Personalized Information as a Tool to Improve Pension Savings: Results from a Randomized Control Trial in Chile

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I. Introduction

Saving for the long term is a challenging task that requires overcoming of commitment and self-control issues and of knowledge barriers that obscure the connection between current costs and uncertain future outcomes. Comprehension of this connection requires financial concepts that individuals often do not

We thank Shlomo Benartzi, Mauro Mastrogiacomo, Olivia Mitchell, Henriette Prast, and seminar participants at the Innovations for Poverty Action (IPA) Financial Inclusion Workshop 2016, Pontificia Universidad Católica de Chile, Universidad Adolfo Ibañez, University of New South Wales, Universidad de San Andres, 24th Annual Colloquium of Superannuation Researchers 2016, Workshop of Financial Literacy and Pension-Related Communication for Better Retirement and Long-Term Financial Decisions 2016, North American Annual Meeting of the Econometric Society 2018, IPA Annual Research Gathering 2018, and RIDGE-LACEA Impact Evaluation of Labor Market Policies for comments received. We thank Diego Escobar, Pamela Searle, and George Vega for excellent research assistance and Pascuala Dominguez and Constanza Palacios for their assistance with the field implementation. We thank the Citi IPA Financial Capability Research Fund for funding (grant no. FCRF109). José Tessada thanks Agencia Nacional de Investigación y Desarrollo de Chile for funding through Proyecto Fondecyt 1191933. Olga Fuentes was employed by the Chilean Pensions

Electronically published November 16, 2023

Economic Development and Cultural Change, volume 72, number 2, January 2024.

https://doi.org/10.1086/720718

understand or know how to apply (e.g., compound interest, inflation, expected returns, market fluctuations, and the timing of investments).[1] Individuals can overcome some of these barriers by relying on advice from external sources, especially advice tailored to an individual's particular circumstances. We conduct a randomized control trial to study how provision of such personalized information affects long-term savings, and we use as a laboratory the savings behavior within Chile's system of individual retirement savings accounts.[2]

The intervention considers a single treatment and compares the effect of provision of personalized information with the effect of receipt of general information. Individuals received the information through self-service modules equipped with pension-simulation software.[3] All participants received information about the three main ways to increase one's self-funded pension component, namely, increasing mandatory savings, increasing voluntary savings, and delaying retirement. On the one hand, members of the control group are given the percentage effect that each of these actions is likely to have "on average" on one's self-funded pension. On the other hand, members of the treatment group receive personalized projections of their pension annuity payouts, assuming no change in behavior, plus forecasts of the difference in the payout for each action (keeping all other decisions constant).

We analyze the results through a conceptual framework that suggests various channels through which our experiment could have affected savings. According to participants' priors, the heterogeneity of the effects provides relevant evidence within that framework. Before the intervention, we elicited the annuity payout that each participant thought he or she would receive upon retirement. Then we contrasted the effect that personalized information had on savings decisions depending on the difference between the estimated pension we provided under the status quo and that expected by the participants. If personalized information affects behavior through channels other than updating each person's beliefs, we should anticipate a uniform effect of the treatment.

Supervisor as Chief of the Studies Division at the beginning of this study and is currently employed by the Chilean Pensions Supervisor as Deputy Chair of Regulation, but her participation has solely been as researcher. All remaining errors are our own. Data are provided through Dataverse at https://doi.org/10.7910/DVN/U6AQ8Y. Contact the corresponding author, Jeanne Lafortune, at jlafortune@uc.cl.

[1] Stango and Zinman (2009) give an example of the potential difficulties associated with grasping these financial concepts and show that individuals tend to linearize exponential functions, leading them to underappreciate the cumulative interest costs of long-term debt and the long-term gains from savings due to interest compounding.

[2] In Chile, the pension system is organized around a scheme of three pillars: (i) a poverty-prevention pillar, (ii) a contributory pillar of mandatory nature, and (iii) a voluntary savings pillar. Our experiment excludes the first one.

[3] See Berstein, Fuentes, and Villatoro (2013) for a description of the software and assumptions used in the simulator.

However, if what is vital is that individuals react to the specific personalized projection they receive and thus readjust their prior, we should see differences in effect depending on the way beliefs were likely updated by the personalized information treatment.

Our intervention should be irrelevant in a neoclassical framework without information friction. In this standard framework (see, e.g., Modigliani and Brumberg 1954, 1980; Merton 1969; Samuelson 1969), individuals are rational decision-makers who are concerned about maximizing their lifelong expected utility and can access and understand a great deal of relevant information (e.g., future wages, interest rates, longevity, returns, and so on). Moreover, these individuals determine their optimal consumption, savings, and investment strategies and commit to their savings plans. In this type of setup, optimal consumption and savings decisions are affected by characteristics such as subjective discount factors, risk aversion, investment horizon, and amount of wealth, among others. Personalized information is unlikely to alter these decisions if well-informed agents make them.

Alternative models suggest that individuals might not make optimal decisions because they have preferences that are non-neoclassical, do not have the information required to make these decisions, or are unable to understand them because of their complexity. Thaler and Benartzi (2004) argue that individuals may lack self-control and may have a tendency to procrastinate. Laibson (1997, 1998) notes that in the presence of hyperbolic discounting, individuals may overestimate their capacity to save tomorrow, and some research asserts that this is consistent with empirical evidence (Brown, Chua, and Camerer 2009). Along these lines, Barr and Diamond (2008) argue that individuals tend to seek short-term gratification, which translates into opting for early retirement even though this reduces their pensions. Another critical factor that influences affiliates' decisions is the existence of inertia and myopic behavior (see, e.g., Madrian and Shea 2001; Agnew, Balduzzi, and Sunden 2003; Mitchell et al. 2006). Even with neoclassical preferences, determining an adequate savings rate can be complex. Benartzi and Thaler (2007) point out that individuals usually do not spend much time calculating a personal optimal savings rate, instead adopting mostly simple rules of thumb, which may lead to systematic biases. Thus, we may alter a participant's decision because the personalized information provided in the treatment is easier to understand than suggestions describing a generic or average individual's condition.

We hypothesize that our focus on personalized information linking savings actions with quantifiable outcomes can help people link today's savings to their self-funded pensions at retirement, thus modifying their long-term savings behavior. We think this hypothesis is a valid one in our context because

Chileans show little financial knowledge and, in particular, insufficient knowledge and understanding of the pension system (see Berstein, Fuentes, and Torrealba 2010). Participants in our sample are more knowledgeable than average Chileans but still have limited information and understanding of the pension system. Low levels of financial literacy may be detrimental for individuals (see, e.g., Mitchell, Todd, and Bravo 2009; Hastings, Mitchell, and Chyn 2010). Furthermore, the lack of financial knowledge is not unique to Chile. Indeed, Lusardi and Mitchell (2008, 2011) find evidence of low levels of financial knowledge for the United States, especially among women, low-income individuals, minorities, and immigrants, and argue that this may be detrimental to pension savings (Behrman et al. 2012).[4] Thus, our results may apply to other regions where similar low financial literacy exists.

In agreement with our hypothesis, we find evidence that voluntary savings significantly increased for those who received personalized information. The estimated effect represents an increase of about 10% in the average amount of voluntary savings made by participants in the first 8 months after treatment. An increase between 0.5 and 1 percentage point in the number of individuals making a voluntary contribution in the period under study drives this result. This rise corresponds to approximately 30% of the fraction of individuals making voluntary contributions. We do not observe a similar effect on mandatory savings, where we find negative and insignificant effects in the first months after treatment. Adding up both types of savings, we find that the increase in voluntary savings was too slight to increase total savings significantly.

However, the fact that voluntary savings did increase in the short term is interesting because most results in this literature (see, e.g., Karlan and Zinman 2018) show little response of savings to factors such as increased rates of returns. We also observe in the treatment group an increase in the probability of retiring a few months after treatment. Finally, our follow-up survey found that personalized information made the intervention more salient and better evaluated by the participants. We also find that it raised their self-reported knowledge and valuation of the pension system.

The contrast in the effect of personalized information on voluntary savings and that on mandatory savings can be better understood once we consider what each participant may have learned from the information. We find that the increase in voluntary savings is concentrated among individuals who had previously overestimated their expected pensions. On the other hand, individuals

[4] However, Hastings, Madrian, and Skimmyhorn (2013) argue that even though there is ample evidence of the positive correlation between financial literacy and retirement planning, savings, and wealth accumulation, more research is needed regarding the causality of that relationship. See Lusardi, Michaud, and Mitchell (2017) for a model of endogenous financial literacy.

who had previously underestimated their expected pensions decreased their mandatory savings (implying lower labor supply, lower formal employment, or lower taxable income).[5] Our results suggest that those who overestimated their pensions responded by increasing their savings through the most accessible mechanism, namely, by increasing their voluntary contributions, whereas those who underestimated their pensions reduced savings in the only way possible, namely, by reducing their mandatory contributions, which required fewer contributions or lower labor income. One result that does not fit our belief-updating framework is the increase in retirement among overestimating individuals. Retirement is only available for a small fraction of the sample, and on top of the self-funded pension, there is a means-tested noncontributory pension complement that decreases as the self-funded component increases. This group may have been disappointed by the projected pension but still found that this might be the best they could aspire to, especially if they were unemployed when participating in the intervention. All in all, we argue that these heterogeneous responses emphasize the role of personalized information, because we should not observe this type of heterogeneity if the treatment mostly made pension savings more salient.

Information provision has been shown to play a role in increasing participation in new pension plans (Duflo and Saez 2003), delaying retirement age (Mastrobuoni 2011; Miranda Pinto 2013), and effectively responding to incentives to increase pension savings (Duflo et al. 2006; Mastrobuoni 2011). Additionally, being exposed to an educational event affects members' savings expectations and their specific retirement goals (Clark et al. 2006), influencing them to make decisions to improve their future pensions. Our innovation lies in going beyond the provision of general information by focusing on the role of information tailored to each individual.

Two existing studies used nonexperimental methods to measure the effect of providing pension projections: Fajnzylber and Reyes (2015), who use matching techniques in Chile, and Dolls et al. (2019), who use an event study in Germany. In addition to experimental variation, our main contribution is to

[5] While observing a decrease in savings may be surprising, the fact that the literature has not agreed upon the optimal savings level for retirement suggests that many individual factors may be relevant in that determination. For instance, the World Bank recommends a replacement rate of 54%, defined in terms of final earnings (see World Bank 1994), and the International Labor Office establishes a minimum of 40% (see International Labor Organization 1952). From an academic perspective, Thaler and Benartzi (2004) suggest that a replacement rate (defined as the ratio of retirement income to preretirement income) between 70% and 100% would be acceptable. However, Skinner (2007) argues that whether optimal consumption increases, decreases, or stays constant at retirement depends on the intertemporal elasticities of household production, consumption, and leisure. Moreover, the same author provides references to empirical studies that have contradicting results regarding the values of these critical parameters.

contrast personalized information and general information instead of use of a control group that receives no information. This comparison allows us to exclude the role of merely making pensions more salient to a recipient's mind. Additionally, our "one-on-one" delivery of the information improves the precision of our estimates compared with that of estimates obtained through use of mail delivery of information in the cases of the two above-named studies. Moreover, our field-experiment design allows us to capture heterogeneity by expectations regarding future pensions, which turns out to be relevant because the effect of the information we provide differs precisely in that dimension.[6]

The work closest to our research is that of Goda, Manchester, and Sojourner (2014), who study the effect of provision of retirement projections on individuals' contributions to retirement accounts in the context of a single firm and for complementary accounts in a country with a defined benefit system. Despite the similarities, our contribution differs from theirs in many ways. First, for most outcomes, they cannot statistically distinguish between the effect of provision of personalized information and the effect of receipt of general information, which is the focus of our paper. Second, our setting allows us to offer more concrete details on "retirement" income and not just about "retirement savings," something impossible to do with only employer-related plan data in the US system. Third, while Goda, Manchester, and Sojourner (2014) focus on voluntary savings, because of the nature of our database we can provide more evidence regarding the labor-market outcomes of our intervention, which include formal employment and retirement decisions. Goda, Manchester, and Sojourner (2014) find that providing income projections increases contributions by about 3.6% on average compared with those of the group that received no information, but providing workers with simple knowledge on how to change one's contribution has a significant effect on contribution density as well. Our estimated marginal effects of provision of personalized information compared with the receipt of general information are larger than the estimates of Goda, Manchester, and Sojourner (2014), which is not surprising if the information is more enlightening than in their case. Finally, our results also represent a broader group among the Chilean population, including low-income and middle-income people, lower-education individuals, informal workers, the self-employed, and inactive system affiliates. It also captures almost all of the pension contributions by these individuals. This group is usually not targeted by employer-sponsored retirement plans in the United States.

While our paper is, to our knowledge, one of the first to explore random assignment of personalized versus general information in the context of long-term

[6] Fajnzylber and Reyes (2015) did not have data on expectations. In contrast, Dolls et al. (2019) showed only that most participants overestimated their pension.

savings, many other works have looked at the role of information on savings in general. Goldberg (2014) reviews a set of existing studies and argues that the effect of financial-literacy interventions on the savings rate is not very sizeable. In particular, two studies for Indonesia—Cole, Sampson, and Zia (2011) and Carpena et al. (2011)—both show no effect of interventions that increased financial literacy on savings. It may be that general information is merely unlikely to change behavior.

The organization of our paper is as follows. Section II describes the experiment in detail. In section III, we document the empirical methodology and the data. In section IV, we present and discuss the results, and our conclusions follow in section V.

II. Experiment

We designed a randomized control trial to estimate the effect of personalized information on long-term savings. This section first presents how we constructed the personalized information set for each participant and then presents the experimental details.

A. Forecasting Long-Run Savings

Retirement savings in Chile mainly stems from two potential sources: mandatory contributions linked to formal labor-force participation and tax-advantaged voluntary contributions.

The mandatory contributions are deposited into individual accounts managed by single-purpose private companies called pension fund administrators (AFPs for their name in Spanish).[7] Since its introduction, the required contribution rate has been set at 10% of taxable income.[8] The coverage provided by the system, measured as the proportion of affiliates to working-age population, is around 85% as of December 2021.

Individuals can increase their pension savings by making voluntary contributions into tax-advantaged accounts. A broader set of firms are allowed to manage these accounts: AFPs, mutual fund companies, insurance companies (through life insurance products that have a savings component), and so forth. Individuals may withdraw their voluntary savings before retirement, but they must pay the corresponding taxes and a surcharge for early withdrawal. Investment decisions

[7] For each AFP, there is a fund choice among five funds, which are differentiated mainly by the proportion of their portfolio invested in equities and fixed-income securities. We do not include any information about these different funds in our experiment.

[8] For the purpose of pension (and health insurance contributions), the income is capped at a monthly wage of approximately US$2,800. Moreover, the cap is adjusted every year, according to the real annual growth in average wages.

concerning voluntary savings are less constrained than in the case of mandatory savings, but take-up of these voluntary-savings accounts is much lower: as of June 2021, approximately 22% of affiliates had such an account. Most of these accounts are opened in AFPs (51%), followed by insurance companies (22%), mutual fund companies (16%), and security brokers (11%).

As we have emphasized before, understanding the effect of long-term savings decisions requires substantial financial sophistication. Survey evidence about retirement planning and financial literacy in Chile shows that a large fraction of the population has a low level of financial literacy and that most of the population is not planning for retirement. The 2009 Social Protection Survey (EPS for its name in Spanish) included a financial literacy module, which had questions comparable to those analyzed in other countries (Lusardi, Michaud, and Mitchell 2011). On the basis of these data, Moure (2016) shows that, relative to respondents from developed countries, Chileans show lower levels of financial literacy. Less than half of respondents answer correctly a simple question about compound interest and risk, while less than 20% answer correctly a question about inflation. Moreover, the correct response rates are positively related to educational attainment and negatively related to age and are lower for female and lower-income respondents (see Hastings and Mitchell 2020). According to these data, Chileans also show poor financial planning practices: less than 10% of the EPS sample takes active planning actions, and within different subgroups of the population only individuals with postgraduate education have a planning prevalence higher than 30%.

Furthermore, results from the EPS indicate that 82% of Chilean affiliates do not know how their pensions will be calculated, and almost half of those who claim to know about this subject give an incorrect description.[9]

Given this low level of pension knowledge, individuals may not have a good estimation of how much their savings decisions today will affect the annuity each one can obtain at retirement.[10] Since 2005, together with the last quarterly AFP statement, individuals receive a personalized pension forecast that goes mostly unnoticed. For instance, the 2009 EPS shows that only 2.7% of the individuals declare looking at content other than account balance, returns, or fees charged.

In order to increase the visibility of this personalized forecast, the Superintendencia de Pensiones (SdP) of the Chilean government has made its pension

[9] Lack of knowledge about the system is general; most individuals do not understand or do not know basic characteristics of the system. For more details on the results from the Social Protection Survey, see the evidence shown by Berstein, Fuentes, and Torrealba (2010).

[10] At retirement, individuals can pick between an annuity or programmed withdrawal. We forecasted the pension that would be provided by an annuity because this is the most common choice among current retirees.

simulator available online at http://www.spensiones.cl/apps/simuladorPensiones/. However, this simulator is complex to use, and a limited number of individuals have accessed it.[11] Our experiment thus aims at simplifying the simulator and facilitating access to it.

The SdP simulator is based on a model that uses a representative affiliate's characteristics: age; gender; level and density of contributions; level of income prior to retirement; retirement age; investment strategy; and number and characteristics of beneficiaries. This model is described in detail by Berstein, Fuentes, and Villatoro (2013). Using information about the current balances in mandatory and voluntary pension savings, the model constructs a consolidated balance. Starting from the affiliate's current age, pension savings growth is driven by monthly contributions (mandatory and voluntary savings) and by the return earned on previously accumulated pension savings. With these and user-provided inputs, the online simulator produces a forecast that corresponds to the monthly after-tax annuity payout an individual would receive in current Chilean pesos. This forecast is only for the self-funded pension component. For low-income individuals, the pension system also includes a subsidy that the simulation does not incorporate in the calculations because it is computed when the person effectively retires, and individuals must fulfill residency and means-tested requirements to become recipients of these benefits.

The pension simulator developed for the experiment is a simplified version of the online SdP pension simulator. In contrast to the online version, we first assume that the user will follow the default investment strategy, which is determined by the age of the participant. The same investment strategy is applied to the mandatory and voluntary pension-savings accounts. In order to calculate the annuity, we assume that all individuals are married and without dependent children at the moment of retirement and that men are 2 years older than their spouses. The simulator further assumes that the future mandatory contributions will equal the average contribution of the past 12 months. Finally, for users that are at least 2 years younger than the legal retirement age (65 years for males and 60 years for females), the simulator assumes that users retire at the legal age. For users that are older, the simulator assumes that retirement takes place in two more years or at age 70, whichever is lower. In line with the SdP simulator, we do not add the potential subsidy for low-income individuals in our simulations. Finally, while the online simulator provides a range of values for the annuity (using a probability distribution), our personalized information report only informs the mean value.

[11] See Antolin and Fuentes (2012) for a description of the simulator.

Besides this "status quo" estimated pension, we also provide participants with an estimate of the effect of three typical suggestions made to individuals who wish to increase their retirement savings. These also correspond to what the online version of the simulator offers. All participants receive the estimated effect for each of the three alternatives and thus cannot explore the effect of modifying the suggestions they receive.

The first of these actions refers to increasing the density of mandatory contributions. This is entirely linked to formal employment. In principle, all workers in the formal sector of the economy (i.e., individuals that have a working contract with a firm) are obliged to contribute 10% of their wages into their pension savings accounts, and 79% of the population has contributed at least once through this channel. In practice, however, it could be possible (and anecdotal evidence suggests that this is the case sometimes) to elude this obligation. For instance, workers can be employed without a contract, thus lowering the frequency of mandatory contributions, and can underreport the wages received, thus effectively saving less than 10% of wages. The simulator calculates the level of annuity payout that a participant could obtain if he or she contributed every month from now until retirement age according to the average monthly wage (conditional on contribution) over the past year (i.e., increasing the number of mandatory contributions to 12 per year). Notice that we do not estimate the effect of reduction of underreporting on the intensive margin (contributing for an amount below one's monthly income), we only address the extensive margin.

The second type of action relates to increasing voluntary contributions. The simulator forecasts the annuity payout under the assumption that the individual voluntarily saves 1% of pretax labor income from now until retirement age.

Finally, the last suggestion refers to postponing retirement. The legal retirement age is 60 (65) years for female (male) workers, and the simulator recalculates the annuity payout if the individual were to delay retirement by one more year. This increases the annuity for two separate reasons. First, the retirement savings finance one less year, which allows higher monthly payouts. Second, the simulator assumes that the individual will save in the same way as in previous years during that additional time, leading to higher savings accumulation.

B. Randomized Control Trial

To test whether receiving this personalized information plays a role, we implemented a randomized control trial. The intervention consisted in installing self-service modules, equipped with the pension-simulation software described above, in locations with a high flow of low-income to middle-income but working individuals. We decided to install these modules in the locations where social payments and services targeted to the needs of these individuals are delivered. In Chile, those services have been assigned to a government agency called "Chile Atiende," which has 153 offices across the country and receives on average 37,000 visits per year. Most of the proceedings or inquiries performed in these offices are related to pensions (26%), information on procedures and benefits (23%), certificates (11%), and buying state-run National Health Fund (FONASA for its name in Spanish) "bonos" with which to pay medical care by a doctor (8%). A quarter of visitors wish to ask general questions or obtain information about some specific topic.

We chose to partner with this government agency because the demographics of its population appeared to match that of our target population. According to the information the agency provided to us for visits in 2013, most users are women (67%), 27% are under 40 years old, 27% are between 40 and 55 years old, 24% are between 56 and 65 years old, and 22% are above 65 years old. With regard to educational level, 48% of users have primary education or incomplete secondary education, 33% completed secondary education, and only 19% have complete or incomplete tertiary education.

Table A.1 (tables A.1–A.6 are available in the online appendix) shows that the individuals who participated in our experiment were demographically closer to all affiliates of the pension fund system than to those who use the simulator's online version. While only 30% of those who used the simulator in its complex version online were women, roughly 52% of our participants were women, much closer to the 47% of affiliates they represent in Chile's pension system. Our participants, as shown in the second column of table A.1, also match almost perfectly the age distribution of all affiliates, while those visiting the online simulator tend to be older. Our participants also have a wage distribution and a savings behavior similar to those of the whole set of affiliates of the pension system, while the online simulator was visited by high-wage, high-savings individuals.

The module was identified as a module from the SdP in order to increase its credibility. As individuals approached the module, each was asked to place his or her national ID card under a scanner and index finger on a fingerprint reader. This was required for us to be able to obtain the individual's data from the SdP database (if that individual had ever affiliated with the system) and to implement the randomization.[12] Each individual was then asked to provide consent.

[12] While national ID numbers are given by birth or immigration date and thus are not random, the last digit preceding the "verification" character is not correlated with age, gender, or any relevant characteristic of the individual. The ID numbers consist of a six- to eight-digit number followed by the verification character, determined by the previous numbers, in a "xx.xxx.xxx-y" format. We use the last digit before the hyphen for the randomization, that is, the last "x" before the hyphen.

Figure 1. Example of information provided to the control group.

At that point, not only the SdP appeared as participating in the project but also the universities of the researchers and the Abdul Latif Jameel Poverty Action Lab. If individuals consented, they were asked to answer a short, 10-minute survey. Once the survey was completed, treatment participants were led to the simulator, while control participants were offered three nonpersonalized tips to increase their pensions. The control participants were reminded that by increasing the number of times one contributes during the year, by making voluntary contributions, and by delaying retirement age, one can increase his or her pension savings. They were given the average effect that each of these measures can have on a typical pension, all in percentage terms. Figure 1 shows the exact screen the control group faced.[13] The control participants had the option of obtaining a printed version of this reminder. They could also have it sent to them by email.

On the other hand, through use of the simulator, each treated individual was given an estimate of his or her current pension and the exact effect that each of the three measures mentioned to the control group would have on one's pension. Figure 2 shows the screen that appeared to a given individual.[14] That individual was anticipated to receive a pension of 130,795 Chilean pesos (CLP\$), or about US\$250 per month at the exchange rate of that year. While low, this is about 50% more than the guaranteed pension offered by the Chilean government at that moment. This woman, in the previous year, had contributed to the pension fund only 5 months out of 12.[15] The simulator shows her that by increasing the frequency of her contributions to all months of the year, she could more than double her pension. It also shows her that by voluntarily saving an extra 1% of her monthly income in an individual voluntary-savings account,

[13] We provide a translation of this figure in fig. A.1.
[14] A translation of this figure is provided in fig. A.1.
[15] We know she is a woman because the assumed retirement age is 60 years.

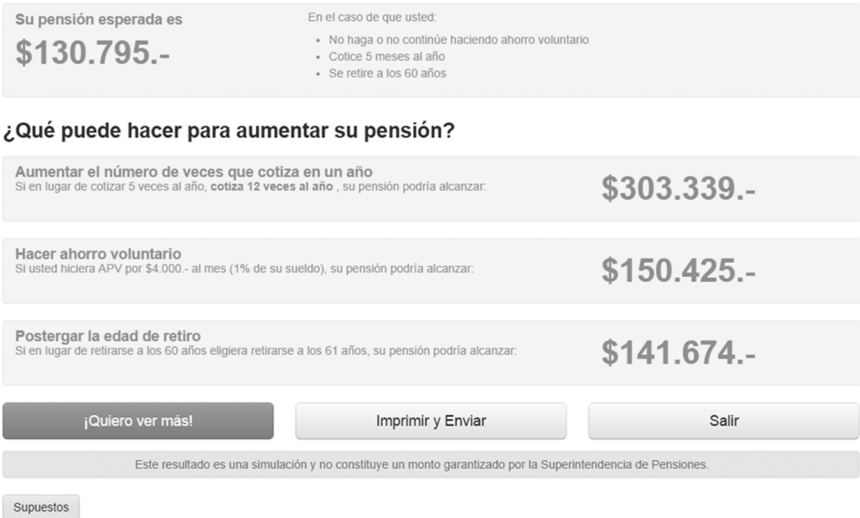

Figure 2. Example of information provided to the treatment group.

she could increase her pension by about 15%. Finally, delaying her retirement age by 1 year would increase her pension by a bit less than 10%. All these estimates are provided for each person by using a person's own data as available in the system. They are also expressed in terms of monetary value instead of percentages.[16] Once at that point, the person can obtain a printed or email version of the estimates. She can also go back and alter the parameters of the simulation to see the effect of other alternatives. For example, she could try to increase voluntary savings by a larger fraction, alter the retirement age by more than what the system suggested, or increase only partially the density of mandatory contributions. The system records those simulations for any individual who chose to do that.[17]

At first, we implemented our modules as self-serving kiosks in eight locations of "Chile Atiende" in the metropolitan region of Santiago and its rural surroundings. The locations were selected on the basis of the demographics of the visitors they would receive, the flow of visits they had, a representativeness of rural/urban areas, and geographic proximity. We ran the experiment in this way for 2 months. However, the flow of individuals completing the process was very small. In particular, most individuals were stopping at the point where the national ID card and the fingerprint reader were required. Observational data suggested that this step was complicated for many users, who

[16] In our discussion of results, we will argue that this is not the reason why personalized information appears to induce savings.
[17] Few individuals pursued that option, which is why we do not explore these data in more detail.

would get frustrated by the process. We thus altered our implementation and randomly assigned to locations and days a module "assistant" who both encouraged participation and helped the person navigate the module. The assistants were undergraduate students who were given basic training on the functioning of the module. The presence of these assistants substantially raised the take-up of the module: more than 93% of our sample completed the experiment with an assistant, implying that our experiment includes the interaction with those individuals. However, the interaction with the assistant was the same whether the individual was in the control group or the treatment group. We thus continue to highlight the fact that our experiment really contrasts the role of personalized versus general information.

III. Empirical Methodology and Data

A. Theoretical Framework

We implemented our experiment aimed at estimating the differential effect of personalized versus general information on long-term savings. However, we recognize that our intervention could have affected savings decisions through a number of alternative channels.

First, the intervention could have had a "nudging" effect. The two types of information that were given to individuals were different, and there were differences in the way information was presented. For instance, it is possible that seeing a screen that has a forecast of one's pension on its own makes treated participants think more of their pensions. The absence of piggy-bank icons could also lead them to pay less attention to the information and thus think less of their pensions. The control group received a message that referred to the anticipated effects in terms of percentages, while the treatment group received a message in terms of pesos.[18] In the case where these differences made treatment group participants more willing to consider their pension savings, we would expect that the treatment group participants would increase their savings by using the channel that may be the easiest to adjust (voluntary savings). We would also think that this effect would be temporary, because being reminded once without a commitment device would not lead to long-term

[18] There is evidence that a change in how amounts are presented may have an effect. Goldstein, Hershfield, and Benartzi (2016) conduct an experiment to explore how individuals' perception of the adequacy of savings varies according to whether their state balances are presented as lump sums or as annuities. The authors report that for low-income levels, annuities are perceived as less satisfactory than their lump-sum equivalents, while the opposite holds for higher income levels. Also, middle-age participants considered a relatively small lump sum as more adequate than its annuity counterpart, and they were less likely to increase savings rates when they were shown a relatively small lump sum instead of the equivalent annuity. The authors argue that the presence of this "illusion-of-wealth" effect may help to explain why individuals seem to under-annuitize upon retirement.

changes in behavior. If some individuals had previously delayed a retirement decision, our intervention could have reminded them of the availability of funds in their retirement accounts, which could lead some individuals to perform the paperwork to access their retirement savings.

Second, the intervention, through its personalized nature, could lead treated participants to update their beliefs about the adequacy of their pension savings. Those who would be told that they were overoptimistic in how much they could receive from the pension system could thus respond by increasing their savings. Given that mandatory savings are linked to one's labor supply and wage, we anticipate that those who wish to make such an increase would do so primarily through voluntary savings. But it is also possible that for some individuals, the update in belief occurs in the opposite direction. Participants who were too conservative in their estimation of how much they would receive from the pension system could actually respond to the intervention by reducing their pension savings. In this case, given that voluntary savings are very rare, the only way in which most participants could decrease their flow of pension savings would be through a lowering of mandatory savings. Doing this is not costless because it involves moving to informality or negotiating that a fraction of one's wage now be paid informally.[19] Finally, we could think that this update in beliefs should lead only those who are given "good news" to retire if they are able, while those who receive "bad news" would delay retirement as a way to increase their pension savings. However, given that there is a means-tested noncontributory pension available for those whose self-funded pension is low, those who are told that the pension they can obtain from their own funds is very low may thus conclude that continuing saving within the system is not beneficial enough and instead choose to avoid postponing retirement to obtain the subsidized amount earlier. This would thus suggest that our intervention could lead to very different outcomes depending on the direction of the updating that is generated by the experiment. To be able to see if this is a possible channel, we elicited the expected pension from all participants. We will thus be able to differentiate the effect depending on the pension we estimated compared with what was expected. This heterogeneity in responses to personalized information based on prior beliefs will be part of our contribution to the literature because previous studies were unable to explore this type of heterogeneity.

Third, the treatment group received a different type of information regarding the actions that could be taken to increase one's pension savings. The personalized nature of the information could thus lead the treatment group participants to

[19] The potential for altering mandatory savings through employment formality has been discussed before. Kumler, Verhoogen, and Frías (2020) show that in Mexico, a pension reform that put more weight on past wages did increase the amount of wage payment officially declared by employers.

undertake actions that are shown to be of greater personal benefit to them than is the personal benefit to the control group participants of the information they received, which represented "average" benefits. It could also decrease incentives to pursue actions that are shown to have little effect. Thus, the type of response we would expect would depend on how the personalized effect differed from the effect of what was provided to the control group. Furthermore, the response of individuals in the treatment group could also be influenced by the relative magnitude of the effect of these actions on their pensions compared with their predicted pensions. While the control group participants are told that the actions could increase their pensions by between 7% and 16%, depending on the action, some individuals may be shown that extra savings produces increases in pensions that are quite limited, in particular for those closer to retirement age who have low wages. This could lead some treated participants to reduce their savings and even consider early retirement given this type of information.

Our empirical strategy and data collection take into account the potential influence of these alternative channels, and we explore which of these effects is likely to be observed.

B. Data

We will measure long-term savings through the same type of actions that the simulator evaluates. We will thus need information regarding mandatory contributions, voluntary contributions, and retirement decisions. We will further look at other decisions of participants within their account (investment decisions) and savings actions, perceptions, and decisions outside of the pension savings system.

Our main source of data for these outcomes is the administrative database of the SdP. This database is constructed from the information that each AFP provides to the SdP about its affiliates. Information regarding the age and gender of affiliates is available among the few demographics the database records. The database also offers a rich set of information regarding the formal-labor-market participation of individuals (because all formal-employed workers are required to contribute to the pension fund system), their pension savings, whether they work as employed or self-employed, and whether they have retired. The data on mandatory and voluntary savings are available at a monthly frequency.[20] Finally, the database also records some information regarding the involvement of individuals in their investment decisions: whether they have asked or changed their password required to access their AFP's website, whether they have changed their savings between types of fund, and whether they have changed their AFP.

[20] If an employer makes a contribution for a worker that corresponds to a payment in month 5, it will be linked to that month, even if the employer makes a late payment in month 8, for example.

We then complemented these data using a phone survey conducted about 10 months after the use of the module. Phone calls were made to the telephone numbers the individuals reported as their contact information in the module and to the telephone numbers they had on file in SdP's administrative data. In this relatively short phone survey, we focused on variables that are invisible to us in administrative data. We measure informal-labor-force participation, savings outside the pension system, and knowledge, intentions, and perceptions regarding that system.

Given the hypothesis that personalized information may alter beliefs, we also wanted to elicit individuals' priors about their retirement savings. We did so in our baseline survey, which was conducted directly in the module before the individual received the treatment information. This survey included questions about current labor supply, education, and position within the household. For individuals who were not registered in the pension system, we also included questions regarding their gender, their age, and their labor earnings because we could not rely on the information provided by the SdP regarding these variables. We also requested information regarding the importance of the pension system for their retirement financing and the amount of savings they had outside the pension system. We then measured their financial knowledge using the three typical questions in this literature (see Lusardi, Michaud, and Mitchell 2011; van Rooij, Lusardi, and Alessie 2011; S&P Global 2014): present value, compound interest, and inflation. We also tested their knowledge of the pension system in Chile. Finally, we elicited their expected and desired pension levels.

As can be seen in table 1, in terms of socioeconomic characteristics, most have at least a high school diploma, and almost a third have some postsecondary education. About 18% of participants have completed a university degree, and 15% did not finish high school. Two-thirds of participants are heads of household, 80% are currently working, and 89% are in the labor force. They earn on average a wage of about CLP$464,000 per month, which is almost twice the full-time minimum wage in Chile, or about US$850 at the exchange rate of the period. Thus, our participants are not very poor but more representative of low-income to medium-income workers in the region of Santiago. Once more, however, this average wage is much lower than that of online users of the pension simulator, as shown in table A.1.

Almost all (95%) of our participants are affiliated with a pension fund. Most of them (83%) consider the pension system as an important source of revenue for their retirement. On average, individuals expect to receive about 58% of their current wage as a pension and wished they could receive about 15% more than their current wage as a pension. On average, they contribute to the mandatory system about 8 months per year, and each has about CLP$10 million in

TABLE 1

BALANCE

	Observations	Mean Control	Mean Treatment	Difference T − C
Descriptive:				
Female	2,546	.510	.526	.019
				(.020)
Age	2,546	39.288	37.820	−1.404***
				(.488)
Primary school	2,538	.150	.159	.007
				(.014)
High school	2,538	.338	.321	−.018
				(.019)
Some postsecondary	2,538	.333	.354	.021
				(.019)
University	2,538	.179	.166	−.010
				(.015)
Head of household	2,538	.706	.680	−.024
				(.018)
Working	2,547	.800	.799	−.000
				(.016)
In labor force	2,547	.906	.882	−.023*
				(.012)
Wage (average M$ past 6 months)	2,547	445.873	481.401	39.229**
				(16.399)
Affiliated	2,547	.954	.954	.001
				(.008)
Savings (past year):				
No. of months voluntary saved	2,547	.402	.434	.035
				(.081)
No. of months mandatory saved	2,547	7.855	8.002	.187
				(.190)
Saved voluntary	2,547	.048	.057	.011
				(.009)
Voluntary savings (M$)	2,547	19.925	30.736	10.740
				(12.750)
Mandatory savings (M$)	2,547	431.390	439.042	12.557
				(19.404)
Balance mandatory account (UF)	2,547	384.199	427.316	46.286*
				(27.670)
Savings (M$) outside system	1,598	2,781.575	2,160.213	−674.995
				(932.853)
Priors:				
Desired pension (M$)	2,510	505.384	570.938	47.995
				(54.617)
Expected pension (M$)	2,510	249.771	290.067	29.825
				(31.092)
Estimated pension (M$)	2,545	261.471	273.941	13.245
				(12.159)
Expected pension mistake (M$)	2,508	11.257	−16.293	−16.027
				(32.210)
Expected pension mistake	2,503	−.104	−.081	.025
				(.020)
AFP important for retirement	2,538	.821	.844	.021
				(.015)

TABLE 1 (*Continued*)

	Observations	Mean Control	Mean Treatment	Difference T − C
Knowledge:				
Ease with system (1–7)	2,410	4.780	4.722	−.061
				(.070)
Knows how pensions are calculated	2,529	.449	.450	.004
				(.020)
Knows percentage of wage discounted	2,529	.433	.435	.004
				(.020)
Financial knowledge score (1–3)	2,531	1.565	1.577	.017
				(.036)

Note. The table displays the mean for each characteristic for the treatment and control groups. The "Difference T − C" column reports the coefficient of a regression of each baseline characteristic against a dummy for treatment and exposition date fixed effects. Robust standard errors are shown in parentheses. T = treatment; C = control; M$ = millions of Chilean pesos; UF = unidad de fomento.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

his or her mandatory pension savings account and less than CLP$2.5 million in savings outside the pension system.

We then turn to their financial knowledge. Fewer than half can properly answer a multiple-choice question regarding how pensions are calculated, and fewer than half correctly answered that 10%–12% of one's income is contributed to the AFP (each pension fund manager sets its own service fee on top of the mandatory savings of 10%). The participants on average answer about half of our financial literacy quiz properly, and they give themselves an average score of 4.7 out of 7 in their self-evaluation of ease with the system.

Regarding the frequency and magnitude of voluntary contributions, on average participants contribute 0.4 times per year (this is less than 1 month per year). For those who make voluntary contributions, the average amount represents roughly between 4% and 6% of their monthly wage. More striking, only around 5% had made at least one voluntary contribution over the past year.

Next, we note that the average pension we simulated for these individuals is on average marginally larger than the one the individuals themselves predicted. Thus, for the average person, we may actually correct their beliefs in a way that decreases their incentives for savings. However, different individuals received a simulation above (below) the ones they expected, implying that we will observe different types of belief update. In order to explore the possibility that different types of news affected individuals in a heterogeneous way, we define the error as

$$\text{Pension Error} = \frac{\text{Simulated Pension} - \text{Expected Pension}}{(\text{Expected Pension} + \text{Simulated Pension})}. \qquad (1)$$

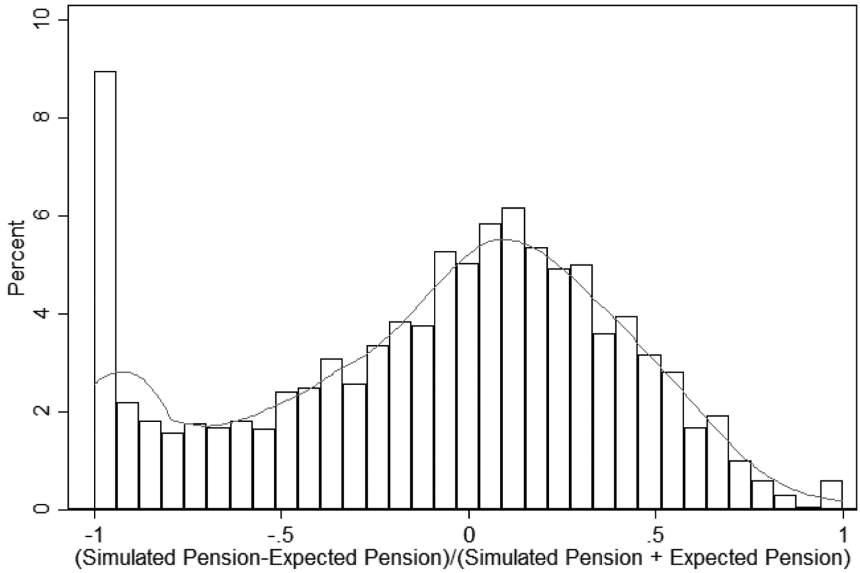

Figure 3. Distribution of difference between predicted pension and expected pension. Shown is the distribution of "Pension Error," as defined in equation (1), in the sample of participants in the experiment. The histogram is completed with a smoothed kernel density estimation represented by the gray line.

Figure 3 shows the distribution of this variable and suggests that while individuals do make mistakes in how they estimate their pension, there is no sense in which they systematically overestimate or underestimate their pension, because the distribution is almost centered at zero.[21] When we examine the error measured in Chilean pesos, we find that the average error is relatively small compared to the amount of the predicted pension. The average absolute value of the error, however, is relatively large, amounting to about 66% of the predicted pension. This suggests that while there is no strong systematic bias in the direction of the mistake, some individuals have a very incorrect estimate of what their future pension is likely to be. We will exploit this heterogeneity later in our empirical analysis.

We can also explore the influence of the type of message that would have been (and, for the treatment group, was) received for each type of action. In figure 4, we show in each panel a histogram of the return to one of the three actions for each participant in our sample. In each histogram, we show by a vertical dotted line the return that the control group was informed of (when a range was given, we show the maximum value). In figure 4*A*, we show what was the return to increasing the density of mandatory contributions to 12 months per year. Given

[21] The mass of individuals at -1 corresponds to people who were predicted to receive an annuity payout of zero but expected a positive amount.

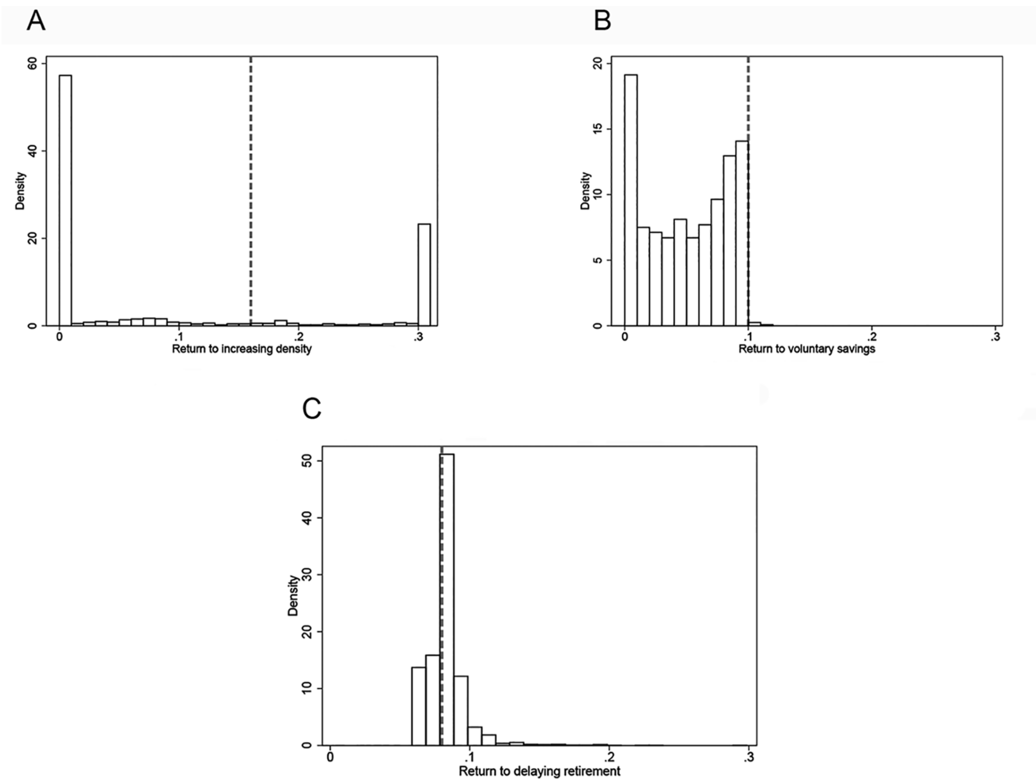

Figure 4. Estimated personalized return to different actions compared with that of the control message. Each histogram presents the return of an action in terms of percentage of baseline annuity payout for each participant in the sample (treatment and control). For the return to increasing density, observations with returns higher than 0.3 were all included in that bin. The vertical dotted line identifies the maximum of the range of returns that was given in the information for the control group.

that the distribution of returns for that action has a large number of very large values, we grouped all of them at 30%. What we observe for this action is a bimodal distribution where a majority of participants gained nothing from increasing density because they were already contributing in all months, while a second minority group could gain very substantially from increasing their very low density. A majority of the treatment group thus received personalized information that showed lower returns to increasing density than those of the control group. In figure 4*B*, we show the distribution of returns to increasing voluntary savings. In this case, we observe that the great majority of the sample would experience a gain of less than 10% if they saved 1% of their annual income in voluntary savings. This would suggest that for this outcome, the treatment group received indications that, on average, their returns were lower than the rates of 7%–10% provided to the control group. Finally, figure 4*C* shows the distribution of returns to delaying retirement age. We observe a much more condensed distribution of these returns, centered just above the value that was provided to the control group. Thus, in this case, the treatment group was probably given more optimistic views on the return to delaying retirement than what was provided to the control group.

Finally, while annuity payments and retirement decisions could depend on marital status and the number of dependents, we do not have this type of information in our survey or in our administrative data to test its balance. We also do not use it in predicting annuity payments because current conditions may not reflect the situation one expects when retiring.

C. Empirical Methodology

Randomized allocation to the treatment allows us to directly compare treated and control individuals. Therefore, we use a simple approach as specified in the following equation:

$$Y_{i,t,z} = \alpha + \beta T_i + \gamma Y_{i,(t-12)} + \delta X_{i,(0)} + \mu_z + \varepsilon_{i,t,z}, \tag{2}$$

where $Y_{i,t,z}$ is the outcome for individual i in month t who was exposed to the module in month z, T_i represents individual i's treatment status, $Y_{i,(t-12)}$ is the same outcome but 1 year before the treatment, μ_z represents exposition date fixed effects, and $X_{i,(0)}$ represents baseline characteristics that we will include to capture potential imbalance in our sample. These controls include gender dummies, age (in years), log of baseline wage, the balance in the mandatory savings account, log of estimated pension, head of household dummy, whether the individual was working in the baseline, as well as dummies for educational attainment. Given that our outcome variables are, for some variables, a monetary value that is equal to zero for many individuals, we use the inverse hyperbolic sine (IHS) transformation of that variable. Results are almost identical when using the log of $1 + y$ as an alternative. We include fixed effects for the month in which the individual was exposed to the module in case contribution behavior exhibited seasonal patterns.[22]

We have 12 months of administrative data after exposure for all the participants in the experiment. We will run our main regression for each month past exposure separately. For heterogeneity analysis, we will use multiple months per individual and cluster our standard errors by individual in that case or use the sum of actions during the first 6 months and use standard errors robust to heteroscedasticity.

Nonresponse in the baseline is very infrequent, and only individuals who consented were randomly allocated to receive personalized or general information, so nonconsent is irrelevant in the administrative data.

In the "Difference T − C" column of table 1, we test whether our randomization generated a balanced sample by running a regression of each baseline characteristic against a dummy for treatment.[23] Overall, table 1 suggests that our randomization worked relatively well. Few baseline characteristics are statistically different between the two groups. Because a few do appear to be statistically different, we will run all of our analysis by including controls for demographic variables and for any baseline characteristic that is unbalanced in table 1.

Attrition is not a problem in the analysis that relies on administrative data because we can capture the universe of participants and know that if they do not appear in the database, this is because they have not contributed during a given month. Furthermore, we can perfectly measure the entry and exit of individuals in the database for reasons such as death, retirement, or affiliation.

Attrition in our postexposure survey is much more severe. Quite a few respondents provided telephone numbers that were incorrect or that had been disconnected by the time we tried to reach them 10 months later. This implied that we only managed to find about 40% of the individuals who were part of the initial survey.

To study the role that attrition could have on our survey results, we contrast the observable baseline characteristics of those that completed the follow-up

[22] In our sample, there is not much evidence that individuals voluntarily contribute more at the end of the calendar year. We observe a higher probability of contributing in November and December, but this corresponds to an increase of 0.2 percentage points, which is relatively small. In average amounts, it is actually in January and March that we observe the largest amounts.

[23] Because we include fixed effects for exposition date, the coefficients do not correspond to the difference between the means in both groups shown in the previous columns of the table.

survey and of those who did not, both in the control and in the treatment group, in table A.2. The last column tests whether attrition is likely to bias our results by contrasting the difference in attriters and nonattriters in the treatment and the control groups. The results in this column suggest that there is no evidence that attrition in the survey is different depending on whether individuals received the personalized or general information. This supports our claim that our problem with reaching participants was not linked with an unwillingness to answer but rather a problem with the telephone numbers provided, which were not correctly entered or had too much rotation to be used 10 months later. We also find limited indication that attrition made our treatment and control groups unbalanced on observables, as shown in table A.2. Still the probability of answering the telephone survey is higher for some individuals. Those who answered our surveys are more likely to be older, heads of households, working, to have higher balances in their pension savings accounts, and to consider the AFPs important for retirement than those who did not answer the survey.

IV. Results

We now present our results through the lens of the theoretical framework presented above.

A. Aggregate Results

We first estimate how savings and other outcomes differed between the control and treatment groups for the average participant. If our treatment is mostly a reminder for participants to think about their pensions, we would anticipate a short-term increase in savings for all. If it operated through an updating of beliefs, given that the average participant has a good estimation of his or her pension, we may not observe much effect. Finally, if what mattered was the information provided through the effect of actions, we may see a decrease in savings due to the fact that the treatment group often received less positive feedback than the control group about the role of increasing savings.

We start by measuring the amounts of savings as presented in figure 5. In figure 5*A*, we show the amount of voluntary savings made every month. In figure 5*B*, we focus on the amount of mandatory savings. Finally, figure 5*C* presents the total contributions made each month to the pension system. Results in figure 5*A* suggest that for the type of savings that was easiest to increase, we observe statistically significant effects for the first 9 months after exposure to the module. These are largest in magnitude the first month after the module visit, being larger than 10% at that moment. It then shrinks until month 6 to then increase again (and become again statistically significant) in months 7

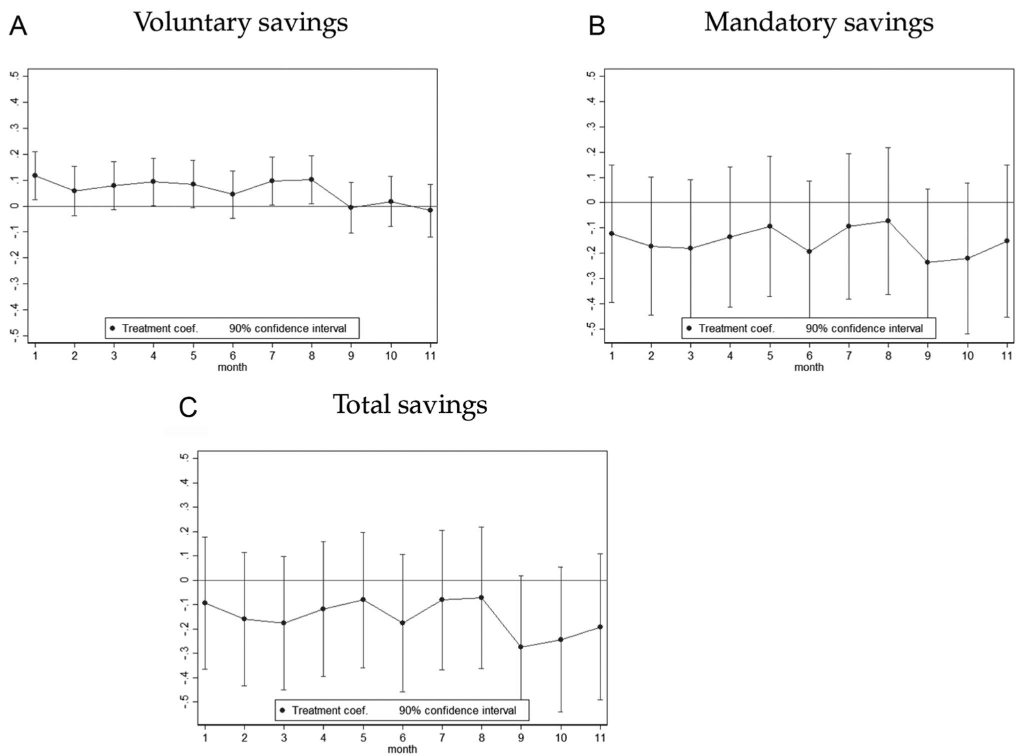

Figure 5. Effect of treatment on amounts saved within the pension system. Each graph presents the coefficient β with its 90% confidence interval when estimating equation (2) separately for each month since exposure to the module. All regressions include controls for gender dummies, age (in years), log of baseline wage, the balance in the mandatory savings account, log of estimated pension, head-of-household dummy, whether the individual was working in the baseline, as well as dummies for educational attainment. They also include fixed effects for the month of the exposure to the module and the value of the outcome variable for the 12 months prior to the period estimated. Standard errors are robust to heteroscedasticity.

and 8. For months 9–11, we see magnitudes much closer to zero and not statistically significantly different from zero. The fact that we observe a positive and nonpermanent increase in savings is more consistent with our intervention functioning as a nudge.

However, voluntary savings are not the main component of pension savings in Chile. We thus turn to mandatory savings in figure 5*B*. For that outcome, we find coefficients that are negative and not statistically significant for each month of analysis. This would be consistent with the fact that for the average participant in our experiment, the update of belief was minor, and the effect of the three strategies to increase one's pension was in general said to be smaller to the treatment than to the control group.

When summing both sources of savings, we observe in figure 5*C* that the increase in voluntary savings was too small to significantly increase the total amount of savings of participants in our study.[24] After all, voluntary savings contributions are, on average, less than 10% of the amount of mandatory savings contributions into the pension fund. Once more, this would be consistent with our intervention acting as more than a simple nudge.

These results are almost identical when using only the unbalanced baseline characteristics as controls, as shown in figure A.2 (figs. A.1–A.4 are available in the online appendix). This suggests that adding characteristics over which randomization was balanced does little to the estimate, as it should. Omitting unbalanced controls would lead us to overestimate slightly the effect of the program, as shown in figure A.3. However, the difference is relatively small. We consider our main estimates as more conservative.

Figure 6 repeats the analysis but this time with three different binary outcomes: whether one contributes voluntarily in a given month (fig. 6*A*), whether one contributes mandatorily (fig. 6*B*), and finally whether one stops contributing and retires (fig. 6*C*). Results in figure 6*A* suggest that the increase in voluntary savings we documented earlier occurred by increasing the fraction of participants making contributions in a given month and not only by those that were already making contributions increasing their savings amounts. We see an increase of 1% in the probability of making a voluntary contribution in the first month after the module visit. This falls to a number that is closer to 0.5% for months 2–8 and becomes only statistically significant at levels larger than 0.1. Finally, as for the case of the savings amounts, coefficients for months 9–11 are basically zero and not at all significant. This is consistent with the fact that we

[24] While not presented here, we have re-simulated the annuity payout of our sample assuming that the changes they made were permanent finding on average limited effects. However, if women were to permanently maintain the changes they made in the first 6 months after their visit, they could increase their annuity payout by 1%–3%, which is sizeable.

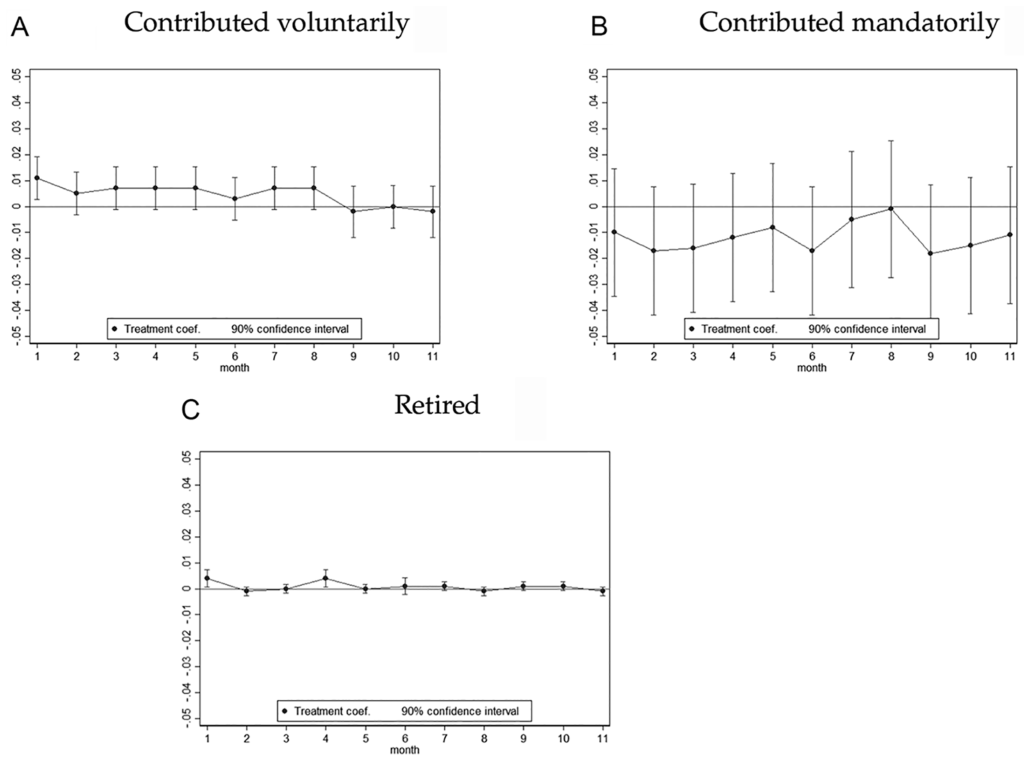

Figure 6. Effect of treatment on savings decisions within the pension system. Each graph presents the coefficient β with its 90% confidence interval when estimating equation (2) separately for each month since exposure to the module. All regressions include controls for gender dummies, age (in years), log of baseline wage, the balance in the mandatory savings account, log of estimated pension, head-of-household dummy, whether the individual was working in the baseline, as well as dummies for educational attainment. They also include fixed effects for the month of the exposure to the module and the value of the outcome variable for the 12 months prior to the period estimated. Standard errors are robust to heteroscedasticity.

do not observe that all individuals who increased savings did so by using the same contribution frequency. Figure A.4 shows the distribution of contributions for the treatment and the control groups. We observe a 30% increase (from 6.2 to 4.7) in the fraction of individuals who contributed voluntarily during the year. We find no evidence that individuals enrolled in an automatic savings program, because the increase in the number of monthly payments is not only concentrated in 12 months but also distributed across a number of payment frequencies. When using regressions, we find that personalized information raised the probability of ever contributing by about 1 percentage point and that this mostly stems from individuals who have made more than one but less than 12 monthly contributions.

Figure 6*B* shows that the nonsignificant decrease in mandatory savings is also visible in the probability of making a mandatory contribution. We observe that the estimated coefficients oscillate around 1% but are not in any way distinguishable from no effects. Finally, figure 6*C* looks at the probability of retiring. We observe that this probability was slightly larger for the treatment group than for the control group in two separate months: 1 and 4. In other months, we see no differences between the two groups. This last result is unlikely to be explained by the fact that our treatment made more salient pension savings or that the treatment group saw less potential benefit of delaying retirement, because the opposite was true for the average participant. It will thus be important to see whether an update in belief can provide a better explanation for the fact that a few more individuals retired from the treatment group than from the control group in two specific months.

In table A.3, we explore whether variables unrelated to saving but part of the choices that individuals may take within the pension system were affected by our intervention. We find no evidence of effects of our treatment on any of these. First, we find no evidence that affiliation was increased. This is comforting as it suggests that our administrative data will not suffer from attrition. It is also consistent with the high levels of affiliation to the system we found in the baseline. We also test whether individuals took some active management decisions related to their pension funds. Specifically, we measure whether the individual changed his or her type of fund within a given AFP, whether the individual changed AFP, and whether the individual changed his or her password. We see no statistically significant effect of personalized information on those variables. The magnitude of these effects is also economically very small, suggesting that the effect we find on savings did not necessarily come hand-in-hand with a more active involvement by the participant in the pension system as a whole. These do not align with the hypothesis that our program only generated a “nudge” leading to pension savings becoming more salient.

Despite the short-lived effect of the intervention and the fact that it was concentrated only in voluntary savings, we argue that being able to increase voluntary savings by only providing personalized information is noteworthy, as previous studies such as those by Bhattacharya et al. (2012) and Madrian (2014) have noted that simply providing information or advice is not always enough for modifying savings behavior. We believe that a more permanent effect on voluntary pension savings may require providing adequate information and introducing some type of commitment device, such as the ones used by Thaler and Benartzi (2004) in their SMarT (Save More Tomorrow) program or by Ashraf, Karlan, and Yin (2006).[25] Another measure that could be considered is simplifying the process for increasing savings as suggested by Beshears et al. (2013). This increase in voluntary savings came at a cost of around US$5 per participant, including the fixed cost of building the module and its infrastructure and the cost of using monitors to lead participants to the modules. Because a fraction of the cost is fixed, it could be lowered if we had continued the program for a longer time period, but it would have remained above US$3 per participant. The additional voluntary savings accumulated over 9 months would correspond to around US$4.

We next explore outcomes related to knowledge and perceptions that we could measure only through survey responses and present these in table 2. We use the same regression as in equation (2), but this time we have only one observation per person, and very few outcomes have baseline information. The first outcome in this table suggests that individuals who received the personalized information treatment were 9 percentage points more likely to remember having interacted with the module. This is a large fraction, because the control average is 82%. We also find that the individuals were much more likely to identify their interaction with the module as involving alternatives to increase pension rather than involving general information or not remembering. Finally, they valued the information they received substantially more than those who received general information. This would suggest that participants seem to have correctly identified the intervention as one where they were provided with personalized information and they valued it more highly.

We then turn to the effect on knowledge. While making pension savings more salient could make individuals learn more about the pension system, updating one's belief could also lead individuals to be better informed about the system. Receiving personalized information appears to increase one's own perceived knowledge about the pension system. However, the performance of the

[25] Save More Tomorrow is a registered trademark of Thaler and Benartzi. See Bryan, Karlan, and Nelson (2010) for a survey on the use of commitment devices in several fields.

TABLE 2
EFFECT OF PERSONALIZED INFORMATION ON KNOWLEDGE AND PERCEPTIONS

Category	Variables	Observations	Control Mean	Effect of Personalized Information
Recall	Module recall	742	.824	.090***
			(.382)	(.025)
Information received	Pensions, wages, etc. (general)	732	.168	−.058**
			(.375)	(.026)
	How to increase pension	732	.092	.036
			(.290)	(.024)
	Module with alternatives to increase pension	732	.106	.290***
			(.308)	(.030)
	Does not remember	732	.633	−.268***
			(.483)	(.036)
	Valuation of information received (1–7)	364	5.500	.500***
			(1.445)	(.146)
Knowledge	Pensions system knowledge (1–7)	737	3.995	.240**
			(1.562)	(.113)
	Informed about system (past 10 months)	737	.299	.023
			(.459)	(.032)
	Knows how pensions are calculated	736	.068	−.003
			(.251)	(.018)
	Knows percentage discounted by AFP	715	.119	.001
			(.324)	(.023)
	Understands voluntary savings (APV)	715	.612	.048
			(.488)	(.035)
	Knows retirement age	715	.751	.071**
			(.433)	(.029)
AFP's valuation	AFP qualification (1–7)	706	3.147	.236*
			(1.807)	(.135)
	Pension is an adequate return (0–1)	682	.131	.066*
			(.338)	(.037)
	Trust in the system (1–7)	716	2.835	.210
			(1.746)	(.133)

Note. All regressions include controls for gender dummies, age (in years), log of baseline wage, the balance in the mandatory savings account, log of estimated pension, head-of-household dummy, whether the individual was working in the baseline, as well as dummies for educational attainment. They also include fixed effects for the month of the exposure to the module. Robust standard errors are shown in parentheses. APV = ahorro previsional voluntario ("voluntary pension savings").
* $p < .10$.
** $p < .05$.
*** $p < .01$.

respondents in the four questions we included to measure that knowledge—namely, how pensions are calculated, the percentage discounted for pension, the role of voluntary savings, and the retirement age for men and women—is positive but significant only for the last two. It could be that individuals felt that by updating their beliefs, they gained knowledge but did not learn about the ingredients that are involved in a pension forecast.

Finally, the measured effect of the experiment on the valuation of the system is positive for the three outcomes we present and statistically significantly different from zero for two of the three. This would be consistent with updating beliefs leading to individuals thinking that the system is more fair.

B. Heterogeneity by Difference in Belief Inaccuracy

Our theoretical framework suggests that if the way our intervention played a role is through belief update, we should observe a strong heterogeneity depending on the direction in which we updated participants' beliefs. We thus evaluate whether individuals who underestimated, overestimated, or rightly estimated their pension had different effects of being exposed to our treatment. We argue that while the response through acquiring information may be very different depending on how far one's estimate is from the information provided, we should not observe this type of heterogeneity if the treatment mostly made pension savings more salient.

Because our intervention seems to have a decreasing effect over time, we will conduct the rest of our analysis by focusing on the first 6 months after the experiment. We then combine the 6 months of data and run the same regression as that of equation (2) but interacting the treatment with an indicator variable for each subgroup as classified by the mistake that was made. We also include a control for each subgroup as an individual control variable.

We can observe in figure 3 that there is heterogeneity in the type and magnitude of a mistake individuals make when forecasting their pension. We start by dividing the sample into quintiles of mistakes. We would have liked to do it by finer subgroups, but given our sample size, additional divisions were very noisy. This implies that the first quintile uses individuals who overestimated their pension by more than 55%; the second, individuals who overestimated by 10%–55%; the third, those whose estimation was within 10% of the correct value; the fourth, those who underestimated their pension by 10%–35%; and the fifth, those who underestimated their pension by more than 35%.

In figure 7, we show the results graphically for four outcomes: total savings, mandatory savings, voluntary savings, and retirement. While we present only savings and not the number of contributions, as was the case previously, these results are similar, thus not adding much to the analysis. We present each coefficient at the average pension mistake for that quintile on the horizontal axis. Asterisks occur in association with the points where the estimate is statistically significant. The graph suggests that the results presented in the aggregate analysis are very close to those observed for individuals who had an accurate estimate of their pension: moderate positive effect on voluntary savings and retirement (although none of them significant) and negative and nonsignificant effect on mandatory and total savings. In addition, the results also show a strong pattern of heterogeneity by mistake. Only the first two quintiles of pension mistake see a

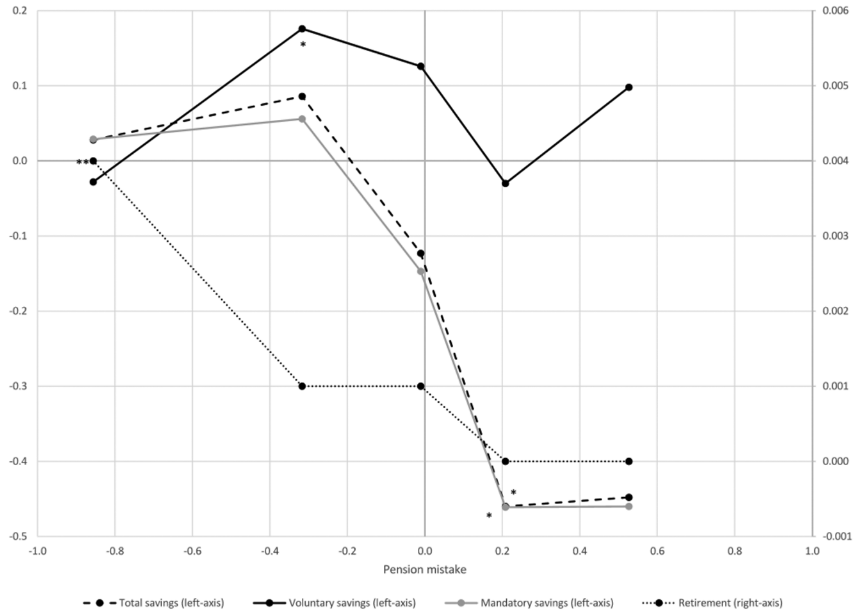

Figure 7. Effect of treatment on savings behavior, by quintile of pension mistake. Each curve presents the coefficient β when estimating equation (2) and interacting with a dummy for the quintile of the pension mistake. All regressions include controls for gender dummies, age (in years), log of baseline wage, the balance in the mandatory savings account, log of estimated pension, head-of-household dummy, whether the individual was working in the baseline, as well as dummies for educational attainment. They also include fixed effects for the month of the exposure to the module and the value of the outcome variable for 12 months previous to the period estimated, as well as a control for the quintile of pension mistake. Standard errors are clustered by individual. One asterisk indicates significance at the 10% level; double asterisk indicates significance at the 5% level.

positive effect of being in the treatment group on their overall, mandatory, and voluntary savings. Because of the fall in sample size, the only statistically significant coefficient is for the second quintile in the case of voluntary savings, but the pattern is very marked. In a mirror pattern, we observe very large negative effects on total and mandatory savings for the two upper quintiles. These effects are significant for the fourth quintile. The effect for voluntary savings is also lower, but the difference is not very large. We obtained similar results when dividing the sample into three groups: those whose simulation was 15% below the sum of their expected and simulated pensions (that is to say, Pension Error > 0.15), those whose simulation was 15% above the sum of their expected and simulated pensions, and those whose simulation came within ±15% of that value.

This is overall consistent with our hypothesis that the intervention helped the treatment group participants update their beliefs. These results are consistent with those in the lowest quintiles thinking that they need to increase their pension savings and using the easiest mechanism to do so (voluntary savings). On the other hand, the lowest higher quintiles appear to want to decrease their savings and do so through a reduction in their mandatory savings, because this is the only way that most individuals in our sample can reduce their contributions. While not reported here, when we look at the effects on mandatory savings over time, the results are long lasting for the lowest quintiles and have limited evidence of a fading "nudge." This would be consistent with treatment working mostly through its effect on updating beliefs for this group. However, for those who had a correct estimation of their pension, we see an initial positive effect on voluntary savings that fades over time. This would be consistent with the treatment playing the role of a nudge in this population. Overall, while we cannot divide the total effect of our treatment into nudge and belief update, these results suggest that both are at play but maybe not for the same population.

Figure 7 also presents a result that does not fit with this framework. We find that the probability of retirement decreases in pension mistake, being positive and significant only for those who had most overestimated their pension. Because retiring is akin to a reduction in savings, how can we reconcile the fact that some individuals in the group that received the worst news are more likely to retire when provided with this information? First, retiring is a decision available only to some very specific individuals who are eligible because of their age or disability. Those individuals are likely to find that they have limited capacity to increase their savings even if we give them "bad news." Second, given that there is a means-tested noncontributory pension that is available for those whose self-funded pension is low, those who are told that the pension they can obtain from their own funds is very low may thus conclude that continuing to save within the system is not profitable and instead rationally elect retirement to obtain the subsidized amount. Third, we find that this behavior was concentrated among those who were unemployed at the moment of their visit to the simulator, and close to 37% of them did not have any income during the previous 6 months. Retiring allows them to unlock their retirement savings. Therefore, this group may have been disappointed by the pension we announced they could receive but still find that this may be the best they could aspire to. We believe this is a strong reality check regarding the possible effects of advising to postpone retirement when individuals may be facing high unemployment and low attachment in the labor market.

We also explore heterogeneity in the survey data. Because our sample size for the survey is significantly smaller than in the administrative data, we divide our sample into three groups on the basis of whether the mistake was more than 15% or within that range.[26] In table 3, we first look at changes in behavior after

[26] Similar results were obtained when joining the first two and the last two quintiles, thus using 10% as the cutoff.

TABLE 3
HETEROGENEITY OF RESPONSES IN SURVEY DATA BY ESTIMATION MISTAKE

			Effect of Personalized Information for Those Who:		
Variables	Observations	Control Mean	Overestimated >15%	Estimated within 15%	Underestimated >15%
Behavior (during the past year considered):					
Affiliating with AFP	732	.035	−.036	−.005	.007
			(.03)	(.01)	(.02)
Starting/increasing voluntary savings	732	.394	.088	.046	.093
			(.06)	(.07)	(.06)
Changing contribution frequency	732	.159	.123***	.005	−.048
			(.05)	(.05)	(.05)
Changing retirement age	732	.256	−.117**	−.034	.014
			(.05)	(.06)	(.05)
Information about system	732	.604	.079	.142**	−.012
			(.06)	(.07)	(.06)
Savings:					
Has other savings for retirement	717	.202	.032	−.051	.076
			(.04)	(.06)	(.05)
Savings outside the system (log)	719	1.115	.204	.153	1.644***
			(.46)	(.65)	(.56)
System's pension important (1–2)	690	.728	.005	.045	−.003
			(.06)	(.06)	(.06)
AFP's valuation:					
AFP qualification (1–7)	701	3.147	.324	.083	.418*
			(.24)	(.24)	(.23)
Pension is an adequate return (0–1)	678	.131	.068	.074	.073
			(.05)	(.08)	(.05)
Trust in the system (1–7)	711	2.835	.371	.216	.150
			(.24)	(.23)	(.22)

Note. All regressions include controls for gender dummies, age (in years), log of baseline wage, the balance in the mandatory savings account, log of estimated pension, head-of-household dummy, whether the individual was working in the baseline, as well as dummies for educational attainment. They also include fixed effects for the month of the exposure to the module and for the group of pension mistake. Each row corresponds to a separate regression where the interaction with each type of mistake is included. Robust standard errors are shown in parentheses.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

the intervention. We find evidence that those who had largely overestimated their pension were more likely to contemplate altering their mandatory contributions but also less likely to change their retirement age. This is consistent with our view that those who received bad news are more likely to consider changing some of their behavior to increase their future pension. That we here find they may be less likely to change their retirement age but also observe that the effect on retirement was positive in the administrative data can be reconciled through the lens of our framework. While those who can retire immediately and are shown the inadequacy of their pension savings may have limited opportunities to increase their savings and thus choose to retire, those that are ineligible to retire immediately are likely to want to increase their savings and do so through a number of channels including anticipating a later retirement age. In the administrative data, we observe only the first group. In the survey, we are likely to capture a much larger fraction of the second group. However, we also find similar coefficients for the three groups on consideration of increasing voluntary savings, which does suggest that the response to voluntary savings may be less dependent on the pension mistake as shown in the administrative data.

We then turn to self-reported savings. For those who increased their savings within the pension system, we find no evidence of a savings movement from outside the pension system since we never observe a negative coefficient. While not significant, the point estimate is positive. On the other hand, those who grossly underestimated their pension (and who were decreasing their savings within the pension fund) may have increased their savings outside the system. This would make sense because pension savings are illiquid and cannot be used for emergencies over the life cycle, while savings outside the pension system have this advantage. Individuals who were shown they were saving appropriately within the system may have diverted savings outside of it. While not reported here, we also find that the decrease in mandatory savings observed in the administrative data appears to stem from a reduction in employment formality and not from a reduced labor-force participation. The probability that the individual reports working is unchanged by the provision of personalized information for any group.

We also found that individuals who had most underestimated their pension were the ones who reported having a higher trust in the system when exposed to the module, although this is not statistically significant. This would be consistent with them updating their belief about the usefulness of the system. However, we also observe a similar-sized and statistically significant coefficient for trust in the AFPs for those who underestimated their pension, which is less consistent with our belief-update hypothesis.

If the reason behind the pattern we document is because we provided new information to individuals and that they were able to update their priors in response to this, we may anticipate that those with less financial savvy would be the ones most affected by the news. Previous studies have found evidence of heterogeneity by knowledge and education (Behaghel and Blau 2012; Hanel and Riphahn 2012). We explore this in table 4 by looking at the effect by estimation mistake and financial-sector knowledge in panel A and by estimation

TABLE 4

EFFECT OF PERSONALIZED INFORMATION BY PENSION MISTAKE AND KNOWLEDGE

		Voluntary Savings		Mandatory Savings		
	Total Savings (1)	No. of Months (2)	Amount (IHS) (3)	No. of Months (4)	Amount (IHS) (5)	Retired (6)
	A. By Financial System Knowledge (N = 2,500)					
Personalized information × Overestimated	−.180	.104	.380**	−.176	−.244	.016
	(.509)	(.070)	(.188)	(.218)	(.509)	(.014)
Personalized information × Correct	−.008	−.045	−.045	.056	−.006	−.004
	(.562)	(.121)	(.276)	(.262)	(.562)	(.031)
Personalized information × Underestimated	−1.036***	.107	.136	−.313	−1.037***	.001
	(.388)	(.101)	(.261)	(.198)	(.387)	(.003)
Personalized information × Overestimated × Medium	.563	−.083	−.345*	.375	.627	.001
	(.682)	(.076)	(.198)	(.285)	(.682)	(.016)
Personalized information × Correct × Medium	−.652	.046	.015	−.347	−.661	.013
	(.720)	(.151)	(.361)	(.335)	(.719)	(.032)
Personalized information × Underestimated × Medium	.482	−.143	−.156	−.002	.450	.001
	(.484)	(.131)	(.346)	(.251)	(.484)	(.010)
Personalized information × Overestimated × High	−.043	−.082	−.190	−.027	−.055	−.005
	(.868)	(.073)	(.234)	(.367)	(.862)	(.020)
Personalized information × Correct × High	.364	.180	1.111**	.115	.044	−.011
	(.787)	(.166)	(.528)	(.368)	(.783)	(.035)
Personalized information × Underestimated × High	1.187**	−.107	−.476	.452	1.241**	.002
	(.585)	(.141)	(.346)	(.293)	(.584)	(.004)
	B. By Education Level (N = 2,508)					
Personalized information × Overestimated	−.845	.089	.188	−.338	−.860	.050**
	(.623)	(.074)	(.155)	(.264)	(.623)	(.021)
Personalized information × Correct	−.479	.114	.108	−.098	−.480	.032
	(.575)	(.144)	(.349)	(.357)	(.575)	(.054)
Personalized information × Underestimated	−1.824***	−.079	−.270	−.925***	−1.818***	.003
	(.571)	(.063)	(.171)	(.319)	(.570)	(.005)
Personalized information × Overestimated × HSD	.488	−.072	−.086	.281	.488	−.052**
	(.818)	(.088)	(.229)	(.353)	(.818)	(.024)
Personalized information × Correct × HSD	.033	−.205	−.202	−.178	.049	−.047
	(.739)	(.176)	(.436)	(.423)	(.738)	(.057)
Personalized information × Underestimated × HSD	1.819***	.065	.316	1.061***	1.816***	−.006
	(.651)	(.112)	(.288)	(.362)	(.650)	(.012)
Personalized information × Overestimated × Some college	1.739**	−.038	.133	.590*	1.715**	−.042*
	(.844)	(.088)	(.208)	(.346)	(.842)	(.022)

TABLE 4 (*Continued*)

	Total Savings (1)	Voluntary Savings: No. of Months (2)	Voluntary Savings: Amount (IHS) (3)	Mandatory Savings: No. of Months (4)	Mandatory Savings: Amount (IHS) (5)	Retired (6)
Personalized information × Correct × Some college	.329	−.158	.018	.120	.136	−.038
	(.834)	(.168)	(.456)	(.445)	(.831)	(.055)
Personalized information × Underestimated × Some college	1.607**	.164	.458	.767**	1.583**	.003
	(.666)	(.105)	(.280)	(.368)	(.667)	(.007)
Personalized information × Overestimated × University	1.088	.013	−.069	.286	.974	−.024
	(.972)	(.105)	(.196)	(.433)	(.978)	(.027)
Personalized information × Correct × University	.534	.099	.700	.154	.496	−.021
	(.929)	(.228)	(.575)	(.473)	(.929)	(.056)
Personalized information × Underestimated × University	.487	.079	−.267	.381	.499	.002
	(.794)	(.181)	(.481)	(.408)	(.792)	(.006)

Note. All regressions include controls for gender dummies, age (in years), log of baseline wage, the balance in the mandatory savings account, log of estimated pension, head-of-household dummy, whether the individual was working in the baseline, as well as dummies for educational attainment. They also include fixed effects for the month of the exposure to the module and the value of the outcome variable for 12 months previous to the period estimated. In panel A, controls for financial literacy and their interactions with pension mistakes are included, as well as controls for pension mistake directly. In panel B, interactions of each education dummy with pension mistakes are included, as well as controls for pension mistake directly. Robust standard errors are shown in parentheses. IHS = inverse hyperbolic sine; HSD = high school diploma.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

mistake and education in panel B. In each regression (represented by individual columns in table 4), we include the main interaction between the treatment and each mistake category and the interaction of these with indicators of financial knowledge and education. We do not include the main effect for personal information because this would be collinear with our interactions with each pension-mistake category.

We find evidence supporting our hypothesis in panel A of table 4. Those with the lowest level of financial knowledge are the ones who increase their savings the most when provided with "bad news" and who respond by reducing their mandatory contributions when receiving "good news." Savings and reduced contribution responses are reduced in groups with higher financial literacy.

In panel B of table 4, we turn to whether the response also depended on formal educational attainment. We observe there a murkier pattern for voluntary savings. Added savings appears to have not been concentrated among those with the lowest levels of education. However, mandatory savings and retirement propensity behaviors suggest a similar pattern as the one in panel A. The reduced savings when faced with good news does appear to be strongest among those without a high school diploma, almost fully disappearing for more educated groups. For retirement, we also find that the provision of bad news increased the retirement probability for those without a high school diploma but not for those with higher levels of education. Thus, this appears to be in line with our hypothesis that the added information through personalization allowed individuals with lower degrees of financial literacy and overall education to update their beliefs.

Overall, we find that these results appear to be consistent with a strong heterogeneous effect of the module depending on the pension mistake, which would suggest an important role for belief update.

C. *Heterogeneity by Difference in Effect of Distinct Actions*

We next turn to look at whether the personalization of the actions that were suggested to participants played a relevant role. As explained before, this could be due to two specific reasons. One, individuals could follow the type of action in which they are shown the most return. Second, individuals could be discouraged when shown that they have limited capacity to alter their future pension given the time they have left or the type of income they experience. To explore the first hypothesis, we obtained estimates of pensions under alternative decisions for the control group and the treatment group. We then divide our population by whether the message that was given was above or below a certain threshold. For voluntary savings, we use 10%, because this was the maximum of the range provided to the control group. For density, we simply divide the sample into groups that had full density and thus were shown no benefit from increasing density and those that were shown a positive effect. Finally, for delaying retirement, we use 8%, because this was the number provided to the control group.

We then run separate regressions in each panel of table 5 where we interact the effect of personalized information depending on whether one was shown a large or a small increase by taking a given action. In panel A, we see that those who were shown a larger potential increase from contributing voluntarily 1% of their income did not experience a statistically significant effect of being shown personalized information, except for an extremely small effect on retiring. A similar conclusion can be reached for those who were shown large increases. Overall, if anything, the size of the coefficients indicate that those who were shown magnitudes below the controls are those that increased their voluntary savings. We thus see limited evidence that the type of recommendation influenced the behavior we noted in aggregate.

Panel B of table 5 separates the sample by those who were fully contributing mandatorily and those who were not. We observe that the decrease in mandatory

TABLE 5

EFFECT OF PERSONALIZED INFORMATION ON SAVINGS BEHAVIOR BY TYPE OF MESSAGE, FIRST 6 MONTHS

		Voluntary Savings		Mandatory Savings		
	Total Savings Amount (IHS) (1)	No. of Months (2)	Amount (IHS) (3)	No. of Months (4)	Amount (IHS) (5)	Retired (6)
	A. By Returns to Voluntary Contributions					
Personalized information × Small increase from voluntary savings	−.098	.006	.078	−.010	−.116	.001*
	(.139)	(.005)	(.051)	(.012)	(.139)	(.001)
Personalized information × Large increase from voluntary savings	−.488	−.025	−.211	−.054	−.489	.031
	(3.865)	(.030)	(.311)	(.319)	(3.866)	(.023)
	B. By Returns to Increasing Density					
Personalized information × No increase from density	−.276*	.007	.091	−.025*	−.289*	.002
	(.151)	(.008)	(.081)	(.013)	(.151)	(.001)
Personalized information × Positive increase from density	−.080	.004	.042	−.008	−.101	.001
	(.248)	(.005)	(.051)	(.022)	(.248)	(.001)
	C. By Returns to Delaying Retirement					
Personalized information × Small increase from delaying retirement	.405	.007	.082	.034	.375	.002
	(.253)	(.006)	(.060)	(.023)	(.253)	(.001)
Personalized information × Large increase from delaying retirement	−.418**	.006	.072	−.038***	−.427***	.001
	(.163)	(.007)	(.072)	(.014)	(.162)	(.001)
Control mean	7.574	.031	.336	.666	7.564	.001

Note. The sample includes six monthly observations for 2,415 individuals for all regressions. All regressions include controls for gender dummies, age (in years), log of baseline wage, the balance in the mandatory savings account, log of estimated pension, head-of-household dummy, whether the individual was working in the baseline, as well as dummies for educational attainment. They also include fixed effects for the month of the exposure to the module and the value of the outcome variable for 12 months previous to the period estimated. In panel A, controls for whether the return to added voluntary savings was above 10% were included. In panel B, control for whether the individual had contributed every month in the 12 months prior was included. In panel C, controls for whether the return to delayed retirement was above 9% were included. Clustered standard errors by individual are shown in parentheses. IHS = inverse hyperbolic sine.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

(and also total) savings is completely concentrated in the group that was fully contributing mandatorily. As specified before, those are the ones who could decrease their frequency by switching, partially or fully, to informality. Thus, we find again limited evidence that the personalized information on the return to each action explains the pattern we observed in aggregate.

Finally, panel C of table 5 differentiates the sample by those who were shown small or large returns to delaying retirement. We do not observe a difference in the probability of retiring between the two groups. We do find large decreases in mandatory and total savings for those who were shown a large return to delaying retirement. This can be explained as in the case of panel B. These are individuals who were more strongly attached to the labor force.

Overall, these results suggest that the personalization of the effect of each action played a much more muted role than the updating of beliefs through the provision of an estimated annuity payout. Similar conclusions are reached when separating the sample by which action was providing the highest amount of additional pension estimate, as shown in table A.4.

Nevertheless, our framework also suggests that there may be an effect linked to discouragement if an individual is shown, overall, limited capacity to alter his or her future pension. To study this in more detail, we return to our division of our sample between overestimators, correct estimators, and underestimators of pension. This time, we additionally interact this by whether the maximum of all actions was shown to be below or above 9%. We call those who were shown all simulations to be below 9% "low possibilities" participants. The opposite is true when that value is above 9%. Table 6 shows these results. In general, we continue to find limited evidence that the personalization of effect of actions matters significantly. Within each group of pension mistake, we observe in general a similar pattern between the two groups. The exception to this is the large effect on retirement within the group of those who had largely overestimated their pension, which we observe is strongly concentrated among the group that was shown to have limited possibilities to alter their future pension. Thus, this would be in agreement with the supposition that this behavior is linked to a discouragement effect of being shown that one's pension will be significantly lower than one's anticipation, coupled with the fact that there appears to be little participants can do to alter this reality. This leads them to step out of the system and obtain the pension they are able to accrue immediately. We see the reverse pattern for those who had underestimated their pension, where the effects are largest and significant only for the group of individuals who were shown they could significantly alter their pension. Again, these are basically individuals who were more able to significantly reduce their savings by taking one of the actions shown to them.

D. Additional Heterogeneity

While our framework suggests that some types of heterogeneity are likely to be more informative than others, one could be interested in exploring heterogeneity with respect to some variables that we know influence decisions related to pensions. It could be that our heterogeneity by beliefs maps to heterogeneity in other characteristics. We explore the role of age and current wages. We cannot explore the role of health shocks (which have been argued to be very relevant for

TABLE 6

EFFECT OF PERSONALIZED INFORMATION ON BEHAVIOR WITHIN THE PENSION SYSTEM BY HOW MUCH PENSIONS COULD BE IMPROVED AND PENSION MISTAKE, FIRST 6 MONTHS

		Voluntary Savings		Mandatory Savings		
	Total Savings Amount	No. of Months	Amount	No. of Months	Amount	Retired
Personalized information × Overestimated × Low possibilities	−.033	.019	.199	−.007	−.050	.008**
	(.459)	(.014)	(.152)	(.042)	(.459)	(.003)
Personalized information × Overestimated × High possibilities	−.045	.008	.071	−.005	−.066	.001
	(.310)	(.006)	(.057)	(.028)	(.310)	(.001)
Personalized information × Correct × Low possibilities	−.176	−.007	−.042	−.012	−.188	.000
	(.317)	(.015)	(.157)	(.028)	(.316)	(.003)
Personalized information × Correct × High possibilities	−.051	.017	.193	−.010	−.072	−.001
	(.441)	(.015)	(.171)	(.039)	(.441)	(.001)
Personalized information × Underestimated × Low possibilities	−.240	−.001	.008	−.023	−.237	−.000
	(.212)	(.014)	(.148)	(.018)	(.212)	(.001)
Personalized information × Underestimated × High possibilities	−.734*	.006	.075	−.063*	−.753*	.001**
	(.406)	(.008)	(.083)	(.035)	(.407)	(.000)
Control mean	7.574	.031	.336	.666	7.564	.001

Note. The sample includes six monthly observations for 2,377 individuals for all regressions. All regressions include controls for gender dummies, age (in years), log of baseline wage, the balance in the mandatory savings account, log of estimated pension, head-of-household dummy, whether the individual was working in the baseline, as well as dummies for educational attainment. They also include fixed effects for the month of the exposure to the module and the value of the outcome variable for 12 months previous to the period estimated. Controls for an interaction between pension mistake and whether the maximum return was above 9% were included in all regressions. Clustered standard errors by individual are shown in parentheses.

* $p < .10$.

** $p < .05$.

retirement decisions) because we do not have any information regarding health conditions.

Table A.5 shows results where we divide participants into three age groups. They suggest that our positive effect on voluntary savings was concentrated among those who are within 10–15 years of retirement. It is the only age group where total savings are not reduced, and it is also the group for which the negative coefficients on mandatory savings are the smallest. We find that decreasing savings in the mandatory account is particularly relevant for the middle-aged group. This thus suggests that while personalized information was able to increase savings, it did so for an age group that may be already more informed and more focused on retiring in the not-so-distant future. While not presented here, we find that pension overestimation is particularly strong for the oldest age group, which could explain in part the pattern we report. The young and the middle-aged are equally represented among those who significantly underestimated their pension, which does not explain why only those in the middle-aged group decrease their mandatory pension contributions. Thus, we argue that this continues to show some distinctive role for belief update, above and beyond closeness to retirement age.

We also explore heterogeneity by current wage. We divide our sample into three groups: those who earned less than CLP$250,000 per month, which corresponds to about the minimum wage at the moment of the study; those who earned between CLP$250,000 and CLP$500,000 (that is to say, two minimum wages); and finally those who earned more than this amount. This would match to thresholds of around US$450 and US$900, respectively. It also allows us to divide our sample into three groups of similar size. Table A.6 shows limited differences by wage group. The only visible effect suggests that the decrease in mandatory savings is concentrated among the highest earners in our sample. This would match again our hypothesis that only those who contribute every month to the pension fund are those who can reduce their contribution by selecting informality.

V. Conclusions

Long-term savings requires commitment, self-control, and a broad understanding of financial concepts, which allows individuals to connect how current costly decisions will lead to uncertain returns in the future. In this paper, we show that individuals saving for retirement in a system that has more than 30 years of existence still have difficulty estimating the annuity payout they will receive and that providing personalized information about this can have a substantial effect on their savings and retirement behavior in the short run, even without any additional nudges or commitment devices. We argue that the effect of providing personalized information appears to have been mostly caused by enabling participants to update their beliefs about the annuity payout they would receive. This would suggest that there may be informational gaps that, when filled, could influence long-term savings decisions.

However, our experiment also shows that personalizing information may lead some individuals to reduce their savings behavior. This is interesting because a recurring topic for academics and policy makers is whether individuals have adequate savings levels for retirement; see, for instance, Munnell, Webb, and Delorme (2006) and Federal Reserve Board (2014) for the US case and OECD, IDB Bank, and World Bank (2014) for Latin America. Overall, our results suggest that individuals appear to have a clear objective and respond to information in a way that is consistent with that objective. This would suggest that the view that individuals are “undersaving” should not be considered universal, because part of our sample appears to have previously been "over-saving." Overall, the heterogeneous responses suggest that personalized and individual expectations should be taken into account when designing nudges and other encouragement interventions.

Furthermore, our paper is silent about what types of nudges or commitment devices could be added to this setup. We leave it to further research to explore the complementarity or substitutability between providing personalized information and offering commitment mechanisms to implement some of the decisions suggested by the personalized simulator. Nevertheless, our results suggest a lower bound for a policy where personalized information could be bundled with additional instruments to increase future savings.

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