Search and Negotiation with Biased Beliefs in Consumer Credit Markets*

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Abstract

How do biased beliefs about the distribution of interest rates affect search, negotiation, and loan terms in consumer credit markets? In collaboration with Chile's financial regulator, we conducted a randomized controlled trial with 112,063 loan seekers where we elicited beliefs about the interest rate distribution, then showed treated participants a price comparison tool that we built using administrative data on the universe of consumer loans merged with borrower characteristics. The tool shows loan seekers a conditional distribution of interest rates based on similar loans obtained recently by similar borrowers. We find that most consumers thought interest rates were lower than they actually were, and the price comparison tool caused them to increase their expectations about the interest rate they would obtain by 55%. Most consumers also underestimated price dispersion, and our price comparison tool caused them to increase their estimates of dispersion by 68%. The price comparison tool did not cause people to search or apply at more institutions, but it did cause them to be 39% more likely to negotiate with their lender, to receive 13% more offers and 11% lower interest rates, and to be 5% more likely to take out a loan.

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1 Introduction

Consumer credit markets feature large amounts of *within-consumer* price dispersion (Stango and Zinman, 2016; Ponce, Seira, and Zamarripa, 2017). Even though many consumers pay substantial costs by borrowing at higher rates than they could, this price dispersion can persist in equilibrium if consumers engage in limited search and negotiation (Stahl, 1989; Hortaçsu and Syverson, 2004; Allen, Clark, and Houde, 2014).[1] Why, then, do consumers not search or negotiate more? The existing literature has focused on *costs* that prevent search or negotiation, including time and travel costs, high rejection rates, cognitive effort to compare complex offers, and the costs of gathering additional quotes to use in a negotiation.[2] We focus instead on the expected *benefits*, and test whether biased beliefs about the interest rate distribution constrain search and negotiation.

Motivated by a model of sequential search and negotiation with potentially biased beliefs, we conducted a randomized controlled trial (RCT) in close collaboration with Chile's financial regulator, the Comisión para el Mercado Financiero (CMF). The RCT, conducted with 112,063 Chileans searching for loans, tests how correcting biased beliefs about the distribution of interest rates affect search, negotiation, and loan terms in consumer credit markets. We both measure beliefs about the interest rate distribution and show loan seekers in the treatment group a price comparison tool designed to correct biased beliefs. We built the price comparison tool using administrative data from CMF on the universe of consumer loans merged with borrower characteristics. The tool shows participants the conditional distribution of interest rates based on similar loans obtained recently by similar borrowers.

In a standard sequential search model (without negotiation), biased beliefs would affect the perceived benefits of search and thus the consumer's reservation rate. If consumers underestimate the first moment of the distribution of interest rates (as we find most consumers do), they will search more than is optimal because they will overestimate the expected benefit from another draw from the distribution of rates. If consumers underestimate the second moment of the distribution (as we also find that most consumers do), they will search less than is optimal because they underestimate the expected benefit from another draw.

Biased beliefs can also affect negotiation. In a model where consumers negotiate with lenders and signal their beliefs about the interest rate distribution they face during the negotiation, we

[1] Consumers incur a substantial cost to borrow at higher rates than they could obtain. In the US auto loan market, the average borrower pays $488 more in present value for a $17,000 car (Argyle, Nadauld, and Palmer, 2023). In mortgage markets, Woodward and Hall (2012) estimate that borrowers pay $1,000 more for a $100,000 mortgage, and Bhutta, Fuster, and Hizmo (2024) estimate that borrowers pay $6,250 more for a median $250,000 mortgage.

[2] On physical search costs across branches, see Allen, Clark, and Houde (2013) and Argyle, Nadauld, and Palmer (2023). On high rejection rates leading to higher per-offer search costs for less-creditworthy borrowers, see Agarwal, Grigsby, Hortaçsu, Matvos, Seru, and Yao (2024). On the cognitive effort required to compare offers, see Galenianos and Gavazza (2022). For evidence on the costs of negotiating in mortgage markets, see Allen and Li (2025).

show that a consumer's reservation rate—which is a function of their beliefs about the interest rate distribution and their search cost—is no longer a sufficient statistic for search. Consumers who underestimate the second moment of the interest rate distribution will (successfully) negotiate less than if they had accurate beliefs. The model's prediction for how biased beliefs about the first moment affect negotiation, however, is non-monotonic. Thus, in a sequential search model with negotiation, correcting consumers' underestimates of the second moment should increase negotiation while not necessarily increasing search, and the effects of correcting consumers' underestimates of the first moment depend on how biased their beliefs about the first moment were.

We test these predictions from the model in an RCT where we recruited participants through Google ads targeted to people searching for keywords related to consumer loans in Chile. After participants clicked on the Google ad and consented to participate in the study, we collected their contact information and national ID numbers, which we use to track participants' future loan outcomes in administrative data.[3] We then had them fill out a baseline survey that randomized whether we asked them their beliefs about the distribution of interest rates (which we refer to as the "elicit beliefs" treatment). After the baseline survey, we cross-randomized whether we showed them a price comparison tool, a simple tool showing our estimate of the cost savings from search in pesos, or a control video. We then elicit beliefs again from those assigned to the elicit beliefs treatment, to measure the extent to which participants update their beliefs after seeing the price comparison or simple tool. To measure real outcomes on search, negotiation, and loan terms, we use a combination of administrative data from CMF and follow-up phone surveys.

We first document that the majority of participants have biased beliefs about both the first and second moments of the interest rate distribution. While there is significant heterogeneity in beliefs, the vast majority underestimated both the interest rate they would get on the loan they took out, as well as the dispersion in rates. We measure whether participants underestimated the rate they would get by comparing actual interest rates obtained by participants after the study (according to administrative data) to their beliefs about the rate they would obtain. Nearly three-quarters (73.2%) thought they would obtain an interest rate lower than what they actually obtained. Furthermore, borrowers who underestimated the rate they would obtain did so by on average 14.9 pp. Their estimates of dispersion in the interest rates banks would offer them are also much lower than suggested by administrative data: specifically, 75% of participants underestimate dispersion in the interest rates bank would offer them compared to administrative data. This finding is robust to various measures of dispersion; following the macroeconomic uncertainty literature (Coibion, Georgarakos, Gorodnichenko, Kenny, and Weber, 2024), our preferred measure is the range be-

[3]Chile's national ID number, or *rol único tributario* (RUT), is commonly used in everyday life. For example, people are asked to give their national ID numbers when they check out at the grocery store. The data merge using national ID number was completed in accordance with the terms of the collaboration with CMF and with the study's IRB protocols.

tween the highest rate a bank would offer them and the lowest rate a bank would offer them, due to its simplicity.[4]

We then test the effects of a price comparison tool designed to correct biased beliefs on (i) beliefs about interest rates, (ii) search behavior, (iii) negotiation, and (iv) loan terms, including whether they were offered a loan, the terms of the offer, and whether they took out the loan. The price comparison tool was built using administrative data on loan and borrower characteristics for the universe of consumer loans in Chile, i.e., data from over 1.8 million loans to approximately 1.2 million borrowers over two years. The tool shows treated participants the conditional distribution of interest rates that similar borrowers obtained for similar loans over the previous six months.

Immediately after seeing the price comparison tool, simple tool, or control video, participants assigned to the elicit beliefs treatment were asked again about their beliefs about the distribution of interest rates. When treated with the price comparison tool, participants update and report expecting to receive a 16 pp *higher* interest rate on the loan they obtain, or a 54.9% increase compared to the control mean posterior belief of a 29.2% expected annual interest rate. The price comparison tool also led participants to increase their expectation of how much price dispersion they face in the market by 16 pp, or 68% relative to the control mean posterior of 23.2 pp dispersion in annual interest rates.[5]

To measure effects on search behavior, negotiation, and loan terms, we combine the administrative data on originated loans with rich data on participants' search histories that we collected in a follow-up phone survey conducted with a subset of 6,441 participants. The price comparison tool did not affect the number of institutions at which participants searched for information (which includes "soft search" such as visiting a bank website or branch to get a sense of the interest rate the bank would offer without formally applying), the number of institutions at which they applied for a loan, nor the specific institutions at which they searched. While the lack of a treatment effect on search could be due to offsetting effects of updating inaccurate beliefs about the first and second

[4]This measure performed better in piloting than more complicated measures of the distribution. Consistent with our piloting, in the inflations expectations literature eliciting a more detailed distribution leads to higher survey dropout (Weber, D'Acunto, Gorodnichenko, and Coibion, 2022), which is a particular concern in our setting given that our participants take the survey online and are not professional survey respondents unlike in some other studies. Following Coibion, Georgarakos, Gorodnichenko, Kenny, and Weber (2024) we also ask participants what percent of loan offers they think are above the midpoint of the distribution (i.e., the midpoint between the lowest and highest rates they reported to us) to capture potential asymmetry, and use the implied standard deviation of the distribution under certain functional form assumptions as an alternative measure of dispersion.

[5]We avoid using the term "prior" to refer to their belief prior to seeing the tool, as some may have already updated their beliefs—for example by visiting a bank's website, seeing a bank ad, or getting an offer from a bank—before participating in our study. To distinguish the belief elicited before seeing the price comparison tool, simple tool, or control video and the belief elicited afterwards, we refer to the latter as a "posterior belief." Although we winsorize responses to these interest rate questions at the 95th percentile, the distributions are nevertheless quite skewed and the medians are substantially lower than the means. The control median posterior of the annual rate people expect to get is 18%, while the control median posterior of dispersion is 10.7 pp.

moments of the distribution, even among subsamples that underestimated the second moment and did not underestimate the first moment we do not find a statistically significant impact on search. Instead, as highlighted by our model, the lack of an effect on search is likely due to the possibility of negotiating, which breaks the sufficient statistic relationship between a consumer's reservation rate and search.

Despite not affecting the number of institutions at which participants searched or applied, the price comparison tool led them to obtain 13% more offers and 11.9% lower interest rate offers (measured in the follow-up survey), and to be 5% more likely to take out a loan (measured in the administrative data). Increased negotiation explains how borrowers obtained more loan offers and more favorable terms: the price comparison tool increases the probability of negotiating by 39%. In a subsequent survey we conducted to gather more data on negotiation, we find that the treatment effect on negotiation occurs prior to receiving a formal offer, which explains why negotiating also affects the probability of receiving an offer: in many cases the lender first informally offers an interest rate, then only if terms are agreed on does the lender issue a formal offer.

We next test two key predictions of our model. In the model, lenders make initial offers that are conditional on borrower characteristics and their desired loan characteristics, without observing the consumer's beliefs. The consumer then signals their beliefs to the lender in the negotiation (e.g., if the lender offers a 30% annual interest rate, a consumer countering with 10% vs. 25% will reveal very different beliefs about the interest rate distribution). The lender can then issue a new take-it-or-leave-it offer at a lower price, but must incur a cost to do so, and thus only does so if the consumer's beliefs indicate that they are likely to accept the new offer.

Assuming that seeing the price comparison tool leads borrowers to partially update their beliefs, our model has two key predictions. First, the price comparison tool should cause consumers who underestimated dispersion to negotiate more (but not necessarily search more). Second, the effect of the price comparison tool on negotiation should be non-monotonic in biased beliefs about the first moment of the interest rate distribution. Intuitively, consumers who overestimate the first moment will not benefit from updating their beliefs closer to the truth, as the lender will assume based on the consumer's beliefs that the consumer will accept the initial offer.[6] Consumers who vastly underestimated the first moment also will not benefit, as their partially updated beliefs will still be too far below the true distribution, and thus the lender will infer that it cannot profitably lend to them, and will not incur the cost of negotiating. Meanwhile, consumers who somewhat underestimated the first moment will successfully negotiate more, as their updated beliefs make it such that not only are they sufficiently likely to walk away if the lender does not offer a lower rate, but also the lender can still profitably lend to them at a lower, negotiated rate.

[6] Our model does not require consumers to report truthfully, but does assume that the way the consumer interacts with the lender provides a truthful signal about the consumer's beliefs.

Our results align with these two predictions. First, the effect of the price comparison tool on negotiation is concentrated among those who underestimated dispersion, increasing their probability of negotiating by 74%. Meanwhile, the price comparison tool did not have an effect on negotiation for those who did not underestimate dispersion, and the difference in the estimated treatment effects on negotiation between the two groups is statistically significant. Second, the treatment effect on negotiation is non-monotonic in how biased beliefs are about the first moment of the distribution. There is no treatment effect for those who overestimated the first moment, nor for those who vastly underestimated it, while the effect on negotiation is concentrated among those who somewhat underestimated the first moment.

Finally, cross-randomizing whether we elicited beliefs about the interest rate distribution led participants to search at 0.13 (4%) more institutions and to obtain 10% lower interest rates than participants who were not asked their beliefs. Unlike the price comparison tool treatment, eliciting beliefs does not have an effect on negotiation or the number of offers received: instead, participants asked their beliefs obtained lower rates by searching more.

Our paper makes three main contributions. First, we contribute to the literature documenting high within-borrower price dispersion in consumer credit markets, the resulting high interest rate costs incurred by borrowers who do not search much, and the constraints to search. High price dispersion faced by the same borrower (or, in some studies, observationally similar borrowers) is a hallmark of consumer credit markets around the world (Zinman, 2015), including markets for credit cards (Stango and Zinman, 2016; Ponce, Seira, and Zamarripa, 2017), consumer loans (Karlan and Zinman, 2019; Cuesta and Sepúlveda, 2021), auto loans (Argyle, Nadauld, and Palmer, 2023), and mortgages (Allen, Clark, and Houde, 2014; Gurun, Matvos, and Seru, 2016; Guiso, Pozzi, Tsoy, Gambacorta, and Mistrulli, 2022; Coen, Kashyap, and Rostom, 2024). Due to this price dispersion, not searching much leads borrowers to pay substantially higher interest costs (Argyle, Nadauld, and Palmer, 2023; Bhutta, Fuster, and Hizmo, 2024).

Existing studies have focused primarily on the role of search *costs* in preventing search, while assuming—due to data limitations—that consumers have correct beliefs about the distribution of prices from which they are drawing, and thus know the *benefits* of search. These search costs include physical search costs across branches (Allen, Clark, and Houde, 2013; Argyle, Nadauld, and Palmer, 2023), high rejection rates (Agarwal, Grigsby, Hortaçsu, Matvos, Seru, and Yao, 2024), and the cognitive effort of comparing offers (Galenianos and Gavazza, 2022)—especially since financial products are often complex (Célérier and Vallée, 2017; Kulkarni, Truffa, and Iberti, 2025) and can include shrouded costs (Campbell, Jackson, Madrian, and Tufano, 2011; Stango and Zinman, 2014; Ferman, 2016; Alan, Cemalcilar, Karlan, and Zinman, 2018). We show that individuals have biased beliefs about the interest rate distribution they face and test the effects of a price comparison tool designed to correct biased beliefs.[7] While we do not find effects of correcting biased beliefs on search, we do find effects on negotiation and interest rates obtained, and show that this can be rationalized when negotiation is added to a model of sequential search with biased beliefs.

Second, we contribute to the literature on the importance of negotiation in markets with price dispersion. Differing negotiation leverage across consumers can cause persistent interest rate dispersion in equilibrium (Allen, Clark, and Houde, 2014). Theoretical models often posit that fixed costs constrain negotiation (Rubinstein, 1982), and there is empirical evidence that this is true (Backus, Blake, Larsen, and Tadelis, 2020). Allen, Clark, and Houde (2013, 2019) and Allen and Li (2025) model negotiation and competition in mortgage markets, and assume that negotiation requires searching for additional quotes. However, negotiating may merely require accurate information on the price distribution (Grennan and Swanson, 2020). We introduce a novel way that beliefs can affect negotiation by allowing for biased beliefs: in our model, the lender learns about the consumer's beliefs about the interest rate distribution from the consumer's response in the negotiation. Thus, correcting biased beliefs can affect the probability that the consumer negotiates successfully and receives a lower interest rate. Consistent with this, we find that the price comparison tool causes borrowers to be more likely to negotiate and to obtain lower interest rates.

Third, we contribute to the literature on the effect of beliefs on financial decision-making, which has been studied on both the assets and liabilities sides of the household balance sheet. On the assets side, individuals who have experienced low stock market returns are more pessimistic about future stock market returns and are less likely to participate in the stock market (Malmendier and Nagel, 2011). Increased expectations about house price growth cause increases in real estate investments (Armona, Fuster, and Zafar, 2019; Liu and Palmer, 2023).[8] On the liabilities side, experiencing inflation leads households to expect more inflation in the future and to borrow more using fixed-rate mortgages (Malmendier and Nagel, 2016). Correcting biased beliefs about the dollar costs of payday loans reduces loan demand among first-time borrowers (Bertrand and Morse, 2011), while more experienced payday borrowers have more accurate beliefs but are willing to pay to avoid repeat borrowing (Allcott, Kim, Taubinsky, and Zinman, 2022). We show how experimentally-induced changes in beliefs about the distribution of interest rates affect search, negotiation, and loan terms in the market for consumer loans.

[7] In other contexts beyond consumer financial markets, Arteaga, Kapor, Neilson, and Zimmerman (2022) and Agte, Allende, Kapor, Neilson, and Ochoa (2024) study how beliefs affect search in the education market, Bandiera, Bassi, Burgess, Rasul, Sulaiman, and Vitali (2025) study this in the labor market, Jäger, Roth, Roussille, and Schoefer (2024) study how beliefs affect job search and negotiation intentions, and Caldwell, Haegele, and Heining (forthcoming) show that workers signal their beliefs to employers by providing their salary expectations before they receive a job offer.

[8] The real estate investments investigated in these papers are incentivized but experimental, i.e., they are stylized decisions made by participants as part of an incentivized survey experiment. In contrast, we study decisions made by loan seekers in the real world after receiving our treatment.

2 Institutional Context

2.1 Chilean Consumer Loans

Consumer loans are a popular credit product offered by banks in Chile: 43% of Chilean households have outstanding consumer credit, with consumer loans and credit cards being equally popular forms of obtaining credit from banks (Banco Central de Chile, 2021). Consumer loans are typically uncollateralized, have fixed interest rates, and are paid in equal monthly installments up until the loan matures.

According to administrative data from the CMF on the universe of consumer loans obtained between November 2021 and February 2024 ($N = 1{,}863{,}087$ consumer loans), the mean and median annual interest rates are 25.8% and 23.9%, respectively. The median loan amount is $4,488 USD, and the median maturity is 3 years. Based on our survey data, consumer loans are most commonly used to pay down other higher-interest debt (23.7% of borrowers), purchase or repair a car (16.3%), invest in their business (10.7%), make home improvements (5.2%), and purchase consumer durables (4.1%).

Unlike in the US and many other countries, Chilean credit bureaus do not report continuous credit scores; rather, they report binary flags of whether people have defaulted on prior loans. In 2012, the government passed legislation requiring a one-off deletion of information on default in response to the financial shock that many households experienced due to a large earthquake; Liberman, Neilson, Opazo, and Zimmerman (2018) study the effects of this policy.

2.2 Regulation in the Consumer Credit Market

Chile has a number of regulatory conditions that must be fulfilled when consumers are offered a loan. In 2011, the Chilean parliament defined a new credit term, *carga anual equivalente* (CAE), which functions as the Chilean equivalent of the annual percentage rate (APR) and must include fees. By law, both the CAE and the interest rate include the costs of all services inherent to loan operations. In addition, the CAE must include any additional costs of the loan, such as any insurance included with the loan (e.g., insurance that will pay off the outstanding balance if the borrower becomes unemployed or incapacitated). As part of the same legislation, borrowers have to be shown a universal credit contract (potentially in addition to another loan being offered) that represents a standardized “plain vanilla” contract and does not include insurance or various other types of fees that are sometimes included in consumer loan offers.[9]

In 2012, a new law mandated that formal loan offers must be made through a standardized

[9]This legislation applied to all consumer loans with loan amounts below approximately $40,000 USD. Thus, it applied to nearly all consumer loans.

disclosure sheet in which the CAE is prominently displayed in large bold numbers in the upper right-hand corner. Additional fees included in the CAE were also itemized in the disclosure sheet. Kulkarni, Truffa, and Iberti (2025) study the impacts of both the 2011 and 2012 regulations.

Finally, in 2013, the Chilean government lowered interest rate caps on consumer loans. Price ceilings have been in place for consumer loans since 1981, but the 2013 law substantially lowered this cap. The maximum interest rate on consumer loans is conditional on the loan terms, and is defined as 1.5 times the "current interest rate," where the current interest rate is calculated as a volume-weighted average of interest rates on originated consumer loans (conditional on loan characteristics). This law also expanded the interest rate caps to not only consumer loans but also other financial products such as credit cards. Cuesta and Sepúlveda (2021) study the effects of this law on both access to credit and interest rates.

2.3 Search for Consumer Loans

Chileans search for loans a number of ways: 92.3% of our follow-up survey respondents visited at least one bank website during their search, 37.3% used a mobile banking app, 33.9% visited a branch in person, 32.9% communicated with a bank by email, and 26.5% communicated with a bank by phone. Soft search (i.e., searching for information without formally applying for a loan) plays an important role: while control participants formally applied to 1.3 institutions on average, they searched across 3.4 institutions.

In our baseline and follow-up surveys, we asked participants what features of a loan were most important to them to better understand search behavior. Figure A.1a shows that in our baseline survey (when participants were looking for a loan), the three most important features of the loan were all functions of the interest rate: 26% of participants reported that the total loan cost was the most important feature, 22% reported monthly payment, and 20% reported the interest rate or APR. Similarly, in our follow-up survey, the most common reason for choosing a particular lender was a lower interest rate, with 46% of participants giving this answer (Figure A.1b).

Another potentially important feature is the probability of being approved for a loan, as consumer credit markets feature high rates of rejection and consumers need to "search for approval" (Agarwal, Grigsby, Hortaçsu, Matvos, Seru, and Yao, 2024). In our context, approval rates conditional on formally applying for a loan are 51.1%. Furthermore, 48.7% of survey respondents reported that the bank gave them some indication of whether their application would be approved before or without formally applying. In our baseline survey, 15% of borrowers named getting approved for the loan as the most important feature of the loan for which they were searching. In our follow-up survey, 30% of borrowers chose a particular lender because they were quickly approved by that institution, while 20% did so because that was the only offer they received.[10]

Less important features that participants reported during the baseline survey when they were searching for a loan included whether the bank branch was nearby (the most important feature for 10% of participants) and whether it is a bank in which they already had an account (8%). In the follow-up survey, when choosing a loan the less important features included whether the loan payment could be automatically deducted from payroll (6%), whether they were a client of that bank (6%), trust in the institution (5%), and getting approved for a higher loan amount (4%).

We also asked participants what strategy they employed while searching for their loan to better understand the prevalence of sequential vs. simultaneous search (De Los Santos, Hortaçsu, and Wildenbeest, 2012) and of searching for approval (Agarwal, Grigsby, Hortaçsu, Matvos, Seru, and Yao, 2024). We find that both sequential and simultaneous search are common (Figure A.2): 60% of participants reported having a target interest rate (consistent with sequential search), while 42% said they planned to search at a target number of banks or until receiving a target number of offers (consistent with simultaneous search). Searching for approval was also common, with 69% of participants reporting that they planned to stop searching after they were approved by one institution. These survey questions were not mutually exclusive, and based on the responses it appears that loan seekers implement a combination of strategies.

2.4 Online Tools

Because our intervention is an online tool that provides information about the distribution of interest rates a borrower faces conditional on their characteristics and the characteristics of the loan they are looking for, we briefly describe other online tools available in the Chilean consumer credit market. We describe two types of tools: (i) tools provided by particular banks on their websites and (ii) third-party comparison platforms. We scraped data from as many of these websites as possible—conditional on the loan and borrower characteristics of our RCT participants—in order to quantify how accurate the information on these websites is compared to the loans participants actually received in the administrative data. We present results from this exercise in Section 5 and describe the details of our procedure in Appendix B. In short, neither bank websites nor third-party comparison websites provide accurate information.

Bank Websites Prospective borrowers can get interest rate quotes from bank websites, usually through online tools provided by the bank that are known in Chile as "simulators." Nearly all (93.2%) of our participants used at least one bank simulator while looking for a loan.

[10] Being quickly approved and only receiving one offer were the second- and third-most common reasons for choosing a particular lender. Reasons were not mutually exclusive, as participants could name more than one.

We identified twelve banks that have consumer loan simulators on their websites. The simulators ask for a range of inputs (Table A.1, panel A). The most common inputs requested by these tools are loan amount and maturity (requested by all banks). All but one bank request the user's national ID number, but we show in Appendix B that the interest rate numbers shown do not vary based on the ID number the user enters. Five out of twelve banks ask for the user's income, and none ask for the user's neighborhood, or *comuna*, despite this being an important predictor of interest rates used by banks in their algorithms.

Third-Party Comparison Websites There are two main third-party comparison websites for consumer loans, but only 12% of participants reported using such a tool when searching for a loan. One is provided by a private company and the other is run by a different government agency. Table A.1, panel B, describes the inputs required by these two comparison websites. In both of these tools, consumers input their desired loan size and maturity and receive quotes for loans from different institutions. However, neither asks for any borrower characteristics, and thus the interest rate quotes they provide are not conditional on borrower characteristics.

3 Experimental Design

3.1 Participant Recruitment

Figure 1 shows the design of the RCT and the funnel of participant recruitment. We recruited 112,063 participants to the RCT from November 2021 to June 2023. We targeted Google ads from the CMF to people who searched for keywords related to consumer loans in Chile. Our Google ads campaign included 4,107,376 ads served from November 2021 to June 2023, and 18.5% of people searching for keywords related to consumer loans in Chile were served our ad. Figure A.4 shows an example of one of the Google ads included in our campaign. Those who clicked on the ads were taken to a landing page with a description of our study and informed consent to participate. The following page asked for their national ID number—which is commonly given out in Chile (e.g., for rewards programs at the grocery store)—and their contact information including email address and phone number. We then conducted a baseline survey prior to showing the price comparison tool, simple tool, or a control video to the participant. Immediately after seeing the treatment, the participant was asked additional survey questions.

The ads we served were clicked 612,945 times, i.e., 14.9% of ads were clicked. From these clicks, 112,063 (18%) consented to participate and continued taking the baseline survey long enough to randomly be asked or not asked the questions on their expectations about the distribution of interest rates banks would offer them and how much they would search; this is our sample for measuring the effects of eliciting beliefs. Many consumers abandoned the survey during this module: 46,051 consumer loan seekers (41.1% of those who consented) continued taking the baseline survey long enough to reach the module where we randomized whether they saw the price comparison tool, simple tool, or control video; this is our sample for measuring the impact of the price comparison and simple tools.[11]

3.2 Elicit Beliefs Treatment

After obtaining their national ID number and contact information, participants completed modules on sociodemographic characteristics and other financial products that they currently have or loans they had in the past. We then randomly assigned 75% of participants to be asked questions about their expectations of (i) the lowest interest rate a bank could offer them, (ii) the highest interest rate a bank could offer them, (iii) the fraction of offers that would have an interest rate above the midpoint between the lowest and highest rates, (iv) the rate they expected the first bank where they searched to offer, (v) the rate they expect the second bank where they searched to offer, and (vi) the rate they expected to get on the loan they ultimately took out. The first three of these are borrowed from the macroeconomic expectations literature (Coibion, Georgarakos, Gorodnichenko, Kenny, and Weber, 2024). In addition, we asked them at how many banks they would search, at which bank they would search first, and at which bank they would search second. We did not ask any of these expectations questions to a randomly selected 25% of the sample in order to test whether these survey questions have a treatment effect, motivated by evidence from Zwane, Zinman, Van Dusen, Pariente, Null, Miguel, Kremer, Karlan, Hornbeck, Giné, Duflo, Devoto, Crepon, and Banerjee (2011) and Stango and Zinman (2014), and indeed we find that asking these questions led people to search more and obtain loans with lower interest rates. After viewing either the price comparison tool, simple tool, or a control video, we again asked the 75% of participants assigned to the elicit beliefs treatment the same interest rate expectation and search questions to test whether their expectations were affected by treatment.

[11] Despite the smaller sample size of 46,051 for measuring the effects of the price comparison tool and simple tool, compared to the sample size of 112,063 for measuring the effect of the elicit beliefs treatment, the research design is internally valid. We do not randomize participants into one of the price comparison tool, simple tool, or control arms until they reach that module of the online survey. As a result, we can simply remove those who do not make it to the tool treatment module from the sample for estimating the effect of the tools, and still have balance across these treatment arms (both in theory and, as we show, in practice). Because we use a cross-randomized design, the results on the effects of the tools are a weighted average of the effect for the subsample who also received the elicit beliefs treatment and the subsample who did not receive the elicit beliefs treatment, relative to a control group in which the same proportions did and did not receive the elicit beliefs treatment; thus, if there is an interaction effect between the tool treatments and the elicit beliefs treatment, it would be reflected in the effect of the tool treatments that we estimate (Muralidharan, Romero, and Wüthrich, 2023).

3.3 Price Comparison Tool and Simple Tool Treatments

Price Comparison Tool Our price comparison tool (Figure 2a) showed participants a conditional distribution of interest rates that similar borrowers had received for similar loans over the past six months. We built the tool using administrative data on loan characteristics merged with borrower characteristics for the universe of consumer loans in Chile, i.e., data from over 1.8 million loans to 1.2 million borrowers over two years. Appendix C provides more details on how the histograms were created given the available data. We refreshed the data every month to show the previous six months of interest rate data, based on tests we conducted to determine the optimal time period of data to show (where we traded off showing accurate information vs. having sufficient data points underlying the histogram shown to each participant). Appendix C.1.1 provides more detail on this trade-off and describes the rationale behind why we showed participants data on loans from the last six months.

Walking through each component of the price comparison tool shown in Figure 2a, the borrower and loan characteristics that the participant already answered in the baseline survey were loaded automatically in the top panel of the tool ("1. Verify that your data are correct"), but these values could be modified by the user. The second panel of the tool ("2. Look at the information") showed the user the distribution of interest rates that similar borrowers had obtained for similar loans in the past six months. We conducted focus groups to test a prototype of the tool. Based on the findings from these focus groups, in order to make the histogram understandable to consumers that may not be familiar with interpreting data from graphs and histograms, participants could hover over the histogram's bars to see a tool-tip that explained what that bar indicated. Specifically, the tool-tip told the participant the number of loans that had that interest rate, gave a cumulative distribution function interpretation of the bar (what percent of loans had an interest rate at or below that rate), and converted the interest rate to a monthly and total loan cost in Chilean pesos based on the loan amount and maturity entered by the participant. In addition, we created a tutorial video that the user could watch to better understand how to use the tool.

In the third panel of the comparison tool ("3. Compare the impact of different interest rates for your wallet"), we compared two interest rates in the histogram to show the participant the implications of these different rates for their monthly and total loan costs (in Chilean pesos). The inclusion of this part of the price comparison tool was inspired by research that it is important to translate differences in APRs into dollar costs (Bertrand and Morse, 2011), that borrowers target monthly payments rather than interest rates (Argyle, Nadauld, and Palmer, 2020), and more broadly that market participants are more perceptive to dollars rather than percentages (Shue and Townsend, 2021). By default, the highest and lowest interest rates were compared, but the user could drag the two triangle markers on the x-axis of the histogram in order to change which interest rates were compared. Alternatively, participants could manually enter interest rate values to see how they would translate into costs. Participants experimenting with this feature should see the concrete consequences of the market's price dispersion, i.e., how they may pay substantially different costs for their loan depending on the interest rate they obtain.

Simple Tool on Benefits of Search Participants in the simple tool treatment arm viewed a simpler tool that provided the user with just two numbers on the estimated benefits of search (Figure 2b). This treatment was designed to be simpler and avoid the information overload that might be present in the price comparison tool. The borrower and loan characteristics that the participant already answered in the baseline survey were again loaded automatically in the top panel of the tool ("1. Verify that your data are correct"). The bottom panel ("2. Look at the information") told the following to the borrower: "Using real data from loans granted to people similar to you, we estimate that shopping at 1 additional bank would lower your monthly payment by $X and the total cost of your loan by $Y, on average." The number of additional banks could be modified using a drop-down menu, which the participant could use to determine the expected benefits of searching at up to five additional banks (i.e., of searching at up to six total banks relative to searching at just one bank).

To estimate the amount they could save in Chilean pesos, we used the conditional distribution corresponding to that participant's characteristics and the characteristics of the loan they were searching for, and simulated consumer searches across 2–6 banks. We then averaged across these simulated searches to calculate how much the participant could expect to save on average. The "More details" link provided the participant with a description of how we calculated the expected savings. Appendix C.2 provides more detail on the calculation of search benefits.

Control Video The control video was a 1 minute and 35 second long animated video created by the CMF describing key credit terms. The video was designed to provide information related to loans that would *not* be useful for search. The video defined what a lender and debtor are, what a loan contract is and what is included in it, and key loan terms like maturity and principal. Figure A.5 shows a screenshot from our control video. In all treatment arms including the control video, the participants were required to stay on the treatment module page for one minute prior to clicking "Next" to proceed to the following module.

4 Data

4.1 Administrative Loan Data

We use administrative data on the universe of consumer loans from 2015–2024 from the Chilean financial regulator, the CMF. We observe the following borrower characteristics that banks use to determine whether to offer loans: age, marital status, gender, income, and neighborhood of residence.[12] Importantly, credit bureaus in Chile do not report continuous credit scores, but rather report binary flags if the borrower has defaulted on prior loans; thus, the interest rates that banks offer are not conditional on continuous credit scores. As for loan characteristics, we see each loan's amount, interest rate, and maturity, as well an anonymized code for the lender. We are also able to follow repayment of the loan in monthly intervals after its issuance to evaluate outcomes such as delinquency and default. We use these data in our construction of the conditional distribution of interest rates for both the price comparison tool and the simple tool on the benefits of search.

By obtaining participants' national ID number, we are able to merge their treatment status and survey responses with future administrative data to measure treatment effects on the eventual loans they obtain. In total, 21,522 out of 112,063 participants from our RCT took out a consumer loan between the time they participated in our RCT and one year later. Of these, 8,988 participants among the sample size of 46,051 participants for measuring the effect of the price comparison tool and simple tool took out a consumer loan within one year of participating.

We also use the administrative data to compare participants in our RCT who took out consumer loans to the universe of consumer loan borrowers in Chile. Figure A.7 shows that borrowers in our RCT are—unsurprisingly—not perfectly representative of the overall population of borrowers in Chile; nevertheless, there is a large amount of overlap in the distributions of characteristics of borrowers in our RCT and the overall population of borrowers in Chile.[13] The two groups are relatively similar on gender (all borrowers: 38% women vs. RCT sample: 37.4%), the percentage who live in the capital Metropolitan Region (all borrowers: 51.5% vs. RCT sample: 50.4%). First-time borrowers (defined as those who did not have a previous consumer loan in the administrative data prior to the RCT) make up 37.46% of the RCT sample compared to 41.58% of the overall population of borrowers. Borrowers in our RCT are relatively better-off than the overall population of borrowers—as the distribution of annual income for RCT borrowers is shifted right of that of all borrowers—though there is extensive overlap in the support of the distributions. The variable with the starkest differences between the RCT sample and the overall population of borrowers is

[12]Note that if applicants already have other products at the bank where they are applying for a loan, the bank might also use that information in its lending decision, and we do not observe these bank-specific data.

[13]We exclude borrowers who participated in our RCT from the "all borrowers" group (i.e., overall population of borrowers in Chile) in order to compare two mutually exclusive groups.

age, where participants in our RCT are younger than the general population of borrowers (median age of all borrowers: 38 vs. RCT sample: 34). These differences are unsurprising considering the online nature of our recruitment process.

The distributions of loan terms (interest rate, loan amount, and maturity) obtained by our RCT participants and all borrowers in Chile also exhibit differences but have a large degree of overlap (Figure A.8). In general, borrowers in our RCT obtain slightly larger, longer-maturity, lower-interest rate loans. For example, the average loan maturity in our sample is 37 months as compared to 34 months in the overall population.

4.2 Baseline Survey

The baseline survey was conducted online after participants who searched for keywords related to loans clicked on a Google ad from the financial regulator and consented to participate in the study. In addition to the questions about beliefs for those assigned to the elicit beliefs treatment (described in Section 3.2), we asked participants about their sociodemographic characteristics and detailed questions regarding their existing banking relationships and other financial products they have.

We also asked participants questions to determine how they form beliefs about interest rates. Specifically, we asked them if they had ever obtained a quote for a consumer loan from a bank website, if they had seen an ad for a consumer loan advertising an interest rate, or if someone they know had told them what interest rate they got for a consumer loan. If they answered yes to any of these questions, we asked them how long ago this was and what interest rates were given by the bank website, advertisement, or person they know. We then asked whether they had searched for a loan before, and if so how long ago it was, how many offers they had received, and the range of interest rates of those offers. Finally, we asked questions on financial literacy, behavioral biases (e.g., financial procrastination), and a set of simple questions used to measure cognitive ability, which are all related to search and the formation of beliefs (D'Acunto, Hoang, Paloviita, and Weber, 2023).

Table 1 reports means for characteristics from the baseline survey, and tests for balance between the 75% of participants who were randomly assigned to the elicit beliefs treatment—i.e. were asked questions on their expectations about the distribution of interest rates and how much they would search—vs. the 25% of participants who were not asked these questions (denoted "control" in the table, although this is different than the control group in the assignment to the price comparison tool, simple tool, or control). Participants in our sample are roughly 36 years old on average with an average monthly income of 1,125,959 pesos (1,142 USD at market exchange rates). Participants have a wide range of education: 3.7% did not complete high school, 36% completed only high school, 21.3% completed a 2-year post-secondary program (equivalent to an associate's degree), and 39% completed a 5-year degree program or higher (equivalent to a bachelor's degree). As for financial experience, 67.8% of our participants had a bank account, and 70.2% had taken out a loan.

As expected due to randomization, our sample of 112,063 participants who were randomized to either receive or not receive the elicit beliefs questions is balanced: the p-value of the omnibus F-test regressing the elicit beliefs dummy on all baseline survey characteristics is 0.463 (Table 1). Furthermore, only one variable—the probability of having a loan already at baseline—is not balanced, as could be expected by chance: those assigned to the elicit beliefs treatment are 0.6 pp less likely to have a prior loan (significant at the 5% level).

Table 2 tests for balance across the price comparison tool, simple tool, and control arms.[14] The sample size in this table, 46,051 participants, is smaller than that of Table 1 because of participant attrition between the module in which we randomized whether we elicited beliefs and the module in which we randomized assignment to one of the tool or control arms. We again find that the sample is balanced across treatment arms. The p-value for our omnibus F-test of whether characteristics jointly predict the price comparison tool treatment is 0.279, and that for the simple tool treatment is 0.207. The only variable that has a statistically significant difference between the treatment arms and the control arm is having a bank account: participants are 1.6 and 1.3 pp more likely to have a bank account in the price comparison tool and simple tool arms compared to the control group (statistically significant at the 5% level).

4.3 Endline Phone Survey

We surveyed participants via phone at least six months after they participated in the RCT. We attempted to contact 42,250 participants (38% of our 112,063 sample), and ultimately collected 6,441 completed surveys, for a 15.5% response rate. Table A.2 shows that response rates are balanced across both the elicit beliefs and tool treatments.

The primary objective of the endline phone survey was to collect rich data on participants' search histories. Search data are poorly captured in most administrative data sets: in many administrative data sets including the CMF data, only originated loans are recorded. Even if all applications were recorded, true search behavior also involves informal quote requests, or even inquiring about the probability of approval at a particular lender (Agarwal, Grigsby, Hortaçsu, Matvos, Seru, and Yao, 2024). For each bank at which consumers searched for information, we

[14]The loan characteristics variables included in Table 2 are not included in Table 1 because they are asked in the same module as the elicit beliefs treatment, and the elicit beliefs treatment caused some participants to stop participating in the survey. (Note that participants who abandoned the survey during the elicit beliefs module are still tracked in the administrative data and included in the sample to estimate the effect of eliciting beliefs.)

ask detailed questions about how they searched (e.g., using the bank's website or mobile banking app, going to a branch in person, emailing, calling by phone), whether they informally received any information about their probability of acceptance or an estimate of the interest rate they would receive, whether they formally applied, whether they were accepted or rejected, what loan terms they were formally offered if accepted, whether they negotiated the offer, and the loan terms they were offered after this negotiation. We also include questions to understand the mechanism behind the potential effect on search, as well as other measures of financial well-being, such as total debt, total savings, and ability to cope with shocks.

5 Results

5.1 Participants Underestimate Rate They Will Obtain and Dispersion

Comparing participants' expectations about the distribution of interest rates with administrative data, we find that prior to viewing the tool, most users thought interest rates were lower than they actually were, and also underestimated price dispersion.

Figure 3 compares the interest rates that participants report expecting to receive on the loan they take out to the interest rates we observe in administrative data for the loan they actually obtained subsequently (restricting to those who did take out a loan after participating in the RCT). It shows that participants have inaccurate beliefs on the interest rate that they will ultimately receive on their loan: 73.2% of borrowers think they will receive an interest rate *lower* than the rate they later receive. Conditional on underestimating, these borrowers think they will get a loan that is 14.9 pp lower than the rate they ultimately receive; the average bias including those who are accurate or overestimate is –6.9 pp.

Figure 4 shows the distribution of the difference between a participant's beliefs on dispersion—measured as the highest rate they think a bank would offer them minus the lowest rate they think a bank would offer them—and the difference in the highest and lowest rates we observe in the administrative data conditional on that participant's characteristics and the characteristics of the loan they are looking for. We use administrative data for similar borrowers and loans over the past six months—i.e., the same data we would show the borrower if assigned to the price comparison tool arm. Unlike in the case of beliefs about the loan they will obtain, for dispersion we cannot compare to offers they actually received, as these would only be a subset of draws from the full distribution of interest rates if they were to search across all banks. We find that the majority of participants (75%) *underestimate* price dispersion, but also that there is a long right tail of participants who substantially *overestimate* dispersion.

Why are beliefs biased? We first test whether biased beliefs about the first moment of the distribution may be due to changes in interest rates over time. We find that even though interest rates were indeed changing over time during the 19 months of our RCT, changes in interest rates do not explain biased beliefs. We then explore whether other sources of information that people have access to lead them to have biased beliefs. We find that various other sources of information indeed provide downward-biased estimates of interest rates, which may explain the bias we observe.

On average, interest rates increased over the 19 months that we conducted the experiment, with the median monthly interest rate among the universe of loans in administrative data ranging from 19.6% to 26.7% over this time period. If consumers are slow to update their beliefs, this could explain why they underestimate the first moment of the interest rate distribution. To test whether consumers are slow to update, we regress beliefs about the rate a consumer will obtain on the monthly median interest rate during the month in which that consumer participated in the RCT. We find that when median consumer loan interest rates are 1 pp higher, beliefs about the rate people expect to obtain are 1.3 pp higher (Table A.3), indicating that people do update as interest rates change over time and that underestimates of the rate people will obtain are not caused by people being slow to update.

Alternatively, consumers might have accurate beliefs about the first moment of the distribution but take a while to obtain a loan. Since interest rates were increasing on average over the course of our experiment, we may then misattribute the difference between their belief and the rate they obtained to a bias about current interest rates, when it is instead due to underestimating future changes in interest rates and/or the amount of time before they would obtain a loan. We show, however, that even when we restrict the sample to the first quartile of the number of days between participating in the RCT and obtaining a loan—those who obtained a loan within 21 days of participating—the bias in the first moment looks very similar, with 74.9% underestimating rates (Figure A.9). Furthermore, while the median interest rates are rising over the course of our sample, consumers' interest rates expectations are persistently lower than the level of rates in the market (Figure A.10), though they do reflect the upward trend in median rates, as confirmed in Table A.3. We conclude that consumers' underestimates of the first moment of the interest rate distribution cannot be explained by changes to interest rates over time.

Participants may have also accurately estimated dispersion *in the offers they expect to receive*, but we provide information on the loans that were actually obtained. For example, one borrower may have interest rate offers of 20 pp and 22 pp, while another borrower with observationally equivalent characteristics, but unobservable to the econometrician higher default risk might have offers of 30 pp and 32 pp. The dispersion of the within-borrowers offers would then be much smaller than the dispersion of the observational interest rates taken by both the borrowers that would be characterized as being in the same histogram. Thus, our measure of dispersion may overestimate the more-accurate within-borrower elicited measure of dispersion.

To test this hypothesis, we compare the dispersion of interest rate offers within borrowers to simulated interest offers based on the admin data.[15] The latter of these numbers should have a mechanically higher dispersion because it is a measure that includes both unobservable borrower characteristics *and* variation across borrowers. If the administrative data is a poor proxy for dispersion that individual borrowers could face, then our simulated dispersion measure should be much larger than the observed dispersion in the rates offered to actual borrowers. The median of the difference between the simulated and observed interest rate offers is 0.01 pp, suggesting that dispersion in the administrative rates accurately captures the dispersion participants could have expected to receive in their own offered interest rates.

What predicts biased beliefs? We estimate a series of regularized regression models using elastic net (Friedman, Hastie, and Tibshirani, 2010) in Appendix E. Table E.1 presents the coefficient estimates from selected elastic net models that render the minimum mean-squared error (MSE). Individuals with more-biased beliefs tend to be younger and have lower incomes, less education, and less experience with financial products (i.e., bank accounts and loans). Furthermore, those looking for a smaller loan amount also tend to have more-biased beliefs.

We next assess what sources of information people use to form their beliefs, and whether these sources provide accurate information. In our survey data, 41% of participants report having seen advertisements by banks, 44% used bank websites, 12% used third-party comparison websites, and 23% asked friends and family about interest rates. To assess whether bank advertisements, bank websites, comparison websites, or family and friends might cause people to have biased beliefs, we compare rates our participants *would* have seen in each of these contexts with the rate they ultimately received in our administrative data (Figure 5).

For advertisements, we cannot observe all bank advertisements but can observe those that banks place on Google. We randomly sample combinations of search terms that led people to the Google ads for our experiment and neighborhoods of participants in our experiment. We then conduct Google searches using that keyword and geolocation pair, and scrape the resulting first page of Google results—including both Google ads and regular Google search results. More details are provided in Appendix D. The difference between interest rates that are shown in Google ads or search results and the rates people actually obtain are heavily negatively skewed with 74.2% of ads advertising rates that were lower than what participants ultimately received from the same bank in our administrative data (Figure 5a).

For bank websites and comparison websites, we use a script to input participants' characteris-

[15] Operationally, we take all the people who got more offer in the follow-up survey and reported their interest rates. Then we run 1,000 simulations on the conditional distribution that corresponds to each participant, drawing as many offers as they received in the actual data in each simulation. We then calculate the standard deviation across the offers in each of the 1,000 simulations. We then subtract the mechanically smaller standard deviation of the offers they actually received.

tics and the characteristics of the loan they are looking for into the interest rate simulator on each bank's website and each comparison website and scrape the resulting loan terms. More details are provided in Appendix B. Bank simulators tend to show inaccurate rates, as the difference between the rate a bank website showed and the rate a participant obtained can be as much as 27 pp lower or 20 pp higher than the rate they ultimately receive. While there is substantial noise in the quotes from bank websites, they are not biased in one direction or the other: 50% of participants would have been shown an interest rate that is lower than the rate they ultimately received (Figure 5b). As for third-party comparison websites, the difference between rates shown on comparison websites and the rates obtained is also negatively skewed, with 74.1% of quotes being lower than the rate the borrower ultimately received. These results suggest that banks have an incentive to provide attractive quotes to borrowers in a context where the borrower is still deciding which bank to apply for an offer from, but that they can subsequently bait-and-switch the customer and offer them a higher rate when providing a formal loan offer (Figure 5c).

Finally, only fifteen of our participants responded in the follow-up survey that they received information from friends and family, reported the interest rates that those friends and family told them, and also received a loan in the administrative data to compare. For this small sample of participants the difference in rates between what friends and family told them and what they ultimately received is also negatively skewed, with 68.8% being lower than the rate the borrower actually received (Figure A.11).

5.2 Price Comparison Tool Leads to Large Updates in Beliefs

After seeing the price comparison tool, did participants revise their expectations about the distribution of interest rates? To test this, we estimate the following specification:

$$Posterior_i - Prior_i = \beta_1 \mathbb{1}(\text{Simple Tool})_i + \beta_2 \mathbb{1}(\text{Price Comparison Tool})_i + \lambda_{b(i)} + \varepsilon_i, \quad (1)$$

where $Prior_i$ is the interest rate expectation participant i reported prior to seeing the tool or control video and $Posterior_i$ is the interest rate expectation they reported after seeing it. In our main specification, interest rates are annualized and measured in levels (e.g., an expected interest rate of 18% per year would be coded as 18). The treatment dummies $\mathbb{1}(\text{Simple Tool})_i$ and $\mathbb{1}(\text{Price Comparison Tool})_i$ equal one if the participant was assigned to that treatment arm and zero otherwise, and $\lambda_{b(i)}$ are bin density fixed effects. The bin density fixed effects are deciles of the number of observations in the tool that were shown or would have been shown to the participant, to control for the fact that people in higher-population neighborhoods or with more borrowers with similar characteristics would see more observations in the price comparison tool and might infer that there is more dispersion than those seeing fewer observations.[16]

Table 3 shows the results. On average, the price comparison tool caused participants to increase their beliefs about the rate they would obtain by 16 pp, or 54.9% relative to the control mean posterior of 29.2%.[17] Comparing posteriors to priors, treated participants' expectations about the entire distribution shift rightward. They update their expectation about the lowest interest rate a bank would offer them by 11 pp and their expectation about the highest interest rate a bank would offer them by 30 pp. Their expectations about dispersion also increase by 16 pp compared to a control mean posterior of 23.2 pp of dispersion, an increase of 68%. Tables A.4 and A.5 show that the same conclusions hold if we use levels of posteriors as the dependent variable, with or without controlling for priors on the right-hand side. Tables A.6 and A.7 show the same pattern when we log-transform beliefs about interest rates.

One concern is that the increased expectations about dispersion are due to scale effects around an increased first moment of the distribution, given that neither the standard deviation nor our preferred measure of dispersion are scale-invariant. To test whether the effects on treatment on expectations about dispersion are driven entirely by a scale effect, we create a normalized measure of dispersion where we divide the highest minus lowest rate that a bank would offer by the midpoint between the highest and lowest rate, which is a scale-invariant measure. Table A.8 estimates the results of the treatment on this normalized measure of dispersion. Even with this scale-invariant measure of dispersion, we find that our comparison tool treatment increases expectations about dispersion (statistically significant at the 1% level). We conclude that the effect of treatment on participants' expectations about dispersion is not just a scaling effect from increasing their expectations of the first moment of the distribution.

Appendix F.1 tests for heterogeneous treatment effects on interest rate beliefs of the price comparison tool using the machine-learning methodology proposed by Chernozhukov, Demirer, Duflo, and Fernández-Val (2025). We reject the null hypothesis of no heterogeneity in treatment effects of the tool for belief updating about the rate participants expect to obtain and the highest rate a bank would offer them, but we fail to reject the null hypothesis of no heterogeneity for the lowest rate a bank would offer them and dispersion (Table F.1). We find that the same characteristics that predict more-biased beliefs also predict a larger treatment effect of the tool on beliefs about both the expected rate and the highest rate. In particular, the treatment effect of the tool is larger for participants who are younger and have lower incomes, less education, and less experience with financial

[16]For those in the simple tool and control groups, the bin density fixed effect is based on how many observations are in the price comparison tool histogram that *would have been shown* to the participant had they been assigned to the price comparison tool arm.

[17]Although we winsorize responses to these interest rate questions at the 95th percentile, the distributions are nevertheless quite skewed and the medians are substantially lower than the means. The control median posterior of the annual rate people expect to get is 18%.

products (i.e., bank accounts and loans), as well as those looking for smaller loans (Table F.3).

In contrast, the simple tool quantifying the benefits of search but providing no direct information on the distribution of interest rates hardly affected priors about interest rates: the coefficient of the effect of the simple tool on expectations about the interest rate the participant will obtain is less than 1 pp, while the coefficient on dispersion is very close to 0 at 0.01 pp, and neither is statistically significant (Table 3).

The predicted effects on search of these empirical findings—of the tool leading consumers to update beliefs about the first and second moments of the interest rate distribution—are ambiguous. In a model without negotiation, since participants increased their estimates about the first moment, this could lead them to search less. In the absence of the price comparison tool, after receiving a draw these participants would think it is a bad offer and continue searching, whereas after seeing the price comparison tool they would know it is a reasonable offer and stop searching. However, since participants also increased their estimates about the second moment, this could lead them to search more because their estimates of the benefits of search have increased.[18]

In our model with negotiation, there are three key insights in terms of the effects of updating biased beliefs on search and negotiation. First, negotiation breaks the sufficient statistic relationship between reservation rates and search, so the relationships between beliefs and search described above no longer necessarily hold. Second, the tool should lead those who underestimate dispersion to negotiate more since it led them to update their beliefs about dispersion. Third, the effect of the tool on those with biased beliefs about the first moment is non-monotonic: it should have no effect on negotiation for those who overestimate or vastly underestimate the first moment, but should increase negotiation for those who somewhat underestimate the first moment.

5.3 Effects of Tool on Search, Negotiation, and Loan Terms

Table 4 shows the effect of our treatments on search behavior and loan outcomes. We run the following regression:

$$y_i = \alpha + \beta_1 \mathbb{1}(\text{Simple Tool})_i + \beta_2 \mathbb{1}(\text{Price Comparison Tool})_i + \varepsilon_i. \tag{2}$$

where y_i are search, negotiation, and loan term outcomes for participant i. The treatment dummies $\mathbb{1}(\text{Simple Tool})_i$ and $\mathbb{1}(\text{Price Comparison Tool})_i$ equal one if the participant was assigned to that treatment arm and zero otherwise. Neither the price comparison tool nor the simple tool led people to search at more institutions or to formally apply for loans at more institutions. Furthermore, Figure A.12 plots cumulative distribution functions (CDFs) of the number of institutions searched by

[18]Merely learning the distribution could also lead participants to search less, as they may have been searching to learn about the distribution in the absence of the tool (De Los Santos, Hortaçsu, and Wildenbeest, 2017).

treatment arm and shows that the null average treatment effect on search is not masking offsetting effects in different parts of the distribution.

We find that despite not searching or applying at more institutions, the tool makes consumers 3.7 pp (39%) more likely to negotiate and to receive 0.069 (13.1%) more offers in the survey data. In a subsequent WhatsApp survey we conducted to further understand the negotiation mechanism, we find that much of this negotiation happens prior to the bank issuing a formal offer, which explains the effect on the number of formal offers received. Participants also receive 11.9% lower interest rate offers, are 3.6 pp (9.7%) more likely to take out a loan, and have 10.5% lower interest rates on the loans they take out according to the survey data.

In administrative data, the tool makes borrowers are 1 pp (5%) more likely to take out a loan, which is a slightly smaller treatment effect compared to the survey data. According to our survey data, borrowers who did not take out a loan overwhelmingly did not make the purchase or investment for which the loan was earmarked.

We do not find a treatment effect on interest rates in administrative data. The discrepancy between the effect on interest rates in survey and administrative data appears to be due to the following. In administrative data, the tool does lead to a reduction in interest rates for loans obtained very shortly after participating in the RCT (Figure 6). During this time period, there is no effect on the probability of taking out a loan. Over time, the tool increases the probability that borrowers who would not have obtained a loan in the counterfactual to negotiate and reach a deal with the bank. However, if these are the less-creditworthy and higher-interest rate borrowers, this creates selection in the interest rate regression that pulls the treatment effect toward zero. This phenomenon can be seen clearly in Figure 6 as the treatment effect increases the probability of obtaining a loan over time and, as this happens, the negative treatment effect on interest rates attenuates towards zero. Finally, among the subset of those we attempted to survey who received a loan according to administrative data, those who obtained a loan within about seven days of participating were more likely to respond to the survey, while those who obtained a loan more than seven days after were less likely to respond (Figure A.13). This leads our estimated treatment effects in survey data to be more heavily weighted towards those who obtained a loan shortly after participating, whom the tool caused to obtain lower interest rates.

Appendix F.2 tests for heterogeneous treatment effects search, negotiation, and loan terms of the price comparison tool using the machine-learning methodology proposed by Chernozhukov, Demirer, Duflo, and Fernández-Val (2025). Across all of the tests on search and negotiation behavior and loan terms, we find no detectable heterogeneity in treatment effects (Table F.4). This suggests that the treatment effects of the tool on negotiation, the probability of taking out a loan, and interest rates do not differ by the characteristics of the participants, but rather tend to be spread evenly across participants.

The simple tool that showed only the benefits of search did not have effects on any search, negotiation, or loan outcomes (Table 4), which is not surprising given that the simple tool did not lead people to update their priors about the interest rate distribution.

Next, we turn to two key predictions from the model about heterogeneous effects of the tool on negotiation, based on biased beliefs about the first and second moments of the interest rate distribution that a consumer faces. The first prediction is that the tool will increase negotiation for consumers who underestimate the second moment of the distribution. We separately estimate specification (2) for those who underestimated the second moment and for all others in Table 5.[19] For those who underestimated dispersion, the tool led to a 7.8 pp (74.3%) increase in negotiation. It did not, however, lead to an increase in search, consistent with the insight from our model that introducing negotiation breaks the sufficient statistic relationship between reservation rates and search. For those who did not underestimate dispersion, on the other hand, there is no effect on negotiation, with a small and not statistically significant point estimate. We interact the terms in specification (2) with a dummy for "underestimated dispersion" to test the difference between the treatment effect on negotiation for those who underestimated dispersion and the null effect for all others, and find that the difference in estimated treatment effects is statistically significant.

The second prediction is that the effect of the tool on negotiation is non-monotonic in how biased the belief is about the first moment. In particular, there should be no effect of the tool for those who (prior to seeing the tool) overestimated the first moment of the distribution. There should also be no effect for those who vastly underestimate the distribution, as they will still underestimate it by enough after Bayesian updating that the bank will determine they likely cannot profitably lend to that customer based on their beliefs, and thus will not incur the cost of lowering their initial offer. In contrast, for those who somewhat underestimate rates, the tool should make them more likely to negotiate by bringing their belief into the range where the bank will determine they can profitably lend and thus will engage in negotiation. This is exactly what we find: those who overestimate and substantially underestimate rates (by more than 30 pp) do not negotiate more in response to the tool, while those who underestimate rates by up to 30 pp are more likely to negotiate (Figure A.15). Furthermore, the difference between the groups is statistically significant.

5.4 Eliciting Beliefs Leads to More Search and Lower Rates

Randomizing whether we elicited priors about the interest rate distribution and the number of institutions at which participants intended to search was motivated by evidence from Zwane, Zinman,

[19] We define underestimating dispersion as beliefs about dispersion prior to treatment being at least 1 pp lower than the observed dispersion in the administrative data. The breakdown of our participants' accuracy in beliefs about both rates and dispersion are presented in Figure A.14. The majority of participants from who we elicited interest rate beliefs underestimate both the levels of rates and dispersion in rates.

Van Dusen, Pariente, Null, Miguel, Kremer, Karlan, Hornbeck, Giné, Duflo, Devoto, Crepon, and Banerjee (2011) and Stango and Zinman (2014) that survey questions can have treatment effects on real-world behavior and outcomes. Table 6 shows the effect of eliciting participants' beliefs on search, negotiation, and loan outcomes. We run the following regression:

$$y_i = \alpha + \beta \mathbb{1}(\text{Elicit Beliefs})_i + \varepsilon_i. \tag{3}$$

where y_i are search, negotiation, and loan term outcomes for participant i. The treatment dummy $\mathbb{1}(\text{Elicit Beliefs})_i$ equal one if the participant was assigned to the elicit beliefs treatment and zero otherwise. We find that merely asking these questions led consumers to search at 0.13 more institutions, or a 4% increase compared to the control mean of 3.357. This increased search led borrowers to obtain 7.1% lower interest rate offers and 9.6% lower interest rates on the loans they took out compared to participants who were not asked these questions, according to survey data. The effect on interest rates is also statistically significant in administrative data (at the 5% level), but lower in magnitude, suggesting 1.2% lower interest rates.

6 Conclusion

We document that consumers have inaccurate beliefs about both the first and second moment of the distribution of interest rates. Almost 73% percent of participants thought they would obtain an interest rate lower than what they actually received and almost 75% percent underestimated the dispersion of interest rate offers they could get. The presence of inaccurate beliefs suggests that consumers are likely to have different search and negotiation behavior than that predicted by models where agents are assumed to perfectly know the distribution of rates from which they are drawing.

We designed a price comparison tool to correct inaccurate beliefs and test their effects on search behavior and loan outcomes using an RCT. The tool showed participants a histogram of interest rates that borrowers with similar characteristics obtained on similar loans in the last six months. We built the tool in collaboration with Chile's financial regulator using administrative data on 1.8 million loans from 1.2 million borrowers over two years. We recruited participants online through Google ads and both surveyed and treated them online. We measure outcomes in administrative data merged with RCT participants using their national ID numbers and a follow-up phone survey we conducted to collect rich data on their search behavior and loan outcomes.

We find that the price comparison tool caused participants to update their beliefs about the interest rate they would obtain upwards by 16 pp or 54.9%. It also caused them to update their beliefs about dispersion in the rates banks could offer them by 16 pp or 68%. The price comparison tool led participants to receive 13% more offers and 11% lower interest rates, by negotiating 39% more. It also made them 5% more likely to take out a loan.

We also cross-randomized whether we asked participants their beliefs about the distribution of interest rates and how much they would search. Merely eliciting beliefs led participants to search at 0.13 more institutions and receive 9.6% lower interest rate offers on average.

These findings show that, on the one hand, there are cost-effective ways to help people obtain lower interest rates without incurring additional search costs (by showing the price comparison tool, which can resolve their uncertainty about the distribution of interest rates and lead them to negotiate better). On the other hand, the price comparison tool requires substantial data that many regulators do not have. Our results also show that a less data intensive and more scalable intervention—merely asking questions about beliefs—also leads people to obtain lower interest rates. However, because eliciting beliefs does not resolve their uncertainty about the interest rate distribution, obtaining lower interest rates with this less data intensive and more scalable intervention does require people to search more (rather than to negotiate better without searching more) in order to obtain those lower rates.

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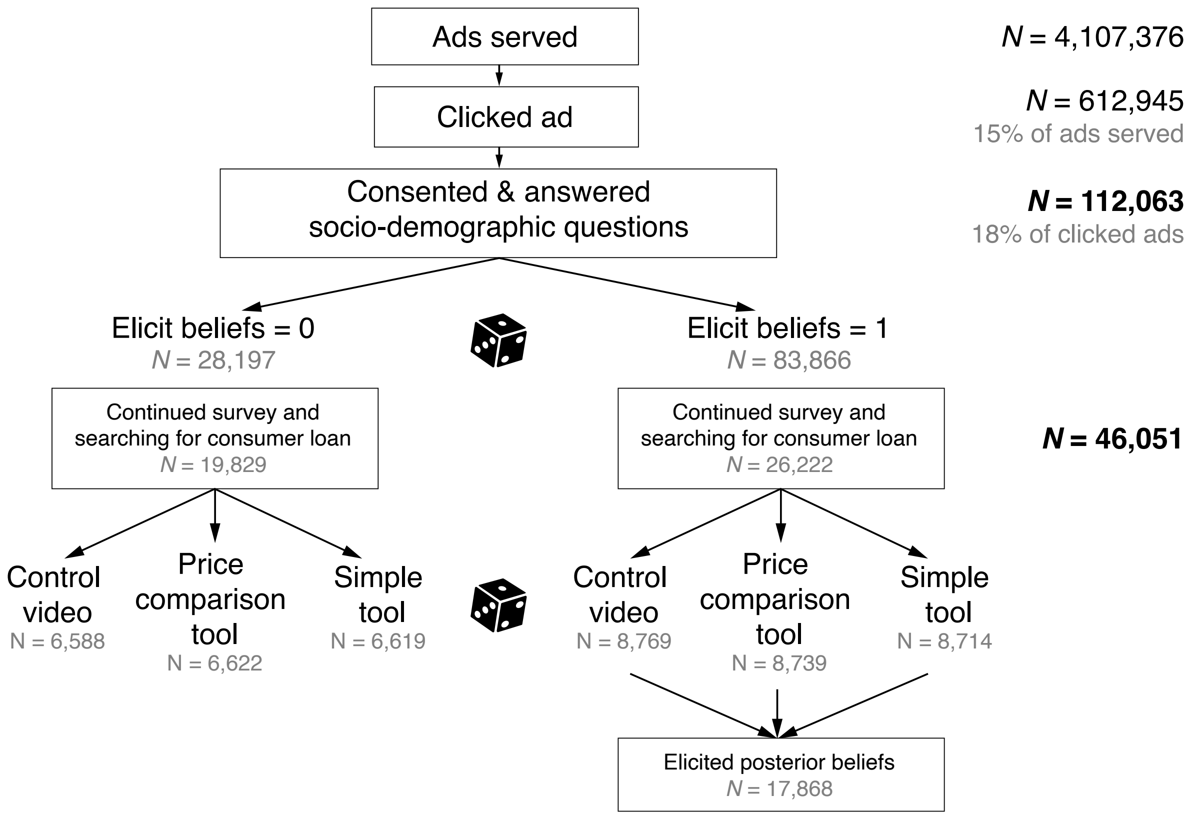
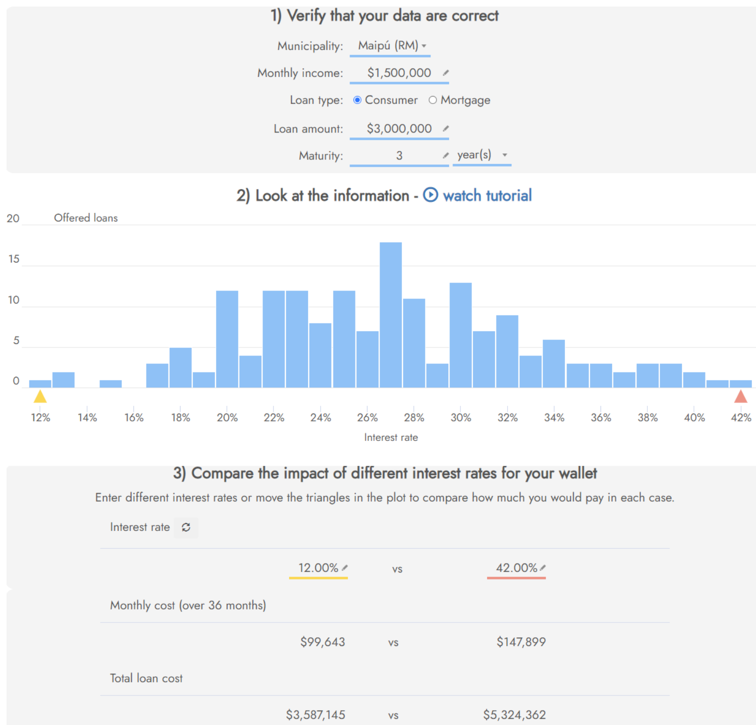
Figure 1: RCT Flowchart



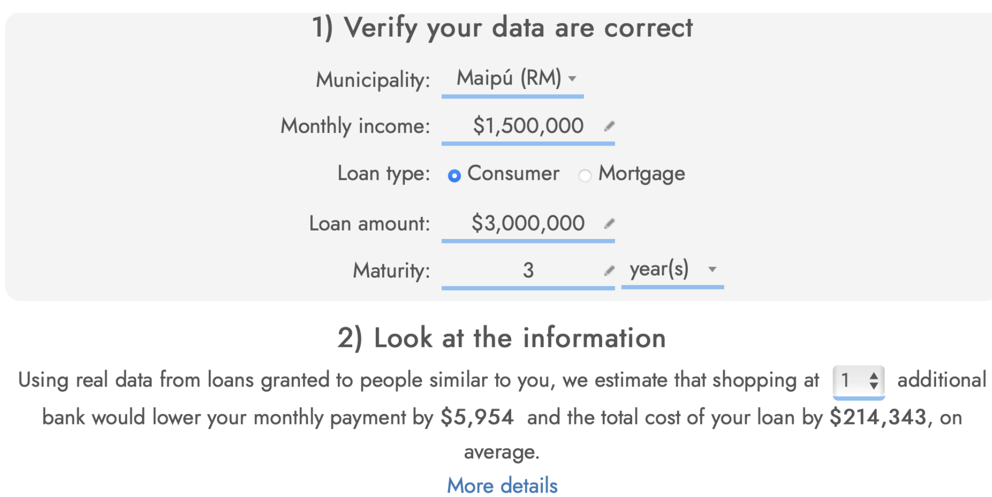
This figure shows the progression of the participants through our study after they reached our landing page from our Google advertisements. They are randomized at two key points: when they are assigned either "Elicit priors = 0" or "Elicit priors = 1" and subsequently when they are cross-randomized to one of our three treatment arms: the control video, price comparison tool, or simple tool.

Figure 2: Screenshot of Comparison Tool Treatments

(a) Interest Rate Price Comparison Tool

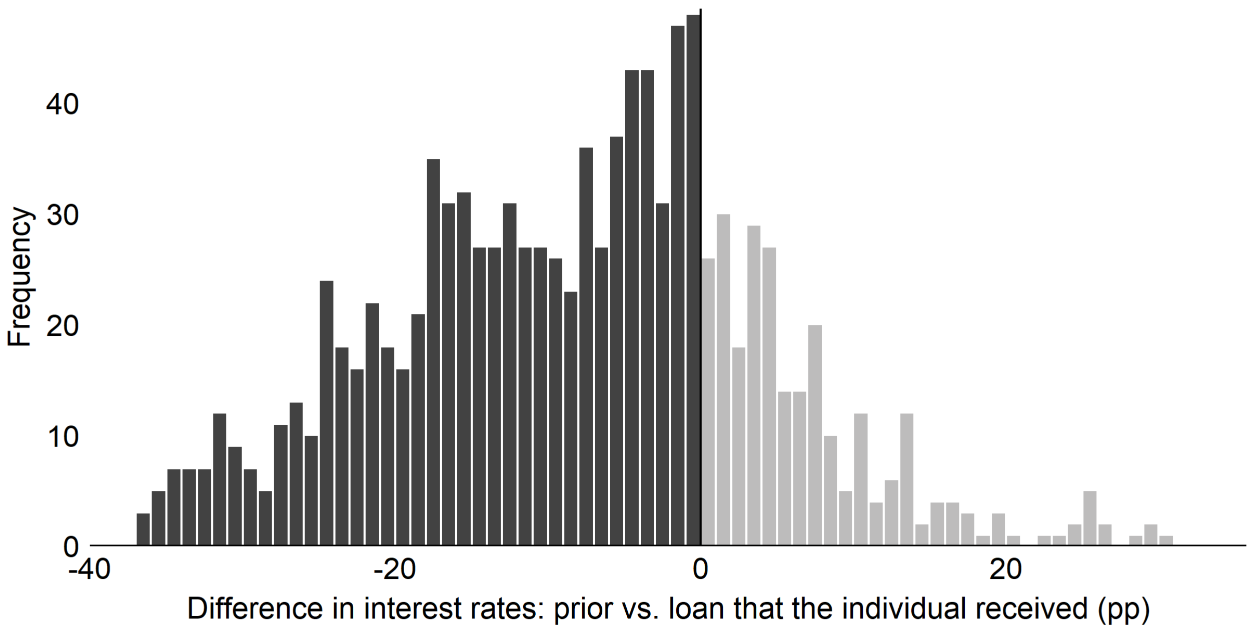


(b) Simple Tool



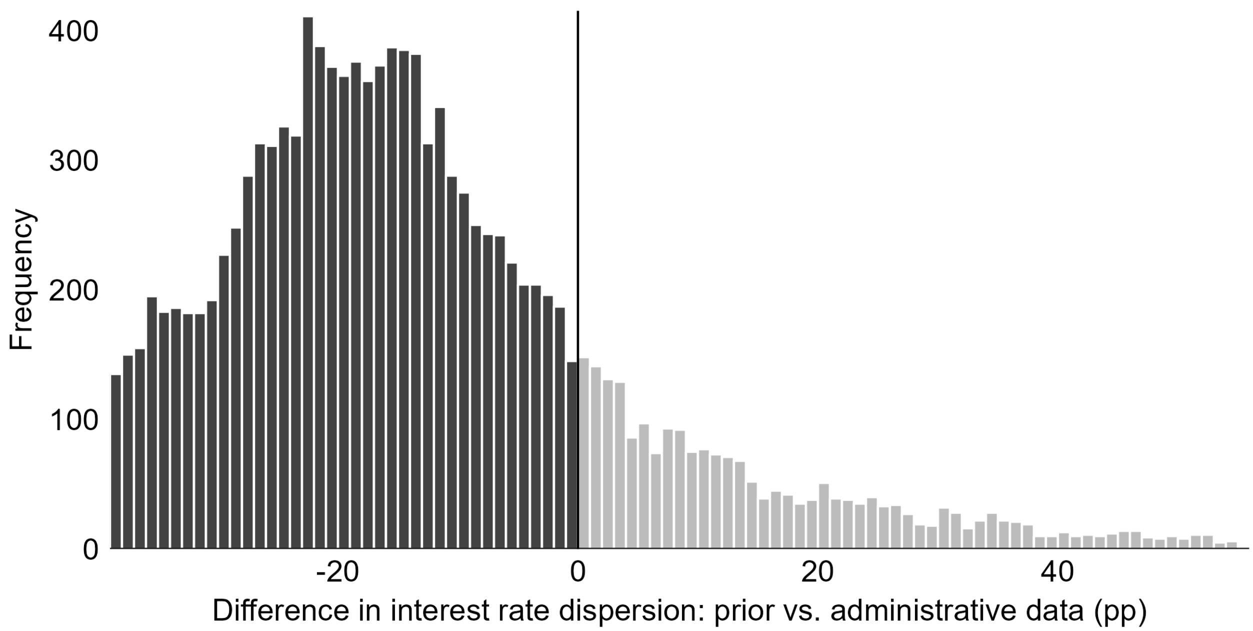
This figure shows a screenshot of English translations of our price comparison tool (panel a) and simple tool highlighting the benefits of search (panel b). For both tools, prospective borrowers already entered the borrower and loan characteristics in the top panel of the tool in our baseline survey; this information is automatically populated for them. Participants can also change this information, in which case the tool is automatically refreshed to show the corresponding data. For the price comparison tool, participants can hover over the histogram bars for more information that helps them interpret and understand the information in the histogram. Participants can also move the triangles along the x-axis to see the implications on monthly and total loan costs. For the simple tool, participants can select from a drop-down menu the number of additional banks they plan to search (up to six banks). The simple tool then displays the amount of money they could save on the monthly and total cost by searching at that many additional banks (more details in Appendix C.2).

Figure 3: Difference in Interest Rates Between Prior and Rate the Individual Received (pp)



This figure shows that participants tend to underestimate the interest rate they ultimately obtain. The figure is a histogram of the difference between a participant's prior expectations about the interest rate they will get on the loan they take out (as reported in our baseline survey) and the interest rate they ended up receiving on the loan they took out in our administrative data. We construct this figure by restricting to the subset of participants in the control group who took out a loan after participating and comparing the interest rate they obtained on the loan in the administrative data to the prior they had reported in the baseline survey. For participants who obtained more than one loan after participating, we restrict to the first loan they obtained after participating. We remove observations beyond the 5th and 95th percentiles in the graph for legibility. The number of observations is 1,198. The percentage of people who underestimated the rate they would receive, i.e., the percentage of the sample in the negative portion of the histogram, is 73.2%.

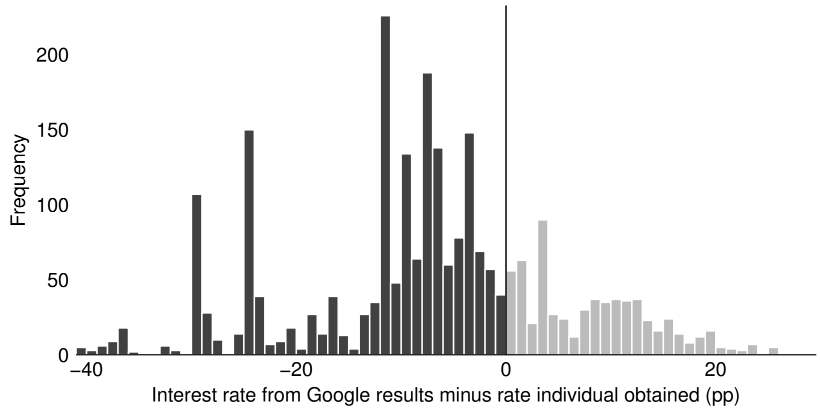
Figure 4: Difference in Interest Rate Dispersion Between Prior and Administrative Data (pp)



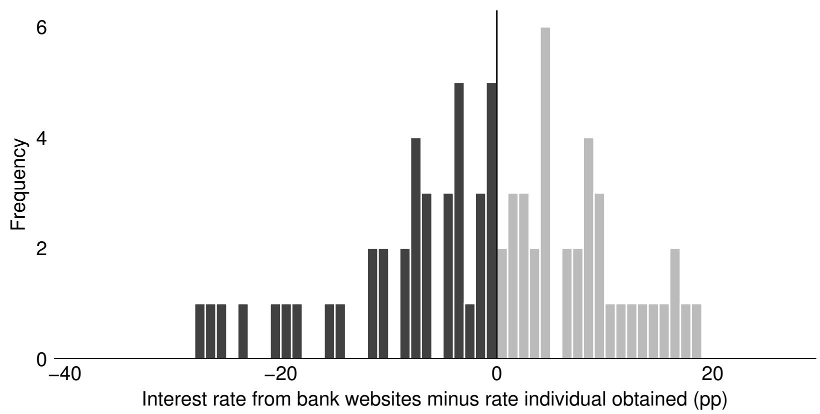
This figure shows that participants tend to underestimate dispersion. The figure is a histogram of the difference between a participant's prior expectations about the dispersion in interest rates that a bank could offer them, measured as the highest rate a bank could offer them minus the lowest rate a bank could offer them, compared to the dispersion we observe based on their characteristics in the administrative data (i.e., the dispersion they would have seen in the price comparison tool if assigned to that treatment arm). We remove observations beyond the 5th and 95th percentiles in the graph for legibility. The number of observations is 14,890. The percentage of people who underestimated the dispersion, i.e., the percentage of the sample in the negative portion of the histogram, is 75%.

Figure 5: Difference in Interest Rates Between Sources and Loan the Individual Obtained (pp)

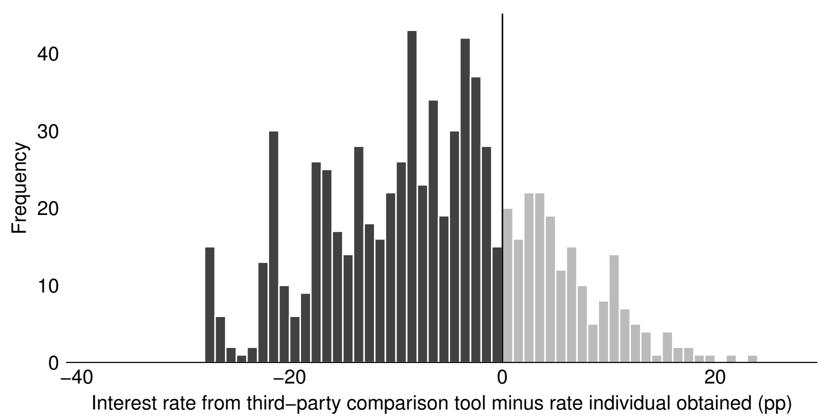
(a) Google Search Results



(b) Bank Websites



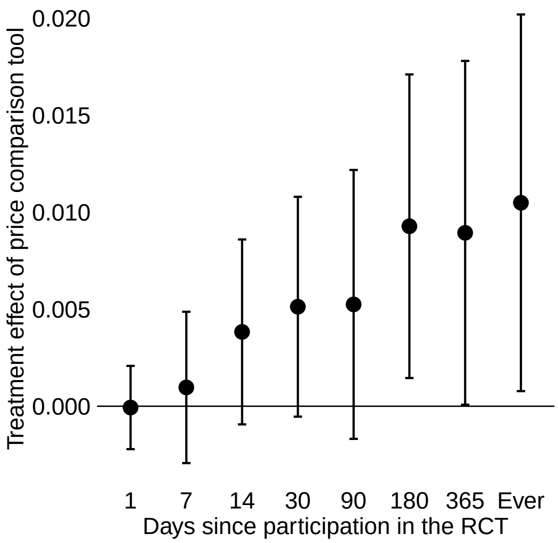
(c) Third-Party Comparison Tool



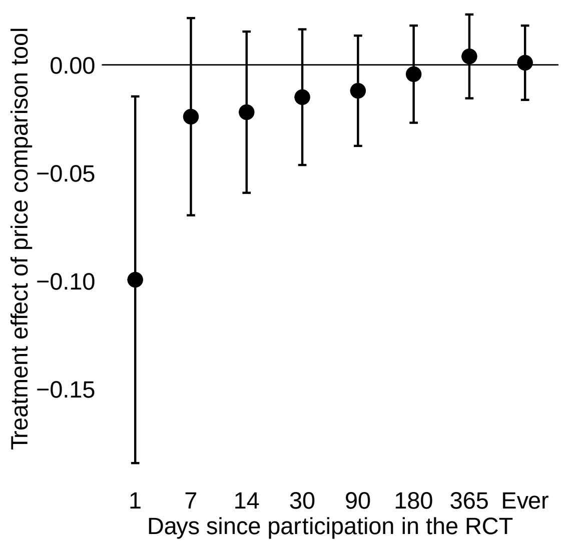
This figure shows histograms of differences between interest rates shown by various sources and the actual interest rate received by participants in the administrative data. Panel (a) shows a histogram of the difference between the interest rate a participant would have seen searching for loan keywords on Google from the bank where they obtained a loan and the rate they actually received from that bank in the administrative data. There are 2,493 observations, of which 74.2% are negative. Panel (b) shows a histogram of the difference between the interest rate a participant would have seen using the bank website of the bank where they obtained a loan and the rate they received from that bank in the administrative data. There are 76 observations, of which 50% are negative. Panel (c) shows a histogram of the difference between the interest rate a participant would have seen on the most popular third-party comparison tool for the bank where they obtained a loan and the rate they received from that bank in the administrative data. There are 749 observations, of which 74.1% are negative. We restrict the time period to loans that were obtained only during the two-month time period that we were running the scrapers, in order to ensure that differences are not due to changes in interest rates over time. See Appendices D and B for more detail.

Figure 6: Effect of Price Comparison Tool over Time (Administrative Data)

(a) Probability of taking a loan



(b) Log interest rate



This figure shows estimates from specification (2) over time relative to when consumers participate in the RCT. For each number of days since participation in the RCT, we measure treatment effects considering only loans obtained within that number of days after participation. RCT = randomized controlled trial.

Table 1: Balance of Pre-Treatment Characteristics by Elicit Beliefs Treatment

	Elicit Beliefs = 0 Mean	Elicit Beliefs	N
	(1)	(2)	(3)
Personal characteristics			
Age	35.939***	-0.106	112,063
	(0.059)	(0.068)	
log(Income)	13.625***	0.001	109,665
	(0.007)	(0.008)	
Incomplete high-school	0.037***	-0.001	108,809
	(0.001)	(0.001)	
Complete high-school	0.358***	0.003	108,809
	(0.003)	(0.003)	
Complete 2-year program	0.214***	-0.002	108,809
	(0.002)	(0.003)	
Complete 5-year program or higher	0.391***	0.000	108,809
	(0.003)	(0.003)	
Financial products			
Bank account	0.677***	0.002	106,220
	(0.003)	(0.003)	
Any loan	0.707***	-0.006**	107,127
	(0.003)	(0.003)	
Omnibus F-statistic		0.979	112,063
		[0.463]	
Number of participants by arm	28,197	83,866	112,063

This table tests the balance of pre-treatment characteristics by elicit beliefs treatment for the full sample. We run the following regression separately for each baseline covariate k: $X_i^k = \alpha + \beta \mathbb{1}(\text{Elicit Beliefs})_i + \varepsilon_i$, where X_i^k is a baseline covariate for participant i and $\mathbb{1}(\text{Elicit Beliefs})_i$ is a dummy indicating whether participant i was assigned to the elicit beliefs treatment. Column (1) shows α which is the mean for the $\mathbb{1}(\text{Elicit Beliefs})_i = 0$ group. Column (2) shows β which is the difference in means between the $\mathbb{1}(\text{Elicit Beliefs})_i = 0$ and $\mathbb{1}(\text{Elicit Beliefs})_i = 1$ groups. Column (3) shows the number of observations in each regression, which can change across covariates due to missing values when the respondent did not answer that question in the baseline survey. For the omnibus F-test to test whether the covariates jointly predict treatment, we run the following regression: $\mathbb{1}(\text{Elicit Beliefs})_i = \delta + \sum_{k=1}^{K} \gamma_k X_i^k + \varepsilon_i$, where $\mathbb{1}(\text{Elicit Beliefs})_i$ is a dummy equal to 1 if participant i was assigned to the elicit beliefs treatment. The omnibus F-statistic is a test of $\gamma_1 = \cdots = \gamma_K = 0$. To retain the full sample in the omnibus F-test, if a participant did not answer a particular question, we create a dummy variable indicating whether the variable was missing, replace the missing value with zero, and include the missing value dummy as an additional X^k covariate in the regression. The p-value of the omnibus F-statistic is included in square brackets. Continuous variables are winsorized at the 95th percentile. "Any loan" is a dummy variable equal to 1 if the participant had any of the following types of loan: consumer loan, mortgage, auto loan, credit card, and cash advance. Unlike in Table 2, we cannot include characteristics of the loan they are searching for in the balance tests since these questions were asked in the same module as the elicit beliefs treatment (rather than a prior module), and the elicit beliefs treatment affected whether participants continued in the survey. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Balance of Pre-Treatment Characteristics Across Tool Treatment Arms

	Control Mean	Difference relative to control mean: Price Comparison Tool	Simple Tool	Joint test F-stat	N
	(1)	(2)	(3)	(4)	(5)
Personal characteristics					
Age	35.773***	-0.145	0.057	1.616	46,051
	(0.082)	(0.116)	(0.116)	[0.199]	
log(Income)	13.460***	0.000	0.004	0.06	44,978
	(0.010)	(0.014)	(0.014)	[0.942]	
Incomplete high-school	0.041***	0.001	0.002	0.426	44,615
	(0.002)	(0.002)	(0.002)	[0.653]	
Complete high-school	0.425***	-0.008	-0.007	1.068	44,615
	(0.004)	(0.006)	(0.006)	[0.344]	
Complete 2-year program	0.222***	0.006	0.005	0.865	44,615
	(0.003)	(0.005)	(0.005)	[0.421]	
Complete 5-year program or higher	0.312***	0.000	0.000	0.002	44,615
	(0.004)	(0.005)	(0.005)	[0.998]	
Financial products					
Bank account	0.618***	0.016***	0.013**	4.566**	43,272
	(0.004)	(0.006)	(0.006)	[0.01]	
Any loan	0.668***	0.002	0.006	0.526	43,675
	(0.004)	(0.006)	(0.006)	[0.591]	
Desired loan characteristics					
log(Loan Amount)	14.737***	0.020	0.017	0.883	43,775
	(0.012)	(0.017)	(0.017)	[0.413]	
log(Maturity (years))	1.320***	-0.003	0.009	1.334	40,920
	(0.005)	(0.007)	(0.008)	[0.263]	
Omnibus F-statistic					
Price Comparison Tool		1.179			30,718
		[0.279]			
Simple Tool			1.277		30,690
			[0.207]		
Number of participants by arm	15,357	15,361	15,333		46,051

This table tests the balance of pre-treatment characteristics across treatment arms for the sample of consumer loan seekers who continued in the baseline survey long enough to reach the module in which they were assigned to one of the tool treatment arms or the control group. We run the following regression separately for each baseline covariate k: $X_i^k = \alpha + \beta_1 \mathbb{1}(\text{Simple Tool})_i + \beta_2 \mathbb{1}(\text{Price Comparison Tool})_i + \varepsilon_i$, where X_i^k is a baseline covariate for participant i and $\mathbb{1}(\text{Simple Tool})_i$ and $\mathbb{1}(\text{Price Comparison Tool})_i$ are dummies indicating whether participant i was assigned to the simple tool or price comparison tool arms, respectively. Column (1) shows α which is the mean for the control group. Column (2) shows β_2 which is the difference in means between the price comparison tool treatment arm and the control group. Column (3) shows β_1 which is the difference in means between the simple tool treatment arm and the control group. Column (4) shows the F-statistic from a joint test of $\beta_1 = \beta_2 = 0$. Column (5) shows the number of observations in each regression, which can change across covariates due to missing values when the respondent did not answer that question in the baseline survey. For the omnibus F-test to test whether the covariates jointly predict treatment, we run the following regression for the participants assigned to simple tool and control groups and for those assigned to the price comparison tool and control groups, separately: $\mathbb{1}(\text{Treatment})_i = \delta + \sum_{k=1}^{K} \gamma_k X_i^k + \varepsilon_i$, where $\mathbb{1}(\text{Treatment})_i$ is either $\mathbb{1}(\text{Simple Tool})_i$ or $\mathbb{1}(\text{Price Comparison Tool})_i$. The omnibus F-statistic is a test of $\gamma_1 = \cdots = \gamma_K = 0$. To retain the full sample in the omnibus F-test, if a participant did not answer a particular question, we create a dummy variable indicating whether the variable was missing, replace the missing value with zero, and include the missing value dummy as an additional X^k covariate in the regression. The p-values of the joint test and omnibus F-statistics are included in square brackets. Continuous variables are winsorized at the 95th percentile. "Desired loan characteristics" refer to characteristics of the loan they are searching for. "Any loan" is a dummy variable equal to 1 if the participant had any of the following types of loan: consumer loan, mortgage, auto loan, credit card, and cash advance. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of Price Comparison Tool and Simple Tool on Beliefs

	Expected rate (1)	Lowest rate (2)	Highest rate (3)	Dispersion (4)
Simple Tool	0.70	0.84**	−0.19	0.01
	(0.43)	(0.35)	(0.79)	(0.66)
Price Comparison Tool	16.18***	10.89***	30.35***	15.93***
	(1.18)	(0.93)	(2.24)	(1.45)
Observations	6,817	6,760	6,661	6,272
Control Mean Posterior	29.22	22.65	47.45	23.18
Control Median Posterior	18	12	25	10.68
Bin Density FEs	Yes	Yes	Yes	Yes

This table shows the effect of the simple tool and price comparison tool on beliefs. It shows results from specification (1). Each column shows β_1 and β_2 for one of the following outcome variables: (1) the interest rate the participant expects to get on the loan they take out; (2) the lowest interest rate the participant expects a bank could offer them; (3) the highest interest rate the participant expects a bank could offer them; (4) the difference between the highest and the lowest interest rates the participant expects a bank could offer them. The sample sizes for each column differ based on the number of participants who responded to the corresponding questions. The bin density fixed effects (FEs) are deciles of how many loans are in the price comparison tool histogram that would have been shown to the participant had they been assigned to the price comparison tool arm, based on their borrower and loan characteristics. If any of the participant's loan amount, maturity, or income is outside of the support of the administrative data, the bin density variable would be a missing value; for these individuals, we create an additional fixed effect category that we include in the regression, i.e., we include an additional dummy equal to 1 if the bin density is a missing value. There are 1,129 observations with dispersion = 0 (highest rate = lowest rate). Continuous variables are winsorized at the 5th and 95th percentiles by treatment arm. Tables A.4-A.8 show alternative specifications including using the prior as a control for the posterior rather than subtracting the prior on the left-hand side, using the posterior on the left-hand side without controlling for the prior, taking the natural logarithm of the interest rate expectations, taking the natural logarithm of the interest rate expectations without controlling for priors, and using a normalized measure of dispersion to test whether the treatment effect on dispersion is solely due to a scaling effect. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effect of Price Comparison Tool and Simple Tool on Search and Loan Terms

	Survey Data							Administrative Data	
	N of inst. searched	N of inst. applied	N of offers	Pr(negotiate)	Log interest rate offered	Pr(take loan)	Log interest rate taken	Pr(take loan)	Log interest rate taken
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(Intercept)	3.450***	1.121***	0.531***	0.097***	3.302***	0.369***	3.213***	0.190***	3.174***
	(0.048)	(0.037)	(0.022)	(0.009)	(0.049)	(0.015)	(0.052)	(0.003)	(0.007)
Simple Tool	0.053	0.018	0.019	0.013	0.000	0.013	−0.031	0.006	0.005
	(0.071)	(0.052)	(0.032)	(0.013)	(0.074)	(0.021)	(0.072)	(0.005)	(0.010)
Price Comparison Tool	0.017	0.025	0.069**	0.037***	−0.127**	0.036*	−0.111*	0.009**	0.004
	(0.071)	(0.051)	(0.033)	(0.014)	(0.062)	(0.021)	(0.065)	(0.005)	(0.010)
Observations	3,283	3,167	3,147	3,114	555	3,143	364	46,051	8,988

This table shows the effect of the simple tool and price comparison tool on search and loan terms using follow-up survey and administrative data. It shows results from specification (2). The outcomes and samples in each column are as follows, and the sample in all columns excludes survey respondents who did not reach the module in which the tool treatments were assigned (but who were nevertheless included in the survey because they did reach the module in which the elicit beliefs treatment was assigned). Column (1) is a count variable of the number of institutions at which participants report searching for a consumer loan; the sample excludes participants who did not know or refused to answer how many institutions they searched. Column (2) is a count variable of the number of institutions at which participants applied for a loan at any time after participating in the RCT; compared to column (1), this sample also excludes participants who did not know or refused to answer whether they applied for a loan at all of the institutions where they searched. Column (3) is a count variable of the number of approved loan applications at any time after participating in the RCT; compared to column (2), the sample also excludes participants who did not know or refused to answer whether the loan was approved for all their loan applications. Column (4) is a dummy variable equal to 1 if the participant tried to negotiate with at least one institution where they applied; compared to column (3), the sample also excludes participants who did not know or refused to answer whether they negotiated for all their loan offers. Column (5) is the natural logarithm of the interest rate offered; compared to column (3), the sample excludes searches that did not generate an offer and offers for which the participant did not report the offered interest rate. Column (6) is a dummy variable equal to 1 if the participant reported taking out a loan at any time after participating in the RCT; compared to column (3), the sample also excludes participants who did not know or refused to answer whether they took out the loan for all loan offers they received. Column (7) is the natural logarithm of the reported interest rate obtained; compared to column (6), the sample excludes offers that the participant did not take and offers for which the participant did not report the interest rate. For columns (5) and (7), each observation is a loan offer or loan taken. Column (8) is a dummy variable equal to 1 if the participant obtained a consumer loan within 1 year after participating in the RCT according to administrative data from the CMF. Column (9) is the natural logarithm of the interest rate on the loan the participant took out according to administrative data from the CMF; compared to column (8), the column excludes those who did not take out a loan in administrative data within 1 year after participating. Continuous variables are winsorized at the 5th and 95th percentiles by treatment arm. Tables A.9 and A.10 show balance tests for the surveyed subsample and subsample receiving loans in the administrative data in this table, respectively. Heteroskedasticity-robust standard errors are reported in parentheses for columns at the individual level, while standard errors clustered at the individual level are reported for columns at the loan offer level or loan level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Heterogeneous Effects of Tools by Beliefs about Dispersion

	Underestimated dispersion				All others			
	N of inst. searched	N of inst. applied	N of offers	Pr(negotiate)	N of inst. searched	N of inst. applied	N of offers	Pr(negotiate)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Intercept)	3.562***	1.188***	0.622***	0.101***	3.391***	1.034***	0.444***	0.078***
	(0.086)	(0.068)	(0.041)	(0.017)	(0.084)	(0.056)	(0.034)	(0.014)
Simple Tool	0.015	0.013	−0.065	0.031	0.289**	0.109	0.104**	0.007
	(0.123)	(0.099)	(0.061)	(0.026)	(0.134)	(0.088)	(0.053)	(0.021)
Price Comparison Tool	0.084	0.033	0.050	0.079***	0.003	0.035	0.068	0.028
	(0.140)	(0.096)	(0.062)	(0.028)	(0.124)	(0.084)	(0.054)	(0.022)
Observations	965	939	935	925	1,063	1,026	1,021	1,013

This table shows the heterogeneous effect of the price comparison tool on search and negotiation using follow-up survey data. It shows results from specification (2) estimated separately for those who underestimate dispersion in columns (1)–(4) and all others in columns (5)–(8). Underestimating dispersion is defined as beliefs about dispersion being at least 1 pp lower than the observed dispersion faced by that consumer in the administrative data. The outcomes and samples in each column are as follows, and the sample in all columns excludes survey respondents who did not reach the module in which the tool treatments were assigned (but who were nevertheless included in the survey because they did reach the module in which the elicit beliefs treatment was assigned). Column (1) is a count variable of the number of institutions at which participants report searching for a consumer loan; the sample excludes participants who did not know or refused to answer how many institutions they searched. Column (2) is a count variable of the number of institutions at which participants applied for a loan at any time after participating in the RCT; compared to column (1), this sample also excludes participants who did not know or refused to answer whether they applied for a loan at all of the institutions where they searched. Column (3) is a count variable of the number of approved loan applications at any time after participating in the RCT; compared to column (2), the sample also excludes participants who did not know or refused to answer whether the loan was approved for all their loan applications. Column (4) is a dummy variable equal to 1 if the participant tried to negotiate with at least one institution where they applied; compared to column (3), the sample also excludes participants who did not know or refused to answer whether they negotiated for all their loan offers. Columns (5)–(8) repeat the outcomes from columns (1)–(4). Continuous variables are winsorized at the 5th and 95th percentiles by treatment arm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

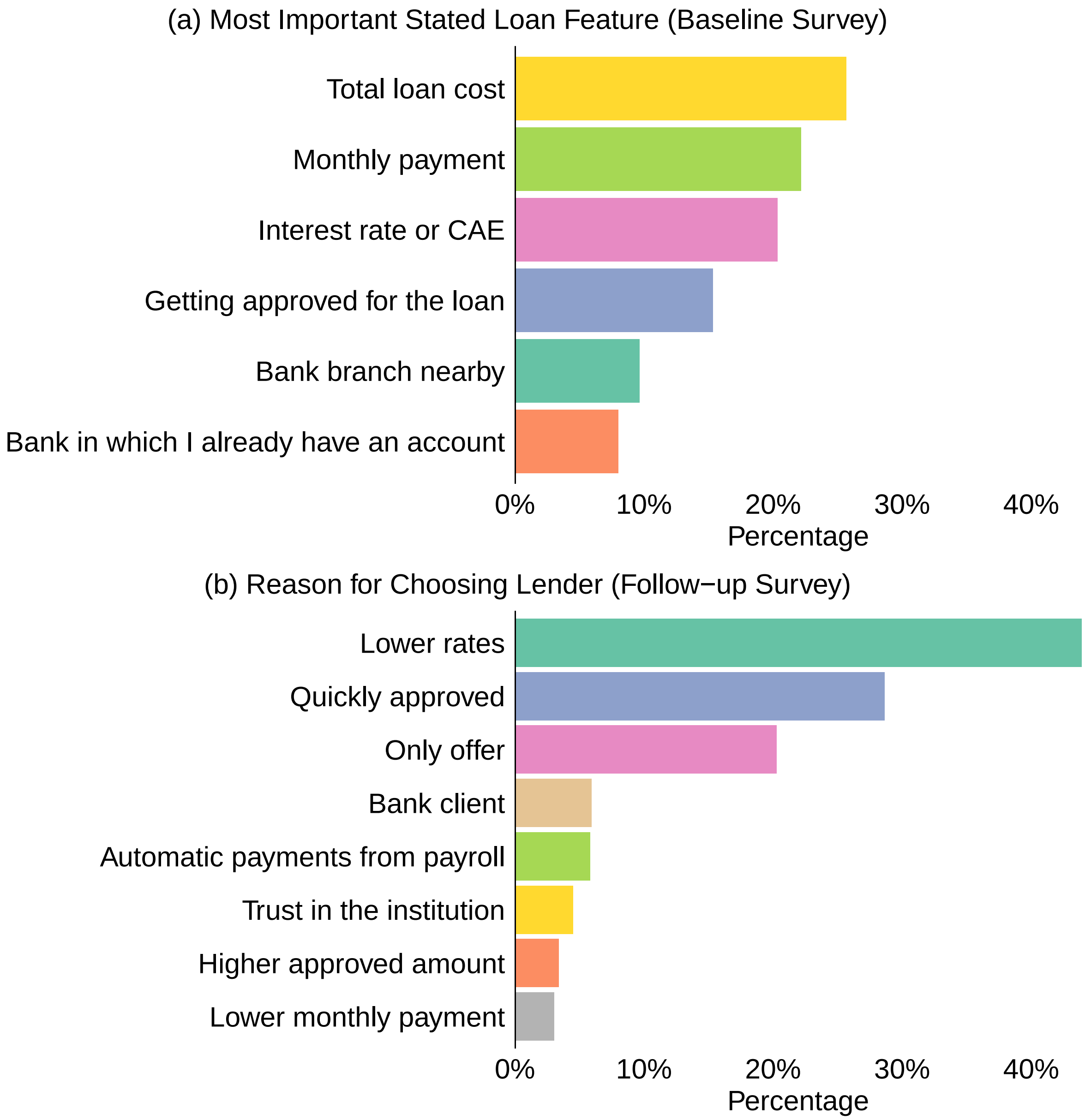
Table 6: Effect of Eliciting Beliefs on Search and Loan Terms

	Survey Data							Administrative Data	
	N of inst. searched	N of inst. applied	N of offers	Pr(negotiate)	Log interest rate offered	Pr(take loan)	Log interest rate taken	Pr(take loan)	Log interest rate taken
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(Intercept)	3.357***	1.192***	0.579***	0.111***	3.553***	0.360***	3.469***	0.195***	3.174***
	(0.040)	(0.033)	(0.021)	(0.008)	(0.035)	(0.012)	(0.041)	(0.002)	(0.005)
Elicit Beliefs	0.130***	−0.031	−0.003	0.011	−0.073*	0.001	−0.101**	−0.004	−0.012**
	(0.048)	(0.038)	(0.024)	(0.010)	(0.042)	(0.015)	(0.048)	(0.003)	(0.006)
Observations	5,774	5,565	5,525	5,465	1,241	5,516	724	112,063	21,522

This table shows the effect of the elicit beliefs treatment on search behavior and loan terms using follow-up survey and administrative data. It shows results from specification (3). The outcomes and samples in each column are as follows. Column (1) is a count variable of the number of institutions at which participants report searching for a consumer loan; the sample excludes participants who did not know or refused to answer how many institutions they searched. Column (2) is a count variable of the number of institutions at which participants applied for a loan at any time after participating in the RCT; compared to column (1), this sample also excludes participants who did not know or refused to answer whether they applied for a loan at all of the institutions where they searched. Column (3) is a count variable of the number of approved loan applications at any time after participating in the RCT; compared to column (2), the sample also excludes participants who did not know or refused to answer whether the loan was approved for all their loan applications. Column (4) is a dummy variable equal to 1 if the participant tried to negotiate with at least one institution where they applied; compared to column (3), the sample also excludes participants who did not know or refused to answer whether they negotiated for all their loan offers. Column (5) is the natural logarithm of the interest rate offered; compared to column (3), the sample excludes searches that did not generate an offer and offers for which the participant did not report the offered interest rate. Column (6) is a dummy variable equal to 1 if the participant reported taking out a loan at any time after participating in the RCT; compared to column (3), the sample also excludes participants who did not know or refused to answer whether they took out the loan for all loan offers they received. Column (7) is the natural logarithm of the reported interest rate obtained; compared to column (6), the sample excludes offers that the participant did not take and offers for which the participant did not report the interest rate. For columns (5) and (7), each observation is a loan offer or loan taken. Column (8) is a dummy variable equal to 1 if the participant obtained a consumer loan within 1 year after participating in the RCT according to administrative data from the CMF. Column (9) is the natural logarithm of the interest rate on the loan the participant took out according to administrative data from the CMF; compared to column (8), the column excludes those who did not take out a loan in administrative data within 1 year after participating. Continuous variables are winsorized at the 5th and 95th percentiles by treatment arm. Tables A.11 and A.12 show balance tests for the surveyed subsample and subsample receiving loans in the administrative data in this table, respectively. Heteroskedasticity-robust standard errors are reported in parentheses for columns at the individual level, while standard errors clustered at the individual level are reported for columns at the loan offer level or loan level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix A Additional Figures and Tables

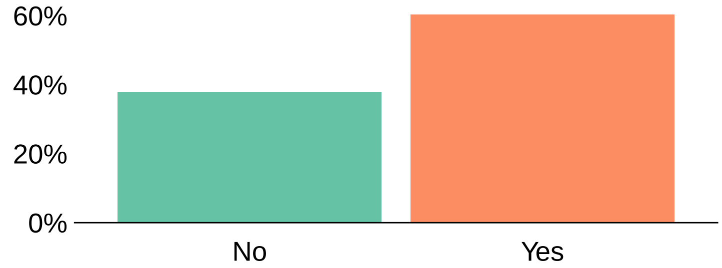
Figure A.1: Stated Importance of Loan Features



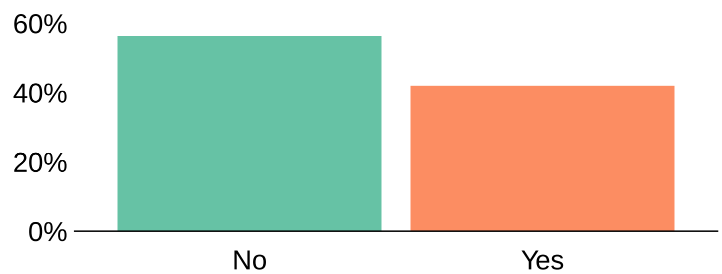
This figure shows the most important features of a loan reported in the baseline survey and the reason they chose to take out a particular offer in the follow-up survey. Panel (a) shows results from the baseline survey, conducted when participants were searching. It shows the reasons that participants ranked as most important in response to the question "What are the most important features of the loan you are looking for?" Panel (b) shows responses in our follow-up survey for the subset of participants who took out a loan. It shows responses to the question "Why did you take the loan from {Bank X} compared to offers you saw or received from other banks?" CAE refers to the *carga anual equivalente* which is analogous to an annualized percentage rate (APR).

Figure A.2: Sequential Search, Simultaneous Search, and Searching for Approval

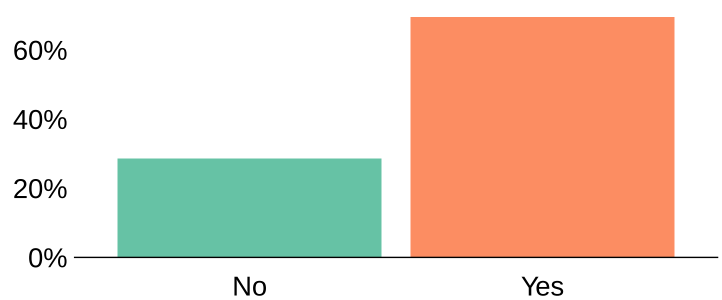
(a) Sequential Search: Target Interest Rate



(b) Simultaneous Search: Target N Offers or N Banks



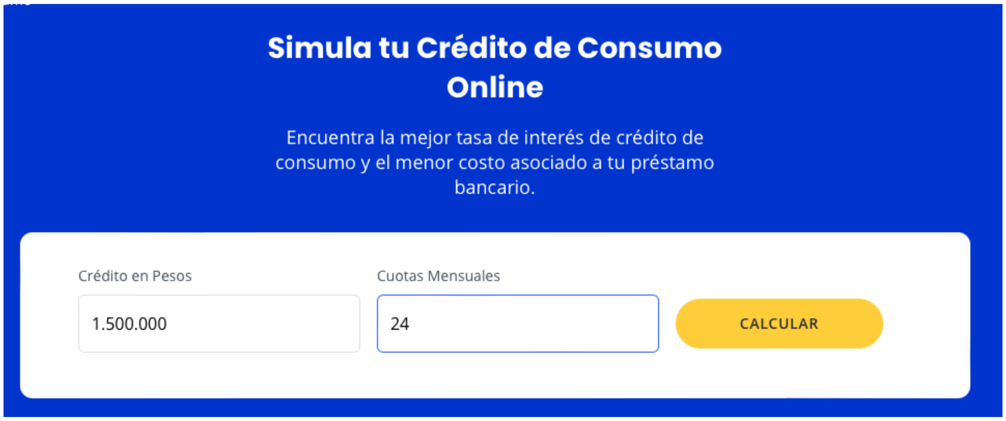
(c) Searching for Approval: Search Until 1 Offer



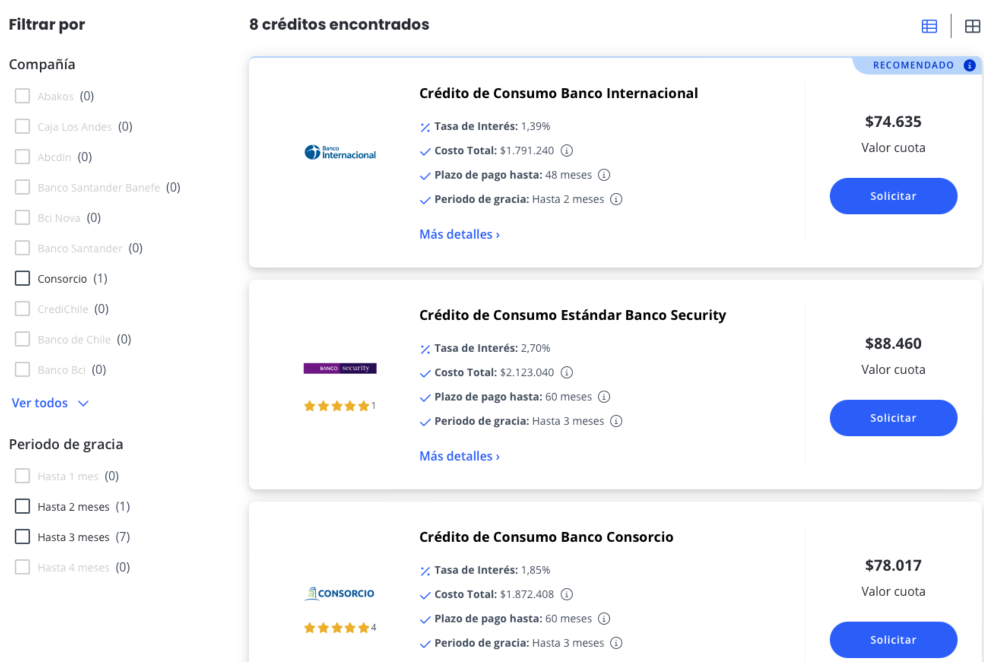
This figure shows the results of asking participants in our follow-up phone survey questions about their search strategy. We asked four yes/no questions to make the three plots: (a) "Did you plan to search until you reached a target interest rate and then stop searching?"; (b) "Did you have a target number of offers you would like to receive from financial institutions to stop looking?' or "Did you have a target number of financial institutions from which you wanted to obtain information about loans?"; (c) "Did you expect to search until a financial institution approved your application and then take a loan from that institution?". For each panel, we counted the number of answers to the questions, and specifically, for panel (b), we reported the number of participants who answered "yes" to either of the two questions.

Figure A.3: ComparaOnline

(a) Input

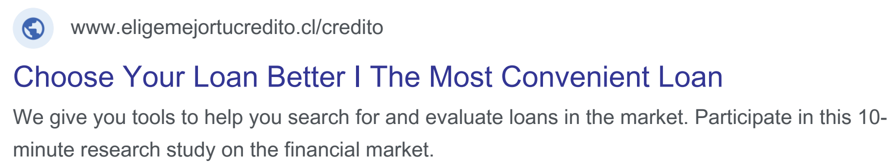


(b) Output



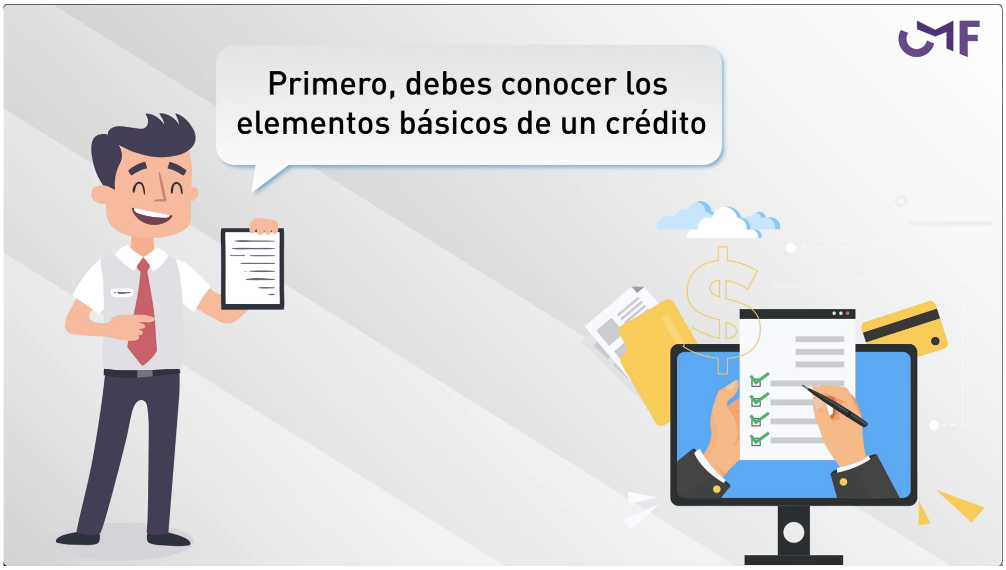
This figure shows the user interface of ComparaOnline. Their website provides rate quotes to prospective customers and direct customers to financial institutions. It functions as a quote aggregator that displays the interest rates that banks report they would (but are not required to) offer. Last accessed on May 15, 2024.

Figure A.4: Sample Google Advertisement for Participants Recruitment



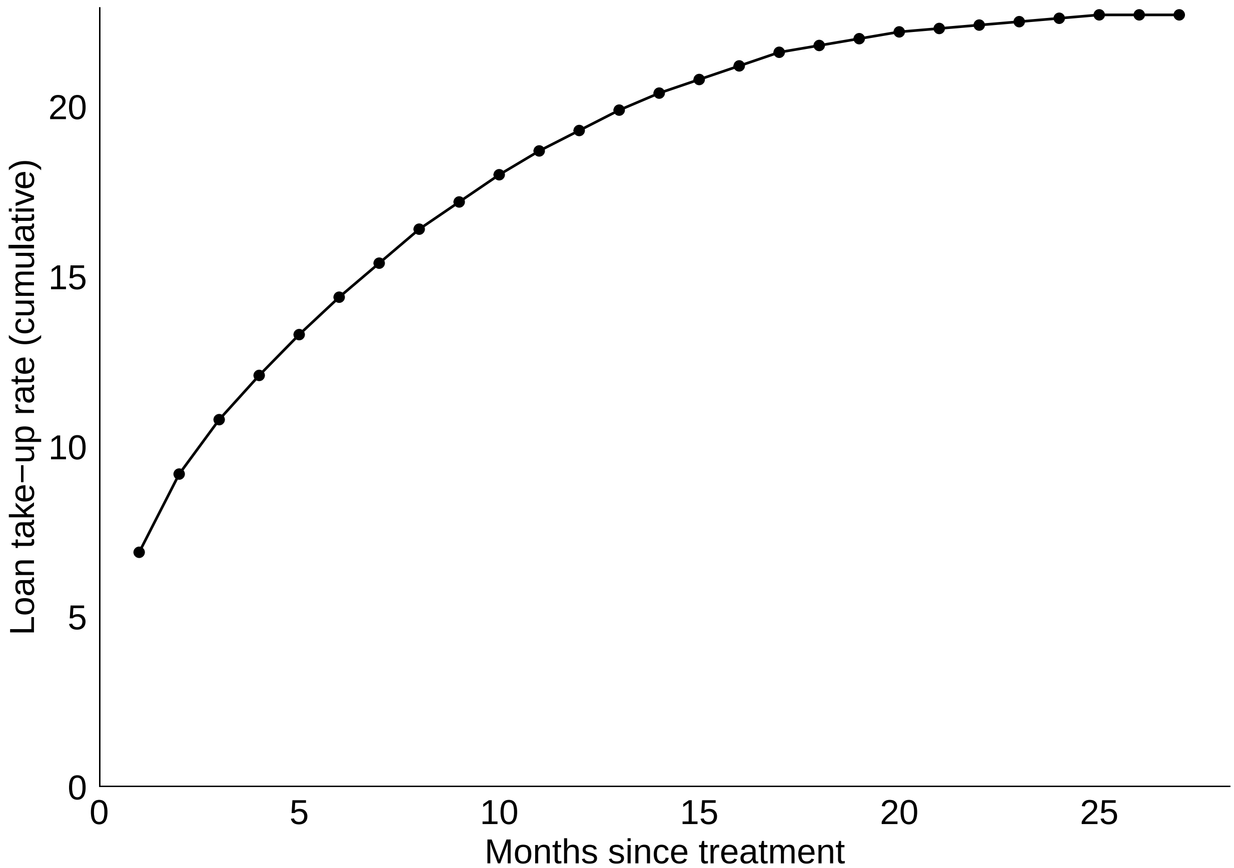
This figure shows an English translation of one of our Google advertisements that we targeted to people searching for keywords related to consumer loans in Chile to recruit them as participants in the RCT.

Figure A.5: Screenshot of Control Video



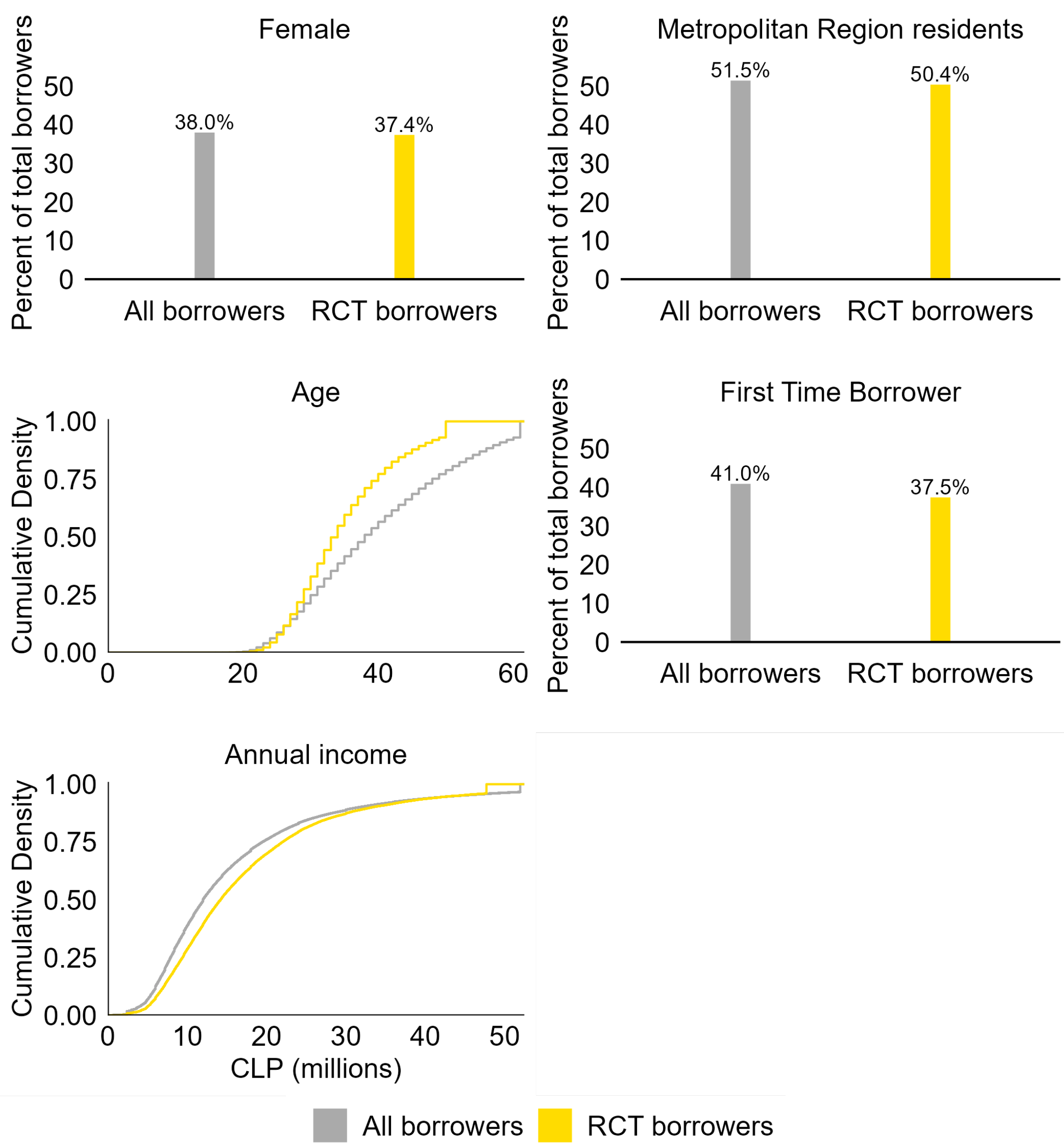
This figure shows a screenshot of the animated video shown to the control group. The video lasts 1 minute and 35 seconds and was developed by the Comisión Mercado Financiero (CMF) to provide basic loan terminology, but not provide information that would affect search.

Figure A.6: Participant Loan Take Up Rate Since Treatment



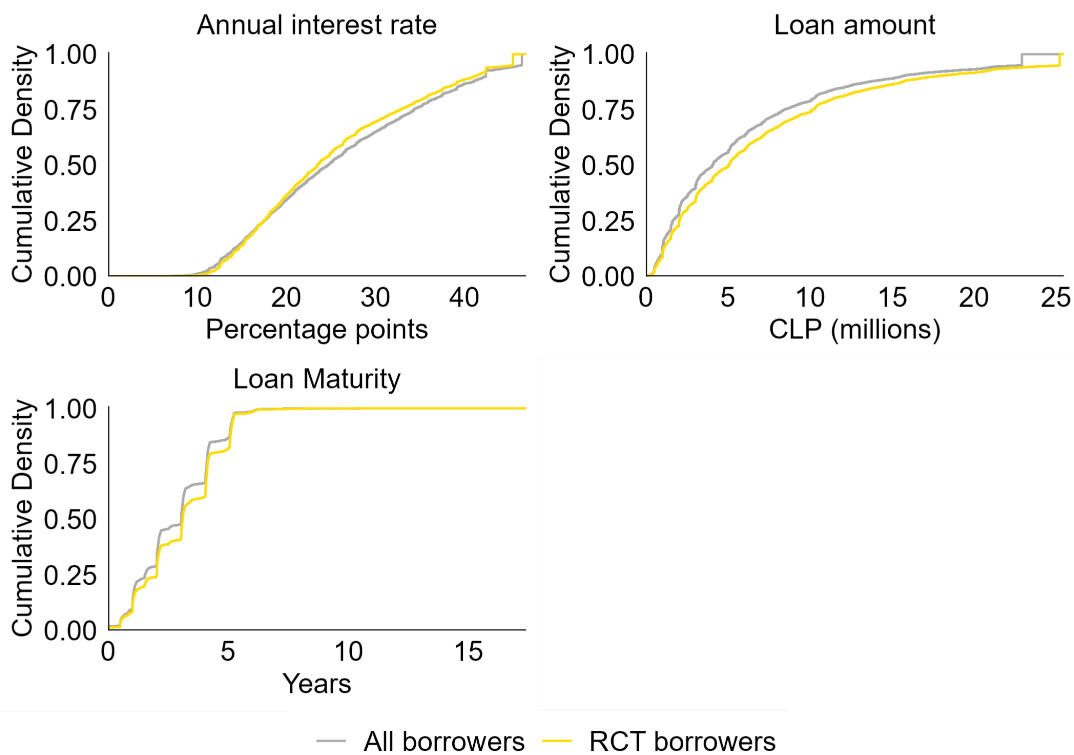
The figure shows the cumulative loan take-up rates of consumer loan borrowers of the 46,051 participants who were assigned to either our control video, price comparison tool, and simple tool. Overall, 10,448 of our RCT participants ended up taking out a consumer loan. We define loan take up as the participants having a loan in our administrative data on bank consumer loans.

Figure A.7: External Validity: Personal Characteristics



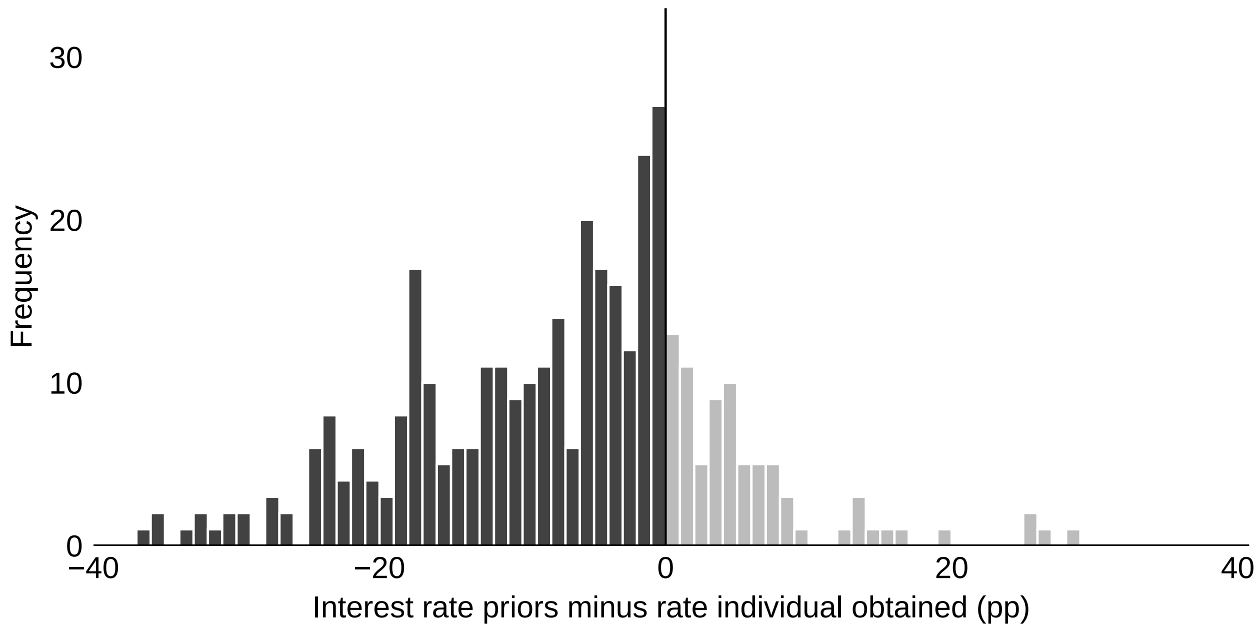
This figure shows the comparison of borrower attributes for all bank consumer borrowers taking out bank consumer loans in the sample period with borrowers who received a bank consumer loan and participated in our RCT. We have 27,130 loans taken out by RCT borrowers and 1,454,216 loans taken out by all consumer loan takers from November 2021 to February 2024.

Figure A.8: External Validity: Loan Terms



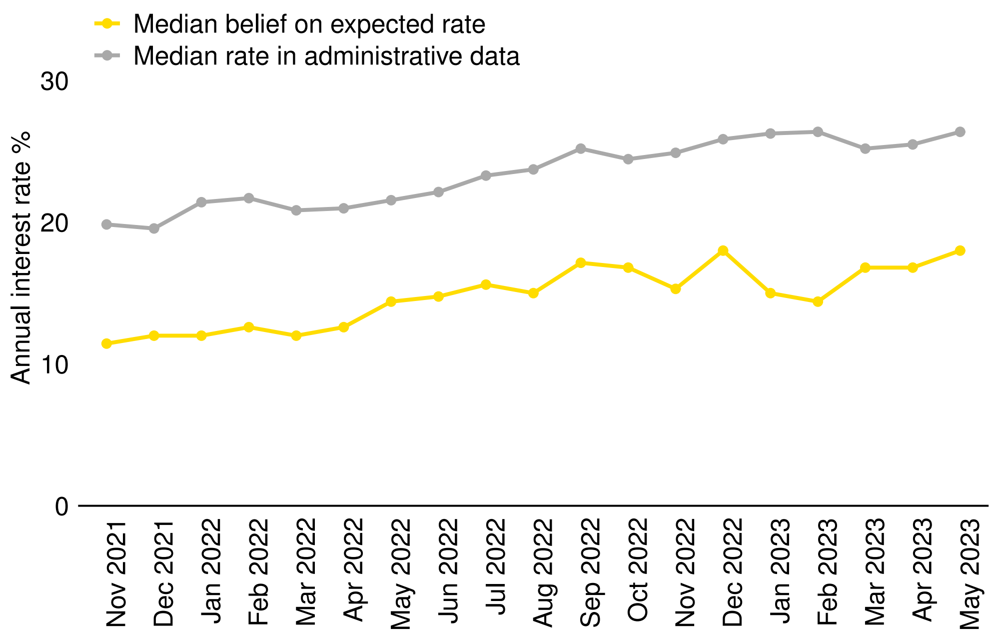
This figure shows the comparison of borrower loan terms for all bank consumer borrowers taking out bank consumer loans in the sample period with borrowers who received a bank consumer loan and participated in our RCT. We have 27,130 loans taken out by RCT borrowers and 1,454,216 loans taken out by all consumer loan takers from November 2021 to February 2024.

Figure A.9: Difference in Interest Rates Between Prior and Rate the Individual Received (pp), Restricted to First Quartile of Time Between Participation and Obtaining Loan



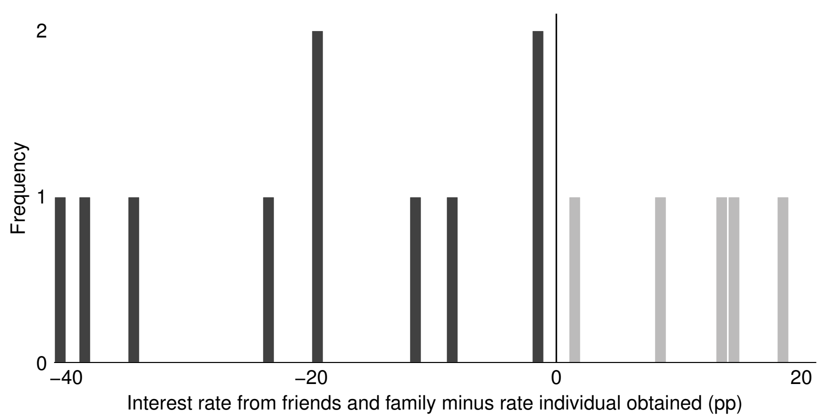
This figure is equivalent to Figure 3 but restricted to the first quartile of the number of days between participating in the RCT and obtaining a loan. It shows that even participants who obtain a loan shortly after participating (within 22 days, which corresponds to the 25th percentile) tend to underestimate the interest rate they ultimately obtain. The figure is a histogram of the difference between a participant's prior expectations about the interest rate they will get on the loan they take out (as reported in our baseline survey) and the interest rate they ended up receiving on the loan they took out in our administrative data. We construct this figure by restricting to the subset of participants in the control group who took out a loan after participating and comparing the interest rate they obtained on the loan in the administrative data to the prior they had reported in the baseline survey. For participants who obtained more than one loan after participating, we restrict to the first loan they obtained after participating. We remove observations beyond the 5th and 95th percentiles in the graph for legibility. The number of observations is 394. The percentage of people who underestimated the rate they would receive, i.e., the percentage of the sample in the negative portion of the histogram, is 74.9%.

Figure A.10: Interest Rates Over Time



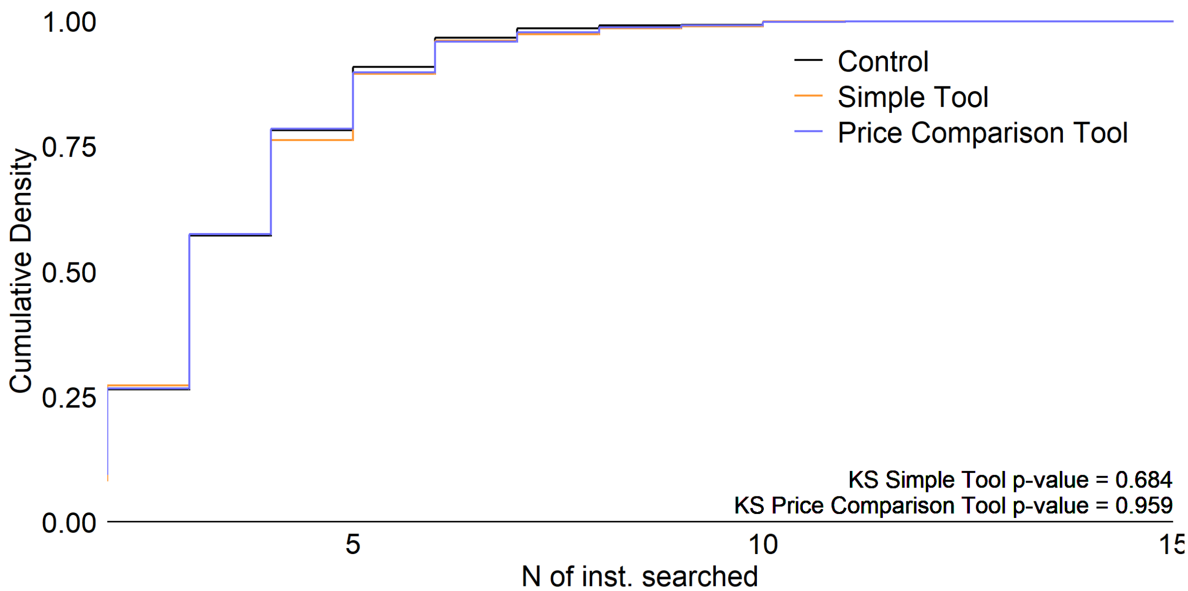
This figure shows the median annual interest rate from administrative data and the median belief about the expected annual interest rate reported by respondents in the baseline survey. For each month, we compute the median across all observed consumer loans (in administrative data) and across all respondents' beliefs (in survey data). Months are based on the response date for survey data and the loan operation date for administrative data.

Figure A.11: Difference in Interest Rates Shared by Friends and Family and Obtained



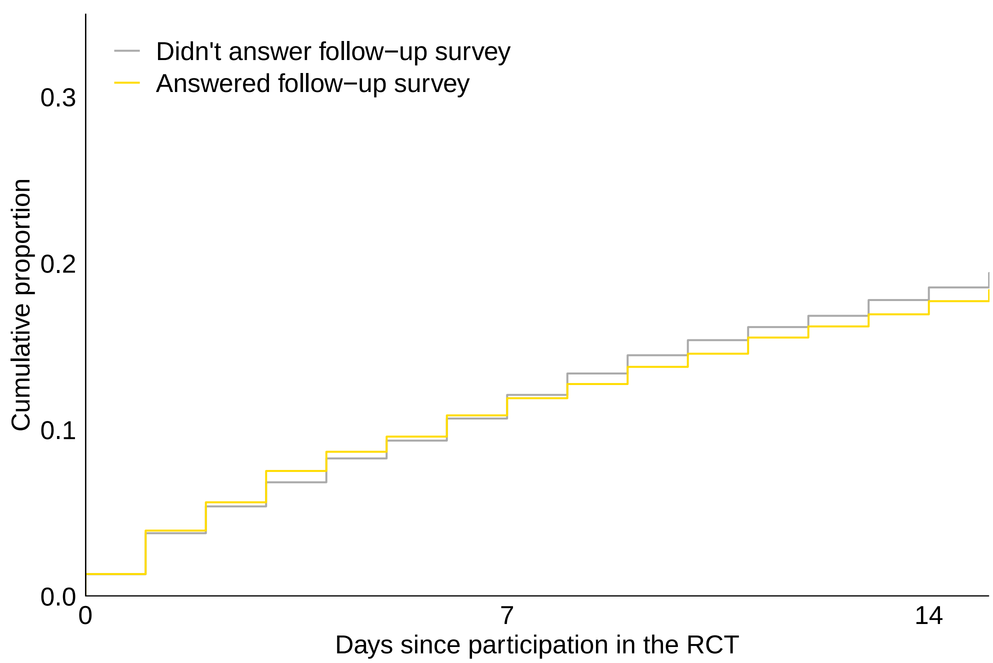
This figure shows a histogram of differences between interest rates shared by family and friends (from survey data) and the actual interest rate received by participants in the administrative data. There are 15 observations, 68.8% of which are negative.

Figure A.12: Cumulative Distribution Function of Number of Institutions Searched



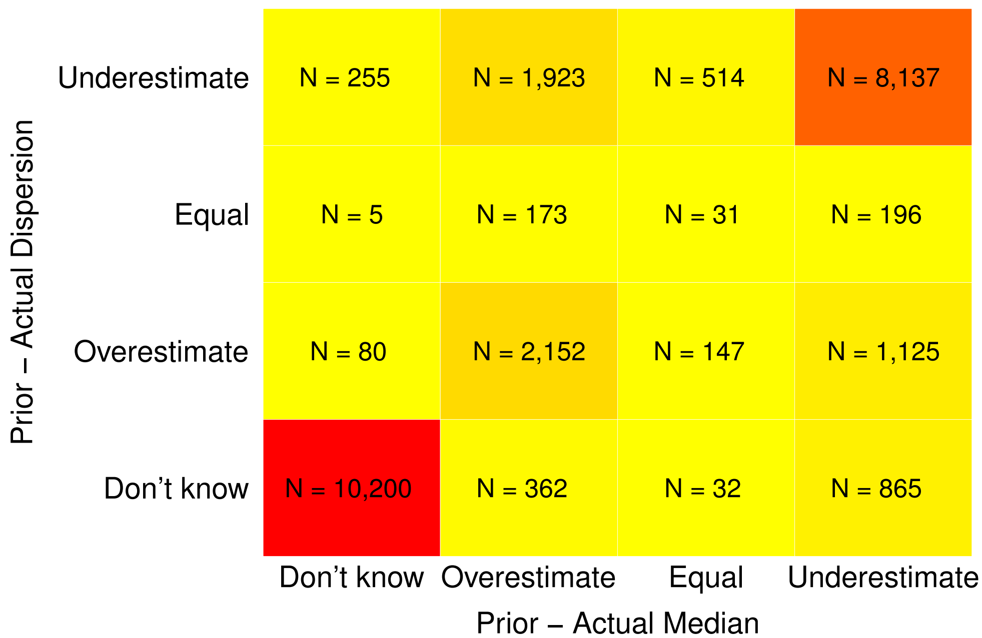
This figure shows the cumulative distribution function (CDF) of the number of institutions searched by treatment arm. N =3,283. The Kolmogórov-Smirnov (KS) test is estimated comparing each tool treatment to the control group and is calculated by Monte Carlo simulations with 10,000 replications.

Figure A.13: Timing of Loan Take-Up by Survey Response



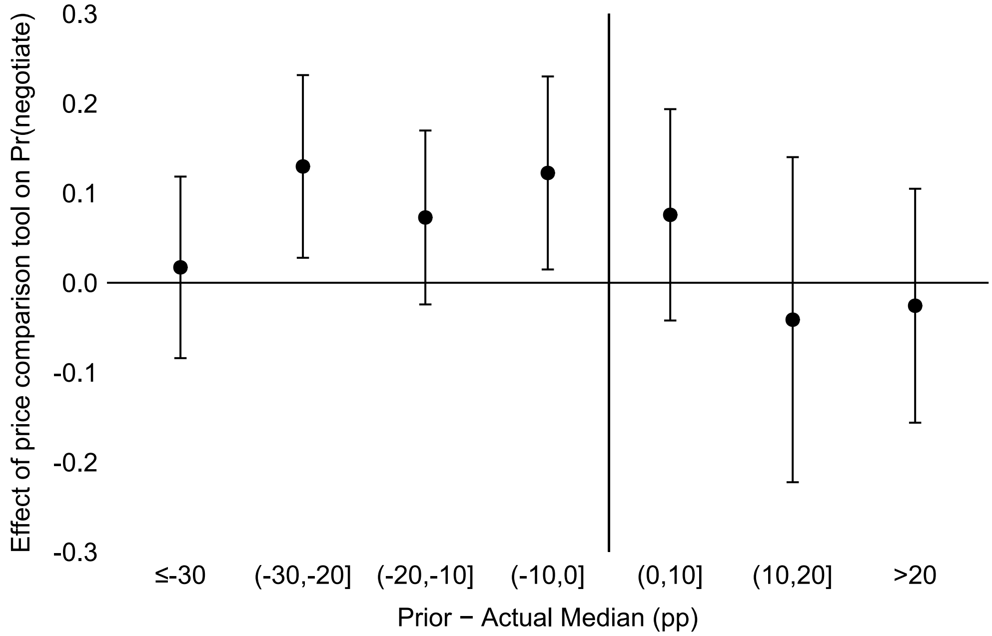
This figure shows the proportion of people who obtained a loan in the administrative data within a certain number of days of participating in the RCT (up to 14), conditional on obtaining a loan. Results are shown separately by those who responded to and did not respond to the follow-up survey, among the subset who we attempted to survey.

Figure A.14: Belief Heterogeneity



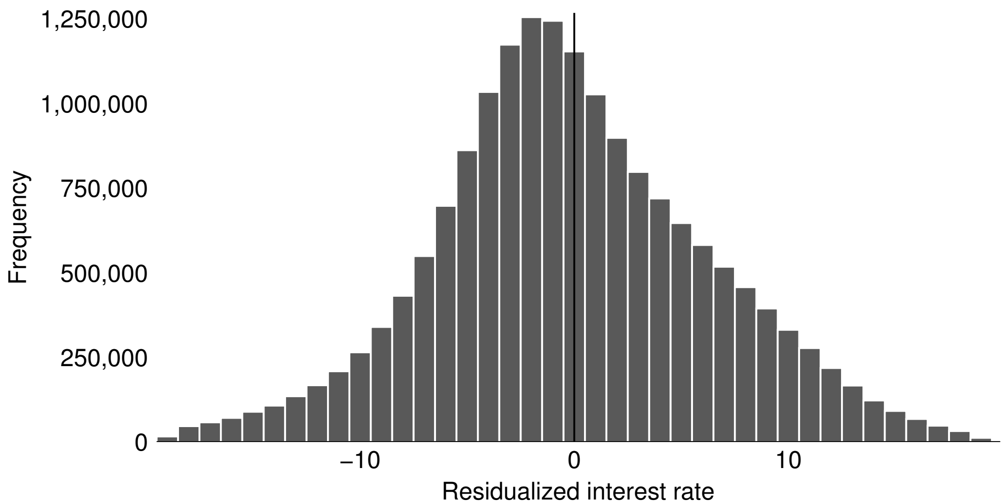
This figure shows participants' beliefs about interest rate dispersion and the interest rate they expected to receive, compared to actual values observed in administrative data, conditional on each participant's characteristics. *Prior – actual median* is the difference between a participant's prior expectations about the interest rate they will get on the loan they take out (as reported in our baseline survey) and the median rate we observe based on their characteristics in the administrative data (i.e., the median they would have seen in the price comparison tool if assigned to that treatment arm). *Prior – actual dispersion* is the difference between a participant's prior expectations about the dispersion in interest rates that a bank could offer them—measured as the highest rate minus the lowest rate they believed they could be offered—and the actual dispersion we observe based on their characteristics in the administrative data. We allow for a tolerance of ± 1 percentage point when defining equality, as it is unlikely participants would report exact matches. Each cell reports the number of participants in each belief category. Colors reflect cell frequencies, with red indicating the most common belief pattern. The sample includes consumer loan seekers who made it far enough in the baseline survey to be assigned to a tool treatment arm or to the control group.

Figure A.15: Heterogeneous Effects of Tool on Negotiating by Beliefs about First Moment



This figure shows heterogeneous treatment effects of the price comparison tool on the probability of negotiating, by how biased participants' beliefs about the first moment were. It shows results from specification (2) estimated separately by bins of the degree to which participants over- or underestimated the first moment of the interest rate distribution. The x-axis measures the participant's belief measured prior to being treated and the observed median of the interest rate distribution they face in administrative data, in percentage points. Bins are each 10 percentage points wide.

Figure A.16: Distribution of Residual Interest Rates



This figure shows the distribution of residualized interest rates from consumer loans. The figure is a histogram of the residuals from a regression of the interest rate on fixed effects that correspond to the specific histogram of the price comparison tool shown (or that would have been shown) to the borrower, based on loan and borrower characteristics and the version of the tool used. We run the following regression: $\text{Interest Rate}_i = \beta_0 + \lambda_j + \varepsilon_i$, where i indexes individual loans and λ_j is a fixed effect for each histogram bin defined by borrower and loan characteristics and the tool version. We include all versions of the price comparison tool, which were constructed using consumer loans taken from June 2021 (six months before the RCT started) to May 2023. The loans used in the regression are those used to construct the histograms shown in the tool, which are based on loans taken in the same *comuna* as the user, or in neighboring *comunas* or the same region, depending on data availability. After residualizing, we restrict the sample to residuals from histograms based on loans taken in the same *comuna* or region, excluding those based on neighboring *comunas*. We remove observations below the 1st and above the 99th percentile to improve legibility. The number of observations is 17,675,794.

Table A.1: Bank Website and Third-Party Comparison Tool Inputs

	Borrower characteristics								Loan characteristics			
	Name (1)	RUT (2)	Document number (3)	Income (4)	Phone number/email (5)	Comuna (6)	Employment condition (7)	Other active loans (8)	Loan amount (9)	Maturity (10)	First payment date (11)	Insurance options (12)
Panel A: Bank websites												
Bank 1		Y		Y					Y	Y	Y	Y
Bank 2		Y							Y	Y	Y	Y
Bank 3		Y		Y					Y	Y	Y	Y
Bank 4	Y	Y		Y	Y				Y	Y	Y	Y
Bank 5		Y		Y					Y	Y	Y	Y
Bank 6									Y	Y		Y
Bank 7		Y							Y	Y	Y	Y
Bank 8		Y	Y		Y				Y	Y	Y	Y
Bank 9		Y										
Bank 10		Y							Y	Y	Y	Y
Bank 11	Y	Y		Y	Y				Y	Y		Y
Bank 12		Y			Y				Y	Y		
Panel B: Comparison Tools												
Third party comparison tool									Y	Y		
Public comparison tool									Y	Y		

This table shows what inputs are required by each bank website's consumer loan simulator and each third-party comparison tool as of April 3, 2024. Column (1) shows whether we were able to scrape data from each bank website or third-party comparison tool. Column (2), "Name", refers to the name of the person searching for information. Column (3), "RUT", refers to the *rol único tributario*, the national ID number in Chile. Column (4), "Document number", refers to the serial number on the national identity card which is distinct from the national ID number or RUT. Column (7), "Comuna", is a geographic area analogous to a neighborhood; we include this column to emphasize that no banks or third-party comparison tools request this information, despite it being an important predictor of interest rates used by banks in their algorithms. Column (12), "First payment date", can be either any specific day chosen by the customer, or the date the simulator is used plus one or more complete months, depending on the simulator. Screenshots providing more details about each bank website and third-party comparison website, as well as the process we used to scrape data from these sites, are provided in Appendix B.

Table A.2: Follow-Up Survey Response

	Pr(answer the survey)	
	(1)	(2)
(Intercept)	0.157***	0.153***
	(0.004)	(0.004)
Simple Tool	−0.004	
	(0.006)	
Price Comparison Tool	−0.006	
	(0.006)	
Elicit Beliefs		0.004
		(0.004)
Observations	20,831	37,286

This table tests for differential response rates to the follow-up survey by tool treatment status and by elicit beliefs treatment status. It uses specifications (2) and (3), where y_i is a dummy variable equal to 1 if participant i responded to the follow-up survey. The sample is restricted to participants whom we attempted to contact in the follow-up survey, and column (1) is further restricted to participants who made it far enough in the baseline survey to be assigned to a tool treatment arm or the control group. We define answering the survey as one in which the participant reached the end of the survey. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Correlation between Beliefs and Monthly Median Rate in Administrative Data

	Expected rate	Lowest rate	Highest rate	Dispersion
	(1)	(2)	(3)	(4)
Median Rate$_t$	1.33***	1.29***	3.25***	1.67***
	(0.15)	(0.12)	(0.31)	(0.19)
Observations	16,015	15,875	15,618	15,045

This table shows the correlation between beliefs and the monthly median rate during the month in which a consumer participated. Coefficients are from a regression of the participant i's belief (measured in percentage points) against the median rate during the month t in which participant i participated in the RCT. The monthly median rate is calculated using the universe of consumer loans in administrative data for each month. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Effect of Tools on Beliefs, Controlling for Priors

	Expected rate (1)	Lowest rate (2)	Highest rate (3)	Dispersion (4)
Prior	0.80***	0.80***	0.68***	0.52***
	(0.02)	(0.02)	(0.02)	(0.02)
Simple Tool	−0.26	0.44	1.07	−1.96**
	(0.86)	(0.70)	(1.46)	(0.80)
Price Comparison Tool	18.94***	14.36***	39.16***	20.61***
	(1.55)	(1.23)	(2.93)	(1.78)
Observations	6,817	6,760	6,661	6,272
Control Mean Posterior	29.22	22.65	47.45	23.18
Control Median Posterior	18	12	25	10.68
Bin Density FEs	Yes	Yes	Yes	Yes

This table shows the effect of the simple tool and price comparison tool on beliefs. We estimate the following specification: $Posterior_i = \theta Prior_i + \beta_1 \mathbb{1}(\text{Simple Tool})_i + \beta_2 \mathbb{1}(\text{Price Comparison Tool})_i + \lambda_{b(i)} + \varepsilon_i$, where $Prior_i$ is the interest rate expectation participant i reported prior to seeing one of the tools or the control video, $Posterior_i$ is the interest rate expectation they reported after seeing it, and $\lambda_{b(i)}$ are bin density fixed effects. Each column shows θ, β_1, and β_2 for one of the following outcome variables: (1) the interest rate the participant expects to get on the loan they take out; (2) the lowest interest rate the participant expects a bank could offer them; (3) the highest interest rate the participant expects a bank could offer them; (4) the difference between the highest and the lowest interest rates. The sample sizes for each column differ based on the number of participants who responded to the corresponding questions. The bin density fixed effects (FEs) are deciles of how many loans are in the price comparison tool histogram that would have been shown to the participant had they been assigned to the price comparison tool arm, based on their borrower and loan characteristics. If any of the participant's loan amount, maturity, or income is outside of the support of the administrative data, the bin density variable would be a missing value; for these individuals, we create an additional fixed effect category that we include in the regression, i.e., we include an additional dummy equal to 1 if the bin density is a missing value. There are 1,129 observations with dispersion = 0 (highest rate = lowest rate). Continuous variables are winsorized at the 5th and 95th percentiles by treatment arm. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Effect of Tools on Beliefs, without Subtracting or Controlling for Priors

	Expected rate (1)	Lowest rate (2)	Highest rate (3)	Dispersion (4)
Simple Tool	−1.01	−0.02	−0.88	−2.95***
	(1.19)	(0.95)	(2.00)	(0.98)
Price Comparison Tool	22.13***	17.22***	43.87***	23.38***
	(1.83)	(1.48)	(3.38)	(1.93)
Observations	7,792	7,640	7,533	7,321
Control Mean Posterior	29.91	22.94	48.12	23.72
Control Median Posterior	17.88	12	25	10
Bin Density FEs	Yes	Yes	Yes	Yes

This table shows the effect of the simple tool and price comparison tool on beliefs. We estimate the following specification: $Posterior_i = \beta_1 \mathbb{1}(\text{Simple Tool})_i + \beta_2 \mathbb{1}(\text{Price Comparison Tool})_i + \lambda_{b(i)} + \varepsilon_i$, where $Prior_i$ is the interest rate expectation participant i reported prior to seeing one of the tools or the control video, $Posterior_i$ is the interest rate expectation they reported after seeing it, and $\lambda_{b(i)}$ are bin density fixed effects. Each column shows β_1 and β_2 for one of the following outcome variables: (1) the interest rate the participant expects to get on the loan they take out; (2) the lowest interest rate the participant expects a bank could offer them; (3) the highest interest rate the participant expects a bank could offer them; (4) the difference between the highest and the lowest interest rates the participant expects a bank could offer them. The sample sizes for each column differ based on the number of participants who responded to the corresponding questions. The bin density fixed effects (FEs) are deciles of how many loans are in the price comparison tool histogram that would have been shown to the participant had they been assigned to the price comparison tool arm, based on their borrower and loan characteristics. If any of the participant's loan amount, maturity, or income is outside of the support of the administrative data, the bin density variable would be a missing value; for these individuals, we create an additional fixed effect category that we include in the regression, i.e., we include an additional dummy equal to 1 if the bin density is a missing value. There are 1,129 observations with dispersion = 0 (highest rate = lowest rate). Continuous variables are winsorized at the 5th and 95th percentiles by treatment arm. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Effect of Tools on Beliefs in Logs, Controlling for Priors

	ln(Expected rate) (1)	ln(Lowest rate) (2)	ln(Highest rate) (3)	ln(Dispersion) (4)
ln(Prior)	0.695***	0.701***	0.684***	0.578***
	(0.010)	(0.010)	(0.010)	(0.013)
Simple Tool	−0.038*	−0.008	−0.041*	−0.091***
	(0.023)	(0.023)	(0.024)	(0.032)
Price Comparison Tool	0.315***	0.273***	0.367***	0.335***
	(0.028)	(0.027)	(0.029)	(0.038)
Observations	6,817	6,760	6,661	6,272
Control Mean Posterior (Levels)	29.22	22.65	47.45	23.18
Control Median Posterior (Levels)	18	12	25	10.68
Bin Density FEs	Yes	Yes	Yes	Yes

This table shows the effect of the simple tool and price comparison tool on beliefs. We estimate the following specification: $\log(Posterior_i + 1) = \theta \log(Prior_i + 1) + \beta_1 \mathbb{1}(\text{Simple Tool})_i + \beta_2 \mathbb{1}(\text{Price Comparison Tool})_i + \lambda_{b(i)} + \varepsilon_i$, where $Prior_i$ is the interest rate expectation participant i reported prior to seeing one of the tools or the control video, $Posterior_i$ is the interest rate expectation they reported after seeing it, and $\lambda_{b(i)}$ are bin density fixed effects. To account for any rate expectations of 0 (which do occur in the data), the transformation $\log(y_i + 1)$ was applied to the annualized interest rates in levels (where, for example, an 18% expected annual interest rate would be coded as 18). Each column shows θ, β_1, and β_2 for one of the following outcome variables: (1) the interest rate the participant expects to get on the loan they take out; (2) the lowest interest rate the participant expects a bank could offer them; (3) the highest interest rate the participant expects a bank could offer them; (4) the difference between the highest and the lowest interest rates. The sample sizes for each column differ based on the number of participants who responded to the corresponding questions. The bin density fixed effects (FEs) are deciles of how many loans are in the price comparison tool histogram that would have been shown to the participant had they been assigned to the price comparison tool arm, based on their borrower and loan characteristics. If any of the participant's loan amount, maturity, or income is outside of the support of the administrative data, the bin density variable would be a missing value; for these individuals, we create an additional fixed effect category that we include in the regression, i.e., we include an additional dummy equal to 1 if the bin density is a missing value. There are 1,129 observations with dispersion = 0 (highest rate = lowest rate). Continuous variables are winsorized at the 5th and 95th percentiles by treatment arm. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Effect of Tools on Beliefs in Logs, without Subtracting or Controlling for Priors

	ln(Expected rate) (1)	ln(Lowest rate) (2)	ln(Highest rate) (3)	ln(Dispersion) (4)
Simple Tool	−0.057*	−0.031	−0.066*	−0.128***
	(0.033)	(0.032)	(0.035)	(0.039)
Price Comparison Tool	0.407***	0.376***	0.459***	0.398***
	(0.034)	(0.034)	(0.036)	(0.043)
Observations	7,792	7,640	7,533	7,321
Control Mean Posterior (Levels)	29.91	22.94	48.12	23.72
Control Median Posterior (Levels)	17.88	12	25	10
Bin Density FEs	Yes	Yes	Yes	Yes

This table shows the effect of the simple tool and price comparison tool on beliefs. We estimate the following specification: $\log(Posterior_i + 1) = \beta_1 \mathbb{1}(\text{Simple Tool})_i + \beta_2 \mathbb{1}(\text{Price Comparison Tool})_i + \lambda_{b(i)} + \varepsilon_i$, where $Prior_i$ is the interest rate expectation participant i reported prior to seeing one of the tools or the control video, $Posterior_i$ is the interest rate expectation they reported after seeing it, and $\lambda_{b(i)}$ are bin density fixed effects. To account for any rate expectations of 0 (which do occur in the data), the transformation $\log(y_i + 1)$ was applied to the annualized interest rates in levels (where, for example, an 18% expected annual interest rate would be coded as 18). Each column shows β_1 and β_2 for one of the following outcome variables: (1) the interest rate the participant expects to get on the loan they take out; (2) the lowest interest rate the participant expects a bank could offer them; (3) the highest interest rate the participant expects a bank could offer them; (4) the difference between the highest and the lowest interest rates the participant expects a bank could offer them. The sample sizes for each column differ based on the number of participants who responded to the corresponding questions. The bin density fixed effects (FEs) are deciles of how many loans are in the price comparison tool histogram that would have been shown to the participant had they been assigned to the price comparison tool arm, based on their borrower and loan characteristics. If any of the participant's loan amount, maturity, or income is outside of the support of the administrative data, the bin density variable would be a missing value; for these individuals, we create an additional fixed effect category that we include in the regression, i.e., we include an additional dummy equal to 1 if the bin density is a missing value. There are 1,129 observations with dispersion = 0 (highest rate = lowest rate). Continuous variables are winsorized at the 5th and 95th percentiles by treatment arm. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Interest Rate Expectations Normalized Dispersion

	Normalized Dispersion
	(1)
Simple Tool	−0.02
	(0.01)
Price Comparison Tool	0.03***
	(0.01)
Observations	6,272
Control Mean Posterior	0.67
Control Median Posterior	0.67
Bin Density FEs	Yes

This table shows the effect of the simple tool and price comparison tool on beliefs. It shows results from specification (1). Normalized dispersion is measured as the highest rate minus the lowest rate divided by the midpoint of the highest rate and lowest rate. The bin density fixed effects (FEs) are deciles of how many loans are in the price comparison tool histogram that would have been shown to the participant had they been assigned to the price comparison tool arm, based on their borrower and loan characteristics. If any of the participant's loan amount, maturity, or income is outside of the support of the administrative data, the bin density variable would be a missing value; for these individuals, we create an additional fixed effect category that we include in the regression, i.e., we include an additional dummy equal to 1 if the bin density is a missing value. There are 1,129 observations with dispersion = 0 (highest rate = lowest rate). Continuous variables are winsorized at the 5th and 95th percentiles by treatment arm. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Balance of Tool Treatment Arms for Survey Subsample in Table 4

		Difference relative to control mean			
	Control Mean	Price Comparison Tool	Simple Tool	Joint test F-stat	N
	(1)	(2)	(3)	(4)	(5)
Personal characteristics					
Age	36.533***	-0.374	0.094	0.669	3,253
	(0.304)	(0.426)	(0.434)	[0.512]	
log(Income)	13.546***	-0.001	0.006	0.012	3,200
	(0.033)	(0.051)	(0.049)	[0.988]	
Incomplete high-school	0.026***	-0.002	-0.001	0.053	3,176
	(0.005)	(0.007)	(0.007)	[0.948]	
Complete high-school	0.381***	-0.027	-0.019	0.88	3,176
	(0.015)	(0.021)	(0.021)	[0.415]	
Complete 2-year program	0.205***	0.037**	0.012	2.119	3,176
	(0.012)	(0.018)	(0.018)	[0.12]	
Complete 5-year program or higher	0.388***	-0.008	0.008	0.288	3,176
	(0.015)	(0.021)	(0.021)	[0.749]	
Financial products					
Bank account	0.648***	0.019	0.026	0.83	3,120
	(0.015)	(0.021)	(0.021)	[0.436]	
Any loan	0.698***	0.031	-0.003	1.868	3,147
	(0.014)	(0.020)	(0.020)	[0.155]	
Desired loan characteristics					
log(Loan Amount)	14.981***	0.033	0.016	0.153	3,083
	(0.041)	(0.059)	(0.058)	[0.859]	
log(Maturity (years))	1.361***	0.008	0.000	0.065	2,945
	(0.019)	(0.027)	(0.027)	[0.937]	
Omnibus F-statistic					
Price Comparison Tool		0.973			2,150
		[0.481]			
Simple Tool			1.34		2,194
			[0.169]		
Number of participants by arm	1,091	1,059	1,103		3,253

This table tests the balance of pre-treatment characteristics across treatment arms for the survey subsample in Table 4. We run the following regression separately for each baseline covariate k: $X_i^k = \alpha + \beta_1 \mathbb{1}(\text{Simple Tool})_i + \beta_2 \mathbb{1}(\text{Price Comparison Tool})_i + \varepsilon_i$, where X_i^k is a baseline covariate for participant i and $\mathbb{1}(\text{Simple Tool})_i$ and $\mathbb{1}(\text{Price Comparison Tool})_i$ are dummies indicating whether participant i was assigned to the simple tool or price comparison tool arms, respectively. Column (1) shows α which is the mean for the control group. Column (2) shows β_2 which is the difference in means between the price comparison tool treatment arm and the control group. Column (3) shows β_1 which is the difference in means between the simple tool treatment arm and the control group. Column (4) shows the F-statistic from a joint test of $\beta_1 = \beta_2 = 0$. Column (5) shows the number of observations in each regression, which can change across covariates due to missing values when the respondent did not answer that question in the baseline survey. For the omnibus F-test to test whether the covariates jointly predict treatment, we run the following regression for the participants assigned to simple tool and control groups and for those assigned to the price comparison tool and control groups, separately: $\mathbb{1}(\text{Treatment})_i = \delta + \sum_{k=1}^{K} \gamma_k X_i^k + \varepsilon_i$, where $\mathbb{1}(\text{Treatment})_i$ is either $\mathbb{1}(\text{Simple Tool})_i$ or $\mathbb{1}(\text{Price Comparison Tool})_i$. The omnibus F-statistic is a test of $\gamma_1 = \cdots = \gamma_K = 0$. To retain the full sample in the omnibus F-test, if a participant did not answer a particular question, we create a dummy variable indicating whether the variable was missing, replace the missing value with zero, and include the missing value dummy as an additional X^k covariate in the regression. The p-values of the joint test and omnibus F-statistics are included in square brackets. Continuous variables are winsorized at the 95th percentile. "Desired loan characteristics" refer to characteristics of the loan they are searching for. "Any loan" is a dummy variable equal to 1 if the participant had any of the following types of loan: consumer loan, mortgage, auto loan, credit card, and cash advance. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Balance of Tool Treatment Arms for Subsample Obtaining Loans in Table 4

	Control Mean	Difference relative to control mean: Price Comparison Tool	Difference relative to control mean: Simple Tool	Joint test F-stat	N
	(1)	(2)	(3)	(4)	(5)
Personal characteristics					
Age	35.082***	0.042	0.245	0.711	8,988
	(0.156)	(0.219)	(0.220)	[0.491]	
log(Income)	13.905***	0.019	0.022	0.777	8,865
	(0.014)	(0.019)	(0.019)	[0.46]	
Incomplete high-school	0.007***	0.002	0.001	0.26	8,832
	(0.002)	(0.002)	(0.002)	[0.771]	
Complete high-school	0.243***	-0.009	-0.015	0.978	8,832
	(0.008)	(0.011)	(0.011)	[0.376]	
Complete 2-year program	0.205***	0.011	0.015	1.023	8,832
	(0.008)	(0.011)	(0.011)	[0.359]	
Complete 5-year program or higher	0.544***	-0.003	0.000	0.039	8,832
	(0.009)	(0.013)	(0.013)	[0.962]	
Financial products					
Bank account	0.863***	0.016*	0.004	1.775	8,844
	(0.006)	(0.009)	(0.009)	[0.17]	
Any loan	0.882***	0.000	0.003	0.069	8,875
	(0.006)	(0.008)	(0.008)	[0.934]	
Desired loan characteristics					
log(Loan Amount)	15.428***	0.054*	0.041	1.779	8,604
	(0.021)	(0.030)	(0.030)	[0.169]	
log(Maturity (years))	1.425***	0.039**	0.021	3.268**	8,373
	(0.011)	(0.015)	(0.015)	[0.038]	
Omnibus F-statistic					
Price Comparison Tool		1.149			5,982
		[0.305]			
Simple Tool			0.661		5,928
			[0.825]		
Number of participants by arm	2,922	3,060	3,006		8,988

This table tests the balance of pre-treatment characteristics across treatment arms for the subsample obtaining loans in the administrative data in Table 4, column (9). We run the following regression separately for each baseline covariate k: $X_i^k = \alpha + \beta_1 \mathbb{1}(\text{Simple Tool})_i + \beta_2 \mathbb{1}(\text{Price Comparison Tool})_i + \varepsilon_i$, where X_i^k is a baseline covariate for participant i and $\mathbb{1}(\text{Simple Tool})_i$ and $\mathbb{1}(\text{Price Comparison Tool})_i$ are dummies indicating whether participant i was assigned to the simple tool or price comparison tool arms, respectively. Column (1) shows α which is the mean for the control group. Column (2) shows β_2 which is the difference in means between the price comparison tool treatment arm and the control group. Column (3) shows β_1 which is the difference in means between the simple tool treatment arm and the control group. Column (4) shows the F-statistic from a joint test of $\beta_1 = \beta_2 = 0$. Column (5) shows the number of observations in each regression, which can change across covariates due to missing values when the respondent did not answer that question in the baseline survey. For the omnibus F-test to test whether the covariates jointly predict treatment, we run the following regression for the participants assigned to simple tool and control groups and for those assigned to the price comparison tool and control groups, separately: $\mathbb{1}(\text{Treatment})_i = \delta + \sum_{k=1}^{K} \gamma_k X_i^k + \varepsilon_i$, where $\mathbb{1}(\text{Treatment})_i$ is either $\mathbb{1}(\text{Simple Tool})_i$ or $\mathbb{1}(\text{Price Comparison Tool})_i$. The omnibus F-statistic is a test of $\gamma_1 = \cdots = \gamma_K = 0$. To retain the full sample in the omnibus F-test, if a participant did not answer a particular question, we create a dummy variable indicating whether the variable was missing, replace the missing value with zero, and include the missing value dummy as an additional X^k covariate in the regression. The p-values of the joint test and omnibus F-statistics are included in square brackets. Continuous variables are winsorized at the 95th percentile. "Desired loan characteristics" refer to characteristics of the loan they are searching for. "Any loan" is a dummy variable equal to 1 if the participant had any of the following types of loan: consumer loan, mortgage, auto loan, credit card, and cash advance. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Balance by Elicit Beliefs Treatment for Survey Subsample in Table 6

	Elicit Beliefs = 0 Mean	Elicit Beliefs	N
	(1)	(2)	(3)
Personal characteristics			
Age	36.822***	-0.307	5,729
	(0.251)	(0.294)	
log(Income)	13.589***	0.035	5,624
	(0.032)	(0.037)	
Incomplete high-school	0.028***	0.000	5,592
	(0.004)	(0.005)	
Complete high-school	0.348***	-0.014	5,592
	(0.012)	(0.014)	
Complete 2-year program	0.210***	-0.001	5,592
	(0.010)	(0.012)	
Complete 5-year program or higher	0.414***	0.015	5,592
	(0.013)	(0.015)	
Financial products			
Bank account	0.682***	0.009	5,491
	(0.012)	(0.014)	
Any loan	0.738***	-0.016	5,538
	(0.011)	(0.013)	
Omnibus F-statistic		0.959	5,729
		[0.482]	
Number of participants by arm	1,563	4,166	5,729

This table tests the balance of pre-treatment characteristics by elicit beliefs treatment for the survey subsample in Table 6. We run the following regression separately for each baseline covariate k: $X_i^k = \alpha + \beta \mathbb{1}(\text{Elicit Beliefs})_i + \varepsilon_i$, where X_i^k is a baseline covariate for participant i and $\mathbb{1}(\text{Elicit Beliefs})_i$ is a dummy indicating whether participant i was assigned to the elicit beliefs treatment. Column (1) shows α which is the mean for the $\mathbb{1}(\text{Elicit Beliefs})_i = 0$ group. Column (2) shows β which is the difference in means between the $\mathbb{1}(\text{Elicit Beliefs})_i = 0$ and $\mathbb{1}(\text{Elicit Beliefs})_i = 1$ groups. Column (3) shows the number of observations in each regression, which can change across covariates due to missing values when the respondent did not answer that question in the baseline survey. For the omnibus F-test to test whether the covariates jointly predict treatment, we run the following regression: $\mathbb{1}(\text{Elicit Beliefs})_i = \delta + \sum_{k=1}^{K} \gamma_k X_i^k + \varepsilon_i$, where $\mathbb{1}(\text{Elicit Beliefs})_i$ is a dummy equal to 1 if participant i was assigned to the elicit beliefs treatment. The omnibus F-statistic is a test of $\gamma_1 = \cdots = \gamma_K = 0$. To retain the full sample in the omnibus F-test, if a participant did not answer a particular question, we create a dummy variable indicating whether the variable was missing, replace the missing value with zero, and include the missing value dummy as an additional X^k covariate in the regression. The p-value of the omnibus F-statistic is included in square brackets. Continuous variables are winsorized at the 95th percentile. "Any loan" is a dummy variable equal to 1 if the participant had any of the following types of loan: consumer loan, mortgage, auto loan, credit card, and cash advance. Unlike in Table 2, we cannot include characteristics of the loan they are searching for in the balance tests since these questions were asked in the same module as the elicit beliefs treatment (rather than a prior module), and the elicit beliefs treatment affected whether participants continued in the survey. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Balance by Elicit Beliefs Treatment for Subsample Obtaining Loans in Table 6

	Elicit Beliefs = 0 Mean	Elicit Beliefs	N
	(1)	(2)	(3)
Personal characteristics			
Age	35.179***	0.012	21,522
	(0.110)	(0.127)	
log(Income)	14.040***	0.004	21,268
	(0.009)	(0.010)	
Incomplete high-school	0.007***	0.000	21,213
	(0.001)	(0.001)	
Complete high-school	0.207***	0.002	21,213
	(0.006)	(0.006)	
Complete 2-year program	0.200***	-0.002	21,213
	(0.005)	(0.006)	
Complete 5-year program or higher	0.585***	-0.001	21,213
	(0.007)	(0.008)	
Financial products			
Bank account	0.888***	0.007	21,238
	(0.004)	(0.005)	
Any loan	0.888***	-0.002	21,303
	(0.004)	(0.005)	
Omnibus F-statistic		0.401	21,522
		[0.956]	
Number of participants by arm	5,503	16,019	21,522

This table tests the balance of pre-treatment characteristics across treatment arms for the subsample obtaining loans in the administrative data in Table 4, column (9). We run the following regression separately for each baseline covariate k: $X_i^k = \alpha + \beta \mathbb{1}(\text{Elicit Beliefs})_i + \varepsilon_i$, where X_i^k is a baseline covariate for participant i and $\mathbb{1}(\text{Elicit Beliefs})_i$ is a dummy indicating whether participant i was assigned to the elicit beliefs treatment. Column (1) shows α which is the mean for the $\mathbb{1}(\text{Elicit Beliefs})_i = 0$ group. Column (2) shows β which is the difference in means between the $\mathbb{1}(\text{Elicit Beliefs})_i = 0$ and $\mathbb{1}(\text{Elicit Beliefs})_i = 1$ groups. Column (3) shows the number of observations in each regression, which can change across covariates due to missing values when the respondent did not answer that question in the baseline survey. For the omnibus F-test to test whether the covariates jointly predict treatment, we run the following regression: $\mathbb{1}(\text{Elicit Beliefs})_i = \delta + \sum_{k=1}^{K} \gamma_k X_i^k + \varepsilon_i$, where $\mathbb{1}(\text{Elicit Beliefs})_i$ is a dummy equal to 1 if participant i was assigned to the elicit beliefs treatment. The omnibus F-statistic is a test of $\gamma_1 = \cdots = \gamma_K = 0$. To retain the full sample in the omnibus F-test, if a participant did not answer a particular question, we create a dummy variable indicating whether the variable was missing, replace the missing value with zero, and include the missing value dummy as an additional X^k covariate in the regression. The p-value of the omnibus F-statistic is included in square brackets. Continuous variables are winsorized at the 95th percentile. "Any loan" is a dummy variable equal to 1 if the participant had any of the following types of loan: consumer loan, mortgage, auto loan, credit card, and cash advance. Unlike in Table 2, we cannot include characteristics of the loan they are searching for in the balance tests since these questions were asked in the same module as the elicit beliefs treatment (rather than a prior module), and the elicit beliefs treatment affected whether participants continued in the survey. Heteroskedasticity-robust standard errors (not clustered since the unit of randomization is the individual) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B Bank Websites and Comparison Websites

In our follow-up survey data, 44% of participants report using bank websites during their search and 12% report using third-party comparison websites (aggregators). Thus, these channels are likely a way that some consumers form their prior beliefs about loan interest rates. We investigate whether the tools on banks' own websites, known in Chile as "simulators," provide accurate information. If not, these simulators potentially contribute to participants' holding inaccurate priors. We scraped data from seven banks' consumer loan simulators and two third-party comparison websites. For each participant in our RCT, we obtained data from their baseline survey of individual and desired loan characteristics. We then ran a script that feeds these inputs into each website (including bank simulators and comparison websites) and scrapes the output. Next, we compared the rates participants would have seen on these websites with the rates they actually received in the administrative data.

B.1 Description of Bank Websites

Many Chilean banks provide a "simulator" on their websites, which allows visitors to see what interest rate they could expect to receive on a loan. Prospective borrowers input their personal information along with desired loan amount, terms, and other details. The simulator then generates loan terms including the interest rate, the "carga anual equivalente (CAE)"—comparable to the annual percentage rate (APR) in the U.S.—the "costo total del crédito (CTC)," which represents the total loan cost, the monthly cost, and the details of application costs and insurance costs. The input variables required by each simulator are also tabulated in Table A.1, panel A. All bank websites require information on loan amount and maturity. All but one bank also require the consumer's RUT (national ID number), but in tests that we show below, we find that the interest rates and other loan terms banks show in these simulators typically do not vary based on the RUT that is entered. Five out of twelve bank websites require the consumers' income as an input. On the other hand, none of them require the users to enter their neighborhood of residence (*comuna*), despite this being an important variable that banks use to price loans.

We were able to scrape data from seven of the twelve bank simulators. The simulators that we were not able to scrape were due to firewalls, returning errors when attempting to obtain a quote, and requiring the user to have a bank account already with that bank or to pay for the quote.

B.2 Description of Comparison Websites

There are two main third-party or government-run comparison websites providing estimated loan terms from multiple banks, also known as aggregators. One is provided by a private company, using information reported to the third-party comparison tool by banks, which report the rates they would offer for different loan types. Banks may have an incentive, however, to report downward-biased quotes to comparison websites as a bait-and-switch technique, as putting lower rates on comparison websites can direct traffic to them over other banks. Table A.1, panel B, describes the inputs required by this comparison website as well as a comparison website run by a different government agency.

B.3 Obtaining Data from Bank and Comparison Websites

We use the loan and consumer characteristics of each consumer-loan seeker in the baseline survey as input to the simulators, thereby replicating what our survey respondents would see should they use these tools. For identification-related inputs, such as RUT (national ID number) and contact information, we use random fake RUT numbers generated by adapting the code at `https://codepen.io/alisteroz/pen/KEoqgQ` for Python. To test whether the outputs shown by the bank websites depend on the RUT entered, we conducted tests where we held all inputs fixed except RUT. In these tests, we set the other characteristics such as loan amount, maturity, and income are set to be the median values and remain constant. We set the test size to 100 observations and tested the five bank websites where randomly generated ID information was used. As shown by Figure B.1, despite occasional variations in interest rates for different RUTs for two rates, the annualized interest rates remain largely identical across a random sample of RUTs. Our data collection period spanned from September 28th, 2023 to October 9th, 2023.

Similarly, four bank websites (three of which we could successfully scrape) require the phone number as an input. We conduct a similar test of whether the interest rates shown by the bank depend on the phone number (e.g., the bank might use the phone number's area code and condition the interest rate on where the consumer lives) by randomly generating phone numbers and again testing 100 observations where other inputs are held fixed. Figure B.2 shows that interest rates do not differ by area code for any of the three banks that require phone number as an input.

Many simulators provide users with the flexibility to select their preferred grace period (i.e. difference between loan origination and first payment date) and insurance options. These choices do not influence the interest rate of the loan, but they impact the CAE (APR) and the total loan cost. Since we did not ask about the preferred grace period or insurance options in the baseline survey (as many respondents would not have known how to respond to these questions), we extract a range of CAEs (APRs) that the user might have seen based on different inputs. In particular, we choose the grace period and insurance option that would either minimize or maximize the CAE (APR) and total cost of the loan, holding other inputs constant. For example, opting for no grace period and declining all insurance resulted in the lowest APR and total loan cost, while choosing the longest grace period and all available insurance yielded the highest APR and total loan cost. Table B.1 shows the details of the minimum and maximum input configurations. Nevertheless, because we observe interest rate (rather than CAE/APR) in the administrative data, the interest rate is the more relevant output that we scrape, and the interest rate is not affected by the choice of grace period or insurance.

Table B.1: Simulators' Min/Max Configuration

	Min	Max
Bank 5	No grace period, no insurance	Maximum grace period (6 months), Seguro Desgravamen (life insurance), and Seguro Cesantía (severance insurance)
Bank 6	No insurance	Desgravamen con ITP (disability insurance) and Protección Laboral (labor protection)
Bank 7	No grace period, no insurance	Maximum grace period (2 months), Desgravamen Hospitalizacion (life and hospitalization insurance)
Bank 8	No grace period, no insurance	Maximum grace period (the end of the next month), Seguro Desgravamen
Bank 10	No grace period, no insurance	Maximum grace period (3 months), Seguro de desgravamen
Bank 11	No insurance	Seguro de Desgravamen

Note: This table shows the min/max inputs we used to get simulation results from banks that allow users to select their preferred grace period and insurance options.

We obtain the following simulated loan outcomes for each consumer-loan seeker: monthly interest rates, equivalent annual charge (carga anual equivalente, or CAE, which is analogous to an APR), and total cost of the loan (costo total del crédito, or CTC).

B.4 Comparison of Websites' Rates and Received Rates

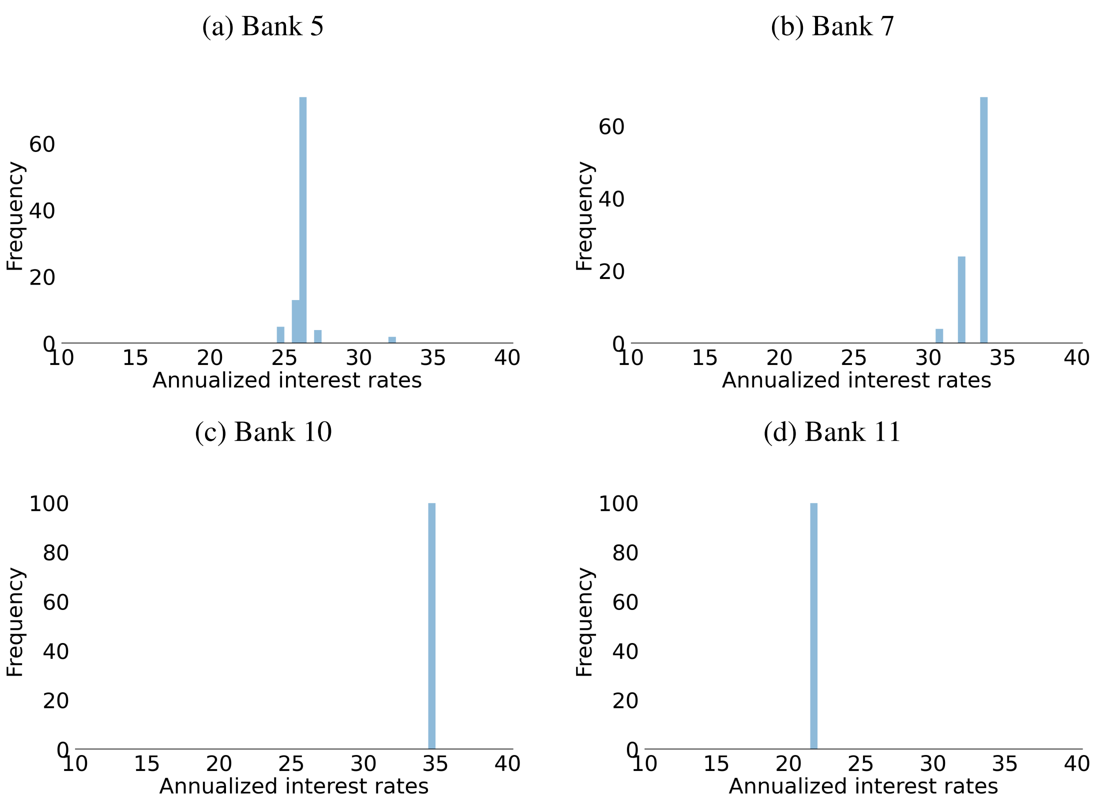
To compare rates to the rate an individual in our RCT would have seen on bank and comparison websites to rates that they actually received in the bank administrative data, we begin by matching the interest rates we scraped from these websites that correspond to what an individual RCT participant would have seen to the interest rates of the loans that these individuals actually received. First, we restrict our sample to the 30,979 people in the administrative data who had taken a loan. Next, for each of these participants, we keep one unique consumer loan from the administrative data, which is the first loan taken after treatment based on the date that the individual participated in the RCT and the date that the loan was taken out. In the rare case that the individual took two loans on the same day (0.55% of the sample), we take the largest loan taken on the first day after treatment that any loans were taken out by that individual. Then, for each individual who took up a consumer loan in the administrative banking data, we match interest rates the individual would have seen on the bank and comparison websites—based on the loan amount, loan maturity, and income they reported in the baseline survey—with the interest rate they obtained in the administrative data.

We merge at the consumer and bank level using encrypted bank identifiers in the administrative data: for example, if the individual took a loan from Bank A, we merge the interest rate they obtained with what they would have seen on Bank A's website, and what the comparison websites would have shown them for a loan from Bank A. If for a given loan, there are multiple matched interest rates quotes from different sources of the same bank, e.g., one from the third-party comparison tool and the other from the bank's website, we keep all quotes. The matched sample has 14,354 observations. We further restrict to loans in the administrative data that were obtained during the same period in which we ran the scrapers, which was from September 1st to October 31st, 2023. This limits the size of the sample but ensures that any observed differences between bank and third-party comparison websites and rates obtained in administrative data are not due to changes in interest rates over time.

The interest rate quotes shown by banks and comparison websites are highly inaccurate (Figures 5b and 5c). This section documents possible explanations for this inaccuracy. The first and most compelling explanation is that these websites do not ask the user for key inputs: none ask for the comuna of residence, and only three out of seven ask for income, both of which are significant predictors of interest rate. Thus, they do not provide quotes conditional on all the relevant borrower characteristics that influence the interest rate. Secondly, the Chilean credit bureau does not provide a continuous credit score; instead, they provide a binary flag for whether a borrower has defaulted on a loan in the past. This is a severe credit event and only happens if the borrower has missed three payments and judicial proceedings have been initiated against them. For the borrower, this flag effectively shuts them out of credit markets. Banks are able to create a proxy for credit risk by creating an average provision score across all banks reporting to the CMF. Each bank sets aside a certain fraction of the loan as revenues in case the borrower misses a payment or defaults as part of their risk management procedures (CMF, 2024). Borrowers are unaware of this number and while banks could pre-populate borrowers' risk scores by RUT in their simulators, in practice they do not. Beyond institutional features, there may be other factors related to the loan search process that can explain discrepancies between rate quotes seen on websites and actual loan rates. First, our scraped simulator data could be different from the loans participants ultimately took out, either due to the change of loan requirements or because our tool endogenously changed their search strategy on desired terms. Second, the discrepancy could be due to banks offering the same loan on different days when the bank might have changed their pricing model. We assess each of these potential explanations.

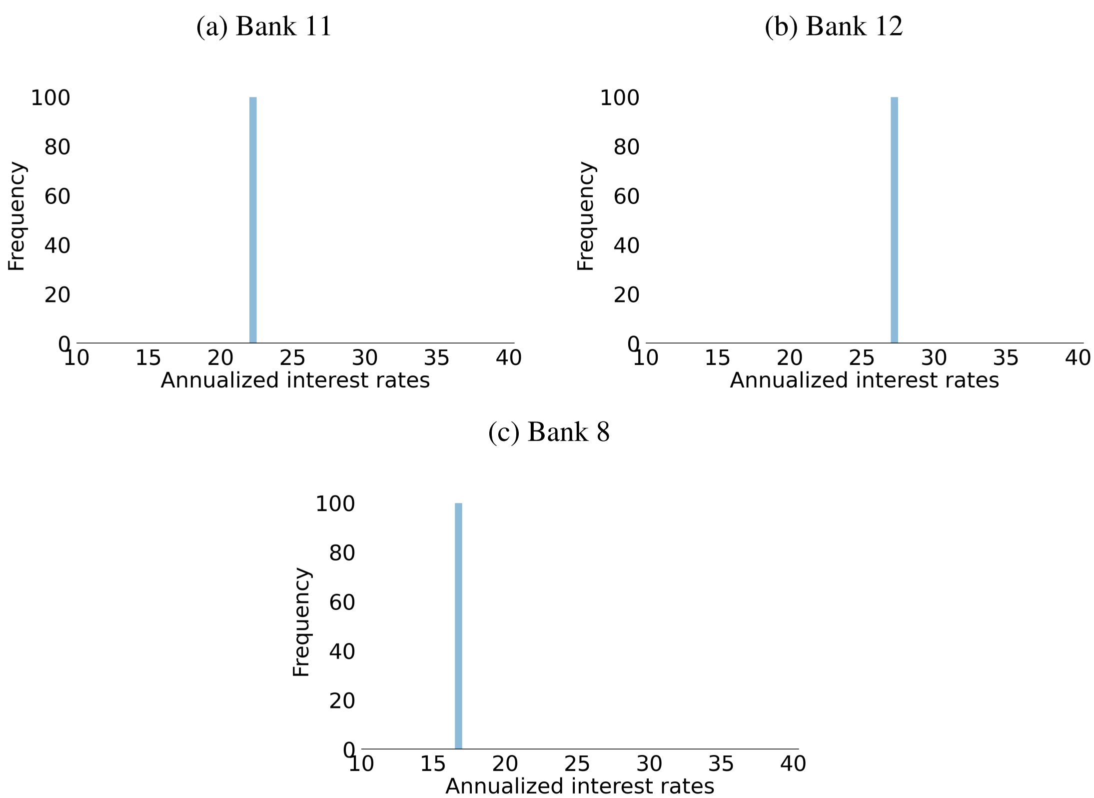
First, we consider the possibility that consumers changed their loan characteristics from their baseline requirements to originated loan terms. For example, a participant might go on a bank website, enter their baseline characteristics, and get an initial rate estimate. Participants may then change the characteristics of their loan in response to this estimate. If the estimate is more than the participant can afford, they may reduce the amount they borrow or extend the maturity of their loan to reduce their monthly payment. Consequently, the discrepancy between simulated and actual rates may be entirely explained by borrowers changing their loan terms. We plot the observed difference between rates participants would have observed on the bank simulator and the rate they took the loan out at the same bank on the difference in loan size and maturity between the baseline survey and their actual loan terms. The scatter plots are presented in Figure B.3 for banks and Figure B.4 for the third-party comparison tool. Loan size differences are presented in panel a and maturity differences in panel b. The majority of difference points between baseline loan size and maturity are clustered along the vertical line at zero. However, there is still substantial variation in interest rates received despite these main loan terms not changing. We regress the rate difference on the differences in loan terms and find that the R^2 of these regressions are 0.173 for loan amount and 0.265 for maturity, respectively. This suggests that the differences between the observed and simulated rates can be explained by participants changing their loan terms throughout their search.

Figure B.1: Test of Whether Bank Simulator Terms Vary by RUT



This figure shows the annualized interest rates from bank websites, derived from a test where we varied only the RUT while maintaining other inputs constant. Specifically, we standardized inputs such as income, loan amount, and loan maturity by using the median values from the baseline sample: a monthly income of $740,000 pesos, a loan amount of $2,500,000 pesos, and a maturity period of 3 years. The x-axis represents the annualized interest rates calculated by each bank's website simulation. Each simulation consists of 100 observations.

Figure B.2: Test of Whether Bank Simulator Terms Vary by Area Code

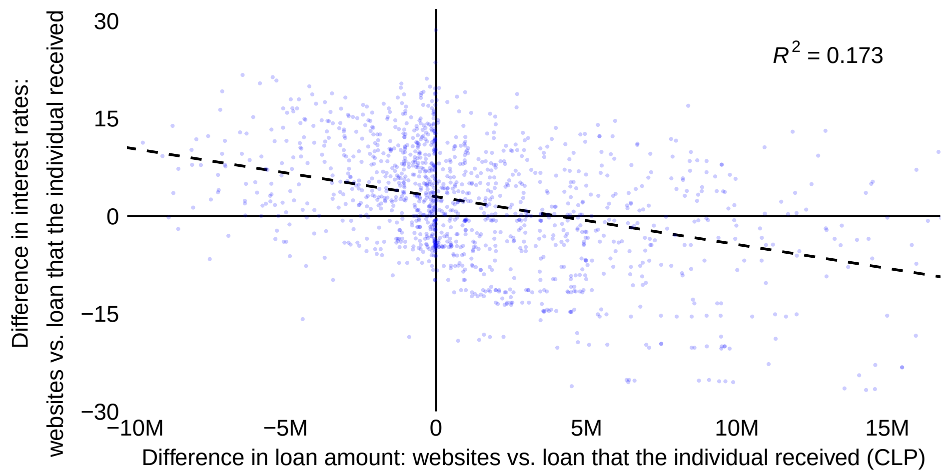


This figure shows the annualized interest rates from bank websites, derived from a test where we varied only the area code while maintaining other inputs constant. Specifically, we standardized inputs such as income, loan amount, and loan maturity by using the median values from the baseline sample: a monthly income of $740,000 pesos, a loan amount of $2,500,000 pesos, and a maturity period of 3 years. The x-axis represents the annualized interest rates calculated by each bank's website simulation. Each simulation consists of 100 observations.

Figure B.3: Difference in Interest Rates Between Bank Websites and Loan That the Individual Received

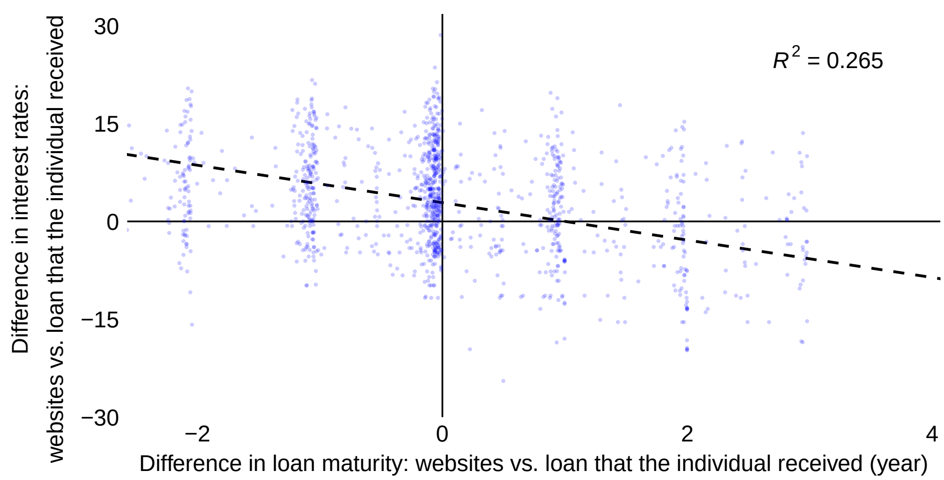
Difference in Interest Rates vs. Difference in Loan Amount

(a) Difference in Loan Amount



Difference in Interest Rates vs. Difference in Loan Maturity

(b) Difference in Loan Maturity



This figure shows the correlation of the difference in interest rates with the difference in loan amount (panel a) or the difference in loan maturity (panel b). Interest rates on bank websites are the rates displayed on consumer loan simulation websites, given an individual's baseline survey inputs. The difference in interest rates is calculated as: interest rates on websites - interest rates of the loan that the individual received. The difference in loan amount (maturity) is calculated as: loan amount (maturity) from the baseline survey - loan amount (maturity) of the loan that the individual received. The variables on the *x*-axis, difference in loan amount (maturity), are winsorized at both the 5th and the 95th percentiles within each treatment group. The dashed line is the line of best fit from a linear regression of the difference in interest rates on the winsorized difference in loan amount or loan maturity. The R^2 at the top-right corner of the plot corresponds to the line of best fit. The number of observations is 1,659. For legibility, the bottom and top 5th percentiles of difference in loan amount are excluded from panel a, and the bottom 5th and top 10th percentiles of difference in loan maturity are excluded from panel b.

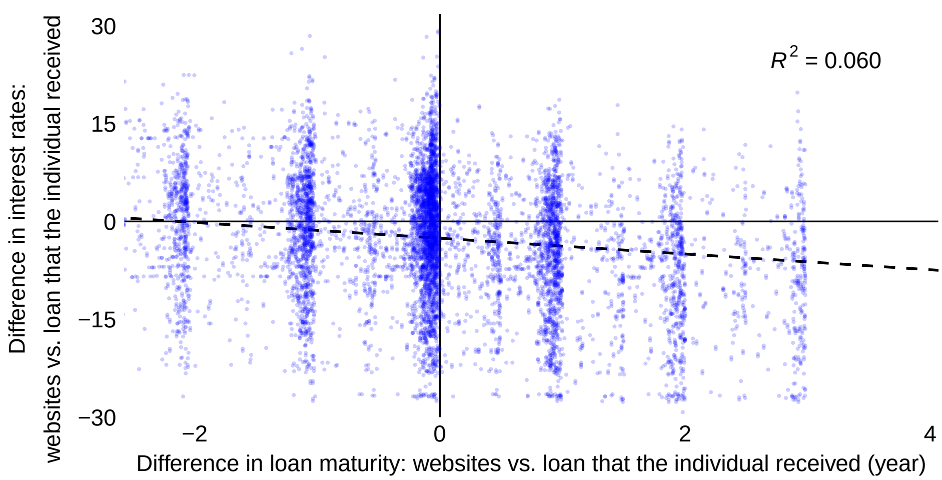
Figure B.4: Difference in Interest Rates Between Third-Party Tool and Loan That the Individual Received

Difference in Interest Rates vs. Difference in Loan Amount

(a) Difference in Loan Amount

Difference in Interest Rates vs. Difference in Loan Maturity

(b) Difference in Loan Maturity



This figure shows the correlation of the difference in interest rates with the difference in loan amount (panel a) or the difference in loan maturity (panel b). Interest rates on bank websites are the rates displayed on consumer loan simulation websites, given an individual's baseline survey inputs. The difference in interest rates is calculated as: interest rates on websites - interest rates of the loan that the individual received. The difference in loan amount (maturity) is calculated as: loan amount (maturity) from the baseline survey - loan amount (maturity) of the loan that the individual received. The variables on the *x*-axis, difference in loan amount (maturity), are winsorized at both the 5th and the 95th percentiles within each treatment group. The dashed line is the line of best fit from a linear regression of the difference in interest rates on the winsorized difference in loan amount or loan maturity. The R^2 at the top-right corner of the plot corresponds to the line of best fit. The number of observations is 12,695. For legibility, the bottom and top 5th percentiles of difference in loan amount are excluded from panel a, and the bottom 5th and top 10th percentiles of difference in loan maturity are excluded from panel b.

Appendix C Price Comparison and Simple Tool Construction

C.1 Price Comparison Tool

We detail how we processed the data from administrative, loan-level data from the CMF into histograms that participants saw in the price comparison tool treatment arm.

First, existing loans are separated to 4x4 matrix by income and loan amount quartiles within a comuna. Thus, for each comuna, there are a total of 16 different histograms a borrower could see based on their loan amount and income inputs, where the quartile cutoffs for both income and loan amount vary by comuna. In order to ensure that the loan histograms do not contain identifiable information, we impose the following two conditions on the histograms:

1. There are at least five data points in total and at least two distinct rounded interest rates.

2. Both her declared income and loan amount fall within the ranges of the data to be shown to her (with ranges calculated as 0.75 * Min – 1.25 * Max, for both variables).

If the above two conditions are met, then the histogram is displayed as in Figure 2a, with the caveat that data points $\geq |5|$ standard deviations away from the rounded interest rate mean will have been removed. The share of comunas that had enough data to show comuna-level histograms was 87.5%. The share of participants that saw comuna-level histograms was 82.2%.

If one of the conditions fails, then the geographic range is expanded to contiguous comunas. Existing loans in the comuna and all contiguous comunas are again sorted into a 4x4 matrix by income and loan amount quartiles within the first-degree neighbours. If the above two conditions are met, a histogram is returned. A message also accompanies the histogram specifying that there was insufficient data for their comuna, and that they are seeing information that includes neighbouring comunas. The share of comunas that did not have enough data for comuna-level histograms but did had enough data to show first-degree neighbour comuna-level histograms was 10%. The share of participants that saw first-degree neighbouring comuna histograms was 4.6%.

If one of the conditions fails, then the geographic range is expanded to second-degree contiguous comunas and the steps in the above paragraph are repeated. A message also accompanies the histogram specifying that there was insufficient data for their comuna, and that they are seeing information that includes neighbouring comunas. The share of comunas that had enough data to show second-degree neighbour comuna-level histograms but not enough data for more fine-grained geography histograms was 2%. The share of participants that saw second-degree comuna matching was 0.6%. If a histogram still could not be created, then a histogram using a 4x4 income and loan size quartile data for the region (equivalent to state) is used with a corresponding message that the information displayed is at the regional level. The share of comunas that required regional data was 0.5%. The share of participants that saw regional data was 8.4%.

How many participant searches?

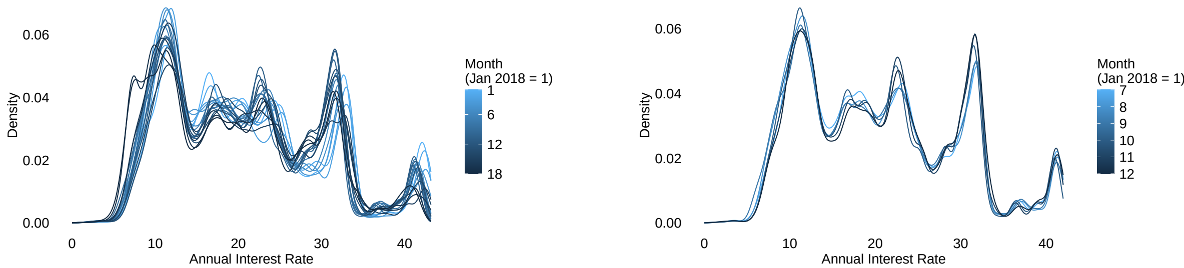
C.1.1 Length of Past Data Shown

Loans differ from other products in that consumers cannot merely compare the current prices of loans at different banks and decide which to buy; instead, they must apply for a loan at each bank that they want to include in their comparison and see whether they are approved. Thus, we view a tool based on actual loans that were obtained by similar consumers in the market as more relevant than a lot of existing price comparison tools that instead collate current information on rates that banks report that they would offer to consumers of different types. The information banks report on current rates that could be collated in this way is (i) not sufficiently dis-aggregated by consumer type and (ii) an inaccurate measure of the rates consumers actually receive, because the information banks are required to report is what loan and interest rate a consumer would be technically eligible for (but this does not reflect the probability that a consumer of that type is approved for the loan in practice). By showing the distribution of interest rates that were actually obtained by similar consumers (based on income and neighbourhood of residence), we provide consumers with a sense of what loans they could actually obtain in the marketplace. Since they see the entire distribution of APRs, they will also have a sense of the probability of banks offering various rates and of being approved for those rates.

In order to provide consumers with this personalized data based on loans actually obtained by similar consumers, we necessarily have to use "historical" data that goes a certain distance back in time. (For example, if we only used data from the past month rather than the past 18 months, there would not be enough observations within each cell defined by consumer and loan characteristics to show the distribution of rates.) Thus, we face a trade-off between how recent the data used by the tool are—which is more relevant if the distribution of interest rates changes over time—and how much information we have to show each consumer. We determined that using 18 months of data goes too far into the past given that the distribution of interest rates does change over time. In Figure C.1a below, we show the distribution of interest rates for consumer loans in each of the 18 months between January 2018 and June 2019.

On the other hand, we determined that the distribution of interest rates is relatively stable over six months, as shown in Figure C.1b. Furthermore, using data from the last six months still provides sufficient observations within each cell to show consumers a distribution of prices faced by similar consumers for similar loans. Furthermore, we will refresh the data underlying the tool each month so that the tool always shows the most recent 18 months of data.

Figure C.1: Distribution of Interest Rates by Time



(a) 18 Months

(b) 6 Months

This figure shows the distribution of interest rates obtained by consumers for consumer loans, with data sourced from the CMF (Financial Market Commission). Panel (a) displays the distribution of interest rates over the 18 months from January 2018 to June 2019. It illustrates how interest rates have varied over time, providing insight into trends in the loan market during this period. Panel (b) shows the distribution of interest rates for the most recent six months from July 2018 to December 2019. It demonstrates that despite monthly variability, the overall distribution of interest rates is relatively stable within this shorter timeframe, allowing for a more accurate and up-to-date comparison for consumers.

C.2 Simple Tool

We use loan-level CMF data to estimate the benefits of searching at more banks. First, we subset the data to originated loans in a given municipality, income quartile, loan size, and maturity for the last six months. This is equivalent to the data the participant would have seen if they were assigned to the price comparison tool. Within a given bin, there are J banks that have originated L loans to borrowers. We randomly draw an interest rate l_0 from bank j and consider it the participant's "first offer". We then draw another quote (l_1) from the remaining $J-1$ banks. We then use the bin maturity and loan size to calculate the monthly interest payments of loan l_0 and l_1. We then consider two cases:

If $l_1 < l_0$, we calculate the present value of the difference in monthly payments over the life of the loan.

If $l_1 \geq l_0$, we set the "benefit of searching at one additional bank" to zero as the participant could take out the first loan.

We then repeat this drawing of two quotes 1,000,000 times. We then set the "benefit of searching at one additional bank" to be the mean of all the present value differences in monthly payments. Thus, the benefit of searching is always a non-negative number, though zeros are included in the average.

To find the benefit of searching at $n \in \{3,4,5\}$ additional banks, we simulate the process for drawing l_n loans from $J-n$ banks 1,000,000 times and take the mean. As before, all benefits are calculated in relation to the first draw l_0, and any differences in monthly payments that are greater than or equal to l_0 are coded as zeros.

We repeat this procedure for all constructed bins. As in the loan price comparison tool, if there are less than 5 loans issued in a bin and less than two unique interest rates in the bin, we expand the bin to include comunas that border our reference comuna. If there are still less than 5 loans and two unique rates, we expand the bin again to include comunas that border the bordering comunas to the reference comuna.

Appendix D Google Search

D.1 Obtaining data from Google Search results

The data were scraped by mimicking users searching from different comunas with various search terms. For each search, we randomly selected a comuna-search term pair. The comuna population data are derived from our baseline survey and weighted by the number of participants from each comuna. The search term population is sourced from our Google Ad campaign. We collected the search terms that led people to our price comparison tool and weighted them by their frequency in searches. During each search, we changed the geolocation parameter in Google to match the selected comuna and searched Google using the selected term. We scraped all available information from each result on the first page, including the content provider, link, title, text snippet, and the position of the results on the page. The scraper ran from November 8th, 2023, to February 4th, 2024, resulting in 6,677,889 Google search results from 101,852 comuna-search term pairs.

We scraped these results using desktop emulator, mimicking what a user would see if they opened Google on a desktop computer. Ideally, we would have also scrape the same results using mobile emulation to check if people see anything different when searching Google on mobile phones. However, we were unable to manipulate location information with mobile emulation. This is because Google adopts different functions to determine the user's location, with the geolocation parameters of the browser used on desktops, and the user's IP address used on mobile phones. It was not operationally feasible to fake the IP addresses of each Chilean comuna, so we were unable to scrape the mobile results.

We used OpenAI's Assistant API to extract variables from raw text scraped from Google Search results pages. We also tested traditional text processing techniques to extract these variables from the Google search results, but found that employing an advanced Large Language Model (LLM) like GPT-4 yielded higher accuracy for this complicated natural language processing tasks. To extract interest rate numbers from raw scraped text with rule-based text processing code, we would have to exhaust every possible pattern in which an interest rate could occur in a sentence, as well as exclude all possible false positive cases. This task becomes increasingly more challenging as the number of observations increases. For instance, in each of the examples presented in Table D.1, the percentage number in the sentence carries a distinct meaning.

On the other hand, a well-trained LLM will be able to comprehend the whole sentence and correctly identify whether it contains a consumer loan interest rate. At the time of our data processing, OpenAI provides two APIs, Chat API and Assistant API. The Assistant API allows users to create and tailor an "assistant" for a specific task and use it repeatedly. It also contains built-in tools tuned for particular tasks, including "code_interpreter", "retrieval", and "function".

We used the "gpt-4-turbo-preview" model of the Assistant API, the state-of-art text processing model at the time, along with its built in tool “retrieval”. The key variables to extract were interest rates and the corresponding banks that offered the rates. We also configured the assistant to identify the language, country, and loan type, so that we could filter only results that were Spanish-language consumer loan-related results from Chile. We also had the assistant identify whether the interest rate is a monthly or annual rate and whether the interest rate excluded fees or was an APR including fees. The prompt we sent to the API can be found in Section D.3.

Given that it is a closed-source LLM, the results generated by our assistant may not be fully reproducible in the future due to the stochastic nature of the model and model updates.

D.2 Comparison of Google Search Displayed and Received Rates

To compare rates that participants in our RCT would have seen on Google to rates that they actually received, we must match our scraped Google Search data with the administrative loan data. Initially, we restrict our sample to the 30,979 individuals in the administrative data who had taken a loan. For each of these participants, we keep one unique consumer loan from the administrative data, which is the first loan taken after treatment based on the date that the individual participated in the RCT and the date that the loan was taken out. In the rare case that the individual took two loans on the same day (0.55% of the sample), we take the largest loan taken on the first day after treatment that any loans were taken out by that individual.

Next, for each individual who took out a consumer loan, we matched the interest rates they would have seen on Google—based on comuna and bank—with the interest rate they received in the administrative data. We then annualized any monthly interest rates by multiplying them by 12 and excluded any scraped results that included only CAE but not interest rates.

We further restrict to loans obtained during the period in which we ran the script to scrape Google search results, which was from October 24, 2023, to February 4, 2024. While this limits the sample size, it mitigate the concern that any observed differences could be due to changes in interest rates over time.

D.3 Prompt for the Assistant API

Task: Analyze text scraped from Google Search results to extract and organize loan-related data, with a focus on interest rates, Equivalent Annual Cost (CAE), and other pertinent details. Given that our research is centered on a sample of Chilean loan takers, avoid relying on “common sense” assumptions typical of English-speaking countries when making inferences in your analysis.

Interest rate data should only be included if explicitly referred to in the context of borrowing or lending. It’s ok if no clear period for the rate is provided. Do not extract percentages or fees that refer to one-time charges, service fees, transaction costs, etc. When unsure, provide a descriptive note regarding the ambiguity rather than extracting incorrect data.

Input Format: JSON-formatted strings sent directly as text snippets.

Output Format: Format your findings in JSON with the following variables:

1. tasa_anual: A dictionary mapping bank names to their respective annual interest rates (include both nominal and effective rates). If specific bank names are not mentioned, mark the bank name as "unknown". Note that if the returned variable is a dictionary, the key should always be the name of a bank. Do not use nested dictionaries. If you try to infer the time frame of the interest rate when it's not given, write your reasoning under the "note" variable described below.

2. tasa_mensual: A dictionary mapping bank names to their monthly interest rates. Similar rules as tasa_anual for unnamed banks.

3. tasa_unidentified: A dictionary mapping bank names to unclear time frame interest rates. Similar rules as tasa_anual for unnamed banks. Avoid categorizing percentages that are not related to interest rates under this variable.

4. cae: A dictionary mapping bank names to their 'Carga Anual Equivalente' (CAE), excluding any values included in apr_number. CAE, a term commonly used in Spanish-speaking countries, is analogous to the APR (Annual Percentage Rate). It denotes the effective annual cost of a loan, encompassing both interest and additional charges. Apply the same rules as for tasa_anual when bank names are not specified. Populate this variable only when the term 'CAE' is explicitly mentioned. If 'CAE' is not directly referred to, use apr_var and apr_number, as outlined below.

5. apr_var: A string indicating a non-CAE APR term (like APR, TEA, CAT).

6. apr_number: A dictionary mapping bank names to their APR value. If it's a "CAE" in the direct term, list the values in "cae" instead. If this variable is not NaN, the previous variable "apr_var" must not be NaN. Similar rules as tasa_anual for unnamed banks.

7. loan_type: Classify the type of loan or financial product as "consumer_loan", "mortgage_loan", "credit_card", "deposit", "policy", or "unknown" (where "policy" refers to the central bank's policy rate). Based solely on the given text. If you try to infer the loan type based on the text, write your reasoning under the "note" variable described below.

8. language: Language abbreviation (e.g., "es" for Spanish).

9. country: Country where the loan is offered, or "unknown" if uncertain.

10. note: If uncertain about the context of a percentage figure, provide a descriptive note to explain the ambiguity. Additionally, explicitly state any implicit assumptions made while interpreting the text.

Special Instructions:

- In cases where multiple banks or entities are mentioned with specific rates, organize this data in a dictionary format under the relevant variable (e.g., tasa_anual).
- Do not perform rate calculations.
- If a search result lacks financial data but is relevant to banks or loans, still return a JSON object with variables 8 and 9. Ensure no variable contains an empty list. Exclude variables from the JSON output if there are no values to report.
- Do not assume the time frame of an interest rate unless it is explicitly mentioned.
- Use your best judgment for determining “language” and “country”. For consumer loans, do not assume that the time frame of the interest rate is annual; in some countries, monthly rates are more commonly used.

Examples:

Example 1:

Input:

{'content_provider': 'condusef.gob.mx', 'link': 'https://www.condusef.gob.mx > ...', 'title': '¿Sabes cuál es la tasa de interés y el CAT que te cobran por tu crédito de ...', 'question': '¿Cuál es el banco que ofrece la mejor tasa de interés?', 'text': 'respecto a este producto. La tasa de interés anual más alta para este tipo de productos la cobra Banorte con 44%, seguida de Banregio con 43%, y posteriormente se ubica HSBC con 39.9%; en tanto que el CAT más alto es igualmente de Banorte con 63.1%.'}

Output:

{“tasa_anual”: {”Banorte”: 44, ”Banregio”: 43, ”HSBC”: 39.9}, ” apr_number”: {”Banorte” : 63.1}, ”apr_var”: ”CAT”, ”loan_type”: ”unknown”, ”language”: ”es”, ”country”: ”Mexico”}

Example 2:

Input:

{'content_provider': 'mrfinan.com', 'link': 'https://mrfinan.com/mx/prestamos/prestamo -hasta-100000-pesos', 'title': 'Préstamo hasta 100 mil Pesos', 'text': 'Préstamos hasta 100 mil pesos. 2 MINUTOS | GRATIS | SIN COMPROMISO. 3- 36 Meses. En Buró de Crédito. CAT mínimo 1.58%'}

Output:

{ "apr_var": "CAT", "apr_number": {"unknown": 1.58}, "loan_type": "consumer_loan", "language": "es", "country": "Mexico" }

Example 3:

Input:

{'content_provider': 'didiglobal', 'link': 'https://web.didiglobal.com/mx/prestamos/', 'title': 'DiDi Préstamos - Rápido, Fácil y Seguro. | DiDi México', 'text': 'Deja tú lo fácil que es solicitar un préstamo, la tasa de interés ordinaria va desde el 5% hasta el 12%. (*Tasa ordinaria mensual estimada).'}

Output:

{ "tasa_mensual":{ "DiDi México": [5, 12] }, "loan_type": "consumer_loan", "language": "es", "country": "Mexico" }

Table D.1: Examples of Non-Interest Rate Percentage Number

Original text (in Spanish)	English translation	Meaning of the percentage number
May 23, 2017 — 776 (Banco Condell). Es decir, una diferencia de $856.692 (33,9%) entre el monto más barato y el más caro. SIMULADOR DE CRÉDITO. El mismo Sernac ...	May 23, 2017 - 776 (Condell Bank). That is, a difference of $856,692 (33.9%) between the cheapest and the most expensive amount. CREDIT SIMULATOR. The same Sernac ...	difference
Requisitos · Impuesto al Crédito: 0.066 % por fracción de mes, aplica sobre el monto total del crédito. · Impuesto al Crédito: Tope máximo 0.8 % equivalente a ...	Requirements - Credit Tax: 0.066 % per fraction of month, applied on the total amount of the credit. - Credit Tax: Maximum cap of 0.8 % equivalent to ...	tax
Sistemáticamente, el Banco de Chile (CHILE) ha sido el banco más rentable de Chile a lo largo de los años, con un ROA medio del 1,8% en los últimos 10 años, superando a toda su competencia local. Como comparación, su ROA es 30 puntos básicos más que Banco Santander Chile (BSANTANDER).	Systematically, Banco de Chile (CHILE) has been the most profitable bank in Chile over the years, with an average ROA of 1.8% over the last 10 years, outperforming all of its local competition. As a comparison, its ROA is 30 basis points higher than Banco Santander Chile (BSANTANDER).	ROA
Ahorra hasta un 15% en tasas de interés en tu crédito para compra vehicular en Compara Online ... El equipo de RadarCupón te aconseja: Compara Online te regala ...	Save up to 15% on interest rates on your vehicle purchase credit at Compara Online ... The RadarCoupon team advises you: Compara Online gives you free ...	discount
El límite que se suele establecer es de entre un 25% y 35% de tus ingresos, es decir, si la cantidad que has pedido supera en este porcentaje a tus ingresos lo más normal es que el banco deniegue tu solicitud y te quedes sin el préstamo o crédito que habías pedido.	The limit that is usually established is between 25% and 35% of your income, that is to say, if the amount you have requested exceeds your income by this percentage, the bank will normally deny your application and you will not receive the loan or credit you had requested.	percentage

This table shows examples of sentences that contain percentage numbers which are not interest rates.

Appendix E Predictors of Biased Beliefs

We first assess whether baseline characteristics predict how biased participants' beliefs are. To do this in a data-driven way that guards against overfitting with many covariates, we estimate a series of regularized regression models using elastic net (Friedman, Hastie, and Tibshirani, 2010). The outcome variables measure how biased an individual's beliefs are, compared to the distribution of interest rates that they face conditional on borrower and loan characteristics. For the interest rate, unlike in Figure 3, we take the difference between their belief about the rate they expect to obtain to the median of the conditional distribution they face rather than to the rate they actually obtained to avoid losing sample size (as the majority of participants did not obtain a loan within one year after participating in the RCT). In other words, we compute

$$r_i^{bias} \equiv r_i^{belief} - r_i^{admin} \tag{4}$$

For dispersion, we take the range between the highest and lowest rates they thought a bank would offer them as their prior about dispersion and subtract the corresponding range from the conditional distribution, as in Figure 4:

$$dispersion_i^{bias} \equiv \left(\overline{r}_i^{belief} - \underline{r}_i^{belief}\right) - \left(\overline{r}_i^{admin} - \underline{r}_i^{admin}\right) \tag{5}$$

For each measure r_i^{bias} and $dispersion_i^{bias}$, we then create four measures: (i) the measure as defined in equations (4) and (5), which can take positive or negative values; (ii) its absolute value; (iii) a binary indicator for underestimating, $r_i^{bias} < -1\,\text{pp}$ and $dispersion_i^{bias} < -1\,\text{pp}$; and (iv) a binary indicator for overestimating, $r_i^{bias} > 1\,\text{pp}$ and $dispersion_i^{bias} > 1\,\text{pp}$.[20]

For each model, we use 20-fold cross-validation to identify the best combination of the regularization strength (λ) and mixing parameters (α), to minimize the mean squared error (MSE), where α interpolates between ridge and lasso penalties. We then extract the estimated coefficients from the model corresponding to the optimal (λ, α) pair for each outcome. This approach allows us to isolate the most predictive features while reducing overfitting.

Results. Table E.1 presents the coefficient estimates from the selected elastic net models that render the minimum MSE. Individuals with more-biased beliefs tend to be younger and have lower incomes, less education, and less experience with financial products (i.e., bank accounts and loans). Furthermore, those looking for a smaller loan amount also tend to have more-biased beliefs.

[20]We allow for slight biases within ± 1 pp without considering these under- or overestimating.

Table E.1: What Predicts Biased Beliefs?

	Prior minus administrative data							
	Interest Rate				Dispersion			
	Level (1)	Abs. value (2)	Under (3)	Over (4)	Level (5)	Abs. value (6)	Under (7)	Over (8)
Personal characteristics								
Age	-0.304	-0.217	0.002	-0.001	-0.485	-0.295	0.003	-0.003
log(Income)	-0.966	-4.852	-0.031	0.014	-3.024	-2.577	0.028	-0.027
Incomplete high-school	11.663	11.119	-0.072	0.061	9.453	8.718	-0.042	0.048
Complete high-school	3.593	4.286	-0.025	0.034	3.963	4.226	-0.021	0.028
Complete 2-year program	1.012	1.907	0.001		0.925	1.259		0.011
Financial products								
Bank account	-0.606	-1.513			-2.585	-2.559	0.021	-0.027
Any loan	-1.315	-2.303		-0.000	-2.137	-1.963	0.004	-0.011
Desired loan characteristics								
log(Loan Amount)	-0.867	-5.866	-0.021	0.006	-0.697	-6.241		-0.007
log(Maturity (years))	0.551		0.004		-2.541	-1.564	0.026	-0.024
Observations	14,401	14,401	14,401	14,401	13,677	13,677	13,677	13,677

This table shows coefficient estimates from regularized regression models using elastic net. For each outcome variable—capturing biased beliefs about the interest rate a participant will obtain (defined in equation (4)) and interest rate dispersion (defined in equation 5)—we regress the outcome on a common set of covariates. The model includes baseline characteristics from Table 2 and missingness indicators, with missing values imputed by sample means (the coefficients for the missingness indicators are omitted from the table for legibility). Among the education categories in the baseline characteristics, we omit the indicator for whether a participant completed a 5-year degree program or higher to avoid perfect multicollinearity. For each outcome, we use 20-fold cross-validation to select the best combination of the regularization strength (λ) and mixing parameters (α), with the objective of minimizing mean squared error. The displayed coefficients correspond to the estimates at the optimal (λ, α) pair for each outcome. Coefficients shrunk to zero are left blank.

Appendix F Testing for Heterogeneous Treatment Effects

To test for heterogeneous treatment effects of the price comparison tool in a disciplined manner, we implement the machine-learning methodology proposed by Chernozhukov, Demirer, Duflo, and Fernández-Val (2025). We focus on whether there is predictable heterogeneity in the treatment effect of the price comparison tool on belief updating and on search behavior and loan outcomes as a function of baseline characteristics.[21] We measure belief updating the same way as in equation (1), i.e., $Posterior_i - Prior_i$ for the rate the participant expects to obtain, the lowest and highest rates a bank would offer them, and the dispersion in rates.

Following the description of Chernozhukov, Demirer, Duflo, and Fernández-Val (2025) in Gertler, Higgins, Scott, and Seira (2025), we consider the conditional average treatment effect (CATE):

$$s_0(Z) := E[Y(1) \mid Z] - E[Y(0) \mid Z],$$

where $Y(1)$ is the potential outcome if treated with the price comparison tool and $Y(0)$ is the potential outcome if assigned to the control group. Z is a vector of baseline characteristics.[22] Chernozhukov, Demirer, Duflo, and Fernández-Val (2025) use machine learning (ML) proxies $S(Z)$ for the CATE, $s_0(Z)$, and provide a method to formally test whether there are heterogeneous treatment effects without data mining.

The best linear predictor (BLP) of $s_0(Z)$ given $S(Z)$ is the solution to

$$\min_{b_1,b_2} \mathrm{E}\left[s_0(Z) - b_1 - b_2 S(Z)\right]^2,$$

which, if it exists, is

$$\mathrm{BLP}\left[s_0(Z) \mid S(Z)\right] = \beta_1 + \beta_2\left(S(Z) - \mathbb{E}[S(Z)]\right),$$

where $\beta_1 = \mathbb{E}[s_0(Z)]$ and $\beta_2 = \mathrm{Cov}\left[s_0(Z), S(Z)\right] / \mathrm{Var}[S(Z)]$. If $S(Z)$ is a perfect proxy for $s_0(Z)$, then $\mathrm{Cov}\left[s_0(Z), S(Z)\right] = \mathrm{Var}[S(Z)]$ and thus $\beta_2 = 1$. In general, $\beta_2 \neq 1$ due to noisy predictions $S(Z)$. If $S(Z)$ is purely noise and uncorrelated with $s_0(Z)$, then $\beta_2 = 0$. Furthermore, if there is no heterogeneity, i.e., $s_0(Z) = s$, then $\beta_2 = 0$ because $\mathrm{Cov}\left[s_0(Z), S(Z)\right] = \mathrm{Cov}\left[s, S(Z)\right] = 0$. Rejecting the hypothesis that $\beta_2 = 0$ therefore indicates that both (i) $S(Z)$ is a relevant predictor and, more importantly for our purposes, (ii) there is heterogeneity in $s_0(Z)$. Therefore, by estimating the β_2 coefficient of $\mathrm{BLP}\left[s_0(Z) \mid S(Z)\right]$ and testing whether the coefficient is significantly different from

[21] We use the same set of baseline characteristics as in Table 2: age, monthly income, education categories, binary variables for whether the participant had a bank account and had previously taken out a loan, and desired loan terms (loan amount and maturity). We omit the indicator for whether a participant completed a 5-year degree program or higher to avoid perfect multicollinearity.

[22] Missing baseline characteristics are imputed with sample means and missingness indicators are included in Z as well.

zero, we can empirically test for heterogeneity in the treatment effect across individuals.[23]

In the ML step, we consider support vector machine, LASSO, and random forest, and choose the model that maximizes $\Lambda \equiv |\beta_2|^2 \operatorname{Var}(S(Z)) \propto \operatorname{Corr}^2(s_0(Z), S(Z))$.[24] Maximizing Λ is equivalent to maximizing the correlation between the ML proxy $S(Z)$ and the CATE $s_0(Z)$, or equivalent to maximizing the R^2 in the regression of $s_0(Z)$ on $S(Z)$. The models that maximize Λ for our analysis on $Posterior_i - Prior_i$ are as follows: LASSO for expected rate, lowest rate, and the highest rate; and random forest for dispersion. The models that maximize Λ for our analysis on search behavior and loan outcomes are as follows: LASSO for number of offers; support vector machine for number of institutions searched, Pr(negotiate), log interest rate offered (all measured in survey data), as well as Pr(take loan) measured in administrative data; and random forest for number of institutions applied, Pr(take loan), log interest rate taken (all measured in survey data), as well as log interest taken measured in administrative data.

F.1 Belief Updating

First, we reject the null hypothesis of no heterogeneity in treatment effects of the tool for belief updating about the rate participants expect to obtain and the highest rate a bank would offer them, but we fail to reject the null hypothesis of no heterogeneity for the lowest rate a bank would offer them and dispersion (Table F.1).

Given that we detected heterogeneous treatment effects for updating about the expected and highest interest rates, we follow Chernozhukov, Demirer, Duflo, and Fernández-Val (2025) to determine what characteristics predict treatment effects of the tool on belief updating. To do this, the data is split into training and estimation samples, and after training the ML model on the training sample, the model is used to estimate predicted individual treatment effects for each individual in the estimation sample. Individuals in the estimation sample are then grouped into quintiles based on predicted individual treatment effects, and "group average treatment effects" (GATES) for each quintile are estimated. Next, differences in the average characteristics Z across the groups with the highest and lowest predicted treatment effects (i.e., the most-affected and least-affected groups) are estimated in a classification analysis (CLAN).

Results. Table F.2 presents a GATES analysis, estimating treatment effects for the quintiles with the lowest and highest predicted treatment effects. Consistent with our findings in Table F.1, we find that the difference in treatment effects of the tool for the most- and least-affected quin-

[23]The coefficient estimates will depend on the data split that takes place before training the machine learning models. The coefficient estimates, p-values, and lower and upper bounds of their confidence intervals that we report in Table F.1 and Table F.4 are the medians of the estimates and confidence intervals from 1,000 such distinct splits, as recommended by Chernozhukov, Demirer, Duflo, and Fernández-Val (2025).

[24]More specifically, since each of the 1,000 data splits leads to a different Λ, we maximize the median Λ from the 1,000 data splits.

Table F.1: Test for Heterogeneous Treatment Effects on Belief Updating

	Expected rate (1)	Lowest rate (2)	Highest rate (3)	Dispersion (4)
ATE	16.04***	10.81***	30.08***	15.83***
	[0.000]	[0.000]	[0.000]	[0.000]
	(13.17, 18.94)	(8.52, 13.10)	(24.55, 35.64)	(12.22, 19.43)
HTE	0.58***	0.35	0.56***	0.15
	[0.001]	[0.141]	[0.001]	[0.211]
	(0.24, 0.91)	(-0.12, 0.83)	(0.22, 0.90)	(-0.09, 0.38)
Observations	4,699	4,651	4,578	4,302
Control Mean Posterior	29.22	22.65	47.45	23.18
Control Median Posterior	18	12	25	10.68
Bin Density FEs	Yes	Yes	Yes	Yes

This table shows average treatment effect (ATE) coefficient estimates and heterogeneous treatment effect (HTE) coefficient estimates from the Chernozhukov, Demirer, Duflo, and Fernández-Val (2025) machine learning procedure described in Appendix F. We conduct this test for the outcomes in Table 3. p-values testing the null hypothesis that the parameter is equal to zero are in square brackets, while 95% confidence intervals are in parentheses. Coefficients, p-values, and lower and upper bounds of confidence intervals are medians over 1,000 data splits. The bin density fixed effects (FEs) are deciles of how many loans are in the price comparison tool histogram that would have been shown to the participant had they been assigned to the price comparison tool arm, based on their borrower and loan characteristics. If any of the participant's loan amount, maturity, or income is outside of the support of the administrative data, the bin density variable would be a missing value; for these individuals, we create an additional fixed effect category that we include in the regression, i.e., we include an additional dummy equal to 1 if the bin density is a missing value. Missing baseline characteristics are imputed with sample means and missingness indicators are included as additional regressors. Among the education categories in the baseline characteristics, we omit the indicator for whether a participant completed a 5-year degree program or higher to avoid perfect multicollinearity. The smaller sample size compared to Table 3 is due to the exclusion of participants treated with the simple tool. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

tiles is statistically significant (column 3). Interestingly, although there are statistically significant heterogeneous treatment effects, the tool leads participants to increase their expectations about the rate they will obtain and dispersion even in the least-effected quintile (by 10.2 pp, compared to 24.7 pp in the most-affected quintile).

In contrast, and again consistent with the results in Table F.1, we find that the difference in the treatment effect between the most affected and the least affected quintiles for belief updating about the lowest rate and dispersion is not statistically significant at the 5% level.

In Table F.3, we present a CLAN analysis to understand which individual traits are associated with the largest impacts on belief updating for the two outcomes for which we observe a statistically significant heterogeneous treatment effect on the tool. We find that the same characteristics that predict more-biased beliefs also predict a larger treatment effect of the tool on beliefs about both the expected rate and the highest rate. In particular, the treatment effect of the tool is larger for participants who are younger and have lower incomes, less education, and less experience with financial products (i.e., bank accounts and loans), as well as those looking for smaller loans.

Table F.2: Heterogeneous Treatment Effects: GATES Analysis

	Most affected quintile (1)	Least affected quintile (2)	Difference (3)	Observations (4)
Expected rate	24.69***	10.23***	14.38***	4,699
	[0.000]	[0.002]	[0.002]	
	(18.31, 30.97)	(3.80, 16.67)	(5.34, 23.46)	
Lowest rate	15.08***	9.01***	6.24*	4,651
	[0.000]	[0.001]	[0.087]	
	(10.04, 20.17)	(3.92, 14.11)	(-0.93, 13.41)	
Highest rate	45.05***	19.22***	25.81***	4,578
	[0.000]	[0.002]	[0.003]	
	(32.94, 57.27)	(6.84, 31.55)	(8.53, 43.10)	
Dispersion	20.22***	13.24***	6.45	4,302
	[0.000]	[0.001]	[0.268]	
	(12.14, 28.24)	(5.18, 21.36)	(-4.93, 17.88)	

The table shows sorted group average treatment effect coefficients for the outcomes in Table 3. The sample is divided into 5 groups, based on the quintiles of the machine learning proxy predictor $S(Z)$. Coefficients, p-values, and lower and upper bounds of confidence intervals are medians over 1,000 data splits. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

F.2 Search, Negotiation, and Loan Terms

Across all of the tests on search and negotiation behavior and loan terms, we find no detectable heterogeneity in treatment effects (Table F.4). This suggests that the treatment effects of the tool on negotiation, the probability of taking out a loan, and interest rates do not differ by the characteristics of the participants, but rather tend to be spread evenly across participants.

Table F.3: Heterogeneous Treatment Effects: Classification Analysis (CLAN)

	Expected rate (1)	Lowest rate (2)	Highest rate (2)
Personal characteristics			
Age	-11.13***	-12.73***	-8.95***
	[0.000]	[0.000]	[0.000]
	(-12.23, -10.02)	(-13.83, -11.63)	(-10.15, -7.77)
log(Income)	-1.69***	-1.37***	-1.69***
	[0.000]	[0.000]	[0.000]
	(-1.88, -1.49)	(-1.56, -1.18)	(-1.88, -1.49)
Incomplete high-school	0.13***	0.05***	0.11***
	[0.000]	[0.000]	[0.000]
	(0.10, 0.16)	(0.03, 0.08)	(0.09, 0.14)
Complete high-school	0.54***	0.44***	0.46***
	[0.000]	[0.000]	[0.000]
	(0.49, 0.59)	(0.38, 0.49)	(0.40, 0.51)
Complete 2-year program	0.03	0.01	0.07***
	[0.173]	[0.559]	[0.004]
	(-0.01, 0.07)	(-0.03, 0.06)	(0.02, 0.11)
Financial products			
Bank account	-0.46***	-0.43***	-0.41***
	[0.000]	[0.000]	[0.000]
	(-0.51, -0.41)	(-0.48, -0.37)	(-0.46, -0.36)
Any loan	-0.32***	-0.38***	-0.34***
	[0.000]	[0.000]	[0.000]
	(-0.37, -0.26)	(-0.43, -0.33)	(-0.40, -0.29)
Desired loan characteristics			
log(Loan Amount)	-2.54***	-2.29***	-2.66***
	[0.000]	[0.000]	[0.000]
	(-2.70, -2.40)	(-2.45, -2.13)	(-2.81, -2.51)
log(Maturity (years))	-0.60***	-0.79***	-0.56***
	[0.000]	[0.000]	[0.000]
	(-0.67, -0.53)	(-0.86, -0.72)	(-0.63, -0.48)
Observations	4,699	4,651	4,578

Classification analysis for the outcomes in Table 3 that exhibit significant HTE coefficients in Table F.1 – expected rate and highest rate. The sample is divided into 5 groups, based on the quintiles of the CATE proxy predictor $S(Z)$. We report the mean difference in the baseline characteristics from Table 2 between the highest and lowest $S(Z)$ quintiles. For example, for age, we subtract the mean age of the lowest $S(Z)$ quintile from the mean age of the highest $S(Z)$ quintile. Among the education categories in the baseline characteristics, we omit the indicator for whether a participant completed a 5-year degree program or higher to avoid perfect multicollinearity. Coefficients, p-values, and lower and upper bounds of confidence intervals are medians over 1,000 data splits. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table F.4: Test for Heterogeneous Treatment Effects on Search, Negotation, and Loan Terms

	Survey Data							Administrative Data	
	N of inst. searched	N of inst. applied	N of offers	Pr(negotiate)	Log interest rate offered	Pr(take loan)	Log interest rate taken	Pr(take loan)	Log interest rate taken
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ATE	0.013	0.025	0.072	0.037*	-0.121	0.030	-0.103	0.008	0.013
	[0.893]	[0.724]	[0.119]	[0.057]	[0.115]	[0.308]	[0.234]	[0.189]	[0.292]
	(-0.183, 0.211)	(-0.117, 0.167)	(-0.019, 0.163)	(-0.001, 0.076)	(-0.272, 0.030)	(-0.028, 0.088)	(-0.276, 0.067)	(-0.004, 0.021)	(-0.011, 0.037)
HTE	0.171	0.314*	-0.010	0.261	0.225	-0.115	-0.390	0.292	0.208
	[0.424]	[0.081]	[0.970]	[0.472]	[0.360]	[0.560]	[0.432]	[0.118]	[0.109]
	(-0.249, 0.596)	(-0.040, 0.668)	(-0.560, 0.532)	(-0.453, 0.960)	(-0.258, 0.706)	(-0.506, 0.281)	(-1.355, 0.584)	(-0.069, 0.686)	(-0.047, 0.464)
Observations	2,172	2,095	2,084	2,065	354	2,081	232	30,718	5,982

This table shows average treatment effect (ATE) coefficient estimates and heterogeneous treatment effect (HTE) coefficient estimates from the Chernozhukov, Demirer, Duflo, and Fernández-Val (2025) machine learning procedure described in Appendix F. We conduct this test for the outcomes in Table 4. p-values testing the null hypothesis that the parameter is equal to zero are in square brackets, while 95% confidence intervals are in parentheses. Coefficients, p-values, and lower and upper bounds of confidence intervals are medians over 1,000 data splits. Missing baseline characteristics are imputed with sample means and missingness indicators are included as additional regressors. Among the education categories in the baseline characteristics, we omit the indicator for whether a participant completed a 5-year degree program or higher to avoid perfect multicollinearity. The smaller sample size compared to Table 4 is due to the exclusion of participants treated with the simple tool. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.