questions 1–5 is normalized by standard deviation of the control group. $p$-values are for Wald test of equal means of three groups. *$p < .1$; **$p < .05$; ***$p < .01$.

Table 3 presents results for financial literacy. More than half answered question 4 correctly, which suggests that they have a basic understanding of inflation and purchasing power. Thirteen percent answered question 2 correctly, and 5.6% answered question 3 correctly, which suggests that most

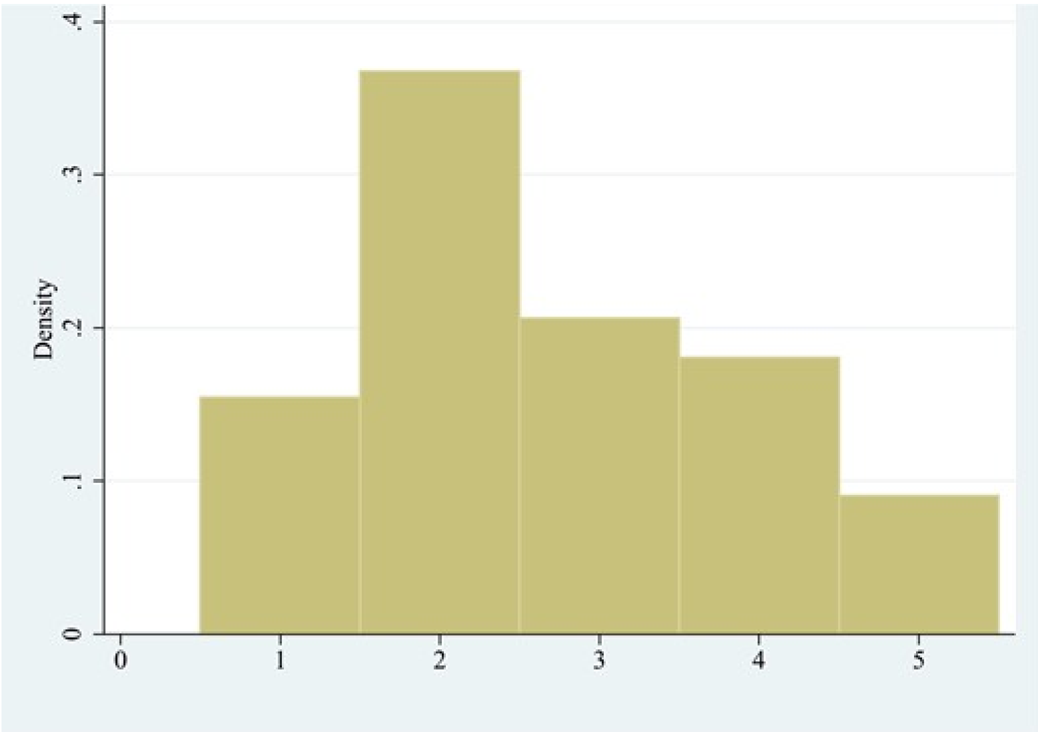
# 4. Empirical Results

In this section, I first show that rural households underestimate the value of savings with compound interest, then demonstrate that financial education about compound interest can increase households' contribution levels. I also analyze possible channels for the effects of financial education about compound interest on pension contributions.

## 4.1 Misunderstanding of compound interest

I measure misunderstanding of compound interest using the compound interest question A that preceded the intervention in the education group. Figure 2 shows responses to the question.

**Figure 2**




Response to compound interest question

This figure shows the distribution of responses to the compound interest rate question before intervention. I asked subjects the following question: You deposit 100 RMB as a certificate of deposit this year at a constant interest rate of 9% per year. Interest is compounded annually. How much money could you receive in 30 years? (1) Less than 300, (2) 300–500, (3) 500–1,000, (4) 1,000–1,500, or (5) more than 1,500? 1, 2, 3, underestimate; 4, correct; 5, overestimate.

Of 369 households in the education group, 201 could not answer the

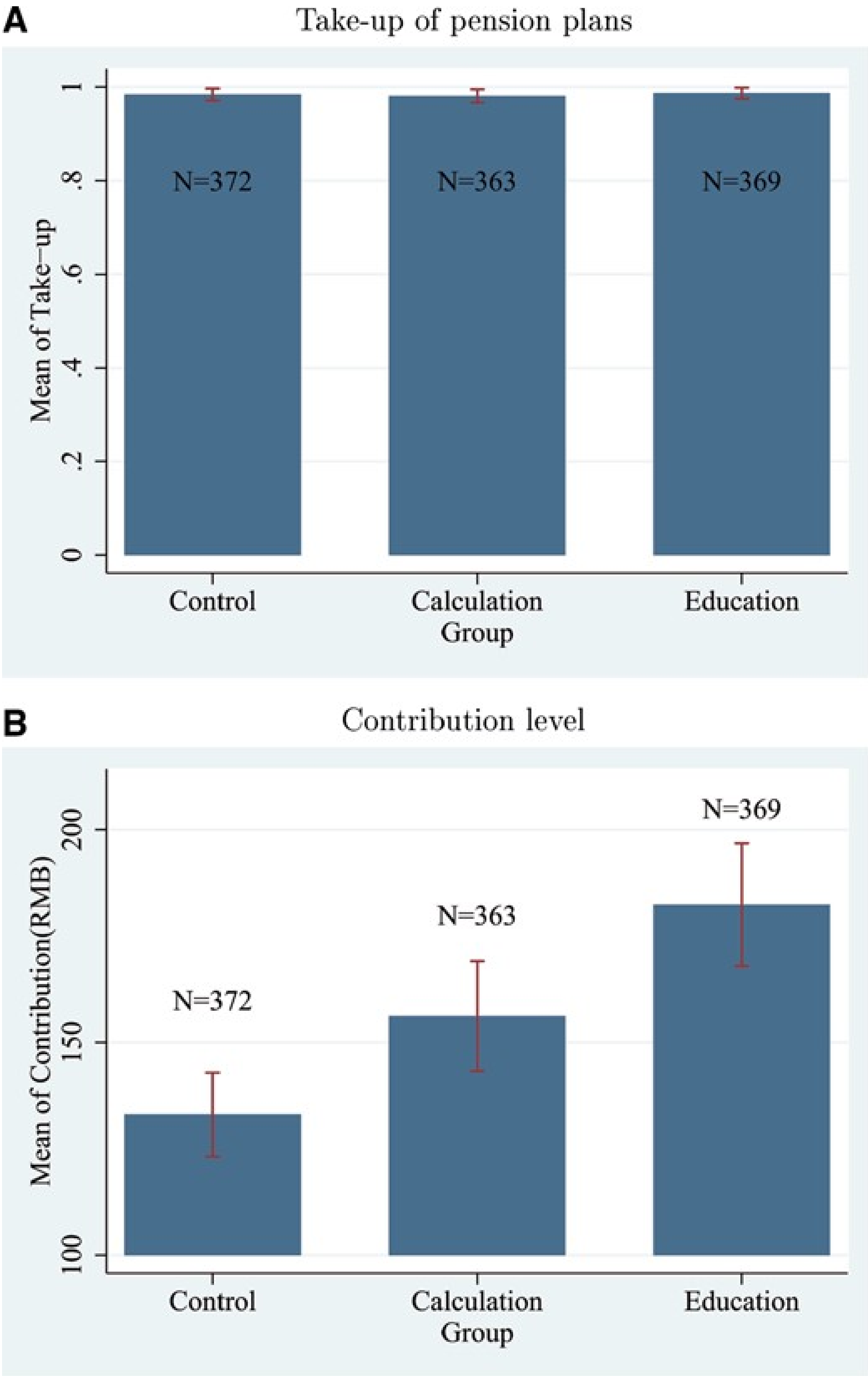
9% chose option 5, which can be characterized as overestimating the value of compound interest. From Figure 2, I see that rural households underestimate the value of savings with compound interest. Although the compound interest questions are different, the results are consistent with those of Lusardi and Mitchell (2007b), who find that only 18% of subjects in the Health and Retirement Study answered the compound interest question correctly.

Online Appendix Figure A1 shows responses to compound interest questions B and C. For question B, 26% underestimated the value of compound interest. For question C, 59% underestimated the value of compound interest. Responses to all three compound interest questions suggest that a significant portion of rural households underestimate compound interest, even at a relatively low interest rate of 2.5%.

## 4.2 Impact of education on pension contributions

Figure 3, panel A, shows that almost all the households in the three groups participated in the pension plan and that there is no significant treatment effect. Figure 3, panel B, shows the treatment effect on contributions. In the control group, the average contribution is 133 RMB. In the calculation group, the average contribution increases to 156 RMB. In the education group, the average contribution increases to 182 RMB. These results suggest that both the education treatment and the calculation treatment increase contribution levels, but that the education treatment is more effective.

**Figure 3**

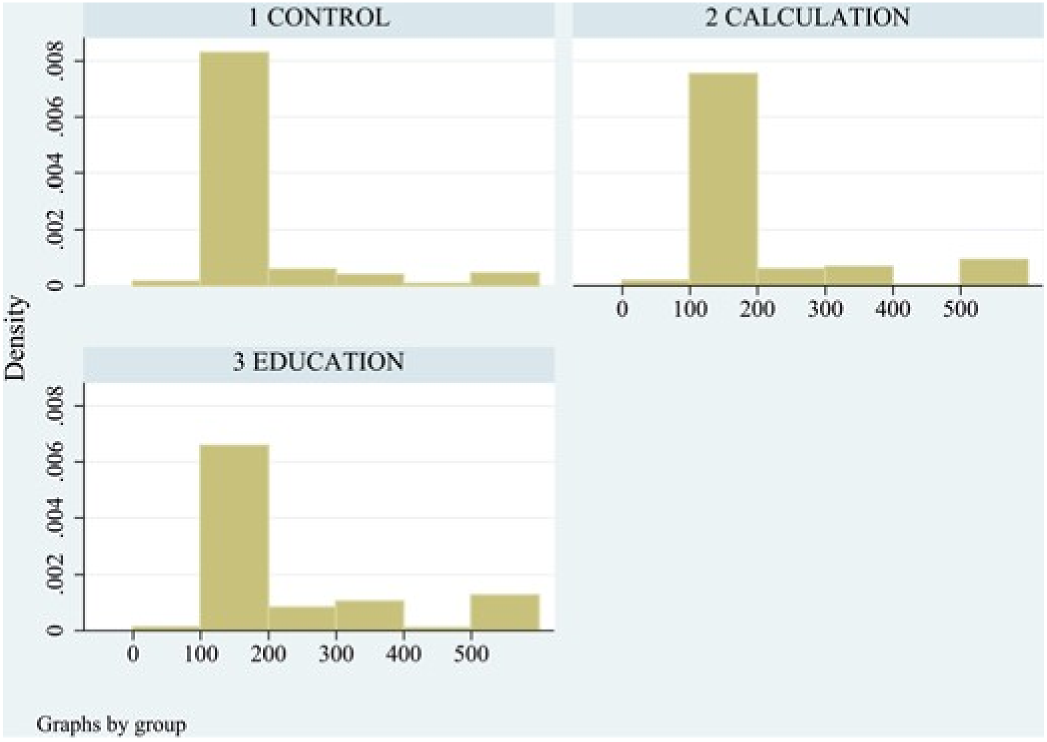



Treatment effects

Panel A shows the treatment effect on take-up of pension plans. In the control group, take-up is 98.4%. In the calculation group, take-up is 98.1%. In the education group, take-up is 98.6%. This suggests that almost all the households in the three groups participate in the pension plans. Panel B shows the treatment effect on contributions to pension plans.

subjects still contribute 100 RMB in the pension. In the education group, however, more subjects contribute 300 RMB and 500 RMB relative to the other two groups.

**Figure 4**




Distributions of contribution levels

This figure shows the distribution of contribution levels for different groups. Contribution level 1 corresponds to 100 RMB, and levels 2 to 5 correspond to 200 RMB to 500 RMB.

I estimate the treatment effect on contributions through an ordinary least squares (OLS) regression:

$$q_{ij} = \alpha_j + \alpha_k + \beta_e \cdot Te_{ij} + \beta_c \cdot Tc_{ij} + \phi \cdot X_{ij} + \epsilon_{ij}, \tag{3}$$

where $q_{ij}$ is the contribution levels after my intervention in 2011 or the changes in contribution levels from 2010 to 2011 for household $i$ in natural village $j$.[9] $Te_{ij}$ is an indicator for the education treatment and $Tc_{ij}$ is an indicator for the calculation treatment. Random assignment implies that $\beta_e$ is an unbiased estimate of the reduced-form intention-to-treat (ITT) education treatment effect, and $\beta_c$ is an unbiased estimate of the ITT calculation treatment effect. $X_{ij}$ are household characteristics (e.g., gender, age, years of education, household size, land for production, car ownership). $\alpha_j$ and $\alpha_k$ are village fixed effects and enumerator fixed effects, respectively.

| Specification: | OLS regression | | | |
|---|---|---|---|---|
| Dep. var.: | Individual adoption of pension | Individual contribution level of pension | Change in contribution level of pension | Individual contributic pension |
| Sample: | All sample | | | |
| | 1 | 2 | 3 | 4 |
| Education | 0.004 | 53.06 | 54.57 | 68.71 |
| | (0.009) | (9.28)*** | (8.94)*** | (14.85)*** |
| Calculation | −0.002 | 25.22 | 22.34 | 18.68 |
| | (0.009) | (9.24)*** | (8.13)*** | (12.97) |
| Education*Overestimation (option 5) | | | | |
| | | | | |
| Education*Correct estimation (option 4) | | | | |
| | | | | |
| Education*Underestimation (option 3) | | | | |
| | | | | |
| Education*Underestimation (option 2) | | | | |
| | | | | |
| Education*Underestimation (option 1) | | | | |
| | | | | |
| Education*Baseline contribution | | | | −0.14 |
| | | | | (0.11) |
| Calculation*Baseline contribution | | | | 0.04 |

| | | | | |
|---|---|---|---|---|
| | | | | (0.08)*** |
| Wald test: $\beta_e = \beta_c$ | | | | |
| *p*-value | .5009 | .0064*** | .0004*** | |
| Obs. | 1,104 | 1,104 | 1,104 | 1,104 |
| Omitted treatment | Control | | | |
| Mean of dep. var. for omitted treatment: | 0.0984 | 133.06 | 28.49 | 133.06 |
| Fixed effects for village and enumerator | Y | Y | Y | Y |
| R-squared | 0.0648 | 0.0895 | 0.0963 | 0.2295 |

Standard errors are clustered by ninety-three natural villages. Robust standard errors are clustered and in parentheses. *$p<.1$; **$p<.05$; ***$p<.01$. In Column 1, the dependent variable is individual pension participation. In Columns 2, 4, and 5, the dependent variable is individual contribution level after my intervention in 2011. In Columns 3, the dependent variable is changes in individual contribution level from 2010 to 2011. In Column 5, I restrict the sample to the control group and the education group only. $\beta_e$ is the coefficient of the education treatment and $\beta_c$ is the coefficient of the calculation treatment.

**Table 6** Effect of the education and calculation interventions on financial literacy

| **Specification:** | **SUR regression** | | | | | |
|---|---|---|---|---|---|---|
| **Dep. var.:** | **Absolute distance to the correct answer** | | | | | **Average absolut answer** |
| | **Question 1** | **Question 2** | **Question 3** | **Question 4** | **Question 5** | **Questio 2 and 3** |
| **Sample:** | **All sample** | | | | | |
| | **1** | **2** | **3** | **4** | **5** | **6** |
| Education | 0.078 | 0.062 | 0.186 | 0.053 | 0.061 | 0.124 |
| | (0.067) | (0.069) | (0.074)** | (0.068) | (0.068) | (0.063)* |
| Calculation | 0.071 | −0.011 | 0.120 | 0.029 | 0.043 | 0.055 |
| | (0.067) | (0.069) | (0.074) | (0.068) | (0.068) | (0.057) |

| | | | | | | |
|---|---|---|---|---|---|---|
| Mean of dep. var. for omitted treatment: | −1.13 | −1.73 | −3.06 | −0.83 | −1.60 | |
| Social-economic variables | Y | Y | Y | Y | Y | Y |
| Fixed effects for village and enumerator | Y | Y | Y | Y | Y | Y |
| R-squared | 0.1752 | 0.1550 | 0.1632 | 0.1446 | 0.1545 | |

In Columns 1 to 5, the dependent variables are the absolute distance between the chosen answers and the correct answers for questions 1 to 5, normalized by the standard deviation of the control group. Column 6 reports average standardized treatment effects on questions 2 and 3, of which both are compound interest questions. Column 7 reports average standardized treatment effects on questions 1, 4 and 5, of which none of them are related to compound interest. Column 8 reports average standardized treatment effects on all questions. Standard errors are clustered by ninety-three natural villages. Robust standard errors are clustered and in parentheses. *$p< .1$; **$p< .05$; ***$p< .01$.

In Column 1, the dependent variable is individual take-up after intervention. There is no evidence of a treatment effect on take-up. This could potentially be due to a high participation rate. In Column 2, the dependent variable is the individual contribution level after intervention. Variables on the right-hand side are treatment indicators, socioeconomic variables, and the fixed effects of natural villages and enumerators. The education treatment increases the contribution by 53 RMB—an increase of 40% relative to the average contribution of 133 RMB in the control group—and is significant at the 1% level. The effect of the calculation treatment (25.22) is positive and statistically significant at the 1% level. The impact of the education treatment is greater than that of the calculation treatment, and the Wald test shows that it is significant at the 1% level. In Column 3, the dependent variable is changes in individual contribution level from 2010 to 2011, and I run the same regression from Column 2. Coefficients are of similar magnitude and have the same direction as those in Column 2. In Column 4, I study the heterogeneous effect with respect to the baseline contribution in 2010. Coefficients of the interaction between treatment indicators and baseline contribution are not significantly different from zero. This suggests that treatment effects are similar across different initial contributions. Online Appendix B3 reports the robustness analysis and shows that the main between the control group and the level implied by the benchmark model. I find that the treatment effect accounts for 55% of the gap between the control group and the benchmark model prediction, with a 95% bootstrap confidence interval [31%, 77%]. This suggests that my interventions reduce the gap between actual decisions and the level implied by the benchmark model.

To study the long-run effect, I also collect the new administrative data for pension contributions in the year 2018 and merge the administrative data with the existing data set. The administrative data includes information about name, gender, age, village, and the contribution levels in the year 2018. Equation (3) estimates the long-run effect. I find that the pension contribution in the education treatment is still 45 RMB larger than that in the control group and is significant at the 1% level. The difference between the calculation treatment and the control group is 21 RMB and statistically significant at the 5% level. Only 9% of participants change their pension contribution in the long run. This is likely due to inertia when choosing their pension plans (Madrian and Shea 2001). These results suggest that the long-run effect is similar to the short-run effect in Table 4, and the effect on contribution persists over time.

In sum, the education treatment increases the contribution by 49–53 RMB, resulting in an increase of around 37%–40% relative to the average contribution of 133 RMB in the control group.[10] The effect on contribution persists over time. This suggests that the financial education has a positive and significant effect on retirement savings for rural households.

## 4.3 Possible channels

For these findings to inform theory, more information is needed to analyze the mechanisms through which this effect could work. Possible explanations include: (1) learning the expected benefits of pensions through a better understanding of compound interest and (2) learning the expected benefits of pensions in general. Grasping the difference between the two channels is important: it will help us understand the factors involved in pension contributions and design better interventions to improve decisions about household retirement savings. Moreover, understanding individual bias about compound interest has important implications in other household finance decisions, such as life cycle consumption (Levy and Tasoff 2016). The experiment is designed to distinguish between these mechanisms.

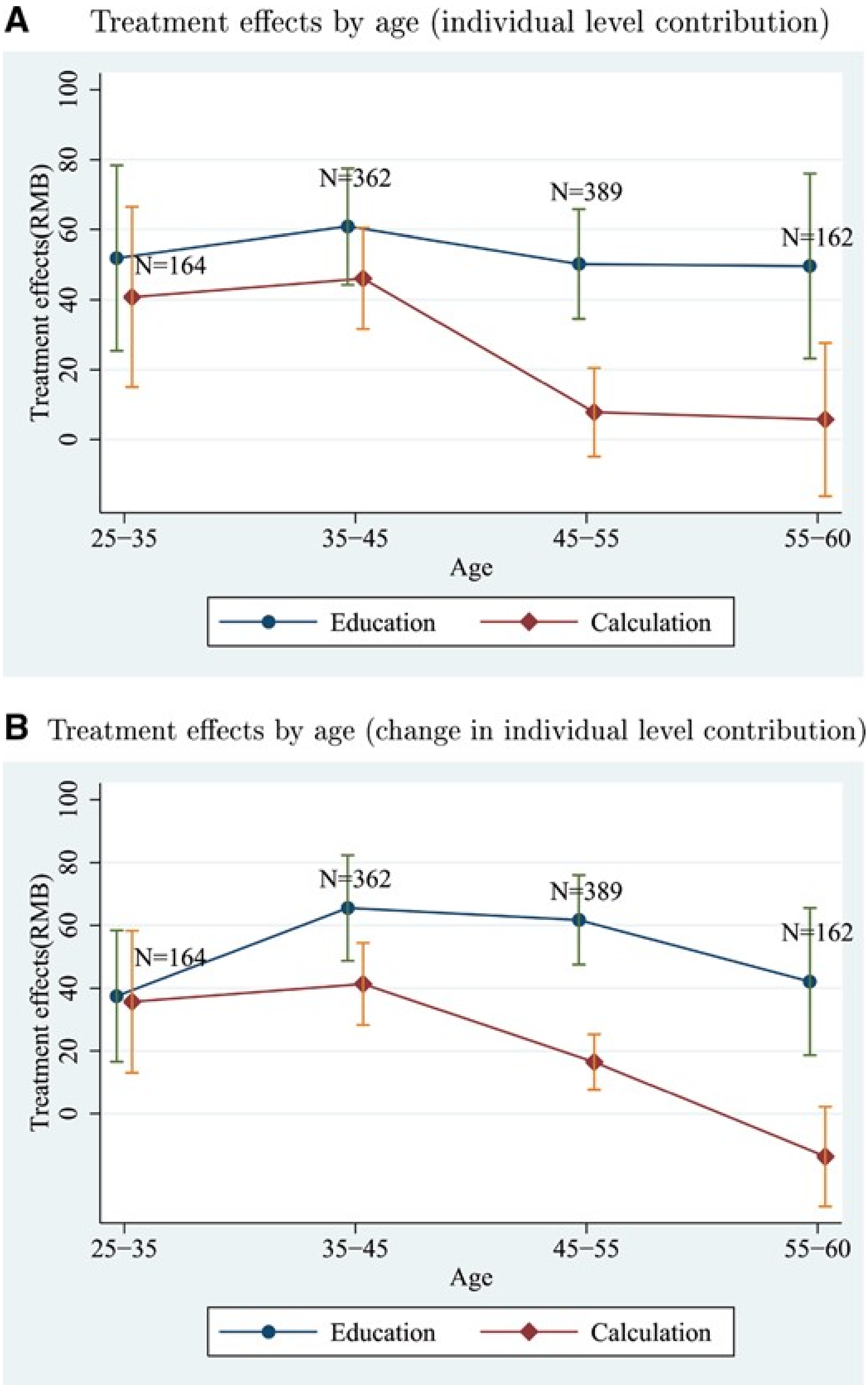
increase household contribution levels by helping households correct their erroneous understanding of compound interest. If learning about the concept of compound interest can explain the treatment effects, I should have several predictions. First, the treatment effects should depend on the initial understanding of compound interest: those who overestimate compound interest should have smaller treatment effects compared to those who underestimate compound interest. Second, I should observe that the education treatment has larger effects than the calculation treatment, because the only difference between these two groups is that the education treatment explains compound interest. Third, the education treatment should increase their understanding of compound interest, but should have no effect on other financial literacy questions.

I first analyze heterogeneous treatment effects. In Table 4, Column 5, I study the heterogeneous treatment effects based on the initial understanding of compound interest. The measurement of compound interest in the calculation group is affected by the intervention, so I restrict my sample to the control group and the education group only. I create five indicators based on the participant's choice on question A before the treatment in the education group or question 3 in the control group. These indicators measure subjects' initial understanding of compound interest, because subjects do not receive any treatment before answering the questions.

I find that the effect of the education treatment is 56 RMB for those who were unable to provide an answer to the compound interest question. However, the treatment effect is 78 RMB smaller if they overestimate compound interest. The effect of the interaction term is significant at the 1% level. In fact, those who overestimate compound interest reduce their contributions by about 22 RMB after the education treatment, though this is not statistically significant ($p$ = .42). The treatment effect is 30 RMB if they correctly estimate compound interest ($p$ = .54), and 70 RMB for those who have the lowest range of estimation ($p$ = .06). Although the treatment effect is not monotonic as participants underestimate more, the pattern is clear: the treatment effect is larger for those who underestimate compound interest than for those who overestimate compound interest. Hence, the evidence is consistent with understanding the concept of compound interest.

Second, I study the comparison between the education group and the calculation group. There could be two explanations for the different treatment effects between the education and calculation groups. First, explaining why the benefit is large might increase the credibility of the from age 30. As such, those who are around 30 can learn the expected benefits directly from the calculation treatment. Those who are closer to 40 or 50, however, only learn the expected benefit for participants who begin at age 30 and, as a result, must link those benefits to their own choices. Therefore, the second explanation has two predictions: (1) the calculation treatment should have similar treatment effects as the education treatment for those around age 30, because teaching compound interest does not provide more information about the expected benefit; (2) the calculation treatment should have lower treatment effects than the education treatment when age increases, because teaching compound interest increases subjects' ability to link the described benefits at age 30 to their own choices. Then I can use the treatment effects for different ages to distinguish these two hypotheses. Figure 5 shows the treatment effects of the education and calculation groups for different ages. In Figure 5, panel A, the dependent variable is the individual level contribution. In Figure 5, panel B, the dependent variable is the change in the individual's contribution level.

**Figure 5**




Treatment effects by age

Panel A shows treatment effects for the education and calculation groups by age group. The dependent variable is individual level contribution. The horizontal axis represents four age groups. The vertical axis is the treatment effects. I report one standard error for each coefficient. Panel B shows the treatment effects for the education and calculation groups

I find that the treatment effects of the education and calculation groups are similar for those who are around age 30, but differ when age increases. The treatment effect of the calculation group is lower than that of the education group for those who are around ages 40, 50, and 60.

I estimate the effects of the education and calculation treatments by age group:

$$q_{ij} = \alpha_j + \alpha_k + \sum_{k=2}^{4} \beta_k \cdot Tk_{ij} + \sum_{k=1}^{4} \beta_{\epsilon k} \cdot Tek_{ij} + \sum_{k=1}^{4} \beta_{ck} \cdot Tck_{ij} + \phi \cdot X_{ij} + \epsilon_{ij}, \tag{4}$$

where $k$ is the indicator of age group. $k$ equals 1, 2, 3, or 4 for those in age groups 25–35, 35–45, 45–55, and 55–60, respectively. $Tk_{ij}$, $Tek_{ij}$, and $Tck_{ij}$ are indicators for age group $k$ in the control group, the education group, and the calculation group, respectively. Therefore, the treatment effects of the education and calculation groups in age group 1 are $\beta_{\epsilon 1}$ and $\beta_{c1}$. The treatment effects of the education and calculation groups in age group $k$ $(k \neq 1)$ are $\beta_{\epsilon k} - \beta_k$ and $\beta_{ck} - \beta_k$.

Table 5, panel A, reports the results for the four age groups. In Column 1, the dependent variable is the individual-level contribution. For those who are around age 30, the difference between the treatment effects of the education and calculation groups is 11 RMB and is insignificant. The differences are 15, 42, and 44 RMB for those who are around age 40, 50, and 60, respectively. The difference is significant at the 5% level for those who are around age 50, and it is close to marginally significant for those who are around age 60 ($p$ = .1179). In Column 2, the dependent variable is the change in individual-level contribution. The results are similar to Column 1. Differences between the treatment effects of the education and calculation groups are significant at least at the 5% level for those who are around ages 50 to 60. Table 5, panel B, also analyzes the results for two age groups to check robustness, and results are similar to panel A. Differences between the treatment effects of the education and calculation groups are 37–41 RMB for those who are between ages 40 and 60, and are significant at the 1% level.

**Table 5** Effect of education and calculation on contribution level by age group

| **Specification:** | **OLS regression** | |
|---|---|---|
| **Dep. var.:** | **Individual** | **Change in individual** |

| | 1 | 2 |
|---|---|---|
| **A. Four age groups** | | |
| Effect of the education treatment for those between age 25 and 35 ($\beta_{e1}$) | 51.89 | 37.48 |
| | (26.47)* | (20.90)* |
| Effect of the calculation treatment for those between age 25 and 35 ($\beta_{c1}$) | 40.76 | 35.68 |
| | (25.79) | (22.60) |
| Effect of the education treatment for those between age 35 and 45 ($\beta_{e2}$-$\beta_2$) | 60.88 | 65.52 |
| | (16.61)*** | (16.83)** |
| Effect of the calculation treatment for those between age 35 and 45 ($\beta_{c1}$-$\beta_2$) | 46.07 | 41.35 |
| | (14.41)*** | (13.06)*** |
| Effect of the education treatment for those between age 45 and 55 ($\beta_{e3}$-$\beta_3$) | 50.20 | 61.70 |
| | (15.65)*** | (14.19)*** |
| Effect of the calculation treatment for those between age 45 and 55 ($\beta_{c3}$-$\beta_3$) | 7.79 | 16.49 |
| | (12.70) | (8.81)* |
| Effect of the education treatment for those between age 55 and 60 ($\beta_{e4}$-$\beta_4$) | 49.62 | 42.09 |
| | (26.41)* | (23.40)* |
| Effect of the calculation treatment for those between age 55 and 60 ($\beta_{c4}$-$\beta_4$) | 5.71 | –13.65 |
| | (21.93) | (15.81) |
| Prob > F(Education=Calculation) for those between age 25 and 35 | 0.6982 | 0.9460 |
| Prob > F(Education=Calculation) for those between age 35 and 45 | 0.4358 | 0.1947 |
| Prob > F(Education=Calculation) for those between age 45 and 55 | 0.0136** | 0.0030*** |

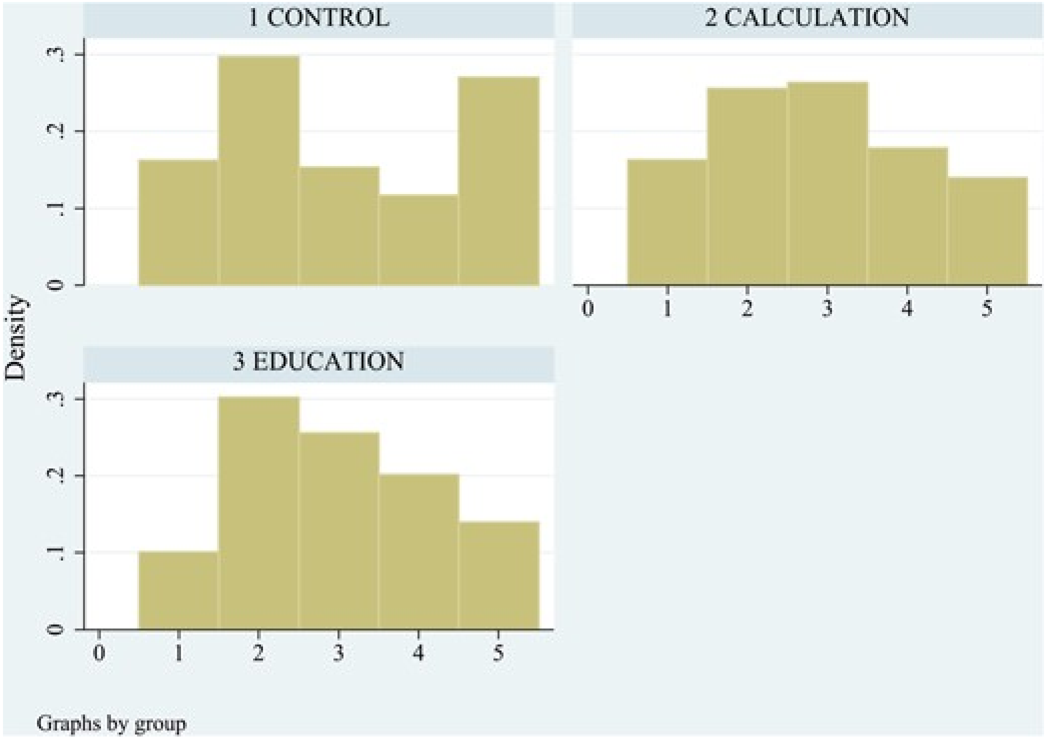
| | | |
|---|---|---|
| Effect of the education treatment for those between age 20 and 40 | 55.34 | 49.11 |
| | (18.03)*** | (17.01)*** |
| Effect of the calculation treatment for those between age 20 and 40 | 43.16 | 35.35 |
| | (17.34)** | (16.97)** |
| Effect of the education treatment for those between age 40 and 60 | 50.05 | 55.25 |
| | (11.46)*** | (11.18)*** |
| Effect of the calculation treatment for those between age 40 and 60 | 13.93 | 14.54 |
| | (10.49) | (8.10)* |
| Prob > F(Education=Calculation) for those between age 20 and 40 | 0.5065 | 0.4406 |
| Prob > F(Education=Calculation) for those between age 40 and 60 | 0.0077*** | 0.0007*** |

Standard errors are clustered by ninety-three natural villages. Robust standard errors are clustered and in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$. Panel A reports the results for four age groups. Panel B reports the results for two age groups as a test for robustness. In Column 1, the dependent variable is the individual level contribution. In Column 2, the dependent variable is the change in the individual level contribution.

I also investigate the qualitative explanation for participants' contribution decisions using the same specification in Table 5. I find that the likelihood of subjects basing their contribution decisions on learning the benefits of compound interest is greater in the education group than in the calculation group for older subjects (see Online Appendix B3 for details).

Third, I study the impact of my interventions on the understanding of compound interest. Figure 6 shows the responses to the compound interest question (question 3) after the intervention in different groups. I only include the 369 households that answered the question. The correct answer is 574 RMB, which is option 4. One can clearly see that rural households underestimate the value of savings from compound interest after the intervention. Although underestimation of compound interest persists after the intervention, there are fewer extremely wrong answers (option 1) and more correct answers (option 4) in the education group than in the other

**Figure 6**




Response to compound interest question

This figure shows the distribution of responses to the compound interest question after intervention. I asked subjects the following question: You deposit 100 RMB as a certificate of deposit this year at a constant interest rate of 6% per year. Interest is compounded annually. How much money could you receive in 30 years? (1) Less than 300, (2) 300–400, (3) 400–500, (4) 500–600, or (5) more than 600? Figures only include those who answered the question and exclude those who did not know. 1, 2, 3, underestimate; 4, correct; 5, overestimate.

To estimate the treatment effect on financial literacy, I follow the specification in Equation 1 and change the dependent variable to financial literacy. I use absolute distance to the correct answer to measure financial literacy. Absolute distance measures how close the respondents' answers are to the correct one, even when they are incorrect. This can help to measure the magnitude of misunderstanding of compound interest. I take the negative sign of the measure so that the higher the measure, the better the understanding of compound interest. Table 2, panel B, reports summary statistics.

Table 6 presents results. I run a seemingly unrelated regression (SUR) with five financial literacy questions. In Columns 1 to 5, the dependent variables are the absolute distance between the chosen answer and the correct answer for questions 1 to 5 (which are shown in Table 3), normalized by the standard deviation of the control group. Because the information in the education not significant. The only exception is question 3, the compound interest question. In Column 3, the effect is positive and significant at the 5% level. Therefore, education reduces the distance from the correct answer by about one-sixth of a standard deviation; this demonstrates that financial education increases people's understanding of compound interest.[11] Robustness checks suggest that other measures of financial literacy show similar results (Online Appendix B.4 provides a complete description of the measurement of absolute distance and robustness checks).

To illustrate the impact of the intervention on overall financial literacy, I follow Kling, Liebman, and Katz (2007) and construct average standardized treatment effects. In Columns 6 to 8, I report average standardized treatment effects on three combinations of questions. In Column 6, I report average standardized treatment effects on questions 2 and 3, both of which are compound interest questions. The effect of financial education is positive and significant at the 5% level. In Column 7, I report average standardized treatment effects on questions 1, 4, and 5, which are unrelated to compound interest. The effect of financial education is positive but not significant. In Column 8, I report average standardized treatment effects on all questions, which is positive and significant at the 10% level. This suggests that financial education has a positive and significant effect on overall financial literacy, and especially on the understanding of compound interest.

In sum, I find that the treatment effect of the education group is larger for those who underestimate compound interest than for those who overestimate compound interest. I also find that the treatment effect of the education group is greater than that of the calculation group for older subjects. The likelihood of subjects basing their contribution decisions on learning the benefits of compound interest is greater in the education group than in the calculation group for older subjects. These results support the notion that the different treatment effects of the education and calculation groups are likely due to participants' ability to link the benefit to their own choices. Moreover, although rural households underestimate compound interest and contribute less to pension plans, education about compound interest can improve people's understanding of compound interest. Therefore, these results demonstrate that the understanding of compound interest is the main channel for the treatment effects.

### 4.3.2 Alternative explanations for treatment effects

An alternative explanation is that learning the benefits of a pension in two interventions indicates that households acquire information about compound interest during the education. Thus, learning the benefits of a pension cannot fully explain the treatment effect of the education group.

Another concern is that enumerators could have put more effort into just persuading households in the treatment groups to contribute more to the pension plan, without explaining the compound interest. However, stronger persuasion is unlikely to explain the better understanding of compound interest after the education treatment. Moreover, there are no incentives for enumerators when participants contribute either more or less. Enumerators are instructed that the contribution is voluntary and that they should not push participants to contribute more or less.

Another explanation is that enumerators provide a more intensive treatment when they perceive that participants are more biased. This is consistent with the finding that the treatment effect is larger for those who underestimate compound interest. To test this explanation, I study the enumerators' efforts when they interact with participants with different degrees of bias. Although I do not have a direct measure of treatment effort for the education group, I have some suggestive evidence to show this is unlikely to be true. First, the main prediction of enumerators' biased treatment is that enumerators should spend more time explaining the question to more biased participants. I use the number of minutes enumerators required to administer the survey as a proxy for effort and find that the average time is 28–30 minutes for those with a different initial understanding of compound interest in the education group. Thus, there is no evidence that enumerators' biased treatment can explain the larger effects of more biased participants. Moreover, enumerators were asked to follow the instructions and provide similar treatment intensity to those in the education group, in order to minimize any effect from enumerators.

## 5. Structural Estimation

The experiment demonstrates that the education treatment increases both understanding of compound interest and pension contributions. In this section, I bridge the gap between the theoretical framework and experimental results by formally estimating risk aversion parameters, time preferences parameters, and parameters of underestimating compound interest in a life cycle model. The experimental design has two advantages to conduct the structural estimation with a life cycle model. First, I have jointly estimate preference and allow for individual heterogeneity in these parameters.

I use both survey data and actual pension contributions from the experimental data to estimate risk aversion parameters, time preference parameters, and parameters of underestimating compound interest, and Table 7 reports the results. The measurement of compound interest in the calculation group is affected by the intervention, so I restrict my sample to the control group and the education group only. I estimate two specifications. In specification 1, I attempt to recover the values of four key preference parameters: the coefficient of relative risk aversion, $\rho$; the discount factor, $\beta$; the magnitude of underestimating compound interest in the control group, $\theta_1$; and the magnitude of underestimating compound interest in the education group, $\theta_2$. The estimation is based on twenty-five moment conditions: five from risk attitude questions, six from time preference questions, twelve from compound interest questions, one from actual pension contributions in the control group, and one from actual pension contributions in the education group (see Online Appendix A.4 for calculations about moment conditions). The parameter estimates, $\hat{\Theta} = (\hat{\rho}, \hat{\beta}, \hat{\theta_1}, \hat{\theta_2})$ , minimize the generalized distance of the sample moments from zero.

**Table 7** Structural estimation

| **Parameter** | **Symbol** | | |
|---|---|---|---|
| | | **(1)** | **(2)** |
| **A. Random coefficients** | | | |
| Risk aversion | $\rho$ | 1.54 | 1.64 |
| | | (0.032) | (0.019) |
| Discount factor | $\beta$ | 0.94 | 0.95 |
| | | (0.034) | (0.083) |
| Exponential growth bias in the control group | $\theta_1$ | 0.67 | |
| | | (0.001) | |
| Exponential growth bias in the education group | $\theta_2$ | 0.09 | |
| | | (0.001) | |

| | | |
|---|---|---|
| Mean of the logarithm of exponential growth bias in the education group | $\mu_2$ | −2.04 |
| | | (0.015) |
| Standard deviation of the logarithm of exponential growth bias | $\sigma$ | 1.07 |
| | | (0.022) |
| **B. Distribution of coefficients** | | |
| Mean of exponential growth bias in the control group | | 0.50 |
| Standard deviation of exponential growth bias in the control group | | 0.25 |
| Mean of exponential growth bias in the education group | | 0.07 |
| Standard deviation of exponential growth bias in the education group | | 0.03 |
| **C. Counterfactual simulation** | | Pension contribution (RMB) |
| Baseline | | 155 |
| Increase the subsidy by 100% | | 158 |
| Financial education | | 177 |

I use both survey data and actual pension contributions from the experimental data to estimate risk aversion parameters, time preference parameters, and parameters of underestimating compound interest. Column 1 estimates four parameters: the coefficient of relative risk aversion, $\rho$; the discount factor, $\beta$; the magnitude of underestimating compound interest in the control group, $\theta_1$; and the magnitude of underestimating compound interest in the control group, $\theta_2$. Column 2 assumes that the parameters of underestimating compound interest are lognormally distributed in both the education group and the control groups and estimate five parameters. $\mu_1$ and $\sigma$ are the mean and standard deviation of the logarithm of exponential growth bias in the control group, and $\mu_2$ and $\sigma$ are the mean and standard deviation of the logarithm of exponential growth bias in the education group. Panel A reports the estimated coefficients. Standard errors are reported in parentheses. Panel B reports the distribution of coefficients by taking draws from the coefficients in panel A. Panel C reports the simulated pension contribution based on three counterfactual scenarios. The first scenario is the baseline when all subjects underestimate the benefit of compound interest. In the second scenario, I consider the subsidy policy that increases the existing subsidy by 100%. In the third scenario, I provide financial education about compound interest, which reduces the exponential growth bias

Column 1 shows the results for specification 1. The estimated coefficient of relative-risk aversion is 1.54. The estimate is close to that of Gourinchas and Parker (2002), but lower than previous estimates without heterogeneity in risk aversion (DeNardi, French, and Jones 2010; Koijen, Nieuwerburgh, and Yogo 2016). The estimated discount factor is 0.94, which is close to the estimates in the literature. The exponential growth bias in the control group ($\theta_1$) is 0.67 and it is 0.09 in the education group ($\theta_2$). The difference between $\theta_1$ and $\theta_2$ is significant at the 1% level, which supports that the education treatment helps to reduce the underestimation of compound interest.

In specification 2, I allow for individual heterogeneity in the parameters of exponential growth bias and estimate a random coefficients model for parameters of underestimating compound interest. I use the method of simulated moments (MSM) to estimate the random coefficients in life cycle models. For each candidate parameter vector, I simulate the individual parameters of underestimating compound interest in the sample. I solve the life cycle model based on these candidate parameters, and then use the resultant optimal consumption rules to simulate the pension contribution of each individual in the simulation sample. I use the simulated data to calculate simulated moments and minimize the distance between simulated moments and empirical moments in both the survey data and actual pension contribution data. I estimate five parameters, $\hat{\Theta} = (\hat{\rho}, \hat{\beta}, \hat{\mu_1}, \hat{\mu_2}, \hat{\sigma})$. $\rho$ and $\beta$ are parameters for risk and time preference, similar to those used in specification 1. I assume that the parameters of underestimating compound interest are lognormally distributed in both the education group and the control group. $\mu_1$ and $\sigma$ are the mean and standard deviation of the logarithm of exponential growth bias in the control group, and $\mu_2$ and $\sigma$ are the mean and standard deviation of the logarithm of exponential growth bias in the education group. I assume that the intervention only influences the mean but not the standard deviation of the underlining distribution due to random assignment. The estimation is based on the same twenty-five moment conditions used in specification 1.

Column 2 shows the results for specification 2. Panel A reports the estimated coefficients of the lognormal distribution. The estimated coefficients of relative risk aversion and discount factor are 1.64 and 0.95, respectively, which are similar to specification 1. The estimated $\mu_1$ and $\mu_2$ are $-0.04$ and $-2.04$, respectively. The difference between $\mu_1$ and $\mu_2$ is significant at the 1% level, which supports that the education treatment helps to reduce the underestimation of compound interest. $\sigma$ is 1.07. In panel B, I generate the distribution of the coefficients by taking draws from the coefficients in panel the education treatment helps to reduce the underestimation of compound interest. It also implies a significant amount of heterogeneity of exponential growth bias in the sample.

I use the estimated parameters to conduct counterfactual welfare analysis under different policies to increase pension contributions. I consider two counterfactual policies: the subsidy policy and the financial education policy. The first scenario is the baseline, where all subjects underestimate the benefit of compound interest and only receive the existing subsidy. This is the case in the control group and close to the real-life situation without interventions. I simulate their pension choices based on the parameters estimated from the control group. In the second scenario, I consider the subsidy policy that increases the existing subsidy by 100%. I simulate their pension choices based on the parameters estimated from the control group and the larger subsidy. In the third scenario, I consider a financial education program about compound interest. I assume that financial education can reduce the exponential growth bias by the same amount in the experiment. Thus, I simulate the pension choices based on the parameters estimated from the education group.

Table 7, panel C, reports the results. In the baseline scenario, the average simulated pension contribution is 155 RMB. When I increase the existing subsidy by 100%, the average contribution increases to 158 RMB, which is about 2%. If I conduct the financial education that has the same effect in the experiment, the average contribution increases to 177 RMB, which is about 14%. One can see that the financial education policy has a much larger effect than a 100% increase in existing subsidy. The counterfactual simulation results imply that financial education about compound interest is more effective than the subsidy policy in increasing pension contributions.

Based on the counterfactual simulation, I can calculate the lifetime utility for each individual and analyze the welfare effect. I first calculate the welfare gain if the understanding of compound interest increases from the level of the control group to the level of perfect understanding. I find that the lifetime utility increases by 10.0% if they estimate compound interest correctly. I also calculate the welfare gain if the understanding of compound interest increases from the level of the control group to the level of the education group. I find that the education treatment increases the lifetime utility by 8.6% compared to the control group.[12]

## 6. Conclusion

and policy topic. Lack of pension savings can have significant consequences on the standard of living of the rural elderly. In this paper, I provide working-age subjects with financial education about compound interest, with the goal of testing for the role of misunderstanding compound interest in rural pension savings in China. I find that the education treatment increases contributions by 49–53 RMB, resulting in an increase of around 37%–40% relative to the average contribution of 133 RMB in the control group. Both reduced form and structural estimation support that the intervention improves the understanding of compound interest.

Evidence on whether financial education can effectively change individual decisions is mixed in the literature. This paper shows that learning the concept of compound interest can help to increase pension contributions in rural areas, and financial education that helps to increase knowledge has a larger effect than simply providing information. Gaurav, Cole, and Tobacman (2011) and Cai and Song (2017) find that financial education with simulated experiences has a positive and significant effect on adoption of weather insurance in developing countries. These findings suggest that one should first identify the barriers to individual participation and then deliver specific financial education to remove these barriers; this seems to work better than general financial education.

This paper has several limitations. First, I do not know whether the intervention crowds savings in or out in other savings accounts and whether it changes overall savings, because I do not have data about subjects' savings accounts.[13] The purpose of the study is to understand why people do not reallocate their savings to high-return pension plans and how to improve their allocation of savings, given their misunderstanding of compound interest. I demonstrate that the intervention changes the allocation of savings in the pension account and discuss possible mechanisms. The second limitation is external validity. The results might apply to the setting where people are well informed about the pension plan and that the time lag between pledging and cash payment is short. Although the experiment was conducted in rural China, I believe that the external validity problem might not be severe for the following reasons: (1) misunderstanding of compound interest is widespread in the United States and is correlated with poor decision-making (Lusardi and Mitchell 2007b); (2) a recent study in the United States shows that mailing printed income projections increases savings in the Voluntary Retirement Plans (Goda, Manchester, and Sojourner 2014). Compared to Goda, Manchester, and Sojourner (2014), this paper examines the effects of a more labor-intensive treatment that includes face-

Avenues for future research include evaluating whether financial education about compound interest influences households' behavior regarding other financial products, such as other long-term borrowing and savings. From a policy perspective, this paper suggests that policy makers should take into account people's biases when designing policies. In particular, policy makers can provide low-cost financial education to overcome individual biases, and thus improve individual welfare.

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## Footnotes

[1] This literature on financial literacy includes, but is not limited to, Lusardi (1999) and Lusardi and Mitchell (2007b, 2014). The literature on procrastination includes Laibson, Repetto, and Tobacman (1998), O'Donoghue and Rabin (1999), Diamond and Kőszegi (2003), Choi et al. (2002), and Madrian and Shea (2001).

[2] The evidence on financial education is mixed. Some studies find no effects or only small effects of financial education on individual decisions (Duflo and Saez 2003; Carter et al. 2008; Cole, Sampson, and Zia 2011; Cole et al. 2012), whereas others find positive and significant effects (Bayer, Bernheim, and Scholz 2008; Agarwal et al. 2010; Carlin and Robinson 2012; Gaurav, Cole, and Tobacman 2011; Cai and Song 2017; Seshan and Yang 2014; Drexler, Fischer, and Schoar 2014).

government pension contract, the government has raised the basic pension for urban employees by about 10% each year since 2005.

[4] In 2010, the contribution from farmers only accounts for about 25% of the total fund in my study county. The central government provides about 50%, and the local government provides the other 25%.

[5] In China, *hukou* is a government system of household registration designed to control the movement of people between urban and rural areas. Urban *hukou* usually offers higher health care, education, and pension benefits.

[6] About 1% of households did not adhere to the contribution level they chose at registration and paid a different amount when village coordinators collected contributions. Changing contribution levels does not incur a penalty; only households themselves and village coordinators need to file some paperwork. I believe that the low rate of changed contribution levels is likely due to participants being familiar with the standard practices used to implement various social programs in rural China and carefully deciding on their contribution level. I will focus on pension contributions from administrative data in later analyses.

[7] Both time preferences and risk attitudes are elicited without monetary incentive.

[8] I cannot conduct within-subject analysis, because I do not have a control group for a pre-/post-analysis.

[9] “Natural village” refers to the actual village, whereas “administrative village” refers to a bureaucratic entity that contains several natural villages.

[10] Intertemporal consumption theory suggests that changes in expected return have two effects: a substitution effect and an income effect. For a net saver, the increase in expected return may increase or decrease current consumption and thus decrease or increase current savings. If the observed effects are, in fact, effects on total savings, this would imply that the substitution effect dominates the income effect. The results do not speak to the theory, because I only have data on savings in the pension plan, rather than on total savings.

[11] Note that the impact of the calculation group on the understanding of compound interest question is not clear. If the information provided in the calculation group surprises the participants and thus changes their behavior, it is likely the participants can infer that they have underestimated the calculation of long-term benefit previously. Thus, it is possible that more participants answer the compound interest questions correctly in the calculation group. I find that, the difference between the calculation group and the control group is not statistically significant ($p$ = .14). The difference between the education group

intervention that teaches compound interest increases knowledge, but has no effect on the quality of decision-making as measured by financial competence.

[13] See Chetty et al. (2014) for a discussion of crowding-out effects in retirement savings.

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