Consumer Misperceptions and Redistribution in Financial Products: Evidence from Brazilian Consórcios*

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Abstract

Financial innovations may expand access to credit, but also introduce complexity that leads to consumer losses and redistribution across consumers. We study *consórcios*, a group savings-and-credit product accounting for 25% of vehicle credit and 12% of housing credit in Brazil. Consumers pool monthly payments and receive funding through lotteries or bids, with bids allowing those with liquidity to obtain funding earlier at the expense of other participants. Using administrative data covering 59 million contracts and a survey of 1,296 consumers, we document four facts. First, 81% of contracts lose money relative to saving and borrowing in the market. Second, losses are regressive. Third, firms' allocation decisions favor bidding, which can improve expected returns, while consumers waiting for lotteries face long waits and large expected losses. Fourth, consumers overestimate the chance of receiving funding early through lotteries. These findings challenge explanations of demand based on the commitment benefits of group savings products and suggest that misperceptions about allocation probabilities may help sustain regressive contract designs. In a randomized information experiment, disclosing accurate information about lottery chances increases liquidity-constrained consumers' willingness to forgo an actual monetary for access to groups with more lotteries.

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1 Introduction

Access to credit shapes major household decisions, such as improving or buying homes and acquiring vehicles. Innovations promise to expand access, especially in developing economies where borrowing costs are high (Karlan and Morduch, 2010). Yet complexity and limited information can make financial products difficult to evaluate (Beshears et al., 2018). This raises the possibility that some consumers face financial losses, while others benefit from contract features at the expense of those who lose (Campbell and Ramadorai, 2026). These risks underscore the importance of consumer protection as financial innovation expands.

This paper studies *consórcios*, a group savings-and-credit product used by millions of consumers in Latin America to finance major purchases.[1] In Brazil, the focus of our study, consórcios account for 25% of new vehicle credit and a growing share of housing credit, and one in seven households holds a contract. Consumers join groups organized by firms and contribute monthly to a common pool. Each month, firms award funding through lotteries and bidding, in which consumers bid to prepay future installments and receive funding earlier. Firms and brokers market consórcios as a cheap, interest-free alternative to traditional credit. But critics argue that high fees make them an expensive form of saving, while unclear lottery odds make them difficult to evaluate. In fact, consórcios can be both: early awards can provide cheap loans financed by other members, while late awards amount to expensive savings due to fees and forgone interest. Given the size of the market, it is central for regulators to understand how value is distributed and whether consumers are properly informed of their lottery prospects.

We examine who gains and loses from consórcios, whether consumers misperceive their lottery prospects, and how accurate information changes their beliefs and valuations. We address these questions using market-wide administrative data, surveys of consumers and brokers, and a randomized information experiment.

Two interconnected features of consórcios guide our analysis. First, consumers with liquidity can bid for earlier funding, generating redistribution from those who wait for lotteries or only bid later. Second, lottery probabilities are difficult to assess in large groups that run for many years, as they depend on firms' allocation decisions and other members' bidding behavior. Together, these features imply that if consumers overestimate lottery probabilities, imperfect information can sustain demand for contracts that generate losses and redistribute value from liquidity-constrained consumers to those with greater liquidity. These features also distinguish consórcios from other rotating savings-and-credit associations (RoSCAs), one of the most prevalent financial institutions in the world (Zambrano et al., 2023; World Bank, 2025; Breza and Kaur, 2026), and require a different explanation for participation. In typical RoSCAs, early access is either independent of

[1] Specifically, consórcio refers to the Brazilian and Uruguayan version of this product. In Argentina and other countries, the same type of product is often called *plan de ahorro* or *grupos de ahorro* (savings plan/group).

liquidity, or waiting is compensated with interest.

We proceed in three steps. First, we combine administrative data on 59 million contracts and surveys of 1,296 consumers and 292 brokers to measure returns, redistribution, and beliefs. We find widespread and regressive losses, as well as long lottery waits that consumers systematically underestimate. Second, drawing on the RoSCA literature, we examine whether commitment motives can rationalize participation. We show that low lottery probabilities limit commitment benefits, while misperceptions can sustain demand despite expected losses. Third, we conduct a randomized information experiment to test how correcting beliefs affects valuations of a lottery-intensive consórcio group. We find that disclosure increases the value consumers place on lotteries, particularly among liquidity-constrained consumers and those who do not save.

To understand the magnitude and distribution of gains and losses, we must account for the dual nature of consórcios: consumers both save and borrow, with returns depending on when they receive funding. To quantify financial returns, we compare each contract's cash flows with those of a market-based alternative that saves the same payments before the award and borrows the remaining amount needed to finance the purchase at market rates on the award date. This benchmark captures the cost of bids, forgone savings interest, and avoided borrowing costs, and is motivated by survey evidence that consórcio participants also use other savings and credit products.

We organize our initial findings into four facts. First, 81% of consórcio contracts incur financial losses, covering both awarded contracts and contracts canceled before participants receive funding. Among awarded contracts, 60% have negative returns. The median awarded contract breaks even only when the benchmark real savings rate is -5% a year. Losses remain widespread even when benchmark borrowing rates are raised to the 90th percentile of market rates. Second, losses are regressive: lower-income participants incur larger losses per BRL paid into the pool, largely because they are more likely to cancel their contracts.

Third, the allocation mechanism transfers value from consumers who wait for lotteries to those with the liquidity to bid. We calculate expected returns by simulating different bidding strategies using observed data. Waiting for lotteries predictably leads to large losses of 14 to 24 cents per BRL paid into the pool. In contrast, strategies with moderate bids lead to early awards and yield higher expected returns. As funding for bids comes from the pool that all members finance, the gains of consumers who can bid come partly at the expense of those who cannot.

Firms prioritize bids over lotteries: bids account for 86% of awards in the first half of a group. This brings fee revenue forward and attracts more bidders, but leaves participants who wait for the lottery with only about an 11% chance of an award by the group's midpoint.[2] Although groups with more lotteries are possible, lotteries account for the majority of awards in fewer than 10% of observed groups. A lottery-only counterfactual with uniform award probabilities would raise

[2]In contrast to other settings where liquidity provisions are tied to risk, we provide evidence that realized default costs do not differ substantially between bid and lottery awards.

non-bidders' expected returns by 11 to 18 cents per BRL paid into the pool.

Fourth, despite the financial importance of award timing, consumers substantially overestimate the probability of early funding. In our surveys, respondents report believing their chance of an award by the group's midpoint is 31%, compared with 10–15% in the data, depending on the asset category. Participants who report a higher likelihood of joining a consórcio soon especially overestimate lottery chances. At the average measured beliefs, a consumer would understate expected financial losses from waiting for a lottery by about one-third. Consumers also report plans that lead to large expected losses, such as waiting for lotteries or delaying bids until later.

Taking stock of the empirical findings, a central question is why some consumers participate despite large expected losses and low chances of early lottery awards. A natural explanation comes from the established RoSCA literature: consumers value commitment, which can justify incurring financial losses to save (Basu, 2011). Yet widespread misperceptions raise the possibility that consumers overestimate the benefits of participation. In a model of hyperbolic discounting, we show that the allocation mechanism is central to the value of commitment itself.[3] Higher probabilities of early awards strengthen commitment by reducing the likelihood that future selves renege on the contract. Moreover, decreasing lottery chances increasingly weakens the commitment mechanism for those who cannot bid, while high fees and financial losses increase the commitment benefits required to justify participation. Together, low lottery probabilities and high fees limit the ability of commitment motives to explain participation in consórcios.

Misperceptions provide an alternative explanation for participation. By inflating the perceived commitment benefits of consórcios, optimistic beliefs can sustain demand even when actual lottery probabilities undermine the commitment mechanism. Such beliefs also reduce consumers' perceived gains from choosing groups with more lotteries, potentially weakening firms' incentives to offer them. Consequently, optimistic beliefs may sustain contracts that undermine the very commitment mechanism some consumers seek.

These results raise a central policy question: can correcting misperceptions change demand for contracts with more lotteries? Accurate information may be particularly valuable for liquidity-constrained consumers, who cannot compensate for low lottery probabilities by bidding. By changing demand for alternative contract designs, disclosure may also affect firms' incentives to offer groups with more lotteries.

To test the effects of correcting misperceptions, we conduct an information provision experiment with 845 consumers. We randomize respondents into an active control group and two information treatments. The *debiasing* (D) treatment uses a short video to convey average bidding shares and lottery wait times, and to explain how more early bid awards increase lottery waits. The *debiasing + group size mechanics* (DG) treatment additionally explains variation in group size

[3] Basu (2011) discusses how the prospect of early access encourages participants not to postpone entry into the rotating mechanism.

and how larger groups reduce individual lottery probabilities. The videos do not mention whether consórcios lead to financial losses or gains or compare them to savings and borrowing, focusing exclusively on the typical design of the allocation mechanism.

Both treatments partially correct misperceptions, closing about 40–60% of the gap between average beliefs and the data. They also increase consumers' confidence in their answers, and change attitudes toward the product: treated respondents have stronger perceptions that consórcios favor bids and say they are less likely to recommend them to friends and family without liquidity.

To measure valuations of groups with more lotteries, we partner with a consórcio firm to create a prospective offer for a group designed to have more lottery awards. We then present the offer to respondents and elicit whether they will forgo a real survey bonus for access to this offer.

We find that disclosure of allocation probabilities can shift demand differently depending on consumers' liquidity. The debiasing (D) treatment raises liquidity-constrained consumers' valuations of the lottery-intensive offer. It increases the share choosing access to the offer over the largest bonus by 8 percentage points ($p = 0.07$) among consumers without liquidity to bid at least half their contract value, a subgroup we pre-registered. The treatment effect is also concentrated among the most optimistic consumers. Furthermore, consistent with our model, the increase in valuation due to disclosure is higher among consumers who reported not saving in the previous year (20 percentage points, $p < 0.01$) or who reported having no access to loans (18 percentage points, $p < 0.01$). The DG information treatment, which contains extra scenes, has no effect, and the difference between the arms' effects is statistically significant.

In side-by-side choices where contract attributes are randomized and explicitly presented, consumers prefer smaller groups with more lotteries. This finding suggests a gap between consumers' stated preferences when attributes are made salient and the design of groups observed in the market. The information treatments do not significantly change the level of any stated preferences, which is consistent with an information channel.

We also find that optimism about lottery probabilities is widespread across consumer characteristics, including financial literacy. Differences in sophistication, at least as captured by these beliefs, therefore appear secondary in explaining who benefits from the contract, in contrast to other settings where sophistication is viewed as a key driver of heterogeneity across consumers.

Our experimental findings show that misperceptions can be corrected, yet they remain widespread. Several factors may explain their persistence. First, contracts span many years and involve relatively few lottery draws, limiting learning from experience. Second, although brokers and firms could inform consumers, their incentives may be misaligned, and they may themselves struggle to assess lottery probabilities given the complexity of allocation decisions over long periods. Indeed, our survey evidence suggests limited learning among both experienced consumers and brokers. Finally, existing disclosure requirements are asymmetric: monthly meeting minutes report winning bids, helping consumers learn how much they need to bid, but provide no comparable information

about their likelihood of receiving funding through lotteries.

Our findings support policies that mandate disclosure of allocation schedules and predictions of lottery chances to consumers. While disclosure policies have often been found to be insufficient across different settings (Campbell, 2016; Garz et al., 2021), they may be especially powerful in contexts in which heterogeneous consumer responses can also have implications for market composition and redistribution. In consórcios, demand responses can affect both contract design and the distribution of returns. If disclosure increases the valuation of lottery-intensive groups, or shifts demand away from consórcios, it could weaken demand for bid-intensive contracts and increase incentives for firms to offer more lotteries. Thus, disclosure may affect not only consumers' individual decisions but also which allocation mechanisms firms can sustain in equilibrium. Establishing the equilibrium effects of disclosure, however, requires accounting for firms' responses and consumers' sorting across contracts.

This paper contributes to four different strands of the literature. First, we contribute to the literature on rotating savings and credit associations, which emphasizes credit frictions and demand for commitment to explain participation and contract design (Besley et al., 1993, 1994; Calomiris and Rajaraman, 1998; Anderson et al., 2009; Anderson and Baland, 2002; Ambec and Treich, 2007; Gugerty, 2007; Basu, 2011; Afzal et al., 2017). Studying the largest formal market for rotating finance, we show how consumers' expectations about allocation timing interact with these explanations, and how liquidity differences and imperfect information shape the distribution of financial returns. We also show how imperfect information can influence the sustainability and regressivity of rotating savings mechanisms. In this sense, we connect group-based financial institutions studied in developing economies to a broader literature on consumer protection and redistribution in financial products (Garz et al., 2021; Campbell and Ramadorai, 2026). Our findings are especially relevant as rotating finance products are increasingly offered by profit-maximizing intermediaries across many countries.

Second, our information experiment connects to other experiments studying consumer protection and disclosure policies (Bertrand and Morse, 2011; Ferman, 2016; Seira et al., 2017; Alan et al., 2018; Adams et al., 2021; Balakina et al., 2026). In particular, the specific mechanism of perceptions about probabilities involving lotteries relates to papers studying demand for prize-linked savings accounts (Kearney et al., 2010; Filiz-Ozbay et al., 2015) and state-run lotteries (Lockwood et al., 2025). We provide experimental data linking beliefs about nonprice attributes to consumers' choices and show that disclosure of nonprice attributes can affect consumers differently based on their ability to take advantage of contract features.

Third, a recent literature studies the distributive effects of retail finance in settings such as life insurance, credit cards, and mortgage refinancing (Gurun et al., 2016; Egan, 2019; Gottlieb and Smetters, 2021; Byrne et al., 2023; Fisher et al., 2024; Berger et al., 2024; Egan et al., 2026; Agarwal et al., 2026). This literature empirically studies how redistribution emerges from supply-side

responses to consumer decision frictions, as suggested by theoretical work (Gabaix and Laibson, 2006; Heidhues and Kszegi, 2017). In a pattern that mirrors cancellations in our setting, Gottlieb and Smetters (2021) show that lower-income households lapse more often on life insurance policies. We provide experimental evidence that disclosure policies can affect consumer valuation of financial contracts in a direction that is consistent with lower redistribution. We also provide evidence that heterogeneity in sophistication may be secondary when consumers with higher liquidity are better placed to take advantage of contract features.

Finally, this paper relates most directly to work on consórcios. To our knowledge, this paper provides the first market-wide evidence on the financial returns to consórcios, along with survey evidence of consumer misperceptions. Other studies measure the effects of durable ownership by using consórcio lotteries as an instrument, examining outcomes such as employment and earnings (Doornik et al., 2024b) and fertility (Doornik et al., 2025). Doornik et al. (2024a) study how credit contracts that mandate a savings period can expand access to credit by screening out consumers, using consórcios as an example of such a contract. Relatedly, we show that cancellations, a form of dynamic screening, can imply large and regressive financial losses.

2 Setting and financial value

2.1 Institutional context: contracts, awards, and firms

When joining a consórcio, a consumer chooses the contract's face value — the funds made available upon award — its term, and asset to be purchased. Consórcios are long-term commitments, typically lasting 4 to 8 years for vehicles and 10 to 20 years for real estate. The contract is then assigned to a group. Groups range from several hundred to over five thousand contracts. Consumers usually do not choose the exact group they join.[4] Appendix B describes the institutional setting in more detail.

The consumer then makes monthly payments to the firm managing the group, covering pool contributions and fees.[5] Because consórcios charge a fee instead of interest, they are often marketed as "interest-free". Fees are set at the group level, so firms do not price discriminate among members of a group, although fees vary across groups.

The key feature of the contract is that the date of award is uncertain, potentially leading some consumers to wait many years for funding. Upon award, participants receive a letter of credit granting them the right to use funds to purchase their desired asset.[6]

[4] Groups may admit new contracts after formation, with terms adjusted to the group's remaining duration. Face values may vary within a group, subject to regulatory limits: the smallest must be at least half the largest.

[5] Payments and face values are indexed to reference prices or price indices and adjusted semiannually or annually.

[6] A credit assessment takes place, and credit may be withheld if deemed risky, until further payments are made.

Awards are made either by lottery or through bids, which allow participants with sufficient liquidity to obtain earlier access to funds by prepaying future installments, reducing future payments one-for-one.[7] A winning bid brings forward both contributions to the group and administration fees. Bids compete in a first-price auction. The lottery consists of random draws among all eligible participants in a given month, including that month's bidders.

Each month, firms choose how to distribute awards between lottery and bids, conditional on the group's budget. The Central Bank mandates at least one lottery award each month. Firms include large commercial banks, carmakers, insurance companies, and independent administrators, such as Honda, Santander, and *Banco do Brasil*.

Finally, nonpayment leads to different consequences before and after award. Before award, three consecutive missed payments lead to cancellation: payments to the pool are refunded at a later date, net of a fine. After award and usage of credit, the contract becomes a collateralized loan, and missed payments constitute a default. In practice, cancellations are common (from 40% to 50% of contracts) while defaults are rare (less than 3%).[8]

2.2 Consumers' access to other financial products and financial habits

Consórcios are widely used in Brazil: in a representative financial literacy survey conducted by the Central Bank in 2023, 147 out of 1,956 adults reported holding a consórcio.[9] We use this survey to characterize consórcio participants and examine whether they also use conventional savings and credit products. First, 91% had a bank account, 78% reported saving money through a formal financial instrument, and 54% reported having some credit product. Usage of financial products is also higher when compared to other respondents, even after controlling for income and education (Appendix Table A.1).

Second, consórcios reach a wide range of consumers: 23% of holders live in households earning up to two minimum wages, 42% live in households earning two to five, and 35% live in households earning more than five. Even in the lowest income bracket, 82% of holders have a bank account, against 59% of other respondents. Both overall and within the lowest income bracket, holders are more likely to report having a financial goal (71% vs. 45% overall), to keep money for bills apart from day-to-day money (70% vs. 61%; 85% vs. 57% in the lowest income bracket), and to agree that they set long-term goals and work toward them (67% vs. 57%). Holders are no more likely than other respondents to buy lottery tickets when short of money, suggesting that a taste for

[7] Some groups allow bids to be partially paid with awarded funds, reducing liquidity requirements. Because this option is available to all members of a group, it does not change their relative bidding opportunities. Some groups also offer "fixed bids", with winners randomly drawn among bidders.

[8] Default rates are lower than market averages, which may be explained by selection into the contract or screening: Doornik et al. (2024a) suggest the pre-award savings period screens out participants by income risk.

[9] With about two adults per Brazilian household, and assuming no household has more than one holder, this translates into about 1 in 7 households.

lotteries is unlikely to be a key explanation for their popularity.

2.3 Measuring financial returns compared to a market replication

We value each consórcio contract by comparing its cash flows with a market replication that saves the same pre-award payments and borrows any remaining amount needed to obtain the same funds at the award date. The comparison captures both the opportunity cost of saving through the consórcio, which pays no interest, and the benefit of cheaper borrowing. The procedure is detailed in Appendix F.

We define the *consórcio value* as the difference in wealth created by the csonórcio, compared to the market alternative, holding the award date constant and expressed in present-value terms:

$$V_i(t^A|\tau, r^S, r^B) := NPV(\text{Consórcio}|t^A, \tau, r^S) - NPV(\text{MarketReplication}|t^A, r^B, r^S), \quad (1)$$

where t^A is the award time, τ is the proportional fee, and r^S and r^B are benchmark savings and borrowing interest rates, with r^S being the discount rate. We refer to the value per BRL paid into the pool as a *return*. For an awarded contract, payments into the pool add up to its face value P_i, so the return is V_i/P_i. For a canceled contract, the denominator is the amount paid into the pool before cancellation, and since there is no award, we set the NPV of the market replication to zero.

We calculate V_i based on average market rates over 2011-2025. We set the savings interest rate to $r^S = 4\%$ (post-tax) and the market borrowing rate r^B to 4%, 19%, and 24% for real estate, cars, and motorcycles, respectively, in real terms.[10] We consider the market average to be a reasonable benchmark given the evidence of financial access among consórcio consumers. For each V_i, we can compute break-even borrowing or savings interest rates, holding the other rate at its benchmark.[11]

The measure accounts for the benefit of avoiding market borrowing while avoiding the ambiguity of internal rates of return, which may be non-unique since consórcio cash flows can change sign twice.[12] When the borrowing and saving interest rates are equal, the measure reduces to the conventional NPV. For awards at the first or last period, the break-even rates are equal to the implicit loan or investment interest rate.

Importantly, the measure does not fully account for the benefits of earlier ownership of the durable, as the discount rate implicitly values possession at r^S. Thus, comparisons across contracts may understate the advantages of receiving an early award.

[10] Data for average interest rates are obtained from the Central Bank. Appendix Figure A.1 presents the corresponding real interest rates alongside government bond yields.

[11] Break-even borrowing rates are undefined for late awards, which require no borrowing, while break-even savings rates are undefined for most early awards, because the saving period is short.

[12] Such cash flows may have multiple internal rates of return, which are also hard to interpret (Hirshleifer, 1958).

2.4 The central aspects of consórcios illustrated by an example

We illustrate typical contract outcomes, the measurement of returns, and the core logic of winners and losers within consórcio groups with an example of three agents. Suppose three agents, L, B, and C, join a consórcio group with three identical contracts to buy a car, with a face value of 10,000, a 60-month term, and a 20% administration fee. Each owes 12,000, initially paid in monthly installments of 200. But each has different outcomes:

- **Agent B bids and receives funding early.** Suppose Agent B has liquidity available to bid. He bids 5,400, equivalent to a 45% bid, and wins (bids are normalized by total payments). The bid amount is deducted from his future contributions, reducing remaining installments to about 109 per month, down from 200 originally.
- **Agent L waits for a lottery and receives funding late.** Suppose Agent L waits for the lottery until she wins in month 48. By then she has paid 9,600, almost the full price of the car, but still owes twelve installments.
- **Agent C waits and then cancels.** Suppose Agent C pays 12 installments while waiting for an award, a total of 2,400, but then stops paying and the contract is canceled. He receives a refund, net of fines and fees. The refund date is based on a random draw, $t = 36$ in this example.

Figure 1 shows the cash flows of each agent. The right column shows the returns, applying Equation (1) to the cash flows. Agent B has positive returns, which means his contract has a borrowing cost below the market rate — a subsidized loan. In contrast, the returns of Agents L and C are negative due to a combination of fees and the forgone opportunity cost of interest.

The example highlights central aspects of consórcios. First, the product can create winners and losers even though contracts are identical. The timing of awards, bids, and cancellations all contribute to substantial differences in financial returns across participants.

Second, bidding can shape the distribution of winners and losers. But bidding is gated by access to liquidity: Agent B's bid amounts to 27 months of installments paid at once. Those without liquidity must wait for a lottery and may cancel before receiving funds. How many lottery awards take place in a group is both a decision of firms and a consequence of consumers' willingness to bid.

Third, transfers may run opposite to the usual compensation for waiting. In conventional financial markets, borrowers compensate lenders for providing funds earlier. In consórcios, however, bidders may simultaneously benefit from higher financial returns and earlier access to funding to purchase an asset, while those who wait bear the cost of this advantage.[13]

[13]Prepayment bidding thus makes this redistribution systematic rather than random, in contrast to other ro-

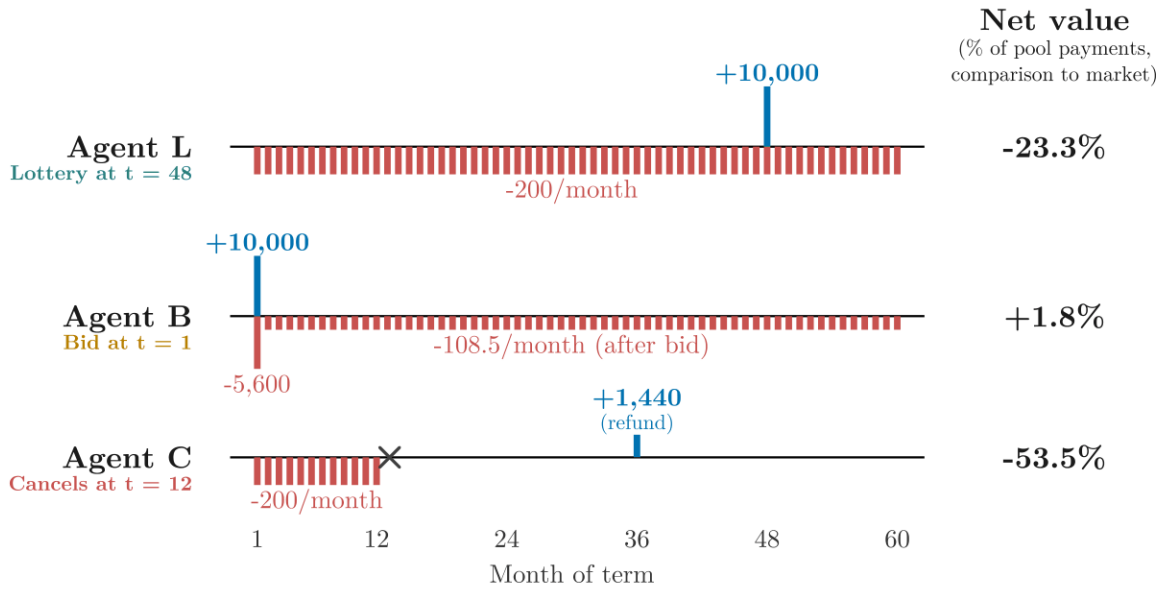


Figure 1: Three identical contracts with distinct outcomes.

Finally, a consórcio group with bidders requires other participants who wait: without them, bidders would have no one to borrow from. Participants who cannot bid therefore have the most at stake: waiting earns no interest and still costs a fee.

A direct consequence is that lottery probabilities are a central component of the expected payoff of consumers who do not bid. For those without liquidity, the lottery is the only source of awards, and will define whether their contract resembles cheap borrowing or expensive savings.

3 Four facts about the consórcio market

In this section, we combine two datasets to document four facts about the consórcio market. We use market-wide administrative data from the Central Bank to document the financial value of consórcios as described in Section 2.3 and the observed lottery probabilities. We then turn to an original survey dataset of 1,296 prospective and past consumers to document misperceptions about lottery probabilities.

First, 81% of contracts face losses, including both awarded and canceled contracts. In particular, 60% of awarded contracts lose. Second, lower-income participants suffer larger losses, largely due to cancellations. Third, bidders can predictably obtain higher returns, while non-bidders suffer large expected losses, driven by low lottery probabilities. Fourth, consumers hold wide misperceptions about lottery chances, and have high uncertainty about beliefs.

tating savings and credit associations (RoSCAs), where allocations are determined by lottery or bids compensate participants who wait.

3.1 Data

The Central Bank data is obtained from SAG *(Sistema de Acompanhamento de Grupos de Consórcios)*. Our sampling frame considers the universe of groups and contracts that started between 2010 and 2025 and contains 48,531 groups with 59,372,033 contracts, spread across 24,955,784 unique individuals and 2,312,928 unique businesses. Appendix C describes the data extraction and sample construction in detail. The survey data has responses from 1,296 prospective and past consumers. The measurement of beliefs and sampling are detailed in Section 5.

The administrative data contains key inputs to calculate the financial value of contracts and award paths of groups. For every contract, we observe its face value, term, administration fees, bid amounts, start date, award date, and cancellation date if applicable. We deflate nominal amounts to 2025 prices. We merge income data from the national credit registry.

Because groups are long, to include ongoing groups in our analysis we predict unobserved award and cancellation outcomes and winning bids, keeping observed outcomes fixed. We restrict predictions to groups observed for at least 25% of their planned duration and estimate duration models using their observed histories. This expands the group sample by 44.7% and the contract sample by 73.5%, with outcomes predicted for about 15% of the contracts in ongoing groups. Out-of-sample validation shows that predictions closely match realized award paths and returns, with small errors that tend to understate differences between bidding and lottery awards. Prediction details are in Appendix D.

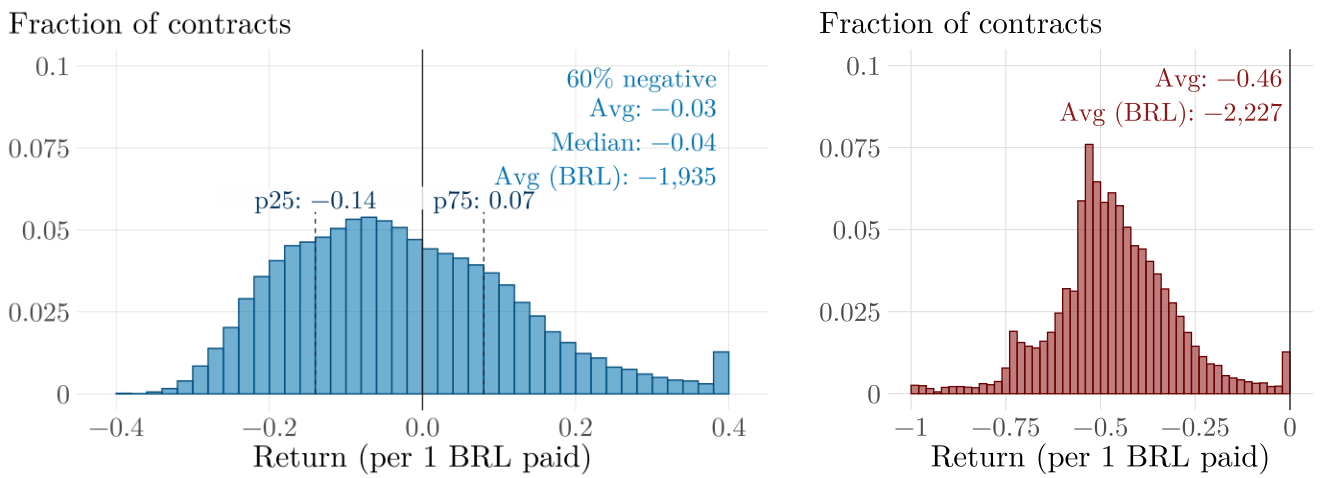
3.2 Summary statistics of individuals, contracts, and groups

Appendix Table A.2 reports summary statistics for contracts and groups by asset category. The summary statistics show how consórcios are major long-term obligations used by a wide range of consumers. First, the median contract's installment, including the administration fee, is about 17.5% of the holder's reported income. Second, the average contract term is 72 months for cars, 56 months for motorcycles, and 184 months for real estate.

Administration fees, which cover the intermediation services of the firm, are high: the average fee charged by a firm is 14% for cars, 20% for motorcycles, and 18% for real estate.[14] Firms also charge reserve fees, a fee meant to pass-through the cost of defaults into group participants. Average reserve fees are around 3% for the three asset categories. In our calculations below, we do not include the reserve fee.

Groups are relatively large, giving firms discretion to choose between bids and lottery awards. They average between 1,000 (cars) and 2,200 (real estate) participants, which implies 12 to 19 participants for each month of duration and multiple awards per month.

[14]For a comparison, chit funds, a type of group-based savings in India, are capped at 7%.



Panel A. Awarded contracts *Panel B. Canceled contracts*

Figure 2: Negative returns are widespread, and cancellations are especially costly.

Notes: This figure shows financial returns per BRL paid into the pool, calculated with Equation (1) across awarded and canceled contracts. For canceled contracts, the market replication is set to zero. The sample includes all contracts in groups started between 2010 and 2025 that are completed or observed for at least 25% of their planned duration. Outcomes for contracts yet to be awarded in ongoing groups are predicted. Returns beyond the axis range fall in the end bins. Dashed lines in panel A mark the 25th and 75th percentiles.

3.3 Fact 1: 81% of contracts face financial losses

Returns are negative for most contracts, and widely dispersed. Among contracts awarded credit, the average return is −0.03 per BRL paid into the pool, and 60% have negative returns (Figure 2, panel A). The median return is −0.04, and the middle half of contracts yield returns between −0.14 and 0.07 per BRL paid into the pool.[15] Returns are negative for motorcycles and real estate and close to zero for cars (Appendix Table A.3). The differences reflect fees, which are smaller for cars, and the outside borrowing option, which is subsidized for real estate.

Negative returns at benchmark rates raise the question of how poor outside options must be for individual consórcios to break even with the market replication. We calculate the savings and borrowing rates that would make each contract's financial value equal to zero. The borrowing break-even applies to early awards, while the savings break-even describes contracts with later awards. Break-even rates can be non-monotonic in the contract term and award time, so they serve primarily to illustrate returns.

For contracts around the median return of −0.04, the median break-even savings rate is −4.9% a year, roughly what cash held at home would lose to inflation over the period (Appendix Table A.4).

[15] Appendix Figure A.2 shows the same distribution varying the sample. Panel A shows groups observed past 50% of their planned duration, which is a stricter criterion for predictions, and panel B shows completed groups, which require no imputation. Completed groups have less negative returns, reflecting composition: those groups are older, with smaller fees and bids, and are almost entirely motorcycles and cars. Appendix Figure A.3, panel A, shows that average fees have increased by 3.6 percentage points since 2010. Panel B shows that, for the same award month, average bids have increased by 2.4 percentage points.

At the 25th percentile return of −0.14, the break-even rate is −7.9%, worse than holding cash. By comparison, a standard savings account pays about 1 to 2% in real terms, while a certificate of deposit or government bond can yield 4%.

For contracts around the median return, the break-even borrowing rate is 10.1 and 5.6 percentage points above the market average for motorcycles and real estate, respectively, and 0.3 percentage points below it for cars. Motorcycle and real estate contracts therefore break even only for participants who would otherwise borrow well above market rates.

We also assess how sensitive these results are to the benchmark rates (Appendix Figure A.4 and Appendix Table A.5). The savings rate matters little: setting it to 0% or 8% moves the average over all contracts, including cancellations, by less than 0.01, and the share of awarded contracts with negative returns stays between 54% and 66%. Borrowing rates 50% above market averages, roughly at the 90th percentile of the distribution of rates, raise the average return of awarded contracts to +0.04 per BRL, but also spread returns further apart, and 47% still lose. Thus, widespread losses are robust to a broad range of plausible outside options.

Figure 2, panel B turns to canceled contracts, which receive a refund rather than an award. These contracts lose, on average, 0.46 per BRL paid into the pool. Fees are not refunded, there are cancellation fines, and refunds take time to be issued. Furthermore, in absolute value, the average loss on a canceled contract is close to that on an awarded contract (BRL 2,227 against BRL 1,935), with the caveat that an awarded contract resulted in the purchase of an asset.

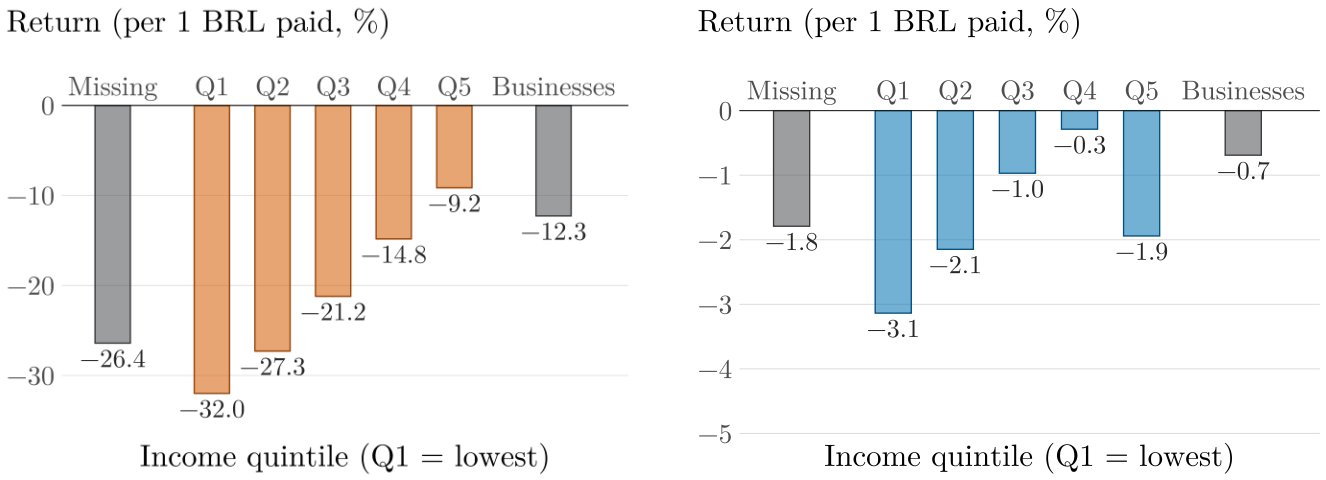
3.4 Fact 2: Lower-income participants suffer larger losses, largely due to cancellations

To study returns by income, we restrict the sample to completed groups. We include both awarded and canceled contracts, measuring returns per BRL paid into the pool as in Section 2.3.

Lower-income participants experience substantially larger financial losses (Figure 3, panel A). Considering cancellations, participants in the bottom income quintile lose 32% of their contributions, compared with 9% in the top quintile. This gradient is largely driven by cancellations: among awarded contracts, returns show an inverse U-shaped relationship with income (panel B). Businesses also appear to have larger returns relative to consumers. Cancellation rates fall from 60% in the bottom income quintile to 29% in the top quintile (Appendix Figure A.5). Although we do not causally link the allocation mechanism with cancellations, it is reasonable to assume cancellations are more likely among those who do not intend to bid.[16]

Higher-income participants are also more likely to receive early awards, defined as awards within the first 25% of group duration (Appendix Figure A.6). This is especially true for real estate

[16] In a 2026 industry survey, participants cited long waits or unclear rules (36%), financial difficulties (35%), and changes in purchase plans (25%) as reasons for cancellation.



Panel A. Awarded and canceled contracts *Panel B. Awarded contracts*

Figure 3: The income gradient in returns is largely driven by cancellations.

Notes: This figure shows financial returns per BRL paid into the pool, calculated with Equation (1) using the subsample of completed groups from Central Bank data. Income quintiles refer to quintiles taken over the distribution of self-reported incomes from the national credit registry, adjusted to 2025 prices. "Missing" refers to missing income. "Businesses" refer to contracts owned by businesses.

groups: the top income quintile is almost three times as likely to receive an early award as the bottom quintile. Conditional on not canceling, the relationship varies across asset categories: the probability of an early award increases with income in real estate but not in cars or motorcycles. Within-group comparisons yield similar results (Appendix Figure A.7, panels A and B). We also find a strong relationship between a contract's face value relative to the group average and the probability of early awards, despite bids scaling with the contract's face value (Appendix Figure A.7, panels C and D).

Bidding imposes a much larger liquidity burden on lower-income participants: within each asset category, a winning bid costs about 15 times as many months of reported income in the bottom income quintile as in the top quintile (Appendix Figure A.8).

3.5 Fact 3: Non-bidders predictably lose, and bidders can predictably obtain higher returns

Bidding allows participants to obtain funds earlier, which improves returns, but costs liquidity and reduces the net amount borrowed from the group, reducing the gains from cheaper borrowing.[17] Bids often range from 30% to 60% of a contract's value in early periods (Appendix Figure A.20). Despite this trade-off, contracts with bid awards obtain higher financial returns. Bid-awarded contracts have average returns 0.04 higher and median returns 0.10 higher than lottery-awarded

[17] At a given award timing, bid awards earn less than lottery awards, most of all early in the term, when winning bids are highest (Appendix Figure A.9, panel A).

contracts (Figure 4). The gap widens as outside rates rise, because early funding is worth more when saving pays more or borrowing costs more (Appendix Figure A.4, right column).

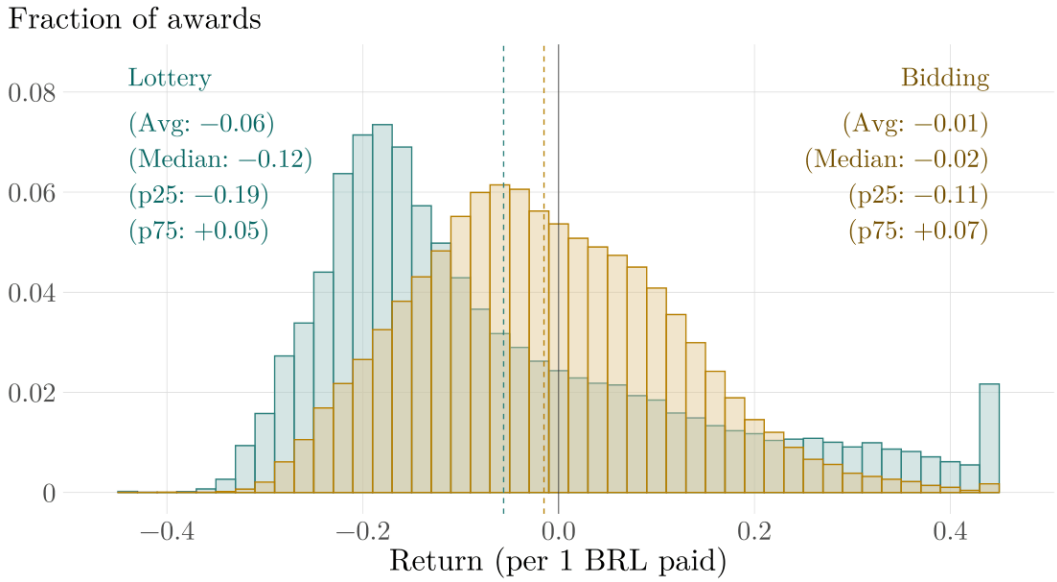


Figure 4: Bid-awarded contracts have higher unconditional returns.

Notes: This figure shows financial returns per BRL paid into the pool, calculated with Equation (1) across bid-awarded and lottery-awarded contracts. The sample includes awarded contracts in groups started between 2010 and 2025 that are completed or observed for at least 25% of their planned duration. Outcomes for contracts yet to be awarded in ongoing groups are predicted. Returns beyond the axis range fall in the end bins. Dashed vertical lines mark each mode's average return.

To evaluate expected returns, we retrospectively compare a strategy that never bids with strategies that submit the same bid, as a share of the contract's total payments, every month until awarded, holding contract characteristics and other participants' bids fixed (for details, see Appendix E). A contract joins a group at its start, pays the category's average fee, and is awarded at the earlier of a lottery draw, which follows the group's estimated lottery hazard, and a bid award, which occurs once its bid exceeds its group's lowest winning bid that month, among bids of at least 30% of face value paid partly with own resources and excluding fixed bids. Table 1 reports, averaged over contracts, the probability of an award within 12 and 24 months, the median award month, and the expected return.[18] Never bidding produces expected returns between −0.14 and −0.24 per BRL paid into the pool, with long expected waits. For the median never-bidding contract, the loss is equivalent to saving at real annual rates of −4.9% for cars, −8.5% for motorcycles, and −2.7% for real estate, against our 4% benchmark. By contrast, bidding substantially advances award timing and improves expected returns (Table 1). For some bidding strategies, expected returns become positive, including for cars at average market fees and borrowing rates.

The large expected losses from never bidding are partly a consequence of firms' allocation de-

[18] We also simulate strategies sampling from contracts' observed date of entry into the group. Both expected wait times, relative to the contract term, and returns remain roughly unchanged.

Table 1: Award timing and returns of never bidding versus bidding monthly.

Strategy	Award probability (%)		Median award time (months)	Expected return
	by 12 mo.	by 24 mo.		
	(1)	(2)	(3)	(4)
Panel A. Cars (14% fee)				
Never bid	3	6	65	−0.14
Bid 40%	61	76	8	0.11
Bid 50%	85	94	3	0.12
Bid 60%	96	99	3	0.09
Panel B. Motorcycles (21% fee)				
Never bid	3	9	54	−0.21
Bid 40%	94	98	3	0.12
Bid 50%	98	99	2	0.09
Bid 60%	99	100	2	0.03
Panel C. Real estate (18% fee)				
Never bid	2	3	158	−0.24
Bid 40%	22	37	45	−0.11
Bid 50%	58	71	10	−0.07
Bid 60%	86	93	6	−0.07

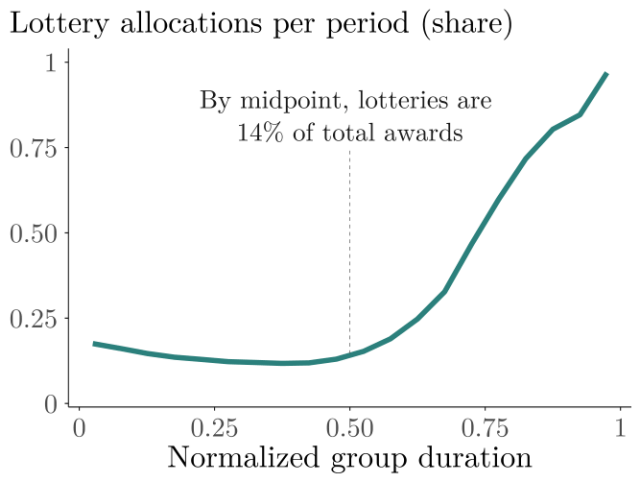
Notes. This table shows simulated outcomes for a contract that joins a group at its start, under different bidding strategies. Bids are assumed to be constant every month until awarded. The contract characteristics are the asset category's average fee, reported in the panel heading, and each group's planned term. Bidding strategies use the observed minimum bids as winning thresholds, with other participants' bids assumed fixed. See Appendix E for details. Non-bidding strategies are calculated using the group's estimated lottery hazard. Returns are calculated with Equation (1) at the benchmark rates. Rows average over contracts, weighting each group by its size and sampling weight.

cisions. Firms choose the mix of lottery and bid awards, subject to a regulatory minimum of one lottery award per month. Lottery awards are back-loaded (Figure 5, panel A): bids account for 86% of awards in the first half of the term, and for a majority of first-half awards in 89% of groups, covering 97% of contracts, as large groups mute the minimum requirement.[19] Firms could favor bid awards because they bring fee payments forward, or because bidders carry lower credit risk. The data do not support the second explanation: bid awards default more often, even at the same award time (Appendix Figure A.10).[20]

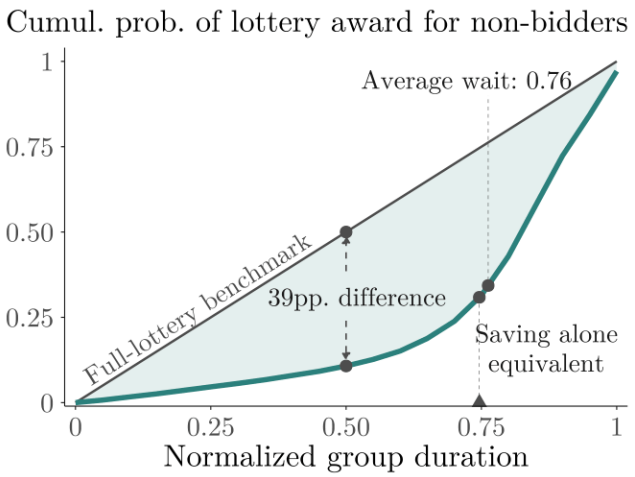
We can use the data to reconstruct the cumulative probability of an award conditional on not bidding. The resulting distribution's median wait is 82% of group duration, and the probability

[19]Between groups started in 2010–2014 and in 2023–2024, the share of early awards made by bid rose from 80% to 90% for cars and from 69% to 82% for real estate, and stayed at 89% for motorcycles (Appendix Table A.6).

[20]Default rates fall steeply with award time, possibly because the pre-award period screens participants (Doornik et al., 2024a). Bidding lowers the outstanding balance but brings awards forward, when defaults are more common. Within three years of the award, bid awards are 1.16 percentage points more likely to default than lottery awards, and 0.38 points more likely at the same award month. A large share of bid awards therefore does not lower overall default rates.



Panel A. Flow of lottery awards



Panel B. Cumulative probability of lottery award

Figure 5: Lottery awards are back-loaded, creating long waits for participants who never bid.

Notes: This figure shows how lottery awards are distributed over a group's duration. Panel A shows the share of lottery awards, pooling the observed awards of all groups started between 2010 and 2025. Panel B shows the cumulative probability of a lottery award for a participant who never bids, from each group's estimated lottery probabilities (Appendix D), weighted by group size. The triangle marks how long a participant who deposited the same monthly installment in a savings account at a 4% real rate would take to accumulate the face value, as a share of the term, averaged across asset categories. Estimates by asset category are displayed in Appendix Figure A.11.

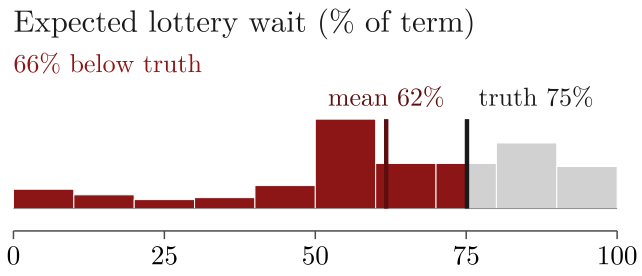
of receiving credit by the group's midpoint is only 11% (Figure 5, panel B). Waits vary across groups, but the expected wait for a lottery award exceeds 70% of the term in groups covering 91% of contracts.

To quantify the importance of the allocation mechanism, we simulate a counterfactual with lottery awards spread uniformly over the group's duration and average contract characteristics. Expected returns of never bidding increase by 0.17, 0.18, and 0.11 for cars, motorcycles, and real estate, respectively, becoming slightly positive for cars and closing 87% and 46% of the loss for motorcycles and real estate.

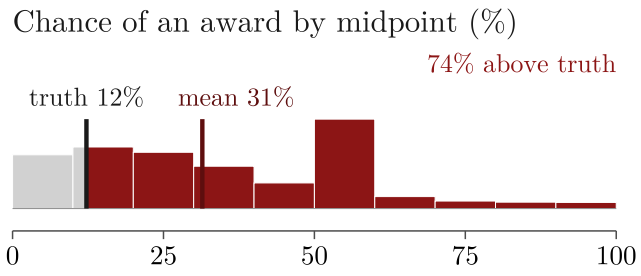
The takeaway is that participants who do not plan to bid, or who plan to bid only late, are buying a contract with large expected losses. Furthermore, those who do not bid face a long wait for a lottery award and a very low chance of early award. A key question is whether consumers appear to be aware of these statistics.

3.6 Fact 4: Consumers hold large misperceptions about lottery chances

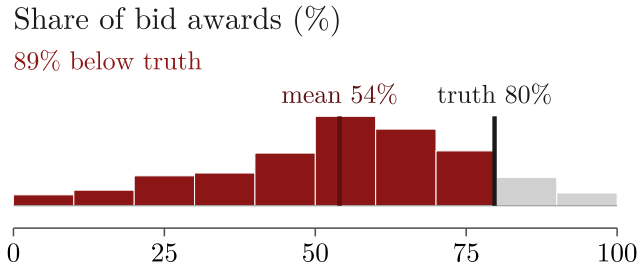
Lastly, using our survey data, we document evidence consistent with optimism about lottery chances, alongside considerable uncertainty about these beliefs, consistent with weakly informed priors. We frame questions under a strategy of never bidding and relying exclusively on lotteries, using respondents' simulated contract terms. We report beliefs and true values re-weighting asset



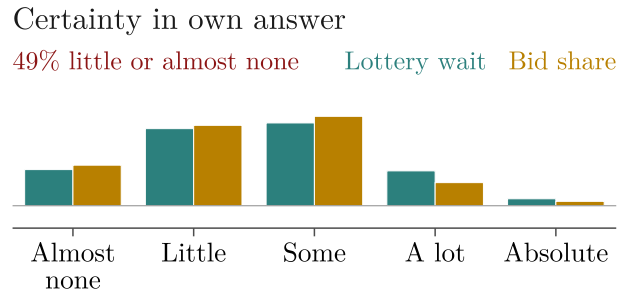
Panel A. Expected wait for a lottery award



Panel B. Chance of a lottery award by the midpoint



Panel C. Share of awards won by bid by midpoint



Panel D. Certainty in own answers

Figure 6: Consumers hold large misperceptions about lottery chances.

Notes: This figure shows consumers' beliefs about lottery wait times and the share of bid awards. True values are calculated from Central Bank administrative data and averaged across asset categories weighted by respondents' asset choices in the contract simulation. The share of respondents whose estimates are optimistic, with respect to lotteries, is calculated separately for each asset category. See Section 5.1.1 for a description of belief measurement. Panel A reports respondents' expected wait for a lottery award as a fraction of the contract's duration. Panel B reports their perceived probability of a lottery award by the group's midpoint. Panel C reports the perceived share of awards won through bidding. Panel D reports respondents' confidence in their answers.

categories to reflect respondents' choices.

On average, respondents expect an award after 62% of the contract duration, compared with 75% in the administrative data (Figure 6, panel A). They also estimate a 31% chance of receiving an award by the group's midpoint, compared with 12% in the data (panel B). A majority of respondents overestimate their lottery prospects. The average beliefs sit roughly at the 10th percentile of the distribution of lottery wait times across groups in the data.

Respondents also report substantial uncertainty. Almost half of the consumers report having little or no confidence in their answers, while only 16% report high or complete certainty (panel D). We also see bunching at 50% across questions, including questions that were quantitative or used visual aids, consistent with uncertainty or poorly formed expectations.

Misperceptions also extend to the allocation mechanism: respondents believe that bid awards account for 54% of awards in the first half of a contract, compared with 80% in the data. Nearly

90% underestimate this share, suggesting consumers are unaware of a key input to understand their lottery chances. Qualitative questions yield similar patterns (Appendix Figure A.12).

How much do these beliefs matter for financial returns? We value a never-bidding contract, at each respondent's chosen asset category and term, under two award-timing distributions: the one in the administrative data, and one that matches respondents' average expected wait and chance of an award by the midpoint. At true lottery odds, never bidding loses 0.20 per BRL paid into the pool. At average beliefs, it loses 0.13. Average beliefs therefore hide about a third of the expected loss.[21]

In Section 5.3, we further examine heterogeneity by financial literacy, education, and experience, as well as beliefs among brokers, and show misperceptions are widespread.

3.7 Takeaways

The facts illustrate how, for a large share of participants, participating in a consórcio group is likely to lead to losses relative to saving and borrowing at market rates. Even under significantly more expensive counterfactual borrowing rates, or lower savings interest rates, a large share of participants would still suffer losses. Furthermore, participants with lower income also tend to cancel more and face relatively larger losses.

Returns are also predictable by bidding strategies. Strategies consisting of bidding from the start of a contract can lead to higher and sometimes positive returns relative to the market. However, these strategies are mostly available for consumers with sufficient liquidity to bid. Not bidding and waiting for a lottery leads to long waits and predictable losses. Furthermore, consumers do not appear to be aware of just how long the wait for a lottery award might be, how low the chance of an early lottery award is, or that bids are much more frequent than lotteries.

4 Participation in group savings: existing explanations and misperceptions

The facts documented in Section 3 raise a question: why do non-bidders join consórcios despite large expected losses and a low share of lotteries? The low share of lotteries, in particular, implies low chances of early access and long wait times. In this section, we draw on a large literature in development economics that studies participation in RoSCAs, considering explanations grounded in commitment and their limitations in this setting. We then show how misperceptions about lottery probabilities can interact with these explanations and help rationalize participation.

[21] The calculation covers financial returns only, not any value of owning the asset early beyond the benchmark rates.

We write a model to examine individual decisions. We focus our discussion on non-bidders, who suffer large expected losses. It is also natural to assume non-bidders are the ones who suffer from time inconsistency, as bidding requires accumulation of resources. Furthermore, their participation is pivotal for the sustainability of groups with bidding, otherwise, bidders would have no one to borrow from.

4.1 Setup: indivisible consumption

Consider a mass of agents indexed by i who want to acquire a lumpy indivisible good with normalized price 1. Let $t \in \{1, 2, 3\}$ denote time periods. Let $\frac{1}{2} > y \geq \frac{1}{3}$ denote per-period income, so the agent can only buy the good alone at $t = 3$. Let c_t denote nondurable consumption, and $d_t \in \{0, 1\}$ denote ownership of the indivisible good at t. The indivisible good yields a flow of ξ in $t \in \{1, 2\}$ and $\Delta \geq \xi$ at $t = 3$, possibly capturing value beyond the model. Let $v(\cdot)$ denote concave utility from nondurables with $v(0) = 0$ and $v'(0) = \infty$. Consumers are quasi-hyperbolic discounters with present-bias parameter $\beta \leq 1$. Period utility is $u_t = v(c_t) + \xi d_t$ for $t = 1, 2$ and $u_3 = v(c_3) + \Delta d_3$. The consumer may save at $r^S = 0$, a normalization.

A rotating finance contract (τ, ℓ) charges a fee τ, leading to installments $q_t(\tau) = (1 + \tau)/3$ and provides a probability $p_t(\ell)$ of obtaining the good at t, which depends on the share of lottery awards ℓ. The random RoSCA benchmark is given by $(0, 1)$. We assume a member who has been awarded pays her remaining installments. A member not yet awarded may cancel at $t = 2$ and forfeits what she paid. To simplify exposition, we omit details of group operation, budget, and the bidding procedure when $\ell < 1$.

The self at t maximizes $u_t + \beta \sum_{s>t} u_s$, taking her later selves' choices as given. The decisions boil down to two options: save alone or join the rotating finance group. In general, an option to borrow at $r^B > r^S$ will reduce the parameter space in which individuals join the group.

4.2 How does the literature understand rotating finance instruments?

The literature broadly discusses the demand and sustainability of rotating finance groups as a result of poor outside options to save and borrow.

First, following Besley et al. (1993), when credit is unavailable, pooling savings offers a chance of obtaining an indivisible good earlier than saving alone. However, limited access to credit alone seems insufficient to explain participation: as Section 3.5 shows, non-bidders would, on average, obtain the good earlier by saving the installment amount. While consumers may still value the small chance of an early award, difficulty saving may be more important than credit constraints in explaining their participation.[22]

[22]The credit rationalization also seems insufficient to explain the rigid allocation mechanism structure and sus-

A second interpretation is more prominent in the literature: rotating finance groups serve as commitment devices to help people save (Bryan et al., 2010; Breza and Kaur, 2026). Demand for commitment can emerge from variants of time inconsistency or other microfoundations, such as household conflict (Anderson and Baland, 2002; Ambec and Treich, 2007; Gugerty, 2007). The proposition below captures this idea, similar to the results in Basu (2011).

Proposition 1. *Fix a contract (τ, ℓ), with $q(\tau) < y$ affordable. Let $\bar{\beta}$ be the lowest β at which a consumer buys the good by saving alone, and consider consumers with $\beta < \bar{\beta}$. Then:*

(i) A random RoSCA, $(0, 1)$, is joined iff $\underline{\beta}^R \leq \beta < \bar{\beta}$.

(ii) A consórcio, (τ, ℓ), is joined iff $\underline{\beta}^C(\tau, \ell) \leq \beta < \bar{\beta}$, where $\underline{\beta}^C(\tau, \ell) \geq \underline{\beta}^R$ is decreasing in ℓ and increasing in τ.

(iii) The ratio $\underline{\beta}^C(\tau, \ell)/\underline{\beta}^R \geq 1$ is decreasing and convex in ℓ.

All proofs are in Appendix H. The lower bound of the interval in β, expressed in (i) and (ii), captures the effectiveness of commitment: the lower it is, the higher is the scope for present-bias to drive demand for rotating finance groups. The upper bound $\bar{\beta}$ is the saving threshold: above it the consumer buys the good by saving alone. It is the same for both contracts and does not depend on τ or ℓ.

As Proposition 1(ii) shows, consórcios are less effective as commitment devices because of the lower share of lotteries $\ell < 1$, and to some extent, of the high fees charged. In particular, the probability of obtaining the good at each period is part of the commitment force of rotating finance, as it incentivizes the participant to make payments to the group while waiting for the award.[23] Furthermore, as (iii) shows, the rate at which consórcios depart from RoSCAs is convex in ℓ, suggesting that greater reductions from $\ell = 1$ increasingly weaken the strength of consórcios as commitment devices. The very low ℓ observed in the market therefore complicates the explanation of consórcios as commitment devices under correctly perceived odds. Furthermore, Basu (2011) shows how sophisticated agents who join should not renege on the contract. Contrary to most other RoSCAs, we see high cancellation rates, suggesting the need for additional mechanisms such as stochastic present-bias (e.g., Duflo et al., 2011). Additionally, partial naivety can also explain cancellations, but would weaken demand for commitment (John, 2020).

tainability of groups. Besley et al. (1994) shows that a credit market composed of the same group resources is often more efficient, therefore raising the question of why a system of peer-to-peer lending would not replace the rotating structure if access to credit was the key motivation.

[23] At $p = 0$, demand for illiquidity is the sole commitment force. With impersonal groups, the consórcio turns into a product that is indistinguishable from many other illiquid savings instruments, such as certificates of deposit.

4.3 Wrong beliefs about lottery probabilities

As shown in Section 3.6, consumers in our sample hold substantial misperceptions about lottery chances, particularly overoptimism. These beliefs may partly reflect how lotteries are presented and discussed in the consórcio market. Firms are not mandated to disclose data about expected chances over time. Moreover, the randomness of lotteries may lead consumers to focus on the possibility of receiving an award at any point, rather than on how their probability of receiving one evolves over time.[24]

Part of the difficulty learning about the true lottery probability over many time periods is that it is an equilibrium outcome: it depends on the composition of bidders and non-bidders, their bidding decisions, and firms' decisions. Evaluating these probabilities requires consumers to anticipate the behavior of other participants under uncertainty. Our reduced-form parameter captures potential failures to form these equilibrium expectations correctly. Misperceptions may also reflect how brokers and firms communicate, advertise, or shroud information about the allocation schedule, such as the number of lottery or bid awards (Gabaix and Laibson, 2006). In this sense, the contract has two prices: the fee charged, reflected in the installment, is the base price, while the lottery odds are the "hidden" price. Under this interpretation, avoiding the hidden price — the "sophisticated" action — does not necessarily rely on information about lotteries, but rather on liquidity.

To capture the role of misperceptions in the model, define the perceived probability of a lottery award as $\hat{p}(\ell) = \frac{1}{3} + \lambda\left(p(\ell) - \frac{1}{3}\right)$, where $\lambda \in [0,1]$ is a reduced-form parameter governing the accuracy of consumers' beliefs. When $\lambda = 1$, consumers correctly perceive lottery probabilities. As λ decreases, they become overoptimistic about early awards and less sensitive to the true odds.

Proposition 2. *Fix a contract (τ, ℓ) with $\ell < 1$, and a consumer who would join a pure lottery contract at fee τ.*

(i) there is $\bar{\lambda}(\tau,\ell) > 0$ such that she takes up the contract (τ,ℓ) whenever $\lambda < \bar{\lambda}(\tau,\ell)$;

(ii) the band $[\underline{\beta}^C(\tau,\ell), \bar{\beta})$ widens as λ falls;

(iii) the highest fee she pays for a contract with lottery share ℓ, $\bar{\tau}(\ell;\lambda)$, rises as λ falls.

As Proposition 2(i) shows, the perception of probabilities can rationalize participation even when there is no demand for commitment, and make consórcios attractive for consumers who might reject them if they knew the right odds. Therefore, misperceptions about lottery odds can rationalize entry when ℓ is low.[25]

[24] Indeed, Time Magazine (1967) describes consórcios as "something that is both safe and lucky", suggesting that the appeal of lottery-based allocation was already prominent in early descriptions of consórcios.

[25] There are other possible rationalizations outside the scope of this paper. Consumers may experience anticipatory utility, hope or aspirations. Consumers may also misperceive their bidding prospects, expecting to receive future windfalls or to save and be able to bid. Or consumers may demand the option value of bidding even if they end up rarely exercising it.

The perception of probabilities interacts with both commitment and early access. First, as Proposition 2(ii) shows, misperceptions and commitment value are complements: an increase in perceived odds widen the band in which sophisticated present-biased consumers join. Equivalently, $\bar{\lambda}$ is increasing in β: the more present-biased the consumer, the more misperception a contract needs to retain her, since more of its value must arrive through the lottery. Second, as Proposition 2(iii) shows, perceived odds raise the willingness to pay for the chance of early access, the access to credit channel of (Besley et al., 1993).

Furthermore, the perception of probabilities can also interact with supply side responses. When designing a group, firms considering a marginal increase of lotteries provided weight the demand response of consumers. If consumers are insensitive to variation in true probabilities, as predicted by most models where consumers face decision frictions due to imperfect information, firms' incentives to compete by providing more lotteries are weakened.

5 Experimental and survey evidence on misperceptions

In this section, we examine heterogeneity in beliefs and whether correcting misperceptions changes consumers' beliefs and decisions. We survey consumers and brokers and randomly assign consumers to short videos that disclose how often awards go to bids and how long lottery waits are, with one version also explaining how group size affects lottery chances. We find optimism about lottery probabilities is widespread, even among experienced consumers and brokers. The information treatment partly corrects beliefs, and the basic video raises the valuation of a lottery-intensive group offer among consumers who cannot bid.

5.1 Survey and experimental design

We design surveys to capture beliefs about relevant attributes of consórcio contracts. We then design an information provision experiment that leverages the Central Bank data to disclose the true share of bid awards and how they relate to the wait time for a lottery.

5.1.1 Belief measurement: lottery wait times and bid award shares

First, we elicit beliefs about the payoff-relevant object for a consumer who never bids: the estimated lottery wait time and probability of early award, defined as an award in the first half of a contract.[26] Second, we elicit beliefs about the shares of bid awards. We tailor these questions to each respondent's desired asset category and contract duration and incentivize quantitative answers

[26]For the market, a $Beta(\alpha, \beta)$ distribution fitted to these two moments tracks the cumulative distribution of lottery-only wait times within 7 percentage points (Figure 5, panel B). Because contracts are long, eliciting the full distribution was impractical, and piloting also showed that more granular measures performed poorly.

with accuracy bonuses worth up to 50% of the base survey payment. We measure each belief using complementary qualitative, quantitative, and probabilistic formulations (Haaland et al., 2023). For the bid share, respondents report the same object using natural language (e.g., "more than half"), a visual share, and the probability that a randomly selected award during the first half of the contract was made through bidding. Appendix Figures G.2 and G.3 provide screenshots of the three types of belief questions.

5.1.2 Information treatments

We designed our information provision experiment with a focus on the allocation mechanism, the share of lottery and bid awards, and the expected wait time for a lottery. The primary goal was to correct misperceptions about consumers' lottery award chances. In addition, we explain mechanics: more bid awards mean fewer lottery awards, and therefore longer lottery wait times.[27]

Importantly, we provided no information about prospective financial returns, how to compare consórcios to savings or borrowing, optimal bid strategies, or any rhetorical arguments focusing on the financial advantages of consórcios.

We design two information arms, labeled D for "debiasing" and DG for "debiasing + group size mechanics". Both information arms present the true bid share at the group midpoint for the respondent's asset category, computed from the Central Bank data as in Figure 5; we refer to this number as the signal. DG additionally highlights that there is variation in group sizes, that group size matters for lottery chances because of the one-lottery-per-month requirement, and that group sizes have doubled in recent years. The intention of arm DG was to provide the link between a useful observable proxy (group size) and lottery wait times.[28] Videos were tailored for each asset category. Script details are available in Appendix G.

To avoid differential attention or attrition due to the video treatment, we make an active control group. We use a public Central Bank video containing very basic information about consórcios.

Appendix Figure A.16 reports the distribution of video completion by arm: median completion is essentially 100% and mean completion is about 95% in each arm, with roughly 91% of respondents watching at least 90% of their video.

5.1.3 Incentivized valuation of a lottery-intensive offer

If consumers misperceive lottery probabilities, it is also possible they are insensitive to changes in the real number of lottery awards. Lower sensitivity connects demand-side frictions with supply-

[27] In piloting interviews with consumers and brokers, they sometimes expressed that bid awards do not delay lotteries since bids add resources to the pool. This is a misperception: bid awards always remove net resources from lottery awards. Appendix Figure A.13 shows a 0.74 correlation between early bid awards and lottery wait times.

[28] Appendix Figure A.15 shows the size gradient in finished groups: by the group midpoint, a never-bidder has a 30% chance of an award in the smallest fifth of groups and 7% in the largest.

side responses: firms have weaker incentives to offer lottery-intensive groups if demand is less likely to respond to these offers. This mechanism is consistent with firms' design of low-lottery groups, as observed in the data. But the scarcity of groups with a high proportion of lottery early awards presents a challenge to test whether correcting misperceptions increases valuation of groups with more lotteries (e.g., 91% of groups have more bids than lotteries, as shown in Appendix Figure A.14).

We partner with a consórcio firm to test whether disclosure increases demand for groups with more lotteries. We create a prospective offer for such a group, and present them to consumers in our survey. We then measure consumers' valuation of the offer through an incentive-compatible mechanism.

The proposed group would have only lottery awards in its first 12 months, at least two lotteries per month thereafter, and a smaller group size. Simulations suggest that 40 to 60% of awards would be allocated by lottery (around the 95^{th} percentile of observed groups), with a 30 to 40% chance of an award by the midpoint, compared with 10 to 15% in observed groups. Respondents were told that choosing access to this real offer could authorize the administrator to contact them about joining. We did not disclose the administrator's identity or fees, keeping the offer centered on lottery allocations.

We elicited valuations using a multiple price list, in which respondents chose between access to the offer and a bonus up to BRL 50 or BRL 100 (two to three times the base survey payment). Participants first answered an example question and were then instructed to treat each choice as final. The price list procedure is simple but can lead to censoring, as participants could have valuations well above BRL 50 or 100, and others could be uninterested. Indeed, valuations are heavily censored: 75% are either the minimum (zero) or maximum possible value. We therefore measure demand as choosing the consórcio over the maximum offer.

5.1.4 Other outcomes

To measure perceptions of bidders' advantage, we asked whether they agreed that consórcios are generally only worth it for people who can bid, whether administrators tend to favor bidders, and whether they would recommend a consórcio to a friend or relative who cannot bid. To measure beliefs and intended bidding behavior, we elicited expected winning bids and the likelihood and maximum size of respondents' own bids, together with that bid's perceived chance of winning and expected time to award.

We also measured hypothetical choices. First, respondents chose between consórcio groups whose fees, monthly lottery awards, group sizes, and expected winning bids varied independently (Appendix Figure A.17), identifying each attribute's causal effect on the stated choice (Hainmueller et al., 2014). Second, respondents chose between a consórcio with average market attributes and

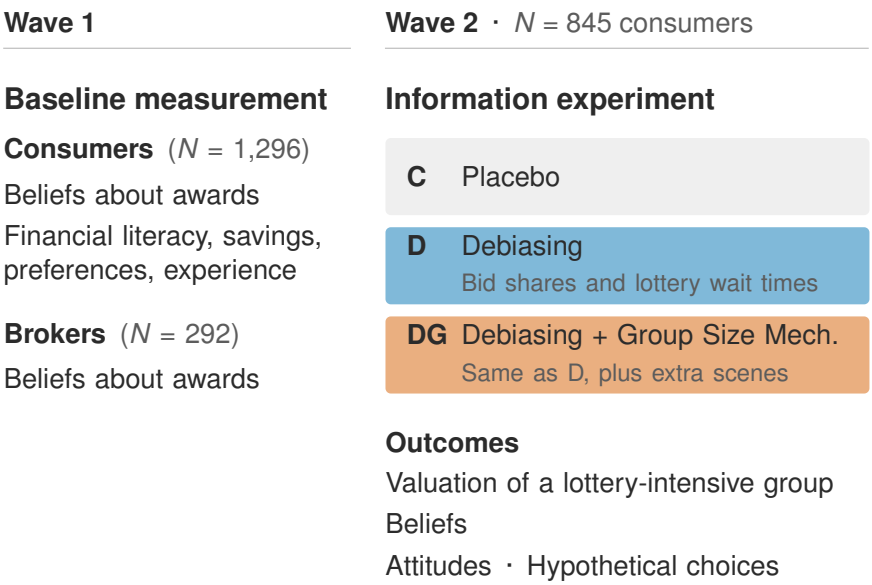


Figure 7: Experimental design.

either financing or saving, with borrowing or savings rates randomized. To facilitate comparison, we reported the downpayment and time required to accumulate it under financing, and the time required to purchase the good by saving an amount equal to the consórcio installment.

5.2 Recruitment, consumer characteristics, and balance and attrition

We recruit consumers and brokers through social media ads explicitly mentioning consórcios, targeting potential customers who have expressed interest in the product ("leads"). This approach reflects the Brazilian market, where social media use is widespread and consórcios are commonly advertised by brokers and firms. To better approximate actual potential customers, the ads did not mention monetary incentives, and we excluded respondents who reported being unfamiliar with consórcios. Recruitment ran from June 3 to August 19, 2026.

For consumers, the study consists of two survey waves (Figure 7). Wave 1 elicits beliefs, consórcio experience, financial literacy, savings behavior, and demographic characteristics. Wave 2 delivers the randomized information provision, repeats the belief elicitation, and measures outcomes.[29] Brokers are recruited alongside consumers but respond to a single survey.

5.2.1 Sample characteristics

Table 2 describes the consumer sample. Respondents are, on average, 47 years old and 59% male, compared with the representative population of 2025 consumers, which averages 41 years and is 65% male. Respondents are mostly interested in real estate (48%) and cars (41%) rather than

[29] Contract choices remain largely stable across waves: 88% of consumers retain the same asset category.

Table 2: Consumer sample description.

	Wave 1 Sample		Population	
	Mean (SD)	Median	Mean (SD)	Median
Panel A. Demographics				
Age	47.2 (12.5)	47.0	40.6 (13.5)	39.0
Male (%)	59.1		65.4	
Employed (%)	87.2			
College complete or higher (%)	69.7			
Primary financial decision maker (%)	82.7			
Panel B. Financial profile				
Financial literacy score (0–4)	3.2 (1.0)	3.0		
Savings buffer (months)	5.1 (4.0)	4.0		
Implied monthly installment (BRL)	1,792 (2,176)	1,250	783 (710)	556
Installment as % of monthly income	27.6 (61.3)	14.8	23.6 (34.7)	15.1
Panel C. Consórcio profile				
Past consórcio participation (%)	56.9			
Currently paying a consórcio (%)	25.5			
Discussed consórcio with vendor (%)	33.9			
Likely to join (≤3 mo.) (%)	56.0			
Would recommend consórcio (%)	69.4			
Already considering an administrator (%)	44.8			
Panel D. Asset of interest				
Real estate (%)	47.7		28.3	
Cars / vehicles (%)	40.8		36.9	
Motorcycles (%)	11.5		34.8	
N	1,296		–	

Notes. This table compares the Wave 1 consumer sample, after the screening quality filter, with a representative sample of Central Bank records for consumers who joined a consórcio in 2025–2026 (Population). Binary variables report the share in percent. Continuous variables report the mean, standard deviation in parentheses, and median.

motorcycles (12%). Consumers in the administrative data split more evenly across assets. Most respondents are employed (87%) and are the primary financial decision-maker in their household (83%). Respondents view the product favorably: 70% report they would recommend consórcios to friends and family. The Wave 2 subsample is similar (Appendix Table A.10).

The sample provides a mix of experienced and new consumers: 57% have participated in a consórcio before and a quarter report currently paying installments. Importantly, respondents report purchase intent: over half say they are likely to join a consórcio within three months, 45% report already considering a specific administrator, and about a third are talking with brokers.

Relevant to our model and mechanism, about 71% of respondents report that they would not have liquidity to bid about 50% of their simulated contract's face value during its first years. We call these respondents liquidity-constrained and the remainder unconstrained. Liquidity constrains correlate with household income, savings behavior, the ability to cope with emergencies, and access to credit (Appendix Table A.7).

Brokers. We survey 292 brokers (Appendix Table A.8). Three-quarters work at businesses dedicated to consórcios: 46% at specialized consórcio firms and a further 31% at consórcio administrators, with the remainder at financial-advisory firms, banks, and car dealerships. At specialized consórcio firms and financial-advisory firms, 60% of brokers sell for two or more administrators, against 9% of brokers employed by an administrator. They are active sellers, with a median of five sales a month, although half have been in the business for three years or less. About 78% advertise on social media, almost all of them on Instagram, and 61% post at least once a week. Consumers and brokers overlap in markets (Appendix Table A.9): they are regionally similar, both lean toward real estate and cars, and 84% of consumers who name an administrator they are considering name one that a broker in our sample sells for.

5.2.2 Balance and attrition

Appendix Table A.11 summarizes balance across covariates. Treatment groups are balanced: a joint test of orthogonality has p-values of 0.63 and 0.39. Appendix Table A.12 summarizes completion rates and attrition. For each treatment arm, we report the number assigned, the number who did not complete the survey, the number excluded by the prespecified quality filters, and the resulting main analysis sample. We distinguish survey attrition from pre-registered quality filters. Non-completion is small, at about 8%, and completion rates are balanced across treatment arms.

5.3 Survey evidence on misperceptions, learning, and strategies

5.3.1 Misperceptions about lottery probabilities are widespread across subgroups

In Section 3.6, we presented evidence that consumers are, on average, optimistic about lottery chances and wait times. When there are winners and losers in identical contracts, one hypothesis is that consumers may be heterogeneous in sophistication: some understand hidden prices or attributes, while others do not (Campbell and Ramadorai, 2026). Proxies for sophistication include financial literacy, education, and past experience.

To measure heterogeneity across subgroups, we combine the lottery wait and bid share questions into two different standardized indices and verify how much closer to the truth each subgroup is. For each consumer characteristic, we regress each belief index on an indicator for that characteristic and report robust 95% confidence intervals. Figure 8 combines the results: higher point estimates represent more precise beliefs.

The data does not support a straightforward sophistication story. Consumers who report they plan to bid early or who have liquidity to bid do not hold substantially more accurate beliefs about lottery probabilities or bid shares. Financial literacy, education, and prior consórcio experience all

predict more accurate beliefs, but none closes the information gap by more than 25%. Financial literacy presents the strongest correlation: respondents score 0.38 standard deviations higher on the bidding-share index and 0.22 standard deviations higher on the lottery-wait index. Respondents with prior consórcio experience also score higher on both indices. But overall, magnitudes are modest. Consumers also do not appear to be overconfident about their own chances: answers do not change when the questions ask about the respondent's own contract rather than a generic one.[30]

The lack of heterogeneity in beliefs about lottery probabilities is consistent with how different information feed into different strategies. A bidder who desires an early award needs to understand whether they can get it, and the sufficient statistic is the bid cut-off in the group.

Brokers' answers provide a comparison point to evaluate consumers' beliefs. Brokers are substantially more accurate about bid shares, sitting at about 50% of the gap between average consumers and true values. However, they do not hold more accurate beliefs about lottery wait times, and inexperienced brokers are more optimistic about lottery chances than the average consumer.

5.3.2 Broker and consumer responses are consistent with limited learning

As displayed in Figure 8, both brokers and past consumers display little learning about lottery wait times, suggesting learning can be slow.

Learning from repeated experience. Since contracts last many years, consumers have few draws to update their beliefs about lottery chances. For example, in the fastest-learning case, a consumer starts from equal odds between the full-lottery benchmark and the true distribution. This consumer would need about six draws of a non-bidding strategy to be 95% confident that the benchmark is wrong. Another issue with learning from experience or from others is that would-be bid winners also compete in lotteries. Thus, averaging over observed wait times for lottery awards produces a downward biased estimate of the expected wait time of waiting for a lottery.[31]

Learning from firm disclosure. Before purchase, firms disclose little information. We simulated purchases on the websites of the top 22 administrators, covering 90% of sales, and documented the information available. In only one case are the planned allocation rules available. After purchase, consumers can learn from monthly meeting minutes, which are required to report winning bids and the breakdown of awards. It would take significant effort for a consumer to take the breakdown of awards, along with the number of participants, and reconstruct the past lottery chances to obtain a forward-looking estimate. Consistent with the readily available information, brokers and experienced consumers are substantially more accurate about winning bids, as reported in the

[30] At endline, we randomized whether the lottery questions asked about "*a* contract" or "*your* contract". The framing has little effect on the index of lottery questions (-0.01 standard deviations, 95% confidence interval -0.13 to 0.12).

[31] Social learning may also be limited by factors such as noise from different strategies and biased sharing.

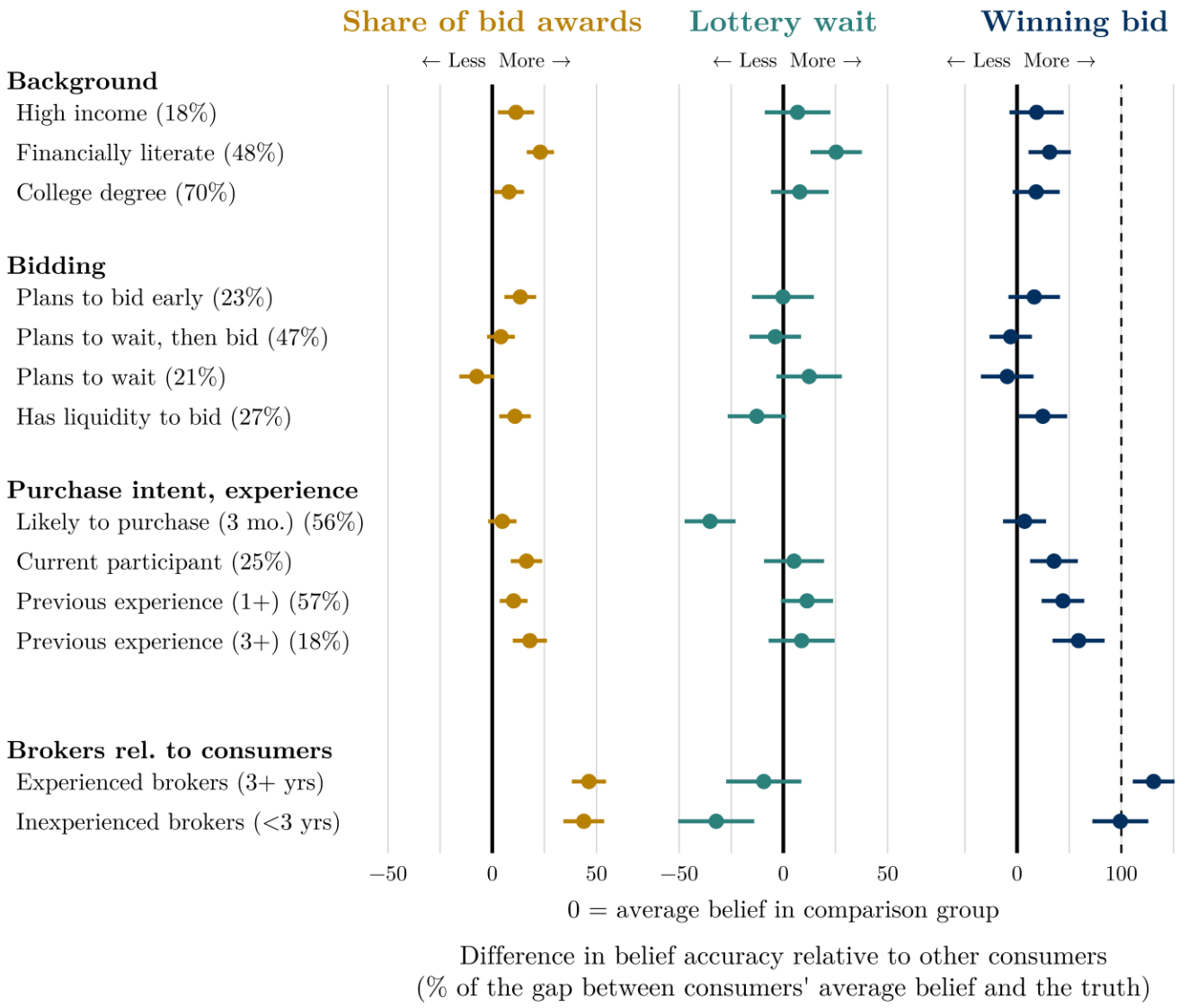


Figure 8: Heterogeneity in beliefs about award allocation across consumer subgroups.

Notes: This figure shows how much more accurate each subgroup's beliefs are than those of other consumers, as a percentage of the gap between consumers' average belief and the truth. Beliefs about the share of bid awards and about the lottery wait are each combined into an index of three standardized measures. Consumer rows compare consumers with and without each characteristic, within asset category; percentages in parentheses are the share of consumers with the characteristic. Broker rows compare brokers with all consumers. Bars show 95% confidence intervals from robust standard errors.

third column of Figure 8.

Learning from brokers. Brokers intermediate most consórcio sales (73% of past purchases in the survey data), but are similarly overoptimistic about lottery wait times, limiting their role in correcting consumers' beliefs.[32] Brokers also earn substantial commissions (about 3% of contract value), potentially distorting their incentives (Anagol et al., 2017; Egan, 2019). These incentives may also vary across consumer types. In a survey experiment, brokers were less likely to believe that longer award waits would reduce future business when the hypothetical client relied on lotteries rather than bids (54% versus 66%, $p = 0.04$).[33] Thus, brokers may have weaker incentives to serve lottery-dependent consumers, in addition to sharing their misperceptions.

5.3.3 Consumers report strategies that would lead to expected losses

Only 23% of respondents plan to bid early. Most plan to wait for the lottery, either bidding if the wait becomes too long (47%) or never bidding (21%), both of which are strategies likely to generate losses, as shown in Section 3.5. Plans vary with reported liquidity to bid, which captures a reasonable early bid threshold. Among consumers with liquidity to bid, 33% plan to bid early, compared with 19% among those without (a 13.7 pp difference, $p < 0.01$). Consumers without liquidity are instead twice as likely to plan to wait for the lottery alone (24% versus 12%, $p < 0.01$). Half of consumers with liquidity plan to wait and then bid, a share similar to those without liquidity (50% versus 46%, $p = 0.23$).

Beliefs about lottery chances are weakly related to the stated strategies. Within each liquidity group, the most optimistic half about an early lottery award, those reporting a chance above the median of 30%, are 4 to 5 pp less likely to plan to bid early. The difference is significant only among consumers without liquidity ($p = 0.04$; $p = 0.41$ among those with liquidity), and optimism is not associated with planning to wait. The difference by liquidity is about three times as large.

5.4 Effects of information experiment on survey outcomes

We next turn to estimate the effects of the randomized information provision on beliefs, choices, and attitudes. If consumers hold mistaken and overoptimistic beliefs about lottery chances, the information treatment should on average correct these beliefs. Our key hypothesis is that it will also change liquidity-constrained consumers' valuation of the prospective offer for a group with more lotteries by making it more attractive relative to the options in the market.

[32] Although brokers may answer strategically, 81% report owning a consórcio themselves, consistent with findings from other settings Linnainmaa et al. (2021).

[33] We randomly varied whether a hypothetical client seeking an award as soon as possible intended to obtain it through a bid or a lottery.

5.4.1 Empirical specification

We pre-registered the estimation of average treatment effects $\tau_D = E[Y|D] - E[Y|C]$ and $\tau_{DG} = E[Y|DG] - E[Y|C]$ using the augmented inverse-probability-weighted (AIPW) estimator, which adjusts for pre-treatment characteristics to improve precision (Athey et al., 2019). The outcome model is a causal forest fit on the randomization strata and on pre-treatment characteristics from the screening survey, including baseline measures of the outcome. For heterogeneity by a binary characteristic M, we project the conditional effect on M, which gives the effect at $M = 0$ and its difference at $M = 1$. We report heteroskedasticity-robust standard errors throughout.

5.4.2 Information corrects beliefs and changes product evaluations

Beliefs are partially corrected. Both treatments shifted beliefs in the expected direction, correcting average misperception gaps by about 40–50% across most measurements (Figure 9).[34] Corrections are similar as a percentage of the prior gap for both bidding shares and lottery wait times. Only bidding shares were numerically displayed in the video, while lottery wait times were only visually suggested. Qualitative responses reveal a similar pattern (Appendix Figure A.12). We cannot reject the equality of treatment effects on beliefs across the two arms.

Changes in average beliefs are accompanied by slightly greater confidence, consistent with uncertainty reduction in Bayesian updating (Augenblick and Rabin, 2021). Both treatments increase respondents' stated certainty in their beliefs by about 0.2 points on a five-point scale ($p < 0.01$).

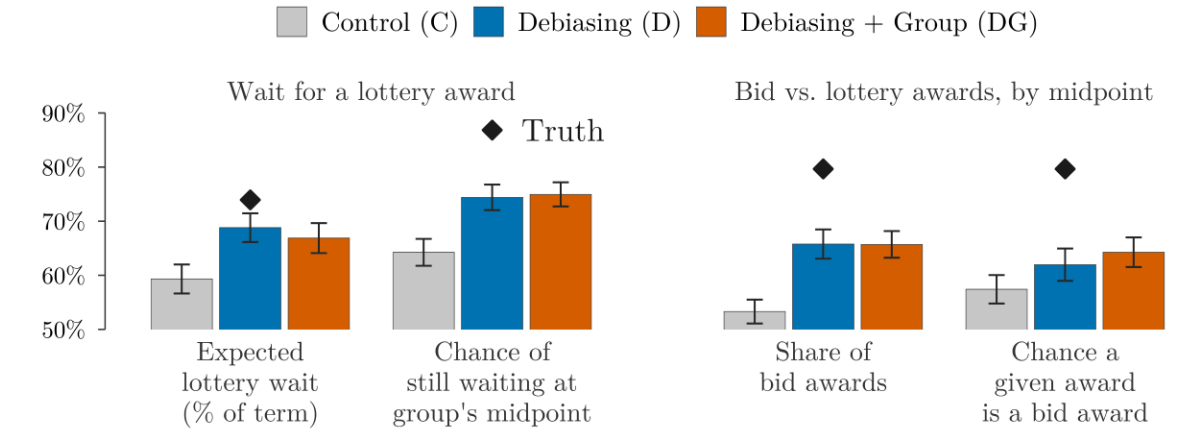
We test whether respondents with larger initial misperceptions revise their beliefs more in response to treatment. We estimate learning rates using the following pre-registered regression,

$$Updating_i = \beta_0 + \beta_1 T_i + \beta_2 Gap_i + \beta_3 Gap_i \times T_i + \varepsilon_i, \tag{2}$$

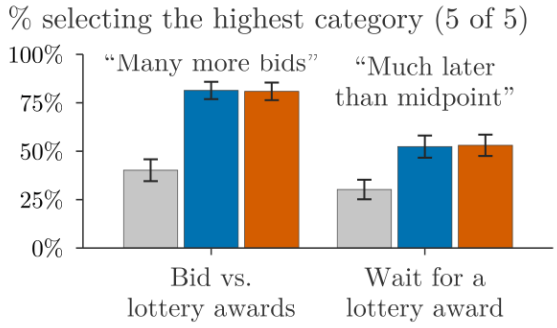
where $Updating = Posterior - Prior$ and $Gap = Signal - Prior$. As shown by Cullen and Perez-Truglia (2022), β_3 can be interpreted as the learning rate of the treatment. We estimate Equation (2) using the obviously-related instrumental variables (ORIV) estimator to account for classical measurement error in priors (Gillen et al., 2019).[35] Appendix Table A.14 displays the results under ORIV and OLS specifications for both treatments. For bidding shares, correcting for measurement error yields an estimated learning rate of 0.49, with no detectable level shift: respondents with larger initial misperceptions revise their beliefs more in response to treatment.

[34] Both treatments also raise perceived group sizes, a key input to lottery chances, likely because the videos show several awards per month. Only DG, which states group sizes explicitly, reduces the mean absolute error relative to the true group size.

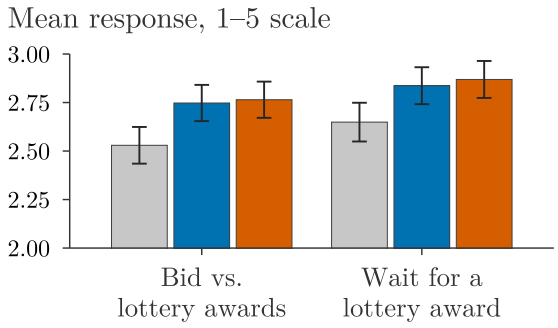
[35] As priors enter both sides of Equation (2), noisy priors can bias the measurement of β_2 and β_3. To see why, imagine a model where $Prior_i = \mu_i + \eta_{0i}$ and $Posterior_i = \mu_i + \eta_{1i}$, where μ_i is a true latent belief and η_i is random noise, independent of the latent belief and survey waves. In this model, there is no learning, but a regression based on Equation (2) recovers $\beta_2 = \frac{\sigma^2_\eta}{\sigma^2_\eta + \sigma^2_\mu}$.



Panel A. Quantitative beliefs



Panel B. Qualitative beliefs



Panel C. Certainty in beliefs

Figure 9: Treatment videos partially correct misperceptions.

Notes: This figure shows Wave 2 beliefs by treatment arm. Bars are arm means estimated with augmented inverse-probability weighting (AIPW), with 95% confidence intervals. Diamonds mark the true values from Central Bank data, averaged across respondents' chosen asset categories. Appendix Table A.13 reports the treatment effects for panels A and C.

For lottery wait times, treatment shifts reported beliefs, but we do not detect a differential response by initial belief gaps. This is consistent with noisy priors and mean reversion in repeated belief measurement.

Information strengthens perceptions of bidder advantage. Both treatments make respondents more likely to view consórcios as favoring those who can bid and make respondents more likely to say they would not recommend them to those who cannot bid (Appendix Table A.15). Agreement with the statement that consórcios are "only a good deal for those who can bid" increases by 10.6 pp under D ($p = 0.01$) and 8.1 pp under DG ($p < 0.05$), against a control mean of 48%. Both arms also increased agreement with a statement that "firms favor bidders" by 9.3 pp under D ($p = 0.02$), with a smaller and noisier effect under DG, against a control mean of 65%. On the recommendation margin, the share who would *not* recommend consórcios rose by 6.5 pp under DG and 6.2 pp under D (both $p < 0.05$, on a control mean of 15%), while the "do not

know enough to recommend" share fell by 5.4 pp under DG ($p < 0.1$, from 23%); we cannot reject equality of the two arms on any of the five outcomes.

Information spillovers to beliefs about winning bids. Respondents also updated their beliefs about winning bids. Relative to the control group, whose beliefs did not change between waves, D and DG raise the expected average winning bid by 5.5 and 6.3 pp (Appendix Table A.19, column 1). This suggests some sophistication: all else equal, learning that more awards go to bids should lower the bid needed to win, implying consumers infer that more bid awards mean stronger competition. Effects on bidding intentions are positive but small and imprecise, with the stated likelihood of bidding rising by 0.16 and 0.12 points on a five-point scale and the reported own bid by 1.7 and 2.2 pp, none statistically significant.

5.4.3 Information and demand for alternative contract designs with more lotteries

In our setting, disclosure informs all consumers, but more fundamentally, it can differentially affect how consumers choose contracts. Our prediction is that correcting misperceptions should increase valuation of the lottery-intensive group offer among consumers who are liquidity-constrained. We test this prediction using our pre-registered liquidity variable and other variables capturing intention to bid. Because of bunching at maximum and minimum values, we define the dependent variable as choosing the prospective offer over the maximum price at the multiple price list (MPL) elicitation.

Consistent with our prediction, the Debiasing (D) treatment increases choices of the offer at the maximum valuation by 8.3 pp ($p = 0.07$), a 25% increase relative to the control average (Table 3, column 2). In column 3, we restrict the sample to participants with monotonic answers in the multiple price list procedure, and the effect is slightly larger (10.4 pp, $p = 0.03$). Results are stronger when using a direct measure of intention to bid (column 4) and robust to using other measures of liquidity.

Consistent with belief correction, the treatment effects of D are also concentrated among the more optimistic half of respondents, those whose perceived chance of an early award is above the median of 30%. The effect among more optimistic respondents is 14.9 pp, 16.2 pp larger than among less optimistic respondents ($p = 0.04$; column 5).

In Section 4, we discussed how commitment and access to credit are two motivations for participating in rotating finance, but the probability of receiving an award early also affects the value of participation. In particular, disclosing true probabilities should make the offer of a group with more lotteries especially attractive to those who have trouble saving on their own or lack access to credit. Consistent with this prediction, the effect of D is concentrated among respondents with limited access to alternative sources of liquidity. Among those who did not save in the 12 months before Wave 1, the treatment increases choices of the offer at the maximum valuation by 20.5 pp,

23.6 pp more than among savers ($p < 0.01$; column 6). Similarly, the effect is 17.8 pp among respondents who expect a bank to deny them a loan, 15.9 pp larger than among those who expect approval at any rate ($p = 0.08$; column 7). The main results are similar when the multiple price list choices are converted to BRL (Appendix Table A.17).

The treatment effects of D are also concentrated among the more optimistic half of respondents, those whose chance of an early award is above the median of 30%. The effect among the more optimistic is 14.9 pp, 16.2 pp larger than among the less optimistic ($p = 0.04$; column 5).

In contrast to the effects of arm D among constrained respondents, the results for arm DG are essentially zero among both constrained and unconstrained respondents. We generally reject the equality of the D and DG estimates in the main treatment effects at the 10% level, suggesting results are not due to noise. This difference is puzzling because both treatment arms led to similar corrections in beliefs. One hypothesis is that adding extra scenes to the video may have led to a change in salience or attention, whereas in D the sole focus is lottery probabilities. Another hypothesis is that the extra scenes explaining the group size mechanism made DG more confident in their choices: we show below that, under DG, the less optimistic non-optimists in the DG group increases their stated choices of consórcios.

Together with evidence that treatment effects are larger among respondents who were initially optimistic about the chance of an early award, we use the moderator test proposed by Kwon and Roth (2026) to investigate whether the valuation effect is consistent with mediation by beliefs. The test cannot reject full mediation through beliefs ($p = 0.4$).

5.4.4 Information does not change stated choices in conjoint analysis

In choices using side-by-side comparisons of groups with randomized attributes (fees, group size, minimum number of lotteries, and average winning bids), consumers consistently prefer smaller groups and more lotteries. In the control arm, consumers are 15 percentage points more likely to choose a group offering at least two lotteries per month rather than one ($p < 0.01$), and 15 and 23 percentage points less likely to choose medium and large groups over small groups, respectively ($p < 0.01$). These preferences contrast with observed market allocations, where groups are typically large and offer few lotteries. This discrepancy suggests that decision frictions, including how products are presented and compared, may drive a wedge between consumers' stated preferences and market choices.

Information provision does not significantly change how consumers choose between attributes. None of the treatment effects on preferences for the four attributes is statistically distinguishable from zero at the 5% level (Appendix Figure A.18). This suggests that the treatment operates through changes in information rather than changing underlying stated preferences (Handel and Kolstad, 2015), consistent with its focus on allocation mechanisms, bidding shares, and lottery

Table 3: Treatment effects on valuation of lottery-intensive groups.

	Valuation of group with more lotteries (chose offer at the top price)						
		Heterogeneity by liquidity			By beliefs	By outside options	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Debiasing (D)	0.059	0.083*	0.104**	0.095**	-0.013	0.205***	0.178**
	(0.039)	(0.046)	(0.046)	(0.045)	(0.053)	(0.064)	(0.078)
× liquidity to bid 50%		-0.095	-0.100				
		(0.087)	(0.088)				
× strategy = bid early				-0.155*			
				(0.091)			
× more optimistic about early award					0.162**		
					(0.078)		
× saved in last 12 months						-0.236***	
						(0.080)	
× bank would approve a loan							-0.159*
							(0.090)
Debiasing + Group (DG)	-0.010	-0.024	-0.032	-0.010	-0.049	0.016	0.012
	(0.038)	(0.044)	(0.045)	(0.043)	(0.051)	(0.061)	(0.072)
× liquidity to bid 50%		0.054	0.086				
		(0.085)	(0.086)				
× strategy = bid early				-0.001			
				(0.090)			
× more optimistic about early award					0.087		
					(0.076)		
× saved in last 12 months						-0.042	
						(0.078)	
× bank would approve a loan							-0.030
							(0.085)
$D = DG$ (p-value)	0.06	0.01	0.00	0.01	0.38	0.00	0.02
Sample	Full	Full	WTP-consistent	Full	Full	Full	Full
Control mean	0.33	0.33	0.31	0.33	0.33	0.33	0.33
Observations	845	845	810	845	845	845	845

Notes. The outcome is an indicator that the respondent chose the consórcio over the gift card at the top of the price list (Section 5.1.3). Estimates are augmented inverse-probability-weighted (AIPW) effects from causal forests that compare each arm with the control group. In columns 2–7, we present effects when the moderator equals zero (no liquidity to bid, no plan to bid early, less optimistic, did not save, expects a loan denial) and the difference when the moderator equals one. Liquidity to bid is the self-reported ability to pay a 50% bid. The more optimistic are respondents whose Wave 1 belief about the chance of a lottery award in the first half of the group's duration is above the sample median of 30%. Saving is any saving in the 12 months before Wave 1; loan approval is the expectation that a bank would approve a loan, at a reasonable or at a high interest rate, as opposed to denying it. Column 3 restricts to respondents with a consistent (monotone) price-list choice. The $D = DG$ row tests equality of the two arms' effects, in the moderator-zero group. Standard errors in parentheses. Control means are control-group averages of the dependent variable. $^{*}\,p < 0.1$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

wait times rather than value judgments about attributes.

The null effects also provide evidence against increased attention to attributes as the primary mechanism of treatment effects (Conlon, 2026). Both treatments emphasized lotteries, while the *DG* treatment additionally highlighted group size. Yet neither treatment changed average choices of these attributes.

5.4.5 Information changes stated choices between financial products

In side-by-side comparisons of a consórcio with a loan or with a saving strategy to purchase the asset, the effects of information depend strongly on prior beliefs about the chance of an early lottery award. These beliefs are also a strong predictor of choices in the control group: the most optimistic half is 15 pp less likely to choose the loan and 24 pp less likely to choose saving over the consórcio ($p < 0.01$, Appendix Table A.16, columns 2 and 5).

Debiasing (D) raises the choice of the loan among the most optimistic. On average, D raises the share choosing the loan by 6.2 pp ($p = 0.06$) from a control share of 18% (column 1), and the effect is 16.5 pp larger among the more optimistic ($p = 0.01$; column 2). On the saving margin the same pattern is weaker (the difference by optimism is 14.1 pp, $p = 0.09$; column 5). Independent of optimism, the information effect is stronger for consumers with liquidity to bid (column 3), although the coefficient is noisier. This is contrary to our priors based on the findings documented by Section 3 that bidding in consórcios can lead to positive returns, equivalent to a subsidized loan. At the same time, our information treatment did not inform consumers about returns or comparisons with outside options. One explanation is that liquid respondents are able to afford the downpayment on a loan immediately. Consistent with this idea, we do not see a stronger treatment effect on choices on savings.

The *DG* arm presents similar patterns of splits by optimism, but is also seems to reinforce the choice of the consórcio among the less optimistic. Among the less optimistic half, *DG* increases the choice of the consórcio over saving by 16.3 pp ($p < 0.01$; column 5). This effect is concentrated among those with liquidity to bid, whose share choosing saving falls by 33.6 pp ($p < 0.01$; column 6), from 74%, compared with no effect under D (difference: $p < 0.01$). Among the more optimistic, *DG* has no significant effect on either margin, but under both arms the more optimistic move away from the consórcio significantly more than the less optimistic (columns 2 and 5). The additional group-size information appears to reinforce the less optimistic consumers' preference for the consórcio. Thus, despite similar average changes in beliefs, the two treatments shift choices among different consumers and on different margins (the treatment arms are statistically different, with $p = 0.01$ and $p = 0.02$; columns 1 and 4).

5.5 Discussion and implications for disclosure policy

Taken together, the experimental results suggest that misperceptions about lottery chances influence both participation in consórcios and the types of contracts consumers value. Correcting these beliefs increased the perceived advantage of bidders and shifted some initially optimistic consumers toward conventional loans. At the same time, liquidity-constrained consumers increased their valuation of a lottery-intensive alternative. And in side-by-side comparisons, consumers already prefer smaller groups and more lotteries, and information does not significantly change these preferences. Thus, information appears to affect consumers' evaluations of the options available in the market, rather than changing their stated preferences over attributes. These findings suggest that imperfect information helps sustain demand for consórcios, and in particular for groups with few lotteries, even when consumers appear to prefer alternative designs when attributes are transparent.

Our results have two implications for disclosure policies. First, we provide evidence that consumers respond to information, which strengthens the motivation for disclosure policies for consumer protection. Second, and more importantly, disclosure of relevant product attributes can have different effects depending on consumers' characteristics, potentially changing the composition of consumers in the market. In our setting, the relevant characteristic is liquidity. Disclosure may be especially effective because the redistribution generated by bidding depends on the participation of both bidders and non-bidders. When non-bidders leave the market or move to lottery-intensive groups, they reduce the scope for bidding and the associated redistribution. Thus, disclosure may induce consumer sorting that, in equilibrium, reduces both bidding and redistribution.

Policymakers may also consider directly regulating the extent of bid awards. Currently, regulations require at least one lottery award. But this leaves room for firms to adjust their contract designs and reduce the effectiveness of this requirement (Campbell, 2016). The first consórcio regulation, introduced in 1972, required at least half of the funds disbursed by a group to be allocated through lotteries. In 1991, this requirement was replaced by the current minimum of one lottery award per month. Because group sizes are unrestricted, firms can increase the number of participants while maintaining the required minimum number of lotteries. Consistent with this possibility, the share of awards allocated through bidding rose from 54–66% in 2008 to approximately 80% in 2026 (Appendix Figure A.19). Therefore, policymakers should consider regulations that mandate lottery awards as a share of total awards or total funds disbursed.

6 Conclusion

We study Brazilian consórcios, the world's largest formal market for rotating savings-and-credit products. We examine how consumers' access to liquidity interact with contract design to create

winners and losers. We show that consumers without liquidity must rely on lotteries to access credit, which are scarce in early periods. We argue the existing explanations for participation in this type of mechanism, based on demand for commitment, are weaker when chances of early access are small and financial losses are large. We then investigate whether misperceptions about award probabilities weaken demand for greater lottery access. Surveys document systematic optimism about lottery chances, and an information experiment shows that correcting these beliefs increases liquidity-constrained consumers' valuation of a lottery-intensive alternative.

Studying consórcios brings firms' contract design choices into the analysis of rotating finance. The findings also contribute to the literature on redistribution through household financial products. While differences in financial sophistication can generate cross-subsidies across consumers, access to higher expected returns in our setting does not require better information about lottery probabilities. Consumers with liquidity to bid can obtain better returns even when they hold substantial misperceptions. Widespread information frictions can therefore have unequal consequences for consumers with different access to liquidity.

Our findings also raise questions about the role of firms and brokers in shaping consumer information. We document limited disclosure of award allocations by firms. Brokers, meanwhile, hold relatively accurate beliefs about winning bids and the share of bid awards, but remain poorly informed about lottery probabilities. Information about bidding is more readily observable and useful in advising consumers who can bid, while information about lottery probabilities is harder to verify. Brokers may therefore transmit information selectively, even without deliberately misleading consumers. Moreover, the difficulty of verifying lottery probabilities may weaken incentives to communicate them accurately. Future research should understand whether supply-side communication sustains differences in consumer information, and thereby reinforces unequal financial outcomes and sustains regressive contracts.

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A Additional figures and tables

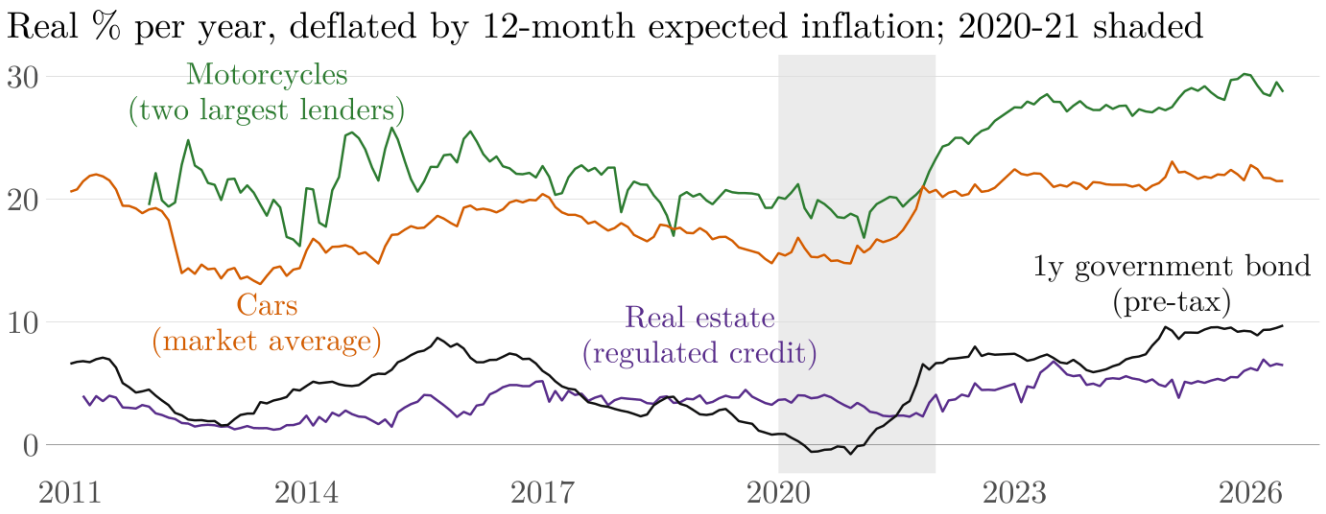
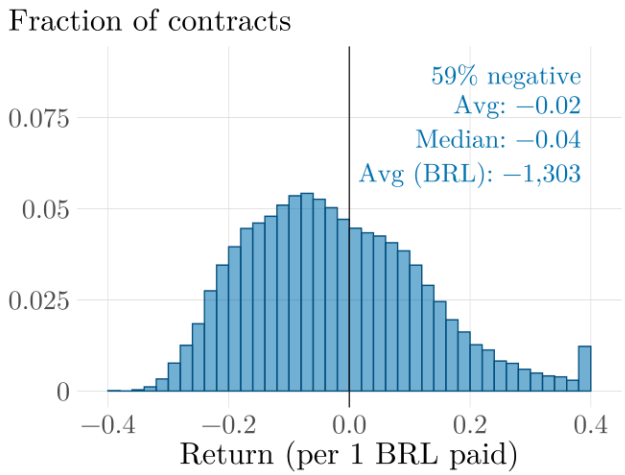
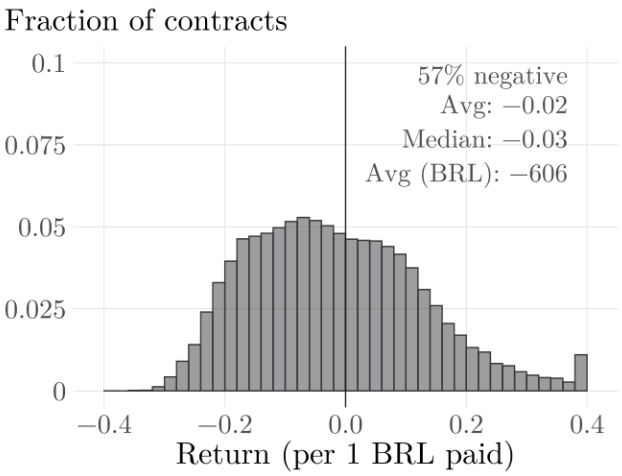


Figure A.1: Market interest rates.

Notes: This figure shows monthly real interest rates on one-year government bonds and on loans for cars, motorcycles, and real estate, from 2011 to June 2026. The government bond rate is the yield on fixed-rate Treasury bonds (*Tesouro Prefixado*), interpolated to one year to maturity and before income tax, from the Treasury's *Tesouro Direto* price history. The car rate is the Central Bank's average rate on new vehicle loans to individuals (SGS series 20749), which car loans dominate. The real estate rate is the Central Bank's average rate on housing loans to individuals at regulated rates (SGS series 20773). No motorcycle series exists, so the motorcycle rate averages the fixed-rate vehicle-loan rates of Banco Honda and Banco Yamaha Motor, the two largest motorcycle lenders, from the Central Bank's interest rates by financial institution. These two lenders also represent 80% of the consórcio motorcycle market. All rates are deflated by the median twelve-month-ahead inflation expectation in the Central Bank's Focus survey. The shaded band marks 2020–2021, when pandemic-era policy pushed short-term real rates below zero; we exclude these years when averaging rates for the benchmarks in Section 2.3.



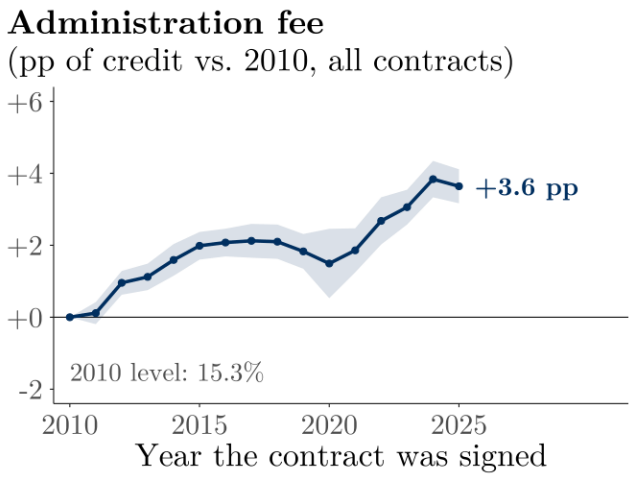
Panel A. Ongoing groups with 50% observed duration



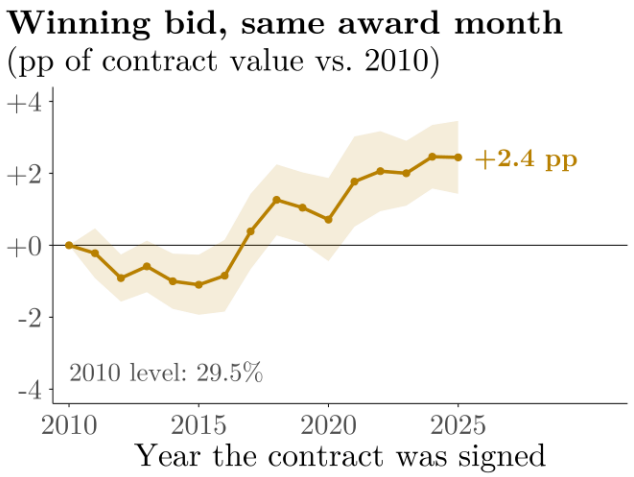
Panel B. Completed groups

Figure A.2: Returns under a stricter observation gate and in completed groups.

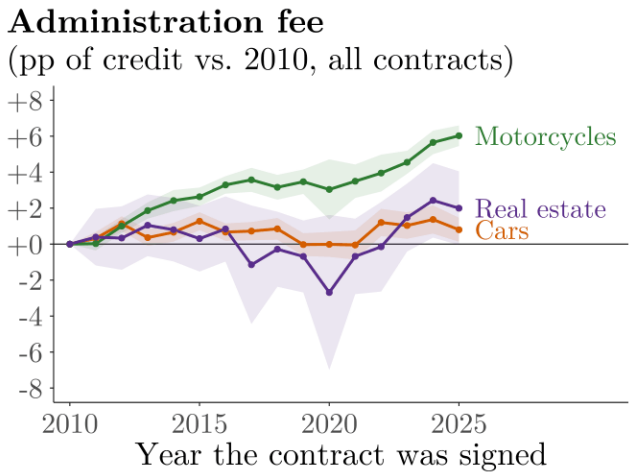
Notes: This figure shows net value calculated with Equation (1) across awarded contracts, as in Figure 2, panel A, in two other samples. In panel A, the sample includes awarded contracts in groups started between 2010 and 2025 that are completed or observed for at least 50% of their planned duration. Outcomes for contracts yet to be awarded in ongoing groups are predicted. In panel B, the sample includes only completed groups, which require no prediction. Returns beyond the axis range fall in the end bins.



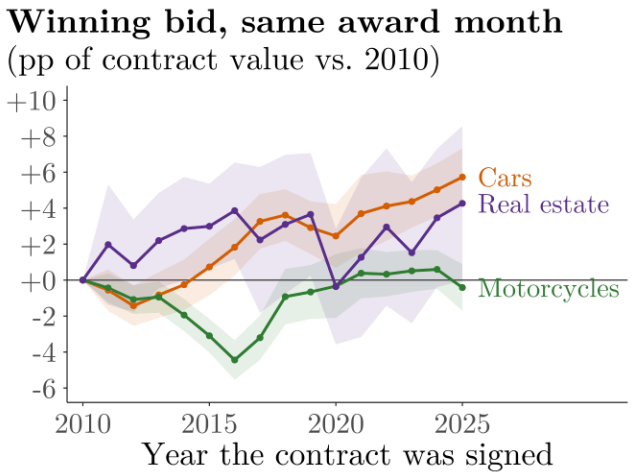
Panel A. Administration fee



Panel B. Winning bid



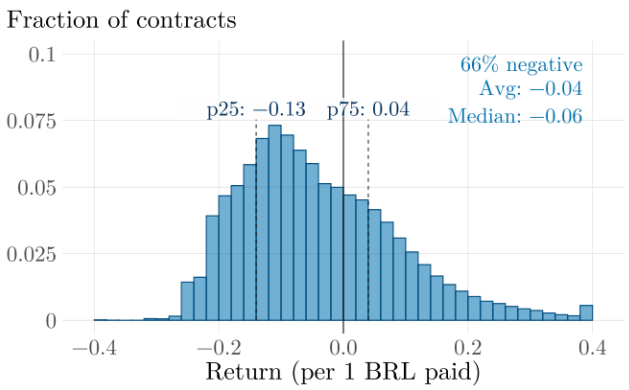
Panel C. Administration fee, by asset category



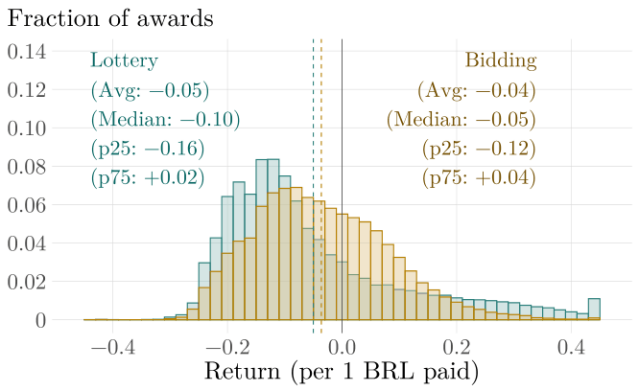
Panel D. Winning bid, by asset category

Figure A.3: Fees and bids by year the contract was signed.

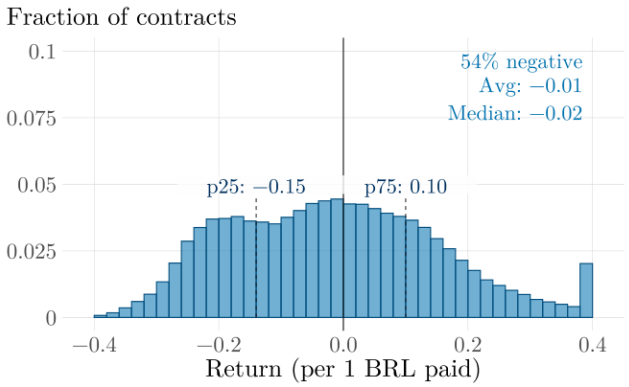
Notes: This figure shows the average change in administration fee and winning bids from 2010 until 2025. Each point is the coefficient on a year-of-signature indicator, in percentage points relative to contracts signed in 2010, from a regression with asset category dummies. Bands are 95% intervals with standard errors clustered by group. Panel A is the administration fee on all contracts. Panel B is the winning bid among contracts awarded by bidding, including fixed-menu bids, holding fixed the month of the contract at which the award happened, so that later cohorts are not compared with earlier awards. Panels C and D run the same regressions separately for each asset category.



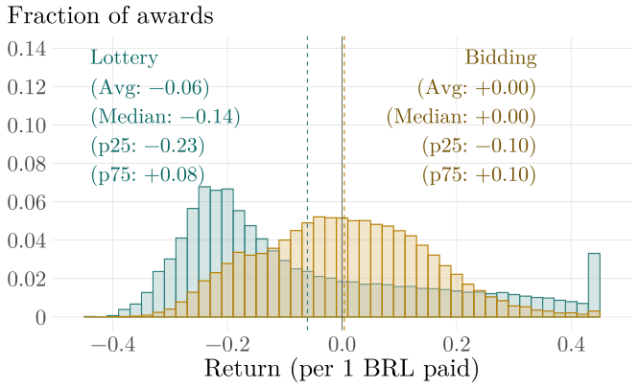
Panel A. Savings 0%, awarded contracts



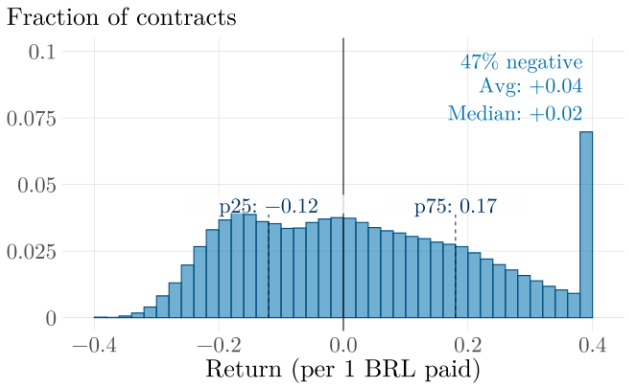
Panel B. Savings 0%, bid vs. lottery



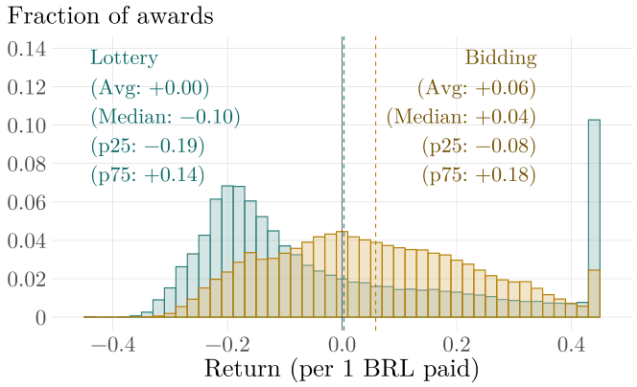
Panel C. Savings 8%, awarded contracts



Panel D. Savings 8%, bid vs. lottery



Panel E. Borrowing ×1.5, awarded contracts



Panel F. Borrowing ×1.5, bid vs. lottery

Figure A.4: Returns under alternative benchmark rates.

Notes: This figure shows the returns of awarded contracts in Figure 2, panel A (left), and of bid and lottery awards in Figure 4 (right), under alternative benchmark rates. In panels A to D, the savings rate is 0% or 8%, and borrowing rates move with it, keeping their spreads over the savings rate. In panels E and F, the savings rate stays at 4% and borrowing rates are 50% higher: 6%, 28.5%, and 36% for real estate, cars, and motorcycles. The sample is the same as in Figure 2. In the right column, the distribution within each award mode in completed groups is approximated from its quantiles under each scenario; the averages are exact. Returns beyond the axis range fall in the end bins.

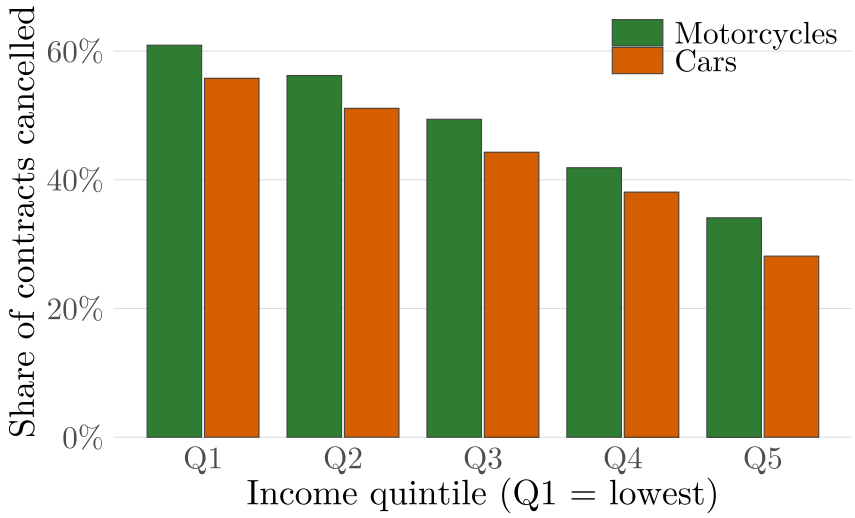
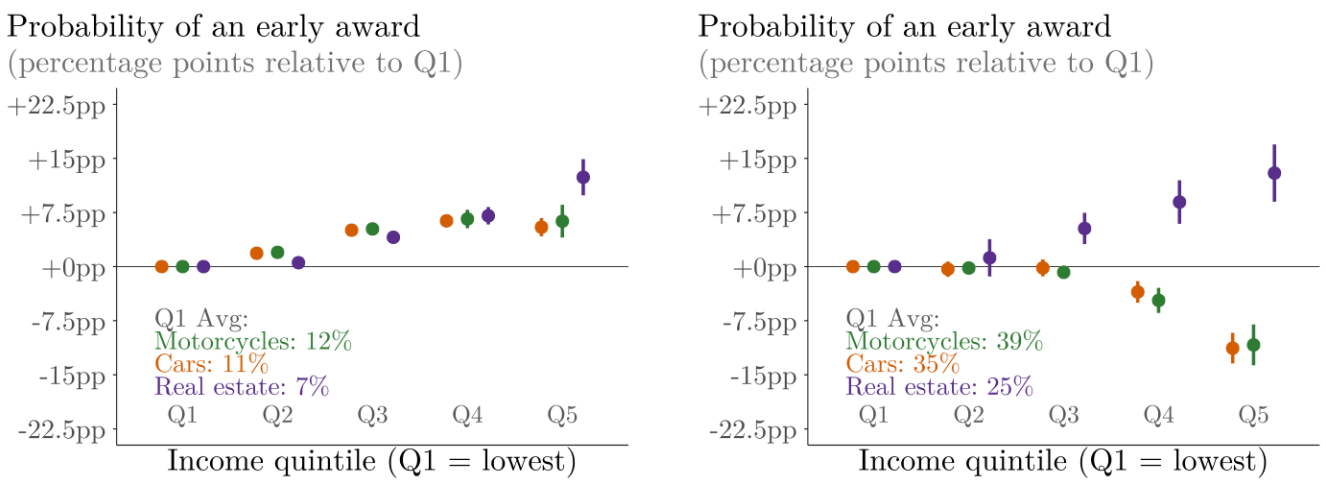


Figure A.5: Share of contracts canceled by income quintile, completed groups.

Notes: This figure shows the share of contracts canceled in each income quintile, in completed groups. Income quintiles are taken over self-reported incomes from the national credit registry. Real estate is omitted because few members of its completed groups have an income record.

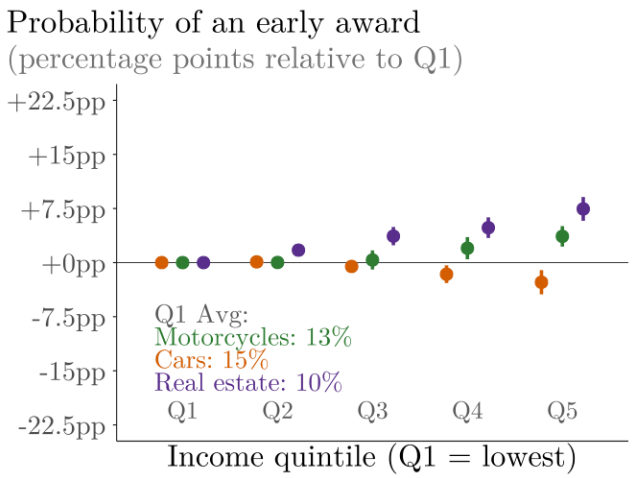


Panel A. All eligible contracts

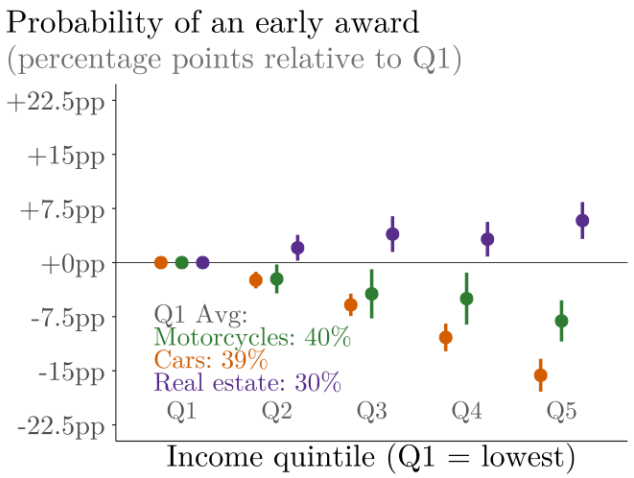
Panel B. Excluding canceled contracts

Figure A.6: Higher-income participants receive more early awards in the aggregate, but not uniformly among contracts that do not cancel.

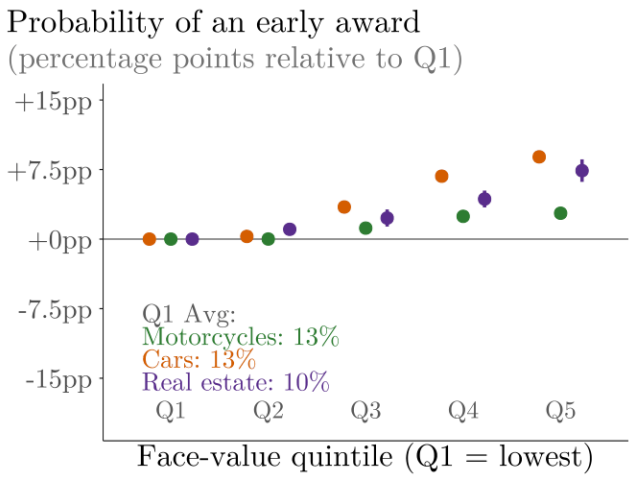
Notes: This figure shows the difference in the probability of an early award between each income quintile and the lowest, by asset category. An early award is an award within the first 25% of the group's planned duration. Panel A includes all contracts, and panel B excludes canceled contracts. Quintiles rank contracts by self-reported income at joining, from the national credit registry. The sample includes groups started between 2010 and 2025 that are completed or observed for at least 25% of their planned duration. The key reports the probability of an early award in the lowest quintile. Vertical lines are 95% confidence intervals.



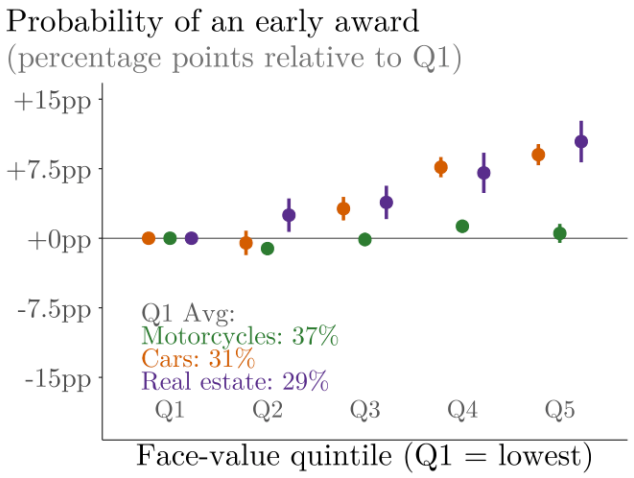
Panel A. Income, all eligible contracts



Panel B. Income, excluding canceled contracts



Panel C. Face value, all eligible contracts



Panel D. Face value, excluding canceled contracts

Figure A.7: Access to early awards by within-group quintiles of income and face value.

Notes: This figure shows the difference in the probability of an early award between each within-group quintile and the lowest, by asset category. An early award is an award within the first 25% of the group's planned duration. Panels A and C include all contracts, and panels B and D exclude canceled contracts. Quintiles rank contracts within their group, by income in panels A and B and by face value in panels C and D. The sample includes groups started between 2010 and 2025 that are completed or observed for at least 25% of their planned duration. The key reports the probability of an early award in the lowest quintile. Vertical lines are 95% confidence intervals.

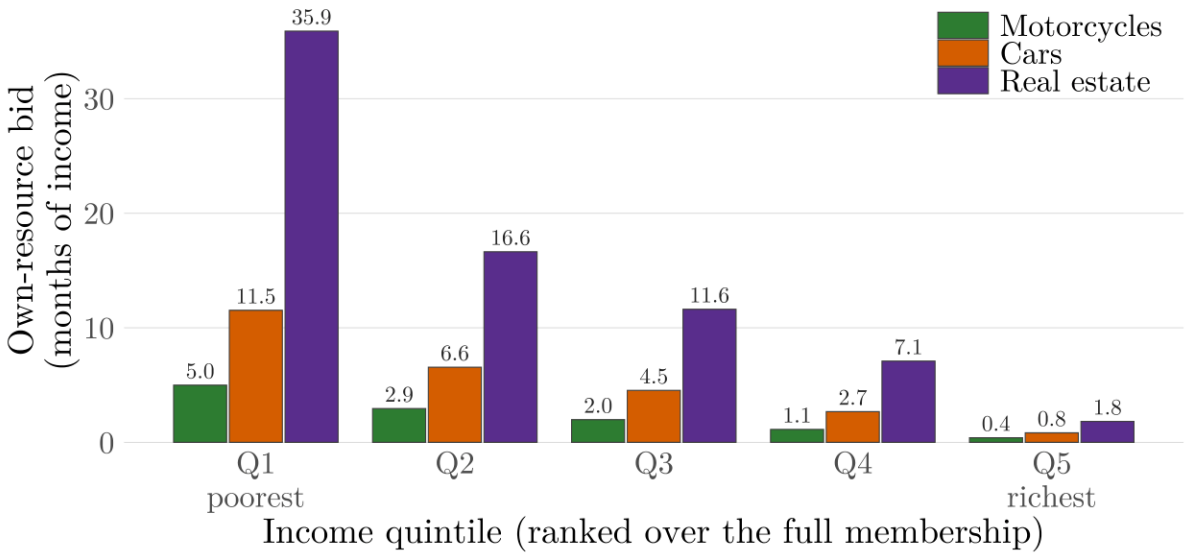
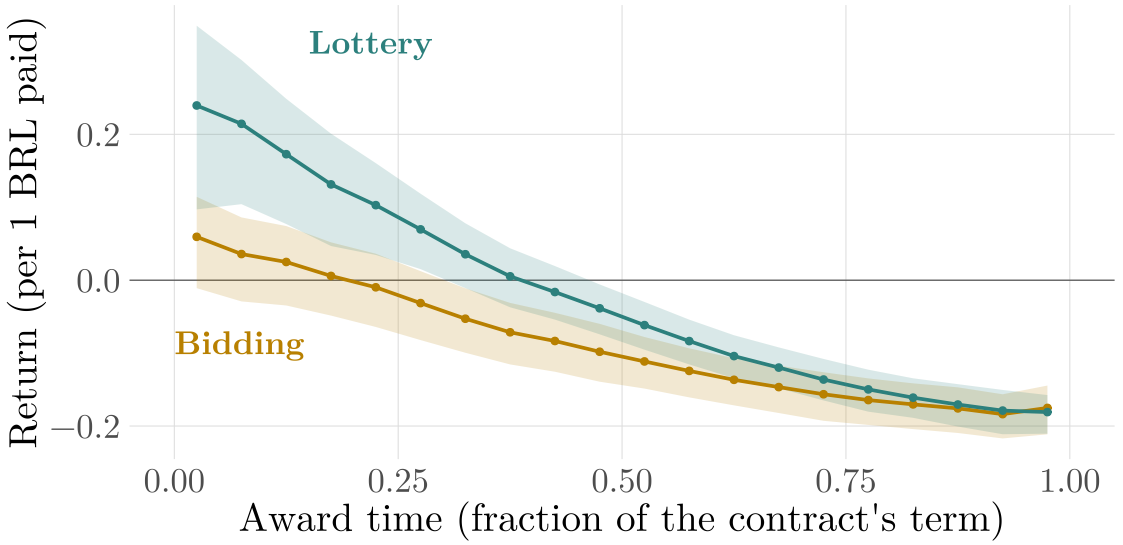
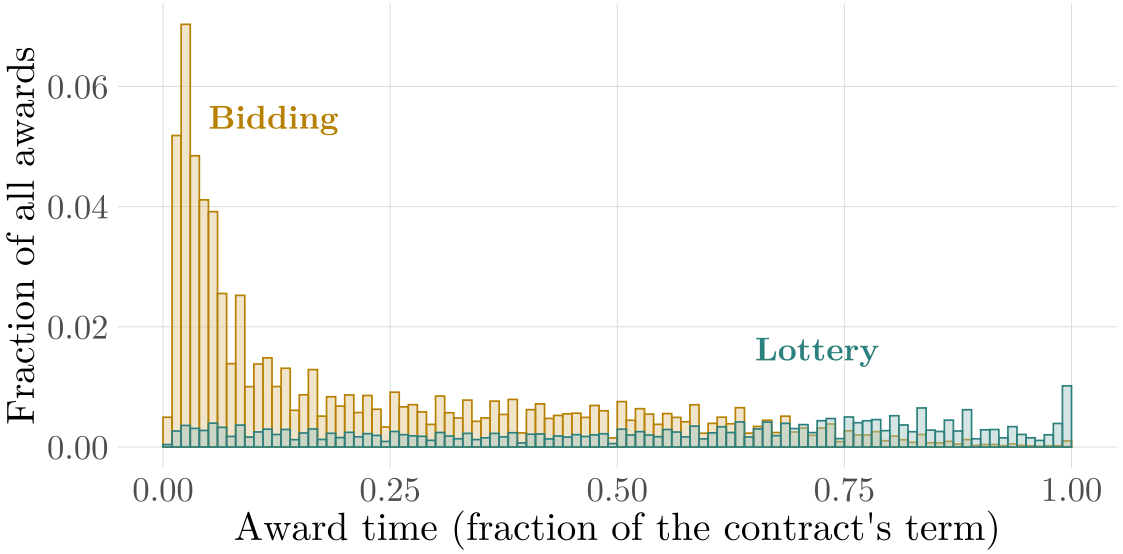


Figure A.8: Median own-resource cost of winning cash bids, in months of income, by income quintile and asset category.

Notes: This figure shows the median cost of a winning bid paid from the member's own resources, in months of income, among contracts awarded by bidding, by income quintile and asset category. The own-resource bid excludes the part of the bid paid out of the credit itself (embedded bid). Income is the real monthly income recorded in the national credit registry at joining. Quintiles are cut over all members of the three categories pooled, so the first quintile holds the same incomes in every category. The sample includes groups started between 2010 and 2025. Medians are weighted by group sampling weights.



Panel A. Returns fall steeply with award timing, and are lower for bid awards than for lottery awards at any given timing



Panel B. Bid awards are concentrated early in the contract term

Figure A.9: Returns and award timing by award mode.

Notes: This figure shows the returns and the timing of awards by bidding and by lottery. Award time is the month of the award as a fraction of the contract's own term. In panel A, lines are the average return in each 5% interval of the term, and bands span the 25th to 75th percentiles. Panel B shows the share of all awards falling in each 1% interval of the term. The sample includes awarded contracts in groups started between 2010 and 2025 that are completed or observed for at least 25% of their planned duration. Outcomes for contracts yet to be awarded in ongoing groups are predicted.

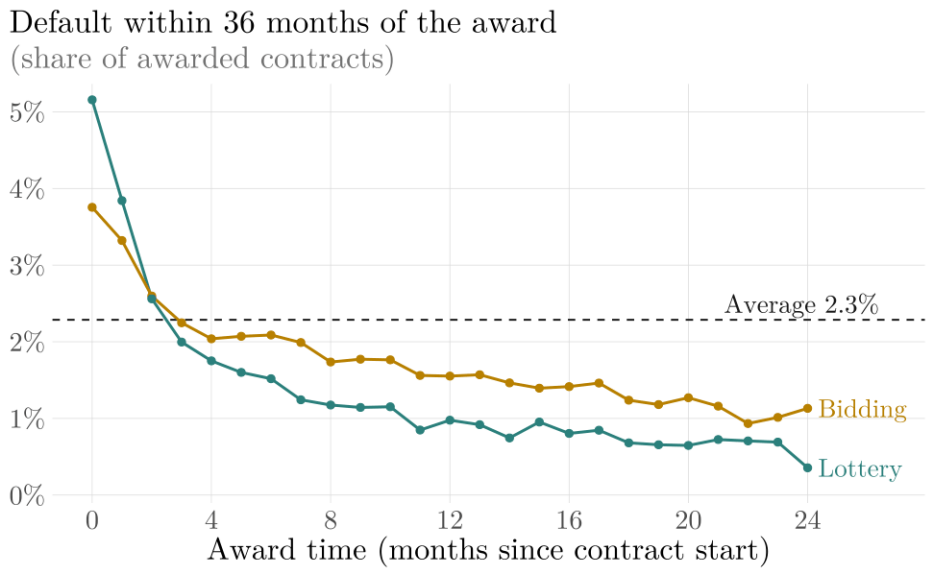


Figure A.10: Default within 36 months of the award, by month of award and allocation mechanism.

Notes: This figure shows the share of awarded contracts that default within 36 months of the award, by the month of the contract in which the award happened (counted from signing, up to 24) and by allocation mechanism. A default is a legal collection action filed against the contract. Only contracts observed for at least 36 months after the award are included, so every contract has the same time at risk. The dashed line is the average over both mechanisms. In a linear regression of default on a bid-award indicator, bid awards are 1.16 percentage points more likely to default (s.e. 0.03), and 0.38 points (s.e. 0.03) with award-month fixed effects; standard errors are heteroskedasticity-robust.

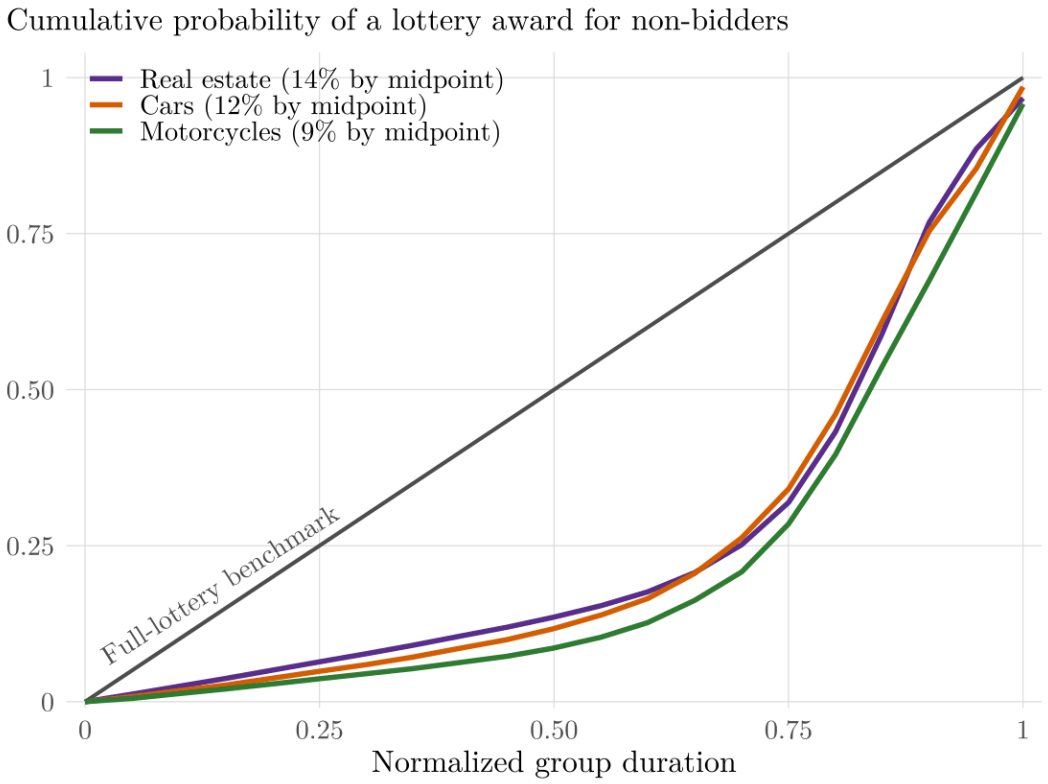


Figure A.11: Cumulative probability of a lottery award for non-bidders, by asset category.

Notes: This figure shows the cumulative probability that a contract that never bids receives a lottery award, by the share of the group's duration elapsed, for each asset category, as in Figure 5, panel B. Curves average the groups' estimated lottery-award probabilities, weighted by group size. The legend reports each category's probability of a lottery award by the group's midpoint. The 45-degree line is the benchmark in which every award is made by lottery.

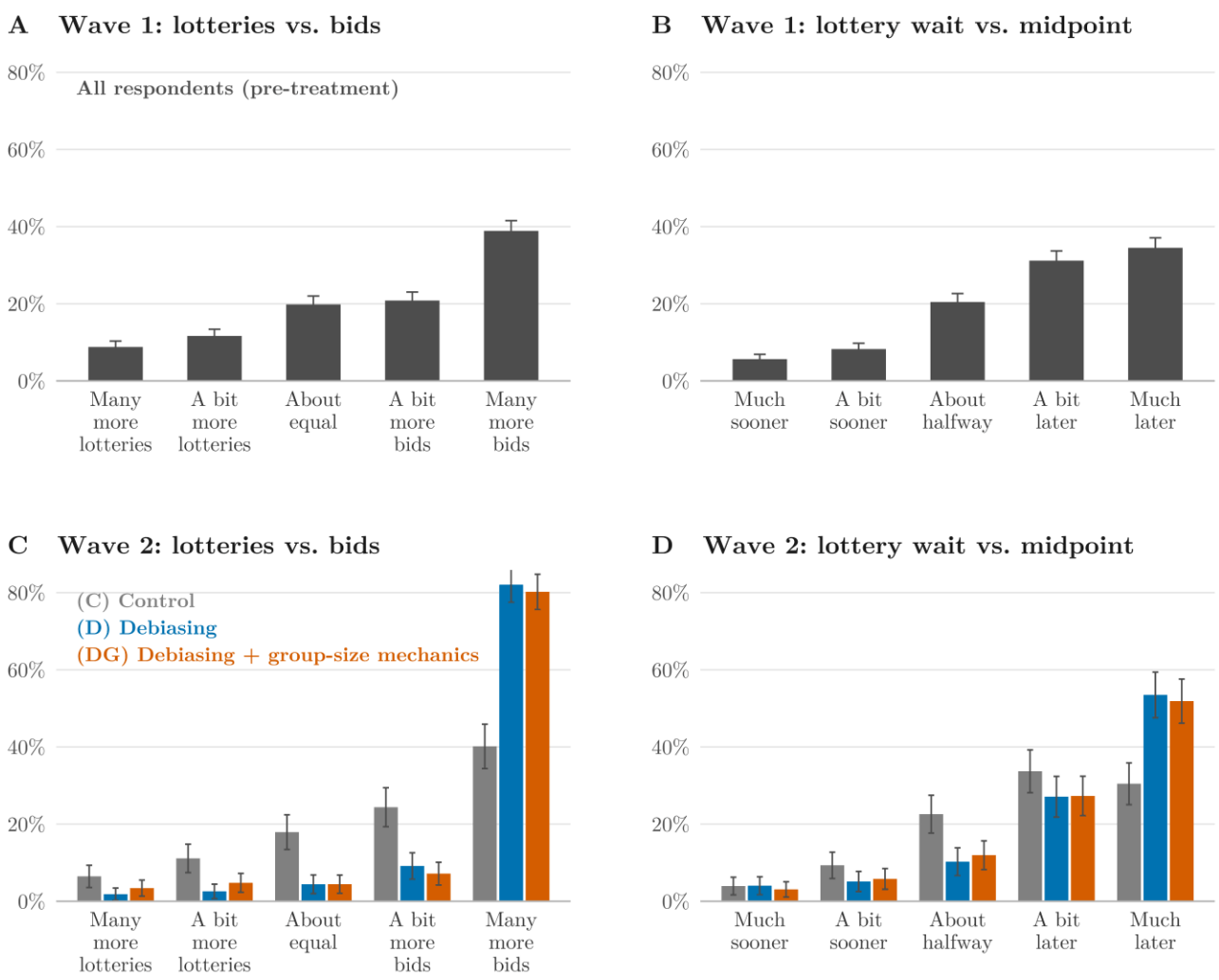


Figure A.12: Qualitative beliefs about award shares and lottery wait times.

Notes: This figure shows answers to the qualitative questions on whether lotteries or bids make up more of the awards in the first half of the group, and on when a lottery award arrives relative to the group's midpoint. Panels A and B show Wave 1, before treatment, for all respondents. Panels C and D show Wave 2, by treatment arm. Bars are shares of respondents, and vertical lines are 95% confidence intervals.

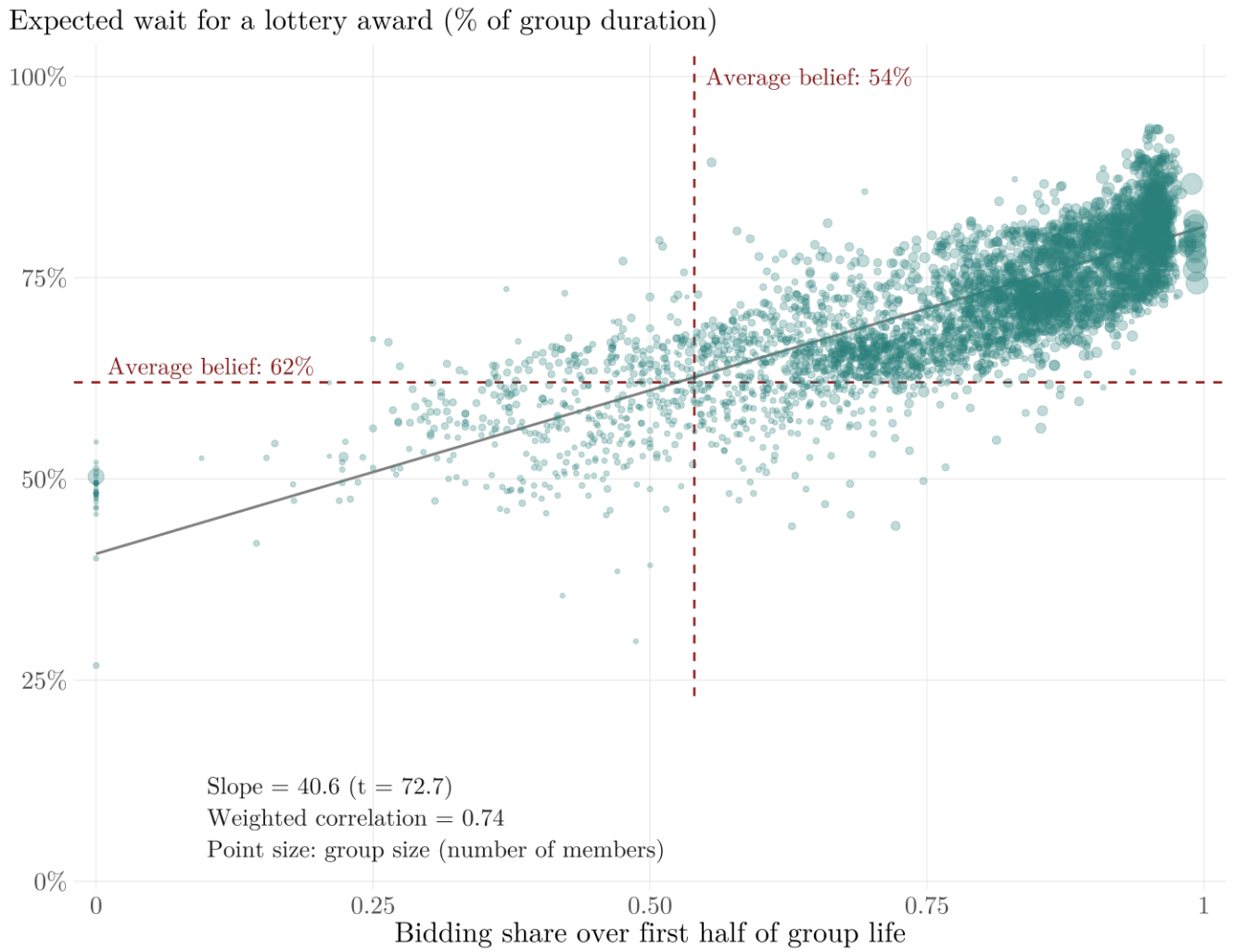


Figure A.13: Bidding intensity and lottery waits across completed groups.

Notes: This figure shows, for each completed group, the share of awards made by bid in the first half of the group and the expected wait for a lottery award, conditional on an award, as a share of the group's duration. Point size is group size, and the solid line is the group-size-weighted least-squares fit. Dashed lines mark respondents' average beliefs about the same two quantities (Section 3.6). Groups whose cumulative probability of a lottery award by the end of the term is below 0.8 (3.7% of groups) are excluded.

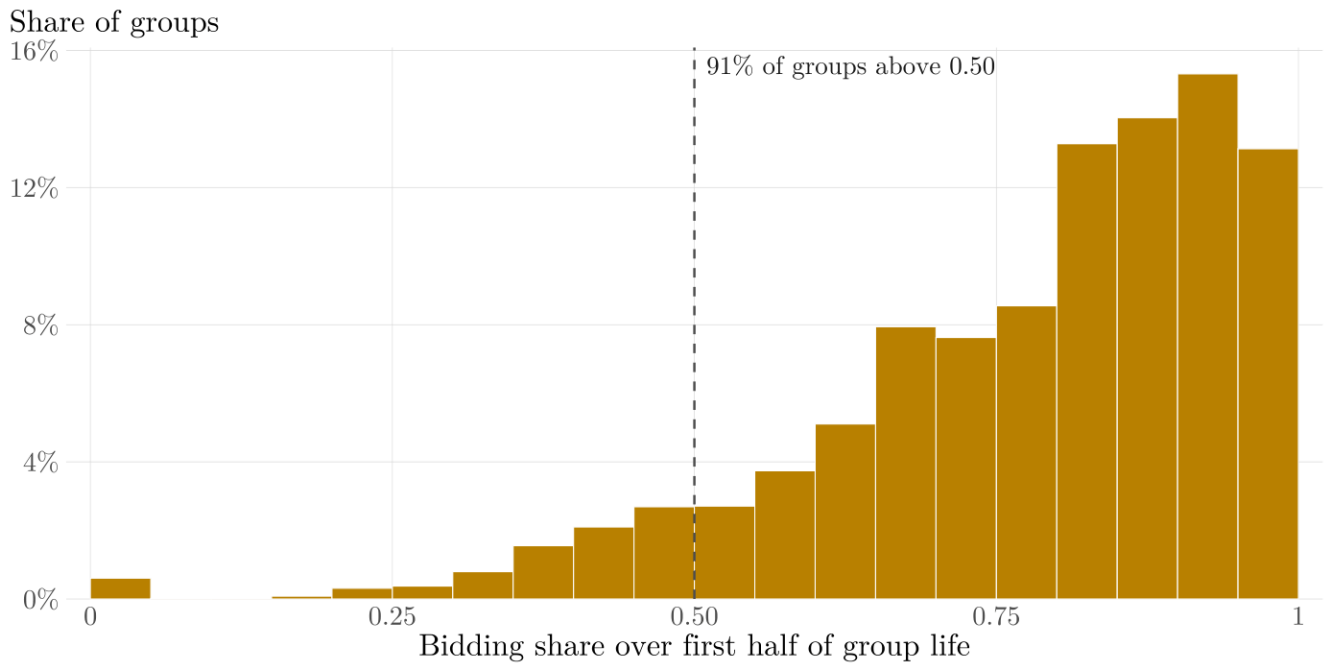


Figure A.14: Distribution of the bid share of early awards across completed groups.

Notes: This figure shows the distribution, across completed groups, of the share of awards made by bid in the first half of the group. Bars are shares of groups in bins of 0.05.

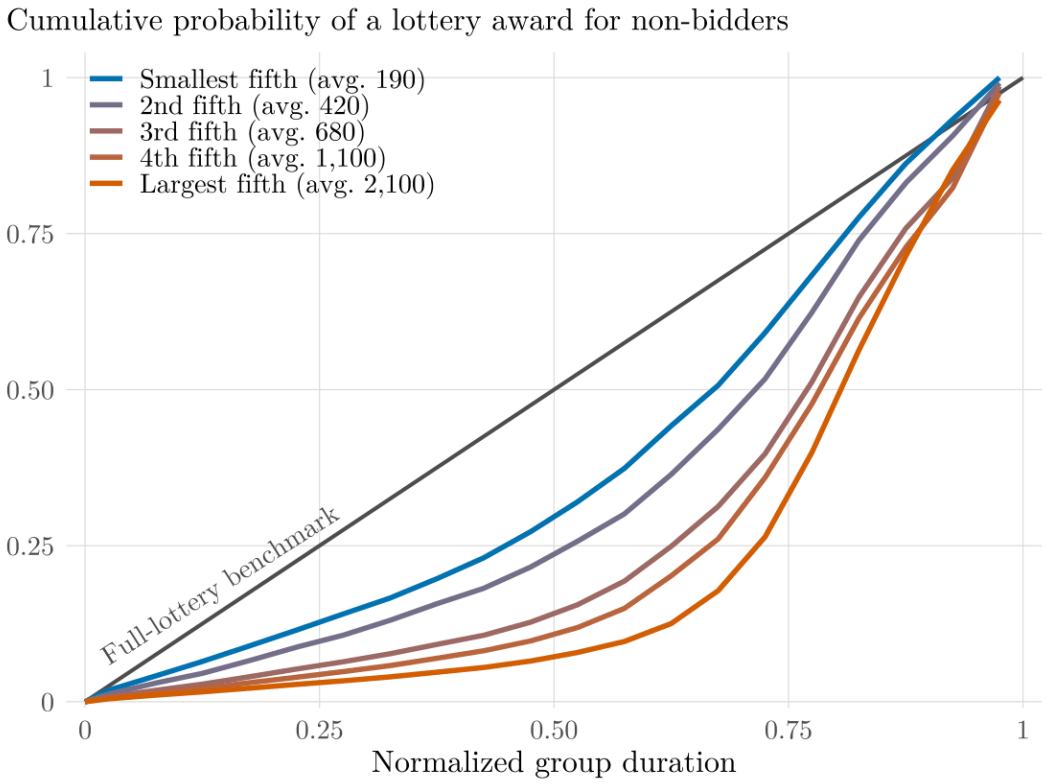


Figure A.15: Cumulative probability of a lottery award for non-bidders, by group size.

Notes: This figure shows the cumulative probability that a contract that never bids receives a lottery award, by the share of the group's duration elapsed, for each fifth of completed groups ranked by size. Curves average the groups' lottery-award probabilities, weighted by group size. The legend reports each fifth's average number of members. The 45-degree line is the benchmark in which every award is made by lottery. The sample includes 4,537 completed groups.

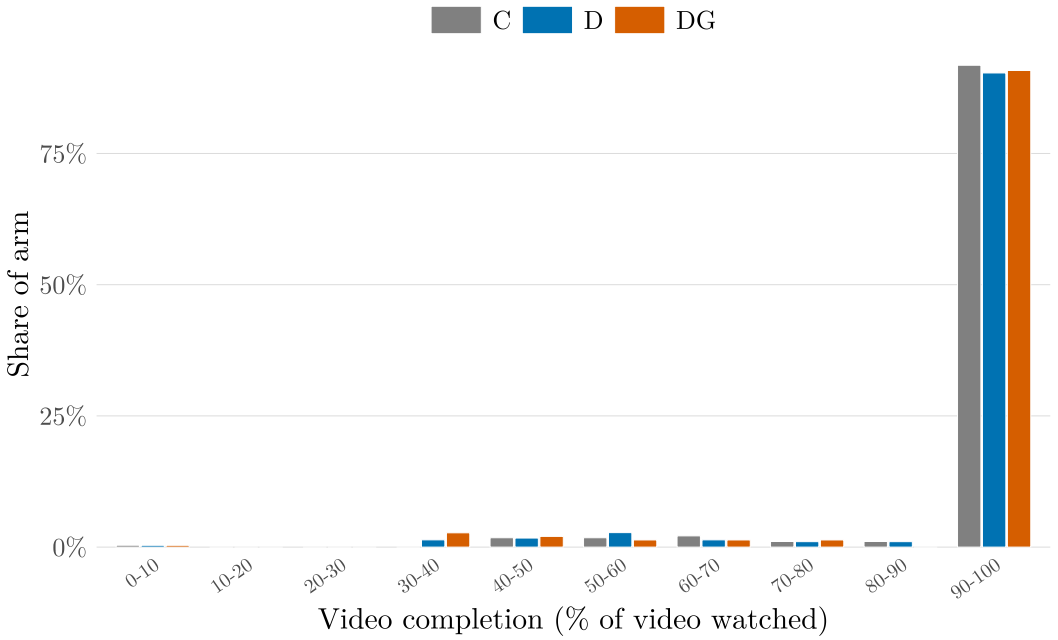


Figure A.16: Distribution of information-treatment video completion, by arm.

Notes: This figure shows how much of their video respondents watched in Wave 2, by treatment arm. Completion is the share of the video watched, so arms whose videos differ in length are comparable. Bars are the shares of each arm's respondents in each 10-point completion bin. The sample includes Wave 2 respondents who passed the quality filters.

Credit details	
Type of consórcio	Cars
Credit amount	R$50,000
Term	72 months

Group comparison	Group A	Group B
Administration **fee**	11.0%	17.0%
Initial **installment**	R$771	R$813
Group **size**	600 contracts	600 contracts
Minimum lotteries before bids	2 lotteries	2 lotteries
Participants' average **bid**	65%	45%

⚠ Important: assume there are no defaults in the groups.

Which group would you choose?

○ Group A ○ Group B

Figure A.17: Hypothetical group-choice task.

Notes: English translation of one of the two group choices each respondent made in Portuguese. The asset, credit amount, and term are those of the contract the respondent simulated earlier; the administration fee, group size, minimum lotteries before bids, and participants' average bid are randomized independently, and the initial installment follows from the fee. In this example, the two groups differ only in the fee and the average bid.

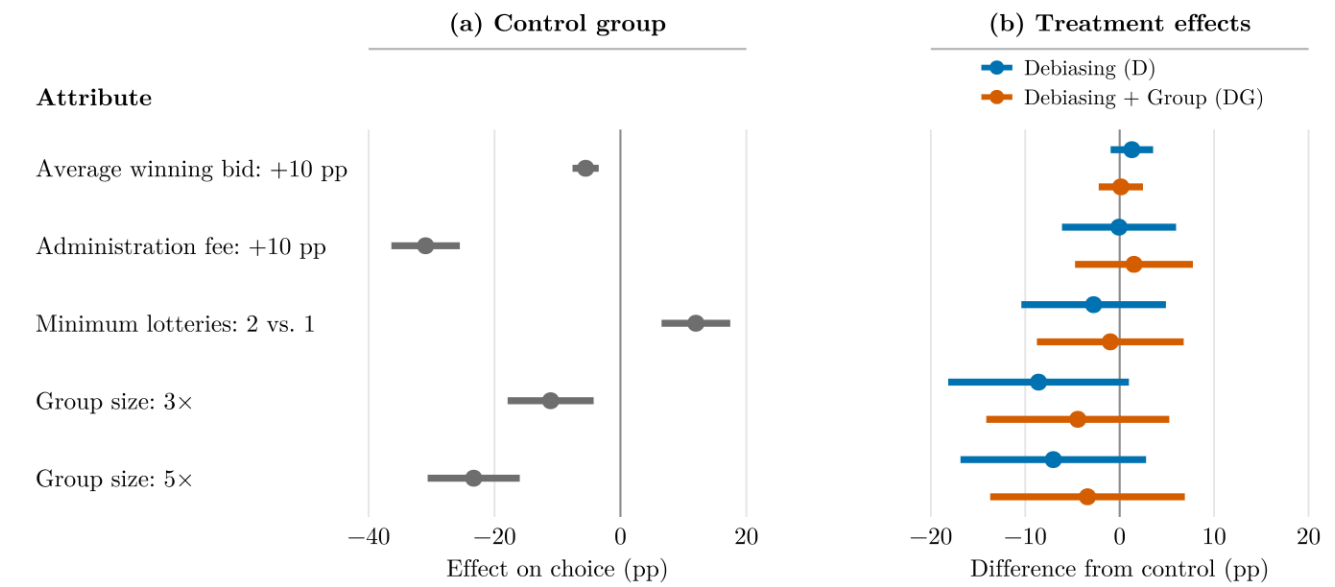


Figure A.18: Attribute valuation in hypothetical group choices.

Notes: Panel A shows the effect of each attribute on the probability that the control group chooses a group; panel B shows how much each treatment arm changes that effect. The average winning bid and the administration fee were randomized as discrete levels; we enter them linearly and report the effect of a 10 pp increase. Minimum lotteries and group size enter as indicators relative to the baseline levels, and group sizes are shown as the ratio to the smallest group, rounded. Linear probability model with PDS Lasso controls; bars are 95% confidence intervals clustered by respondent.

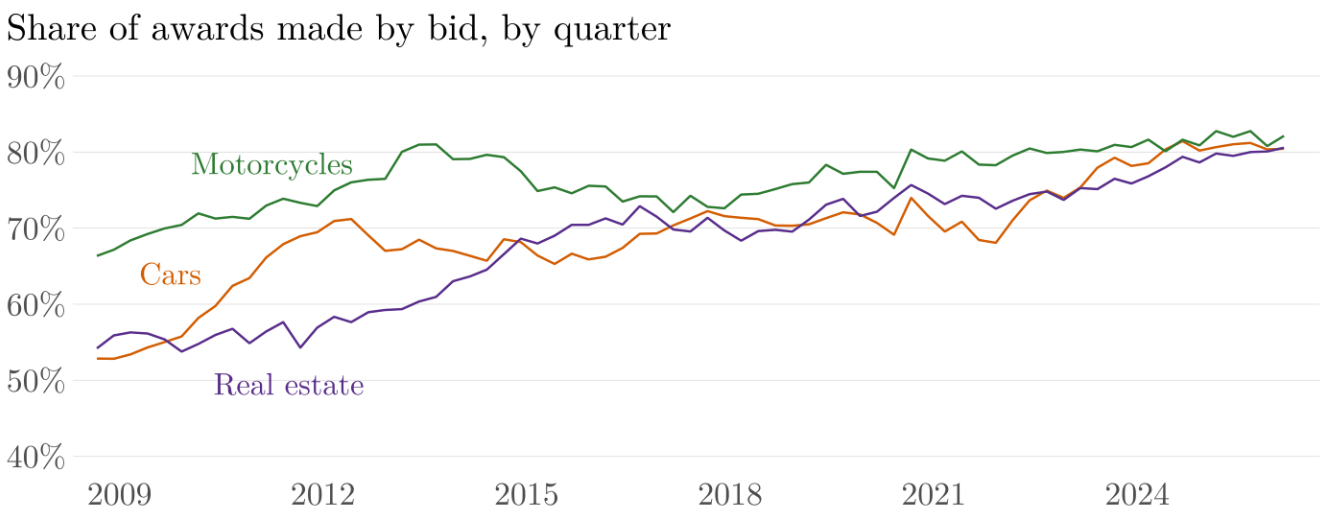
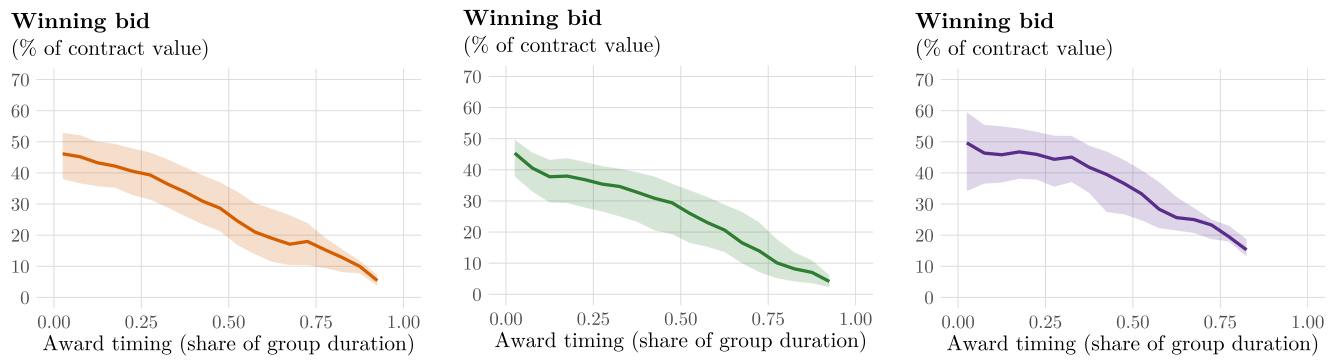


Figure A.19: Share of awards made by bid, 2008–2026.

Notes: This figure shows the share of each quarter's awards made by bid, by asset category, from the Central Bank's public consórcio data. Each quarter pools the awards of all active groups, regardless of how far each group is into its duration. Earlier data do not separate bid and lottery awards.



Panel A. Cars *Panel B. Motorcycles* *Panel C. Real estate*

Figure A.20: Winning bids are highest early in the group's life.

Notes: This figure shows winning bids as a share of total contractual payments (face value plus administration fee), the base on which bid percentages accrue, against the share of the group's duration elapsed at the award. Award timing follows the group's clock, from the group's start. Lines are medians and bands the 25th to 75th percentiles within twentieths of the group's duration, in all groups of the three asset categories. Bids include contracts with fixed-bid clauses. Cells with fewer than 30 winning bids, all in the last fifth of the duration, are omitted.

Table A.1: Financial products held by consórcio holders and other respondents.

	Consórcio holders	Other respondents	Difference: No controls	Difference: Income+Educ controls
	(1)	(2)	(3)	(4)
Panel A: Accounts and cards (currently holds)				
Bank account	91.2	68.2	22.9***	14.7***
			(2.6)	(2.6)
Savings account	56.5	33.6	22.9***	16.7***
			(4.2)	(4.1)
Credit card	77.6	49.5	28.0***	18.7***
			(3.6)	(3.7)
Panel B: Savings (previous 12 months)				
At least one savings product	77.6	52.5	25.1***	15.6***
			(3.6)	(3.5)
At least one, excluding bank accounts	55.2	28.6	26.6***	20.6***
			(4.3)	(4.1)
Savings or checking account	63.9	41.4	22.5***	12.9***
			(4.1)	(4.0)
Treasury bonds or CDs	17.1	6.2	10.9***	7.9**
			(3.2)	(3.1)
Stocks	18.5	6.0	12.5***	10.7***
			(3.3)	(3.3)
Private pension	10.2	2.8	7.4***	5.8**
			(2.5)	(2.5)
Panel C: Credit products (currently holds)				
At least one of the products below	53.7	16.7	37.1***	34.0***
			(4.2)	(4.2)
Vehicle financing	31.3	3.9	27.4***	26.0***
			(3.9)	(3.8)
Home-equity loan	6.8	0.7	6.1***	5.9***
			(2.1)	(2.1)
Mortgage	16.3	3.2	13.2***	11.2***
			(3.1)	(2.9)
Unsecured bank loan	15.0	4.5	10.4***	9.4***
			(3.0)	(3.0)
Payroll loan	23.8	8.1	15.7***	15.3***
			(3.6)	(3.6)
Respondents (N)	147	1,809	1,956	1,956

Notes: This table shows the financial products held by consórcio holders and other respondents in the Central Bank of Brazil Financial Literacy Survey (*Pesquisa de Letramento Financeiro*, https://dadosabertos.bcb.gov.br/dataset/pesquisa-de-letramento-financeiro), among adults aged 18 and over. Consórcio holders are respondents who hold a consórcio or acquired one in the previous two years. Columns 1–2 report the share (%) holding each product. Columns 3–4 report OLS coefficients on a consórcio-holder indicator, in percentage points; column 4 adds income-bracket and schooling indicators. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.2: Contract and group characteristics by asset category.

	Cars	**Motorcycles**	**Real Estate**
	Mean (SD)	Mean (SD)	Mean (SD)
Panel A: Contract level			
Credit value at joining (USD thousand)	8.2 (5.9)	2.3 (1.4)	34.4 (30.2)
Admin fee (%)	13.5 (4.7)	20.4 (4.5)	18.3 (5.4)
Term (months)	72 (18)	56 (17)	184 (37)
Installment incl. fee (% of income), median	17.9	17.2	17.3
Individual holder (vs. firm) (%)	71.2	97.3	77.4
Year of joining	2018.2 (4.3)	2017.7 (4.7)	2020.3 (4.3)
Awarded, not canceled (%)	39.3	54.6	15.9
Canceled (%)	43.3	32.2	50.4
Active (no award, no cancellation) (%)	17.4	13.2	33.7
Contracts (N)	1,039,222	748,342	333,650
Contracts (share, %)	49.0	35.3	15.7
Panel B: Group level			
Group size (quotas)	999 (1,280)	1,080 (1,026)	2,214 (1,746)
Planned duration (months)	72 (13)	57 (15)	186 (30)
Formation year	2016.3 (4.4)	2016.1 (4.9)	2018.6 (4.9)
Quotas awarded (%)	64.1 (30.0)	72.0 (32.1)	25.0 (24.9)
Quotas canceled (%)	40.0 (18.7)	46.6 (16.2)	46.3 (22.5)
Groups (N)	26,750	17,809	3,972
Groups (share, %)	55.1	36.7	8.2

Notes: This table shows means of contract and group characteristics by asset category, with standard deviations in parentheses. Panel A uses a random sample of about 4% of contracts in groups started between 2010 and 2025. Credit value is the face value at joining, in thousands of USD at 5.5 BRL per USD. The installment row reports the median monthly installment, including the administration fee, as a share of the holder's income in the national credit registry, among contracts with reported income. Panel B uses all groups started between 2010 and 2025. Group size counts every quota reported over the group's life, including canceled quotas that are resold, so it exceeds the number of members at any one time. Shares of quotas awarded and canceled are measured at the group's last filing.

Table A.3: Average returns by asset category

		Decomposition of fee-free return		
	Return	Transfer	Borrowing advantage	Fee
	(1)	(2)	(3)	(4)
Panel A. Awarded contracts				
Cars	0.008	0.011	0.123	0.111
Motorcycles	−0.043	0.016	0.151	0.185
Real estate	−0.095	0.031	−0.000	0.126
All segments	−0.025	0.015	0.124	0.146
Panel B. Canceled contracts				
Cars	−0.045	−0.029	–	0.017
Motorcycles	−0.060	−0.034	–	0.024
Real estate	−0.034	−0.027	–	0.019
All segments	−0.051	−0.032	–	0.021

Notes: This table shows average returns per unit of face value by asset category, for awarded and canceled contracts. Canceled contracts are also normalized by their face value, not by the amount paid in as in Figure 2, panel B, so their returns and transfers are small relative to that figure. The sample includes contracts in groups that are completed or observed for at least 25% of their duration; outcomes of contracts not yet resolved in ongoing groups are predicted (Appendix D). Column (1) is the return from Equation (1). Columns (2)–(4) decompose the fee-free return, which equals (2) + (3) − (4). Transfer is the present value of the installment plan at the savings rate. Borrowing advantage is the additional value of receiving the funds at the award date, at the asset category's market borrowing rate. Fee is the present value of the administration fee. Canceled contracts still awaiting their refund draw enter column (1) with a predicted refund and are excluded from columns (2)–(4).

Table A.4: Break-even outside rates across the return distribution

Return percentile	Return	Median award timing	Break-even borrowing		Break-even savings	
			Rate	Spread over market (pp)	Rate	Spread over market (pp)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. All segments						
10th	−0.21	0.73	–	–	−10.0%	−14.0
25th	−0.14	0.55	103.5%	+82.3	−7.9%	−11.9
50th	−0.04	0.17	29.7%	+7.0	−4.9%	−8.9
75th	+0.07	0.06	12.3%	−7.0	–	–
90th	+0.17	0.05	8.0%	−11.6	–	–
Panel B. Cars						
10th	−0.18	0.77	–	–	−5.9%	−9.9
25th	−0.12	0.60	109.8%	+90.8	−5.9%	−9.9
50th	+0.00	0.24	18.7%	−0.3	4.5%	+0.5
75th	+0.10	0.05	9.7%	−9.3	–	–
90th	+0.20	0.05	6.5%	−12.5	–	–
Panel C. Motorcycles						
10th	−0.23	0.72	–	–	−13.3%	−17.3
25th	−0.16	0.52	132.3%	+108.3	−18.1%	−22.1
50th	−0.06	0.10	34.1%	+10.1	–	–
75th	+0.04	0.07	19.3%	−4.7	–	–
90th	+0.16	0.04	12.8%	−11.2	–	–
Panel D. Real estate						
10th	−0.24	0.68	–	–	−5.2%	−9.2
25th	−0.17	0.48	–	–	−8.7%	−12.7
50th	−0.09	0.12	9.6%	+5.6	–	–
75th	−0.02	0.06	5.2%	+1.2	–	–
90th	+0.03	0.07	2.9%	−1.1	32.6%	+28.6

Notes: This table shows break-even interest rates at percentiles of the return per unit of face value, for awarded contracts. The sample includes contracts in groups that are completed or observed for at least 25% of their duration; outcomes of contracts not yet awarded in ongoing groups are predicted. The break-even borrowing rate is the borrowing rate at which the return is zero, holding the savings rate at 4%. The break-even savings rate is the savings rate at which the return is zero, holding the borrowing rate at its benchmark (19% for cars, 24% for motorcycles, and 4% for real estate). Spreads are the break-even rate minus the benchmark rate. A positive borrowing spread means the contract breaks even only for a participant who would borrow at that many points above the market rate. Award timing is the median award month as a share of the term. Each row takes the contracts with returns within 0.01 of the percentile and reports their median break-even rate; a dash marks a cell where the rate is undefined for more than half of these contracts. The same loss spread over more periods is offset by a smaller rate difference, so break-even rates can move closer to market rates as the term increases and need not be monotonic in the return.

Table A.5: Average returns under alternative outside rates

	Market borrowing rates			Borrowing rates 50% higher		
Savings rate	0%	4%	8%	0%	4%	8%
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Finished and ongoing groups (at least 25% of term observed)						
Cars	−0.015	−0.015	−0.013	+0.030	+0.024	+0.020
Motorcycles	−0.049	−0.048	−0.041	−0.010	−0.010	−0.010
Real estate	−0.089	−0.056	−0.030	−0.076	−0.043	−0.021
All segments	−0.041	−0.036	−0.029	−0.003	−0.001	+0.000
Panel B. Finished groups only						
Cars	−0.010	−0.010	−0.009	+0.039	+0.033	+0.028
Motorcycles	−0.044	−0.040	−0.036	−0.004	−0.004	−0.004
Real estate	−0.084	−0.066	−0.052	−0.069	−0.054	−0.042
All segments	−0.031	−0.028	−0.025	+0.012	+0.010	+0.009
Panel C. Awarded contracts, finished and ongoing groups, all segments						
Mean	−0.040	−0.025	−0.013	+0.039	+0.044	+0.047
Median	−0.061	−0.040	−0.020	−0.001	+0.016	+0.032
Share negative	66%	60%	54%	50%	47%	45%

Notes: This table shows returns per unit of face value when the same contracts are repriced under alternative benchmark rates. Panels A and B report the average over all contracts, awarded and canceled; panel C reports the mean, median, and share negative of awarded contracts. The baseline is column (2): a 4% savings rate and borrowing rates of 4%, 19%, and 24% for real estate, cars, and motorcycles. Columns (1) and (3) change the savings rate and move borrowing rates with it, keeping their spreads over the savings rate. Columns (4) to (6) raise borrowing rates by 50% at a 4% savings rate, to 6%, 28.5%, and 36%, and keep those spreads when the savings rate changes. Canceled contracts still awaiting their restitution draw are valued at a predicted restitution. Outcomes not yet observed in ongoing groups are predicted as in Appendix D.

Table A.6: Group size and early awards per month, by group start year.

		Early awards		
Start year	Size	Bids	Lotteries	Bid share
Panel A. Cars				
2010–2014	483	4.31	1.10	0.80
2015–2018	635	6.10	1.04	0.85
2019–2022	786	6.11	1.09	0.85
2023–2024	1,194	9.10	1.06	0.90
Panel B. Motorcycles				
2010–2014	529	7.28	0.91	0.89
2015–2018	772	7.64	1.03	0.88
2019–2022	955	9.89	1.27	0.89
2023–2024	978	10.26	1.28	0.89
Panel C. Real estate				
2010–2014	1,109	2.58	1.14	0.69
2015–2018	1,211	2.78	1.19	0.70
2019–2022	2,032	4.80	1.26	0.79
2023–2024	2,807	6.04	1.30	0.82

Notes: This table shows group size and early awards by asset category and the year the group started. Size is the average number of contracts sold by the time a group reaches 10% of its duration. Bids and Lotteries are awards per group per month in the first 25% of the group's duration, and Bid share is the share of these awards made by bid. The sample includes groups started between 2010 and 2024; groups not yet observed through 25% of their duration contribute the months observed.

Table A.7: Correlation between self-reported liquidity and income and savings variables.

	All respondents				Simulated contract ≥ BRL 1,000			
	Income (1)	Coping (2)	Savings (3)	Loan (4)	Income (5)	Coping (6)	Savings (7)	Loan (8)
Household income (reference: up to BRL 2,000)								
BRL 2,001–5,000	-0.036				-0.032			
	(0.054)				(0.053)			
BRL 5,001–10,000	0.033				0.028			
	(0.054)				(0.054)			
BRL 10,001–20,000	0.106*				0.116**			
	(0.058)				(0.057)			
BRL 20,001–100,000	0.159**				0.174***			
	(0.063)				(0.063)			
BRL 100,001 or more	0.285***				0.297***			
	(0.074)				(0.073)			
Emergency-coping capacity (1–5)		0.160***				0.161***		
		(0.010)				(0.010)		
Any savings in the last 12 months			0.213***				0.223***	
			(0.023)				(0.023)	
Access to a personal loan				0.218***				0.226***
				(0.025)				(0.025)
R^2	0.040	0.167	0.056	0.059	0.046	0.174	0.061	0.065
Observations	1296	1296	1296	1296	1262	1262	1262	1262

Notes: This table shows how self-reported liquidity to bid correlates with income and savings measures among Wave 1 consumers. The outcome is an indicator that the respondent could bid about 50% of the simulated contract's face value with their own resources in the first years of the contract. Each column is a separate linear probability model on one measure; household income enters as indicators for income brackets. Coping capacity is a 1–5 rating of the ability to handle a financial emergency. Columns (5)–(8) exclude the 34 respondents whose simulated contract is below BRL 1,000. Robust standard errors in parentheses. $^{*}\,p < 0.1$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A.8: Broker sample description.

	Mean (SD)	Median
Panel A. Demographics		
Age	43.8 (11.7)	44.0
Male (%)	75.7	
College complete or higher (%)	64.4	
Panel B. Employer (%)		
Specialized consórcio sales firm	45.9	
Consórcio administrator	31.2	
Financial advisory firm	11.3	
Commercial bank	6.5	
Car dealership	5.1	
Panel C. Business		
Years of experience	6.8 (8.2)	3.0
Sales per month	8.3 (10.5)	5.0
Number of administrators	1.9 (1.3)	1.0
Sells for two or more administrators (%)	40.1	
Sells consórcios exclusively (%)	57.2	
Advertises on social media (%)	78.4	
Observations	292	

Notes: This table describes the brokers who completed the survey. Binary variables report the share in percent; continuous variables report the mean, standard deviation in parentheses, and median. Sales per month is trimmed to 0–100, and the number of administrators codes "5 or more" as 5.

Table A.9: Distributional overlap between the consumer and broker samples.

	Weighted Jaccard	Cosine	Overlap
Segment	0.91	1.00	0.95
Region	0.84	0.99	0.91
Administrator	0.49	0.91	0.66

Notes: This table compares the distributions of consumers and brokers across asset categories, regions, and administrators. For distributions p and q over categories k, the weighted Jaccard index is $\sum_k \min(p_k, q_k) / \sum_k \max(p_k, q_k)$, the cosine similarity is $\sum_k p_k q_k / (\|p\| \, \|q\|)$, and the overlap is $\sum_k \min(p_k, q_k)$; each equals one for identical distributions and zero for disjoint ones. Consumers are classified by their asset of interest and brokers by their sales mix. Regions are located from each response's IP address. Administrators are named in free text: the one a consumer is considering (45% of consumers name one) and up to three that each broker sells for. The sample includes 1,296 Wave 1 consumers and 292 brokers.

Table A.10: Wave 1 and Wave 2 consumer samples.

	Wave 1 Sample		Wave 2 Sample	
	Mean (SD)	Median	Mean (SD)	Median
Panel A. Demographics				
Age	47.2 (12.5)	47.0	47.1 (12.5)	47.0
Male (%)	59.1		57.5	
Employed (%)	87.2		86.2	
College complete or higher (%)	69.7		70.3	
Primary financial decision maker (%)	82.7		81.1	
Panel B. Financial profile				
Financial literacy score (0–4)	3.2 (1.0)	3.0	3.2 (1.0)	3.0
Savings buffer (months)	5.1 (4.0)	4.0	5.1 (4.0)	4.0
Implied monthly installment (BRL)	1,792 (2,176)	1,250	1,792 (2,376)	1,250
Installment as % of monthly income	27.6 (61.3)	14.8	27.4 (64.7)	14.3
Panel C. Consórcio profile				
Past consórcio participation (%)	56.9		56.4	
Currently paying a consórcio (%)	25.5		25.9	
Discussed consórcio with vendor (%)	33.9		33.4	
Likely to join ($\leq$3 mo.) (%)	56.0		55.9	
Would recommend consórcio (%)	69.4		69.2	
Already considering an administrator (%)	44.8		46.6	
Panel D. Asset of interest				
Real estate (%)	47.7		46.0	
Cars / vehicles (%)	40.8		43.3	
Motorcycles (%)	11.5		10.7	
N	1,296		845	

Notes: This table shows the characteristics of the Wave 1 and Wave 2 consumer samples, measured in Wave 1. The Wave 1 sample includes respondents who passed the screening quality filters and completed Wave 1. The Wave 2 sample is the experimental sample, after completion and quality filters; 801 of its 845 respondents are also in the Wave 1 sample, and the remaining 44 passed only the Wave 2 quality filters. Variables are defined as in Table 2.

Table A.11: Balance of Wave 1 covariates across information-treatment arms.

	Control mean (SD)	$D - C$	$DG - C$	N	Test: $D = C$	Test: $DG = C$	Test: $D = DG$
Panel A. All assigned in Wave 2 ($N = 985$)							
Age	47.09 (12.35)	-0.36 (0.96)	0.16 (0.99)	985	0.705	0.875	0.597
Male (%)	54.46 (49.88)	7.54 (3.83)	-1.02 (3.87)	985	0.049	0.792	0.024
Employed (%)	87.69 (32.90)	1.29 (2.53)	-4.44 (2.74)	985	0.611	0.106	0.033
College complete or higher (%)	70.77 (45.55)	-2.00 (3.51)	-1.13 (3.52)	985	0.568	0.748	0.805
Race: White (%)	52.03 (50.06)	-2.68 (4.52)	6.52 (4.49)	732	0.553	0.147	0.042
Race: Brown (%)	35.37 (47.91)	2.83 (4.36)	-3.96 (4.28)	732	0.516	0.355	0.117
Income bracket 1: $\leq$ BRL 5,000 (%)	32.92 (47.07)	-0.77 (3.58)	1.30 (3.60)	985	0.829	0.717	0.565
Income bracket 2: BRL 5,001–20,000 (%)	50.46 (50.07)	-0.72 (3.89)	-5.03 (3.89)	985	0.852	0.196	0.268
Household income floor (BRL 1,000s)	12.32 (23.38)	0.71 (1.83)	1.40 (1.86)	985	0.699	0.454	0.718
Primary financial decision maker (%)	84.31 (36.43)	-0.20 (2.86)	-6.73 (3.02)	985	0.943	0.026	0.032
Financial literacy score (0–4)	3.19 (0.96)	-0.06 (0.08)	0.01 (0.08)	985	0.446	0.872	0.358
Prepared for a financial emergency (%)	18.15 (38.61)	7.38 (2.88)	3.01 (2.92)	985	0.011	0.304	0.154
Access to a personal loan (%)	40.92 (49.25)	3.99 (3.70)	1.52 (3.72)	985	0.281	0.683	0.512
Any savings in the last 12 months (%)	59.08 (49.25)	3.58 (3.63)	1.90 (3.68)	985	0.325	0.605	0.648
Financial risk tolerance (0–3)	1.24 (1.27)	-0.00 (0.11)	-0.03 (0.11)	732	0.986	0.814	0.827
Financial patience (0–5)	1.90 (1.83)	-0.02 (0.16)	0.11 (0.17)	732	0.906	0.499	0.431
Emergency-coping capacity (1–5)	2.52 (1.12)	0.18 (0.08)	0.09 (0.08)	985	0.025	0.245	0.294
Bought a public lottery ticket (%)	30.46 (46.10)	-0.73 (3.60)	-0.61 (3.58)	985	0.841	0.866	0.973
Bets or gambles (%)	9.85 (29.84)	-1.27 (2.27)	2.69 (2.46)	985	0.576	0.273	0.096
Likely to join ($\leq$3 mo.) (1–4)	2.53 (0.96)	0.09 (0.07)	0.07 (0.07)	985	0.239	0.349	0.810
Past consórcio participation (%)	54.46 (49.88)	2.28 (3.87)	1.78 (3.88)	985	0.555	0.646	0.896
Currently paying a consórcio (%)	24.31 (42.96)	4.24 (3.46)	-0.66 (3.34)	985	0.220	0.844	0.152
Family/friends in a consórcio (%)	75.38 (43.14)	-1.44 (3.40)	-2.00 (3.41)	985	0.671	0.557	0.870
Discussed consórcio with vendor (1–4)	2.15 (1.08)	0.13 (0.08)	0.00 (0.08)	985	0.123	0.987	0.120
Lottery vs. auction preference (1–5)	2.07 (1.36)	0.08 (0.11)	0.19 (0.11)	984	0.450	0.087	0.346
Credit-timing preference (1–4)	2.22 (0.89)	-0.06 (0.07)	-0.07 (0.07)	985	0.420	0.282	0.826
Willing to bid $\geq$50% (%)	42.77 (49.55)	-1.19 (3.55)	0.40 (3.60)	985	0.738	0.911	0.661
Unconstrained: can fund a 50% bid (%)†	26.77 (44.34)	-0.39 (3.47)	-0.12 (3.45)	985	0.911	0.972	0.938
Bidding-share belief index (SD)	-0.01 (0.67)	0.09 (0.05)	-0.05 (0.05)	985	0.083	0.371	0.011
Lottery belief index (SD)	0.09 (0.73)	-0.08 (0.06)	-0.20 (0.06)	985	0.207	0.001	0.066
Bidding-belief certainty (1–5)	2.41 (0.94)	0.11 (0.07)	0.14 (0.07)	985	0.124	0.055	0.699
Lottery-belief certainty (1–5)	2.51 (0.98)	0.13 (0.08)	0.16 (0.08)	985	0.113	0.041	0.689
Hypothetical WTP in Wave 1 (%)	64.31 (47.98)	1.63 (3.73)	-1.67 (3.77)	985	0.662	0.658	0.376
Joint orthogonality (p)					0.634	0.392	0.074

Notes: This table shows the balance of Wave 1 covariates across treatment arms. Panel A includes all respondents assigned to an arm in Wave 2, and panel B the analysis sample, after completion and quality filters. Each row regresses the covariate on arm indicators with randomization-strata fixed effects; $D - C$ and $DG - C$ are differences from the control group, with robust standard errors in parentheses, and the test columns report p-values. The first column reports the control mean and standard deviation. Belief indices average standardized Wave 1 beliefs, with higher values more favorable to bidding. Hypothetical WTP is the share choosing consórcio access over a BRL 50 bonus in Wave 1. †Liquidity to bid is a randomization stratum, so it is estimated without strata fixed effects and excluded from the joint test. The joint test is a homoskedastic F-test that the covariates predict assignment, with strata fixed effects. Binary variables are in percent.

Table A.11: Balance of Wave 1 covariates across information-treatment arms (continued).

	Control mean (SD)	$D - C$	$DG - C$	N	Test: $D = C$	Test: $DG = C$	Test: $D = DG$
Panel B. Final analysis sample ($N = 845$)							
Age	47.30 (12.67)	-0.63 (1.04)	-0.13 (1.08)	845	0.546	0.902	0.635
Male (%)	54.84 (49.85)	7.66 (4.14)	-0.14 (4.14)	845	0.065	0.972	0.057
Employed (%)	86.02 (34.74)	3.82 (2.78)	-3.07 (3.03)	845	0.170	0.311	0.017
College complete or higher (%)	70.97 (45.47)	0.31 (3.79)	-1.44 (3.79)	845	0.934	0.704	0.643
Race: White (%)	51.50 (50.10)	-2.17 (5.04)	6.82 (4.93)	595	0.666	0.167	0.072
Race: Brown (%)	36.00 (48.12)	1.64 (4.85)	-4.25 (4.74)	595	0.736	0.369	0.216
Income bracket 1: $\leq$ BRL 5,000 (%)	30.82 (46.26)	1.71 (3.87)	3.09 (3.84)	845	0.659	0.420	0.723
Income bracket 2: BRL 5,001–20,000 (%)	51.25 (50.07)	-3.70 (4.23)	-5.10 (4.17)	845	0.382	0.221	0.737
Household income floor (BRL 1,000s)	13.12 (24.42)	0.32 (2.04)	0.88 (2.06)	845	0.874	0.669	0.789
Primary financial decision maker (%)	83.15 (37.49)	-0.69 (3.22)	-5.45 (3.27)	845	0.831	0.096	0.154
Financial literacy score (0–4)	3.19 (0.98)	-0.02 (0.08)	0.03 (0.08)	845	0.775	0.681	0.478
Prepared for a financial emergency (%)	16.85 (37.49)	9.45 (3.07)	4.48 (3.08)	845	0.002	0.146	0.130
Access to a personal loan (%)	41.94 (49.43)	4.98 (4.03)	-0.70 (4.00)	845	0.216	0.861	0.159
Any savings in the last 12 months (%)	59.50 (49.18)	5.53 (3.94)	2.95 (3.94)	845	0.160	0.455	0.510
Financial risk tolerance (0–3)	1.25 (1.28)	0.02 (0.13)	-0.02 (0.12)	595	0.882	0.885	0.768
Financial patience (0–5)	1.89 (1.80)	0.06 (0.18)	0.13 (0.18)	595	0.739	0.490	0.726
Emergency-coping capacity (1–5)	2.49 (1.10)	0.24 (0.08)	0.12 (0.08)	845	0.003	0.152	0.149
Bought a public lottery ticket (%)	31.90 (46.69)	-0.84 (3.97)	-1.85 (3.87)	845	0.832	0.633	0.796
Bets or gambles (%)	8.60 (28.09)	-0.53 (2.35)	2.96 (2.51)	845	0.822	0.238	0.161
Likely to join ($\leq$3 mo.) (1–4)	2.51 (0.97)	0.10 (0.08)	0.08 (0.08)	845	0.204	0.323	0.762
Past consórcio participation (%)	54.12 (49.92)	3.53 (4.20)	3.16 (4.17)	845	0.401	0.449	0.929
Currently paying a consórcio (%)	24.01 (42.79)	5.74 (3.78)	0.33 (3.60)	845	0.129	0.927	0.148
Family/friends in a consórcio (%)	74.19 (43.84)	-1.89 (3.76)	-2.35 (3.71)	845	0.615	0.527	0.904
Discussed consórcio with vendor (1–4)	2.13 (1.07)	0.19 (0.09)	-0.04 (0.09)	845	0.037	0.658	0.010
Lottery vs. auction preference (1–5)	2.06 (1.34)	0.13 (0.12)	0.17 (0.12)	844	0.287	0.141	0.714
Credit-timing preference (1–4)	2.23 (0.92)	-0.06 (0.08)	-0.06 (0.07)	845	0.444	0.411	0.999
Willing to bid $\geq$50% (%)	40.14 (49.11)	1.01 (3.90)	0.17 (3.87)	845	0.795	0.965	0.830
Unconstrained: can fund a 50% bid (%)†	25.09 (43.43)	-0.18 (3.69)	0.85 (3.65)	845	0.961	0.816	0.779
Bidding-share belief index (SD)	-0.02 (0.68)	0.11 (0.06)	-0.02 (0.06)	845	0.052	0.757	0.027
Lottery belief index (SD)	0.08 (0.73)	-0.03 (0.07)	-0.13 (0.06)	845	0.597	0.035	0.144
Bidding-belief certainty (1–5)	2.41 (0.92)	0.09 (0.08)	0.15 (0.08)	845	0.240	0.057	0.465
Lottery-belief certainty (1–5)	2.49 (0.96)	0.13 (0.08)	0.16 (0.08)	845	0.128	0.040	0.668
Hypothetical WTP in Wave 1 (%)	64.87 (47.82)	0.57 (4.04)	-0.72 (4.01)	845	0.889	0.858	0.749
Joint orthogonality (p)					0.412	0.639	0.217

Notes: This table continues Table A.11; see its notes.

Table A.12: Wave 2 attrition and quality-filter flow, by information-treatment arm.

	Control (C)	Debiasing (D)	Debiasing + Group Size Mechanism (DG)	p-value (C vs. D)	p-value (C vs. DG)
Assigned in Wave 2	325 (33.0%)	326 (33.1%)	334 (33.9%)		
Survey attrition	26 (8.0%)	30 (9.2%)	22 (6.6%)	0.593	0.480
Quality filter	20 (6.2%)	23 (7.1%)	19 (5.7%)	0.605	0.825
Final sample	279 (85.8%)	273 (83.7%)	293 (87.7%)	0.440	0.487

Notes: This table shows Wave 2 completion by treatment arm, among the 985 respondents assigned to an arm. Entries are counts, with percentages in parentheses: the arm's share of all assigned respondents in the first row, and the share of the arm's assigned respondents in the other rows. Survey attrition counts respondents who did not finish Wave 2, and Quality filter those who finished but failed the Wave 2 quality checks. p-values are from regressions of each indicator on arm indicators with randomization-strata fixed effects and robust standard errors.

Table A.13: Treatment effects on beliefs (AIPW).

	Bidding Allocation				Lottery Wait Time				Group Structure
	Qualit.	Quanti.	Probab.	Certainty	Qualit.	Quanti.	Probab.	Certainty	Group Size
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Debiasing (D)	0.852***	0.125***	0.045**	0.218***	0.439***	0.095***	0.102***	0.188***	164.6***
	(0.087)	(0.018)	(0.020)	(0.064)	(0.085)	(0.019)	(0.017)	(0.066)	(59.8)
Debiasing + Group (DG)	0.771***	0.124***	0.069***	0.235***	0.480***	0.076***	0.107***	0.220***	93.9*
	(0.092)	(0.017)	(0.019)	(0.064)	(0.081)	(0.019)	(0.017)	(0.067)	(53.7)
D % Gap Correction		47.4%	20.6%			66.0%	45.2%		23.5%
DG % Gap Correction		47.1%	31.0%			52.7%	47.7%		13.4%
$D = DG$ (p)	0.288	0.974	0.265	0.792	0.612	0.315	0.738	0.621	0.235
Control Mean	3.806	0.533	0.576	2.495	3.774	0.596	0.644	2.613	596.1
N	845	845	845	845	845	845	845	845	845
Measurement	Scale 1–5	Share 0–1	Chance 0–1	Scale 1–5	Scale 1–5	Month 0–1	Chance 0–1	Scale 1–5	Members

Notes: This table shows treatment effects on Wave 2 beliefs. Estimates are augmented inverse-probability-weighted (AIPW) effects from a multi-arm causal forest (Athey et al., 2019) with the Wave 1 covariates and the randomization strata; standard errors in parentheses. Gap correction is the arm's effect as a share of the gap between the true value and the control mean, and is blank for outcomes without a true value. The $D = DG$ row reports the p-value for equal effects in the two arms. Group size is winsorized at the 99th percentile. $^{*}\,p < 0.1$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A.14: Learning rates from the information treatments.

	Bid share		Lottery wait		Lottery chance
	OLS	ORIV	OLS	ORIV	OLS
	(1)	(2)	(3)	(4)	(5)
Panel A. Pooled treatment					
Treatment	0.198***	-0.080	0.351***	0.333***	0.434***
	(0.076)	(0.191)	(0.073)	(0.090)	(0.092)
Gap	0.503***	0.150	0.496***	0.423***	0.586***
	(0.055)	(0.151)	(0.053)	(0.077)	(0.065)
Treatment × gap	0.246***	0.490***	0.070	0.103	0.085
	(0.066)	(0.173)	(0.069)	(0.101)	(0.078)
Panel B. By treatment arm					
D	0.183**	-0.181	0.339***	0.286***	0.448***
	(0.089)	(0.205)	(0.080)	(0.100)	(0.103)
DG	0.214**	0.041	0.363***	0.381***	0.419***
	(0.087)	(0.224)	(0.082)	(0.103)	(0.104)
D× gap	0.249***	0.571***	0.091	0.169	0.074
	(0.079)	(0.188)	(0.083)	(0.119)	(0.090)
DG× gap	0.241***	0.400**	0.053	0.047	0.095
	(0.075)	(0.202)	(0.081)	(0.118)	(0.090)
Respondents	845	845	845	845	845

Notes: This table shows how much of the initial belief gap treated respondents close. The dependent variable is the change in belief from Wave 1 to Wave 2; each column regresses it on a treatment indicator, the gap between the true value and the Wave 1 belief, and their interaction, with asset category fixed effects. The interaction is the learning rate. Changes and gaps are in Wave 1 standard deviations. OLS columns average the questions on each belief (three for the bid share, two for the lottery wait), with robust standard errors; ORIV columns instrument the gap in each question with the gap in another (Gillen et al., 2019), with standard errors clustered by respondent. Lottery chance is the probability of no award in the first half of the group's duration, so higher values mean a longer wait. For the qualitative questions, the true value is the top category. $^{*}\,p < 0.1$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A.15: Treatment effects on attitudes and recommendation (AIPW).

	Bidding-Advantage Beliefs		Would Recommend to Non-Bidders		
	Only Good if Bidding	Admin. Favors Bidders	Yes	No	Don't Know
	(1)	(2)	(3)	(4)	(5)
Debiasing (D)	0.106**	0.093**	-0.031	0.062**	-0.036
	(0.042)	(0.039)	(0.036)	(0.029)	(0.029)
Debiasing + Group (DG)	0.081**	0.050	-0.016	0.065**	-0.054*
	(0.041)	(0.039)	(0.036)	(0.029)	(0.029)
$D = DG$ (p)	0.546	0.251	0.668	0.923	0.512
Control Mean	0.477	0.649	0.624	0.151	0.226
N	845	845	845	845	845
Measurement	Share 0–1	Share 0–1	Share 0–1	Share 0–1	Share 0–1

Notes: This table shows treatment effects on Wave 2 attitudes toward consórcios, coded 0/1. Estimates are AIPW effects from a multi-arm causal forest, as in Table A.13; standard errors in parentheses. The first two columns are the shares agreeing that consórcios are a good deal only for those who can bid, and that administrators favor bidders. The Would recommend columns are the shares who would, would not, or do not know enough to recommend a consórcio to someone who cannot bid. The $D = DG$ row reports the p-value for equal effects in the two arms. $^{*}\,p < 0.1$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A.16: Information treatment and hypothetical choices of a loan or saving over a consórcio, by liquidity and optimism.

	Loan over consórcio			Saving over consórcio		
	(1)	(2)	(3)	(4)	(5)	(6)
Debiasing (D)	0.062*	-0.012	-0.052	0.040	-0.024	-0.030
	(0.033)	(0.047)	(0.051)	(0.042)	(0.056)	(0.065)
× optimist		0.165**	0.157**		0.141*	0.120
		(0.066)	(0.073)		(0.084)	(0.098)
× liquidity to bid			0.182			0.028
			(0.122)			(0.130)
× optimist × liquidity			-0.022			0.063
			(0.164)			(0.189)
Debiasing + Group (DG)	-0.019	-0.084**	-0.084*	-0.060	-0.163***	-0.114*
	(0.030)	(0.042)	(0.049)	(0.041)	(0.055)	(0.064)
× optimist		0.145**	0.126*		0.228***	0.131
		(0.059)	(0.067)		(0.082)	(0.096)
× liquidity to bid			0.001			-0.222*
			(0.098)			(0.122)
× optimist × liquidity			0.064			0.385**
			(0.137)			(0.183)
Optimist		-0.150***	-0.153***		-0.243***	-0.200***
		(0.043)	(0.049)		(0.060)	(0.070)
Liquidity to bid			0.033			0.185*
			(0.091)			(0.101)
Optimist × liquidity			0.015			-0.165
			(0.105)			(0.136)
$D = DG$ (p-value)	0.012	0.097	0.483	0.015	0.012	0.175
Control mean	0.18	0.24	0.23	0.48	0.58	0.55
Observations	845	845	845	845	845	845

Notes: This table shows treatment effects on hypothetical choices of a loan (columns 1–3) or saving (columns 4–6) over a consórcio with average market attributes, with the loan rate or the savings return randomized. Estimates are AIPW effects from one multi-arm causal forest per outcome, with the randomization strata and the randomized rate as covariates. Arm rows are effects in the reference group: all respondents in columns 1 and 4, the less optimistic in columns 2 and 5, and the less optimistic without liquidity to bid in columns 3 and 6; × rows are differences from it. The optimist and liquidity rows are differences within the control arm. The $D = DG$ row and the control mean refer to the reference group. Optimist rows refer to the most optimistic respondents, who report a Wave 1 chance of a lottery award in the first half of the group's duration above the median of 30%; liquidity to bid means having own resources to bid 50% of the contract's face value. Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.17: Information treatment and willingness to pay for the lottery-intensive offer.

	Willingness to pay (BRL, incentivized price list)						
		Heterogeneity by liquidity			By beliefs	By outside options	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Debiasing (D)	4.77*	5.77*	7.05**	6.79**	2.63	15.67***	12.09**
	(2.89)	(3.33)	(3.41)	(3.27)	(4.00)	(4.56)	(5.72)
× liquidity to bid 50%		-4.00	-4.28				
		(6.76)	(6.73)				
× strategy = bid early				-8.72			
				(7.01)			
× more optimistic about early award					4.78		
					(5.79)		
× saved in last 12 months						-17.60***	
						(5.87)	
× bank would approve a loan							-9.78
							(6.63)
Debiasing + Group (DG)	-0.16	0.24	0.01	-1.12	0.63	1.84	6.59
	(2.71)	(3.14)	(3.24)	(3.10)	(3.75)	(4.46)	(5.34)
× liquidity to bid 50%		-1.58	1.28				
		(6.22)	(6.20)				
× strategy = bid early				4.55			
				(6.35)			
× more optimistic about early award					-1.75		
					(5.42)		
× saved in last 12 months						-3.28	
						(5.62)	
× bank would approve a loan							-9.24
							(6.20)
$D = DG$ (p-value)	0.08	0.09	0.05	0.01	0.62	0.00	0.37
Sample	Full	Full	WTP-consistent	Full	Full	Full	Full
Control mean	36.6	36.6	34.8	36.6	36.6	36.6	36.6
Observations	845	845	810	845	845	845	845

Notes: This table shows treatment effects on willingness to pay for access to the offer, in BRL, from each respondent's price list (up to BRL 50 or BRL 100, depending on the offer-value arm). Columns, estimator, and tests are as in Table 3. Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.18: Information treatment and valuation of the lottery-intensive offer: further measures of liquidity.

	WTP = maximum (chose offer at the top price)				
	(1)	(2)	(3)	(4)	(5)
Debiasing (D)	0.130**	0.078*	0.119**	0.075*	0.073
	(0.062)	(0.045)	(0.057)	(0.043)	(0.049)
× savings buffer ≥ 6 months	-0.173*				
	(0.095)				
× prepared for an emergency		-0.087			
		(0.090)			
× could cope with an emergency			-0.117		
			(0.078)		
× income > BRL 20,000				-0.083	
				(0.099)	
× income ≥ BRL 10,000					-0.035
					(0.081)
Debiasing + Group (DG)	-0.050	-0.006	0.002	-0.017	-0.010
	(0.060)	(0.043)	(0.055)	(0.042)	(0.047)
× savings buffer ≥ 6 months	0.001				
	(0.093)				
× prepared for an emergency		-0.019			
		(0.089)			
× could cope with an emergency			-0.023		
			(0.076)		
× income > BRL 20,000				0.040	
				(0.096)	
× income ≥ BRL 10,000					0.001
					(0.079)
$D = DG$ (p-value)	0.00	0.04	0.02	0.02	0.07
Control mean, moderator = 0	0.33	0.35	0.34	0.33	0.32
Observations	595	845	845	845	845

Notes: The outcome is an indicator that the respondent chose the consórcio over the gift card at the top of the price list (Section 5.1.3). Estimates are augmented inverse-probability-weighted (AIPW) effects from causal forests that compare each arm with the control group. In columns 1–5, we present effects when the moderator equals zero and the difference when the moderator equals one. The moderators are Wave 1 measures. Savings buffer is the report that, with no income, current savings would sustain the household's standard of living for at least six months; column 1 restricts to the 595 respondents who answered this item. Prepared for an emergency and could cope with an emergency are two cutoffs of the same five-point rating of how well the respondent could handle a financial emergency: "a lot" or "completely" (prepared), and "a bit" or above (could cope). Income is monthly household income; the sample median bracket is BRL 5,001–10,000. The $D = DG$ row tests equality of the two arms' effects, in the moderator-zero group. Standard errors in parentheses. Control means are control-group averages of the dependent variable. $^{*}\,p < 0.1$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A.19: Treatment effects on bidding beliefs and intentions.

	Expected winning bid (1)	Likelihood of bidding (2)	Competitiveness of own bid (3)	Reported own bid (4)
Debiasing (D)	5.53***	0.157	-0.046	1.71
	(1.71)	(0.098)	(0.087)	(1.54)
Debiasing + Group (DG)	6.25***	0.120	0.063	2.16
	(1.71)	(0.094)	(0.085)	(1.56)
$D = DG$ (p-value)	0.680	0.695	0.193	0.777
Control mean	44.51	3.018	2.989	30.92
Wave 1 measure included	Yes	–	–	–
Observations	845	845	845	845
Measurement	% of face value	Scale 1–5	Scale 1–5	% of face value

Notes: This table shows treatment effects on Wave 2 bidding beliefs and intentions. Estimates are augmented inverse-probability-weighted (AIPW) effects of each arm relative to the control group, from a multi-arm causal forest that conditions on pre-treatment covariates, randomization strata, and, in column (1), the Wave 1 value of the outcome. Expected winning bid and reported own bid are percentages of face value; likelihood of bidding and competitiveness of own bid are on 1–5 scales. The $D = DG$ row reports the p-value for equal effects in the two arms. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B Institutional context

This appendix describes the rules that govern consórcio contracts, groups, awards, and firms. Consórcios are regulated by Law 11,795/2008 and by Central Bank rules: Circular 3,432/2009, in force for most of our sample period, and Resolution BCB 285/2023, which replaced it in July 2024 and keeps the provisions we describe below.

Contracts. The monthly installment has up to four components: the contribution to the pool, the administration fee, a contribution to a reserve fund, and an insurance premium. Each is set as a percentage of the price of the asset referenced in the contract, so all of them move with that price. Contracts traditionally referenced a specific asset, such as a given car model, so that payments and credit followed that model's list price. This practice is now less common: contracts usually reference a credit value updated by a price index or a price table set in the contract. The administration fee is a fixed percentage spread over the months of the contract, but firms may collect part of it at signing to pay sales representatives and brokers. Moreover, firms may generally anticipate the collection of fees, charging higher fees in the earlier months of a contract. Sales commissions can therefore be paid when a contract is sold, while the rest of the fee accrues over the contract's life.

Contracts are often bundled with credit life insurance. If a participant who has not yet been awarded dies and the insurer's payment covers the remaining balance, the payment counts as a winning bid in the next monthly meeting with enough funds, and any surplus goes to the participant's beneficiaries. Coverage beyond death, such as disability or job loss, varies by contract.

Groups. A group must be formed within 90 days of the first contract sold; otherwise, the firm refunds any amounts collected, with returns. Participants who join a group already under way must complete their payments within the group's remaining duration, so their contract's term is constrained to the duration of the group.

The pool legally belongs to the group's participants, and not to the firm. It must be deposited in a commercial bank and invested only in federal government bonds or in short-term fixed-income funds, excluding funds in which the firm itself invests. Funds waiting to be awarded therefore earn approximately the base interest rate, and any of these returns accrue to the group. In practice, the common pool is usually kept at small levels, so the money accrued is not significant.

Groups may also hold a reserve fund. It covers shortfalls in the pool, insurance against participants' defaults, and collection costs. When it is not needed for these, it can fund additional lottery awards. Within 60 days of the last award meeting, the firm must notify participants that unused credit and outstanding refunds are available in cash, and the group must close within 120 days. Balances left in the pool and the reserve fund are returned to active participants in proportion to

their payments.

Awards. Awards take place at monthly meetings. Only participants who are current on their payments are eligible. Lottery awards are drawn first, and bids are awarded only after the first lottery award, or when the pool is too small for lotteries. A bid must be paid before the award is confirmed. Some contracts allow "embedded bids", which is a mode of payment in which part of the credit being awarded pays the bid, and the winners' effective funds are only the difference. Other contracts allow "fixed bids", where a pre-defined bid amount is set, and a lottery takes place among participants submitting that fixed bid.

Usage of funds. The award can be used with any seller, and the firm pays the seller directly. In vehicle groups, the credit can buy any vehicle, new or used, as well as aircraft, boats, and capital machinery. Often, vehicles that are too old are not accepted. In real-estate groups, it can buy a finished or off-plan property, or land, and, if the contract allows, pay for construction or renovation. In every category, the credit can also pay off an existing loan on an asset of the same category. If the asset costs less than the credit, the difference can cover related costs (transfer taxes, registration, and insurance) up to 10% of the credit, prepay future installments, or be paid out in cash once the participant has settled their obligations to the group. The asset bought serves as collateral, and the firm may require additional guarantees before releasing the credit.

Awarded credit is not necessarily used immediately. The firm must make it available within three business days of the award, and until it is used it remains invested with the group's funds and accrues their returns. A participant who waits for a better purchase therefore earns approximately the Selic rate on the credit in the meantime. Therefore, considering financial returns, an early lottery award is always more profitable even if the asset is not desired to be acquired immediately. After settling all remaining payments and waiting 180 days, the participant can take the award in cash instead of purchasing an asset, so consórcios do not function as a source of liquidity or insurance. Because cashing out requires paying off the remaining balance, the award is in practice a right to buy an asset of the category, not a cash loan.

Cancellations and refunds. A contract is canceled when the participant files a cancellation explicitly or misses three consecutive payments. Canceled contracts do not receive their refund on leaving: they remain in the group's lottery draws, and a canceled contract that is drawn receives a refund instead of credit. The refund equals the share of the asset's price the participant had paid into the pool, valued at the price on the date of the draw, plus the pool's returns, minus contractual fines. Since 2024, fines cannot exceed the administration fee the firm would have collected had the participant stayed. Canceled contracts that are never drawn are refunded when the group closes.

Once the credit has been used, the contract can no longer be canceled, missed payments are a

default, and the firm must move to seize the collateral after more than one late installment.

Secondary markets. Contracts can be transferred to a third party with the firm's consent, and the firm assesses the buyer's ability to pay. An informal market trades contracts that have been awarded but whose credit is unused: the buyer pays the seller and takes over the remaining installments, obtaining credit at once without bidding. This market also lets participants join as investors, buying contracts in order to resell them after award.

Firms. Firms that manage groups must be authorized and supervised by the Central Bank. Their revenue is the administration fee: returns on the pool accrue to the group, and at least half of any fines and late-payment interest must go to the group, with the remainder to the firm. Firms also bear credit risk at award, as they must compensate the group for losses from guarantees they approved that prove insufficient. This liability gives firms a reason to screen participants before releasing credit. In practice, the cost of defaults is passed through the reserve fund fee.

C Central Bank data: extraction and sample construction

This appendix describes how we extract, clean, and filter the administrative consórcio data reported to the Central Bank of Brazil, and how we build the analysis sample.[36] The source is the quarterly contract-level filings that administrators are required to submit. We merge this data at the taxpayer-ID (*CPF*) level with the national credit registry, the federal revenue individual and firm registries, formal labor-market records, and the social-assistance registry. Our paper restricts analysis to the real-estate, automobile, and motorcycle assets, which represent 90% of contracts sold. We therefore exclude heavy vehicles (trucks, farm machinery) and services.

Our two main datasets include a contract-level sample of 8,642,735 contracts in 7,328 groups and a corresponding longitudinal group-level dataset, which covers 6,887 of these groups after filtering.

C.1 Extraction, sampling, and de-duplication

We sample contracts by stratifying and extracting smaller files at the group-year level, ranging from 2010 to 2025.[37]

We aggregate observations to obtain a cross-section of contracts. A contract is also referred to as a "share", and we turn to this term because there's an identifier called "subshare". In the dataset, a contract is formally identified by an *asset–administrator–group–share–subshare* key. The subshare number exists to allow a canceled contract to be resold and replaced while preserving the same share number. Typically, the subshare number is a chronological sequence from 1, 2, .., and so on up until the current or last owner. In practice, when a canceled contract is replaced, a subshare is first reported as `0` and then switch to a nonzero code, spuriously multiplying observations of what is essentially a single contract. We therefore ignore the subshare dimension and extract data at the *asset–administrator–group–taxpayerID–share* as the unit of analysis, which removes the double-counting artifact while still distinguishing the legitimate case of a number resold to a new holder. One consequence is that public published aggregate totals, which count share–subshare pairs, will not match our contract–CPF counts exactly.

C.2 Contract-level cleaning

Cleaning reduces the filings to one row per contract–CPF. As noted above, filings can carry more than one row per contract–CPF, both from the subshare artifact and from the monthly panel, and these rows need not agree. We therefore keep only contracts whose fields are invariant across all of

[36] The SQL scripts that extract the datasets are available from the authors for replication, pending Central Bank authorization.

[37] Sampling whole groups, rather than individual contracts, preserves the within-group allocation dynamics that the group-level analysis studies.

their rows — the enrollment, award, and cancellation dates, the allocation type, the planned term, and the administration fee — and drop the 19% of contract–CPF units that are not.[38] Monetary values are deflated to constant reais by the enrollment-month price index. Table C.1 defines the variables the analysis relies on.

Table C.1: Key variables in the contract-level data

Variable	Definition
Contract–CPF	Unit of analysis: a contract collapsed across subshare codes and identified with its holder's CPF.
Enrollment, award, cancellation dates	Dates the contract enters the group, is awarded credit, and (if applicable) is canceled.
Allocation type	Whether the award came by lottery (*sorteio*) or bid (*lance*).
Wait for award	Months from enrollment to award, for awarded contracts.
Bid size	Bid amount as a share of the total contract value (the credit value plus the administration fee).
Contract value	Value of the asset at enrollment (pre-correction) and at its final level (post-correction), in constant BRL.
Individual/business dummy	Whether the contract is held by an individual or a firm.

- *Group start.* A group's start is the date of its first award — the earliest *contemplação* observed across the group's contracts.
- *Entry timing.* A contract's entry timing is its enrollment date relative to the group start, scaled by the group's planned duration: near zero for a contract that joins around the first award, near one for one that joins at the group's planned end.
- *Default and repossession.* We flag a default when a judicial collection action is recorded against the contract, and a repossession when the financed good is returned. They therefore represent defaults on obligations, not missed payments.

C.3 Group-panel construction

For the group-level analysis, we collapse the cleaned contracts into a panel with one row per group and award month. A group starts at its first award date and ends at the later of either (i) its planned end (start plus the planned duration) or (ii) its last observed award. The planned duration is defined as the maximum term among its contracts. We index periods in relative time, as a fraction of the group's duration, and measure group size both at fixed fractions of the planned duration (5, 10, and 20 percent) and over the first 5 percent of *observed* periods, the latter so that young groups are not dropped for want of history; active size nets out cancellations. A group is finished when its end date precedes the panel cutoff (31 December 2025).

[38] The share is 15% for cars, 26% for motorcycles, and 10% for real estate, and 35% for groups started in 2010–2014 against 2% for groups started in 2024–2025.

C.4 Group inclusion criteria

We keep groups that behave like standard, well-populated consórcio groups. We drop any that meets one of the following criteria:

1. a cancellation share of 80 percent or more;
2. a group smaller than its planned duration in months (N < T);
3. a finished group that lasted less than 80 percent of its planned duration;
4. groups where a single CPF holding more than 10 percent of the contracts;[39]
5. groups where firms represent 40 percent or more of total contracts;
6. total of non-canceled awards is larger than number of active contracts at any point;
7. very short group durations (below 100 months for real estate, below 30 for cars and motorcycles).

The population census of groups is cross-sectional, so we apply only three rules computable from census columns: (i) cancellation share; (ii) size versus duration; (iii) standard duration. The remaining screens require a monthly panel and cannot be reproduced from the census due to computing constraints.

C.5 Weighting and representative samples

Because the estimation sample is a cluster sample of groups, we reweighted groups according to the population samples. The census fixes the population of groups and their observable margins. From these frames we construct a survey weight, normalized to mean one. A group's base weight is the inverse of its sampling probability, known by design as the number of population groups over the number sampled in its (start-year, asset) stratum. The group-level weight w_g is raked to the group census on start year, group size, asset, and planned duration, and makes the sampled groups representative of the population.

[39] This criterion is also a regulatory requirement from the Central Bank.

D Prediction of censored group histories

This appendix documents the predictions introduced in Section 3 in four steps. First, what is estimated: seven objects, of which four are rates at the group-by-period level, two are prices of an imputed exit, and one is the cumulative bid share that completes panel A of Figure 5. Second, the data and covariates used for estimation: the cut at each group's own first quarter of planned duration, along with a general time profile estimated from all groups of the same asset category. Third, how we estimate: one binomial logit specification for the four rates, prices as cell means or a regression, and then recursions that map rates and prices into a contract's exit and return. Fourth, the validation: in completed groups censored at the cut and predicted from models estimated without them, the lottery wait of a participant who never bids and the pace of awards are matched within a percentage point for cars and motorcycles, and the imputed return distribution of contracts still waiting sits two cents below the realized one. Predictions are made only for groups observed through the cut, and only for the part of each group's history that is not observed.

D.1 Prediction objects

Table D.1 lists the first six objects to be estimated. The first four are rates at the group-by-period level: three hazards giving the probability that: (i) a waiting contract is awarded; (ii) an active contract cancels; and (iii) a waiting contract is drawn in the lottery; and the bid share of a period's awards. The next two are prices: the winning bid amount paid (when the award is won by bid), and the refund value a contract recovers on cancellation.

These six objects are combined to predict the outcomes of a censored contract. The cancellation hazard first sets the exit outcome. Conditional on not canceling, the award hazard predicts the probability of an award in each remaining period. The bid share splits an award between bid and lottery. Then, for each of the three outcomes, the two prices are components of the return calculation: a bid award is priced at the winning bid (a lottery award sets the bid to zero) and a cancellation at the refund value. We call the exit assigned this way an imputed exit; with the contract's own terms, it becomes a return.

Finally, we estimate the cumulative bid share of a group over time.

Everything the paper uses is derived from these objects. The first three rates give each waiting contract a probability of leaving by bid, by lottery, or by cancellation in each remaining period, and the two prices turn those exits into a return. The lottery hazard gives the wait of a participant who never bids. It equals the award hazard times one minus the bid share, but we estimate it directly, because the never-bidder's wait is the object the paper reports and a direct fit can be selected and validated on it; the product of the first two rows is not used.

Table D.1: Predicted objects, definitions, and uses.

Object	Definition	Use in the paper
Panel A. Group-by-period rates and shares		
1. Award hazard	Awards by cash bid or lottery divided by contracts eligible for an award	Award timing of waiting contracts
2. Cash-bid share	Bid awards divided by all awards	Division of awards between cash bid and lottery
3. Cancellation hazard	Cancellations divided by contracts at risk of cancellation	Cancellation timing and probability
4. Lottery hazard	Lottery awards divided by contracts eligible for an award	Waiting time of a participant who never bids
Panel B. Prices assigned to imputed exits		
5. Winning bid	Mean winning bid within asset category and period	Payment at an imputed bid award
6. Cancellation value	Predicted net recovery for a canceled contract	Value of an imputed cancellation

D.2 Data and covariates used for estimation

A group's planned duration is divided into twenty equal periods. For each group and period, the data record lottery awards, bid awards, cancellations, active contracts, and waiting contracts, where waiting contracts are the active contracts not yet awarded. Completed groups contribute all twenty periods while ongoing groups contribute their observed periods.

We construct seven group-level predictors using the first 25% of the groups planned duration: (i) log group size at the cut; (ii) group size at the cut divided by planned duration in months; (iii) log planned duration; (ivvi) the average monthly numbers of lottery awards, bid awards, and cancellations during the first quarter; and (vii) the log ratio of the waiting-contract pool (at the start of the period) to group size at the cut.

Of the 6,887 groups observed through the cut, 43 have lottery awards and no bid award within it. We treat them as pure lotteries by definition, therefore requiring no fit, and they enter neither the estimation nor the validation.

D.3 Estimation procedure

The four rates share one specification. For rate h_{gt} of group g in period t, in asset category s,

$$\log \frac{h_{gt}}{1 - h_{gt}} = \alpha_t + \beta_t \ln N_g + \gamma_s + \delta_s \, \mathbf{1}[t = 20] + \theta' X_g + \sum_k \phi_k(t) \, x_{gk}, \tag{D.1}$$

where α_t are period effects, β_t is a period-specific loading on log group size N_g, γ_s is a category effect, δ_s is a category-specific effect for the final period, and X_g holds the remaining predictors. The three pace predictors x_{gk} (lottery awards, bid awards, and cancellations per month) are multiplied by natural cubic splines $\phi_k(t)$, so that a group's early pace can change over its life. Each rate is a binomial logit with the numerator and denominator of Table D.1 as successes and trials. It is estimated on all groups, completed and ongoing, with periods weighted by the group's sampling weight.

The winning bid is the mean winning bid in each category and period. The cancellation value is a linear regression, on canceled contracts, of the net value recovered on the timing of the cancellation and the shares paid in; predictions are clamped to the range observed among canceled contracts. The cumulative bid share is a binomial logit of cumulative bid awards over cumulative awards on a natural cubic spline in relative time with three degrees of freedom, log group size, and the bid share within the first period, estimated by category on completed groups only, since its outcome is defined at completion.

We combine the fitted rates in two steps. In both, a group's observed periods keep their realized values, and only its unobserved periods take fitted values. First, for a contract that never bids, the probability of a lottery award in period t is $p_{gt} = h^L_{gt} \prod_{\tau<t}(1 - h^L_{g\tau})$: the lottery hazard in t times the probability of not having been drawn earlier. Summing p_{gt} over periods gives the probability of a lottery award by any point in the group. Second, for each contract still waiting when its group's history ends, we follow the remaining periods one at a time. In each period, the contract may first cancel; if it does not, it may be awarded, and the bid share splits that award between bid and lottery. This gives, for every remaining period, the probability that the contract exits by bid, by lottery, or by cancellation. We value each exit as in the main text, by the net present value of the contract's cash flows per unit of face value: a bid exit pays the category's winning bid for that period, a lottery exit carries a zero bid, and a cancellation exit recovers the predicted refund. A contract's expected return averages these values, weighted by their probabilities. For the return distribution, however, we keep every combination of exit and period of every contract as a separate observation, weighted by its probability. Using one expected return per contract would compress the tails of the distribution.

We compared alternative time profiles, links, predictor blocks, and weighting schemes. The comparison emphasized the objects used in the paper: the cumulative probability of a lottery award at the midpoint and at completion, and expected waiting time conditional on an award. We also examined held-out deviance, giving equal weight to relative-time bins and separately evaluating the second half of the term. These criteria assess both the accuracy of the reported objects and errors that an exposure-weighted score can obscure. The chosen specification had lower pooled deviance than the by-category alternative under all three aggregations, although the differences between these finalists were small.

D.4 Results and validation

For cars and motorcycles, the lottery wait of a participant who never bids and the pace of awards are predicted within a percentage point of their realized values; the imputed exits of contracts still waiting reproduce the realized return distribution to within two cents, on the pessimistic side. The design is the one used for selection: completed groups are censored at the cut and predicted from models estimated without them, under five-fold cross-validation by group and under a vintage split that estimates on groups started through 2013 and predicts later ones. Cars and motorcycles have 2,299 and 2,179 completed groups. Real estate has 59, too few to validate at completion; its lottery path is checked at the midpoint on the 201 groups observed through at least half of their term, 142 of which are ongoing and enter the estimation, so this is an in-sample check of the midpoint and not a validation.

Table D.2 reports the lottery path and Figure D.1 overlays predicted and realized cumulative lottery-award curves. For cars, 19.4% of never-bidding contracts are predicted drawn by the midpoint against 19.8% realized, 98.1% by the end of the term against 98.2%, and the expected wait given a draw is 69.4% of the term against 68.4%, with a group-level mean absolute error of 3.6% of the term, under three months. For motorcycles the same figures are 12.0% against 11.9%, 94.1% against 95.9%, 75.2% against 74.4%, and 2.2 months. The vintage split gives the same accuracy. Relative to a model with only the category time profile, the group predictors lower held-out deviance by 21% in cars and 20% in motorcycles (column 7).

The pace of awards is predicted at least as closely (Table D.2, panel C): 58.3% of waiting car contracts are predicted awarded by the midpoint against 57.1% realized, and 60.5% of motorcycle contracts, predicted and realized alike; the expected award time is 45% and 43% of the term, within half a point of the realized values.

The one systematic miss is in the timing of lottery awards late in a group's life. Lottery awards come in a burst after three quarters of the term, and the model under-predicts it: by three quarters of the term, 33.3% of never-bidding motorcycle contracts are predicted drawn against 37.5% realized, and 45.9% against 47.8% for cars. The error rises with a group's bid share after the cut, which the cut does not forecast: groups that bid more than their first quarter suggested leave more of their lottery awards for the end.

Table D.2: Predicted against realized lottery path and award pace in held-out completed groups censored at the cut.

Sample	Groups	Share awarded by			Mean time to award	MAE	Held-out deviance
		Midpoint	Three quarters	End			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Lottery path, five-fold cross-validation							
Cars	2,299	0.194 / 0.198	0.459 / 0.478	0.981 / 0.982	0.694 / 0.684	0.036	−21%
Motorcycles	2,179	0.120 / 0.119	0.333 / 0.375	0.941 / 0.959	0.752 / 0.744	0.036	−20%
Real estate†	201	0.191 / 0.216					−8%
Panel B. Lottery path, vintage split							
Cars	1,064	0.193 / 0.189	0.493 / 0.462	0.981 / 0.979	0.684 / 0.692	0.040	
Motorcycles	1,020	0.108 / 0.112	0.343 / 0.356	0.950 / 0.961	0.756 / 0.748	0.029	
Panel C. Award pace, five-fold cross-validation							
Cars	2,299	0.583 / 0.571	0.843 / 0.835	0.995 / 0.996	0.446 / 0.450	0.022	
Motorcycles	2,179	0.605 / 0.605	0.866 / 0.866	0.994 / 0.995	0.432 / 0.433	0.014	
Real estate	59	0.583 / 0.599	0.851 / 0.879	0.987 / 0.995	0.440 / 0.431	0.025	

Notes. Every cell in columns (2)–(5) is *predicted / realized*. Groups are completed groups held out of estimation, censored at the cut and predicted from models fit without them. Columns (2)–(4) are the cumulative share awarded by the midpoint, three quarters and the end of the planned term: of contracts that never bid in Panels A and B, of all waiting contracts in panel C. Column (5) is the mean time to award, a fraction of the planned term — the never-bidder's wait in panels A and B, the award time of any waiting contract in panel C. Column (6) is the group-level mean absolute error of that mean time, also as a fraction of the term. Column (7) is the change in held-out deviance against a model carrying only the category time profile, so a negative number is an improvement from the group predictors; it is reported for the cross-validated lottery fits only. †Real estate is an in-sample check at the midpoint, on groups observed through at least half of their term, so its later columns are undefined.

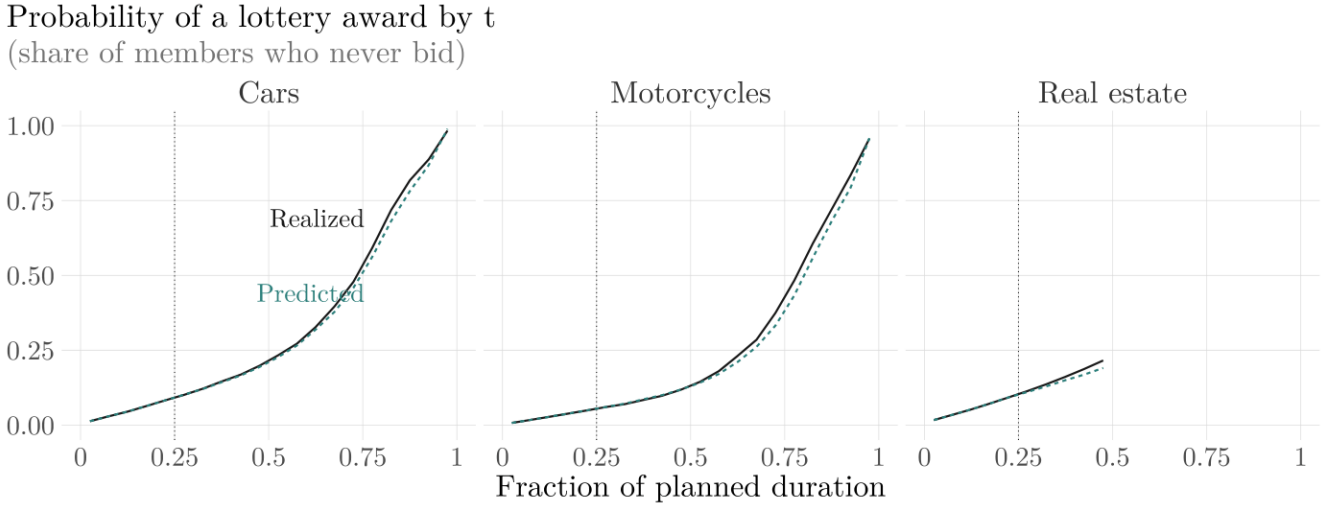


Figure D.1: Cumulative probability of a lottery award, realized and predicted from the cut, in held-out completed groups (cars, motorcycles) and in groups observed through at least half of their term (real estate).

Table D.3 turns to the contracts still waiting at the cut and compares their imputed exits and returns with the realized ones. Pooled over categories, the imputed median return is −0.087 per unit of face value against −0.069 realized, the mean −0.075 against −0.071, and 83% of imputed returns are negative against 79% realized: the imputed median sits about two cents below the realized one. The exit mix is close for bids, 48% imputed against 47% realized pooled and within two points in cars and motorcycles, 6 points low in real estate; cancellations are 38% against 33% pooled, within one point for motorcycles but 8 points high in cars and real estate; lotteries are low, 15% against 20% pooled, by 8 points in cars, 4 in motorcycles, and 2 in real estate. The errors by exit type offset. Imputed bid awards return 4 cents less than realized ones pooled, and 3 to 4 cents less in cars and motorcycles, because the winning bid of the category and period is pooled over all vintages and exceeds what these older completed groups paid; imputed lottery awards return 4 cents more pooled, and 3 to 7 cents more in cars and motorcycles, because exits are placed 1.3 months too early on average. The imputed winning bid is too high for older groups, which is why imputed bid returns fall short of realized ones.

Table D.3: Imputed against realized exits and returns of contracts still waiting when their group is censored at the cut, completed groups.

	Pooled (1)	Cars (2)	Motorcycles (3)	Real estate (4)
Panel A. Return of waiting contracts				
Median	−0.087 / −0.069			
Mean	−0.075 / −0.071			
Share negative	0.83 / 0.79			
Panel B. Exit shares				
Cash-bid award	0.48 / 0.47	0.47 / 0.47	0.49 / 0.47	0.38 / 0.44
Lottery award	0.15 / 0.20	0.19 / 0.27	0.11 / 0.15	0.18 / 0.20
Cancellation	0.38 / 0.33	0.34 / 0.26	0.40 / 0.39	0.44 / 0.36
Panel C. Mean return by exit				
Cash-bid award	−0.072 / −0.035	−0.050 / −0.008	−0.086 / −0.054	−0.121 / −0.101
Lottery award	−0.072 / −0.115	−0.061 / −0.089	−0.085 / −0.151	−0.122 / −0.154
Cancellation	−0.081 / −0.100	−0.080 / −0.099	−0.082 / −0.100	−0.076 / −0.095

Notes. Every cell is *imputed / realized*. The sample is contracts still waiting when their group is censored at the cut, in completed groups. Exit shares are shares of those waiting contracts; returns are per unit of face value. Column (1) pools the categories, weighting each by its number of waiting contracts. Panel A is reported pooled only.

E Simulated bidding strategies

This appendix describes the simulation behind Table 1. We simulate a contract through each group in our dataset under different bidding strategies, assuming bid amounts are constant every month until awarded, and record when it is awarded and the returns. Other participants' bids and the group's lottery awards stay at their observed or predicted paths.

First, the contract joins at the group's start, assuming its term is the group's planned duration and the asset category's average administration fee. We rule out canceling or defaulting. Each award is valued with Equation (1) at the benchmark rates, per BRL of payments to the pool.

Each month, the contract can be awarded by lottery using the group's estimated lottery probability (Appendix D). The model gives one probability per twentieth of the group's duration, spread evenly over its months, with any remainder in the term's last month, when every remaining contract receives its funds.

A bid of b wins the award in a given month if it exceeds that month's threshold: the lowest winning bid above 30% of face value, excluding fixed bids (a value that accounts for more than 20% of a group's bid awards). Each contract faces its own group's thresholds: for each twentieth of duration, we record whether any month's threshold is below b, and the bid wins in the first such twentieth, spread evenly over its months. In a completed group whose thresholds never fall below b, the contract can only be awarded by lottery. In an ongoing group, thresholds after the last observed period are unknown; there, a Kaplan-Meier estimator by asset category, weighting each group by its sampling weight times its size and censoring ongoing groups at their last observed period, gives the probability that b wins in each later twentieth, given that it has not won yet. This applies to 3% of group and bid combinations.

With h_t^L and h_t^B the probabilities of a lottery and a bid award in month t given no earlier award, the contract is awarded in month t with probability

$$a_t = \prod_{s<t} \left(1 - h_s^L\right) \left(1 - h_s^B\right) \left[1 - \left(1 - h_t^L\right) \left(1 - h_t^B\right)\right], \tag{E.1}$$

of which a share $\theta_t = h_t^B / (h_t^L + h_t^B)$ is a bid award. With $V_t(b)$ the value of an award in month t with bid b paid, the expected return is $\sum_t a_t \left[\theta_t V_t(b) + (1 - \theta_t) V_t(0)\right]$. Never bidding sets $h_t^B = 0$.

The statistics in Table 1 are computed from a_t group by group and averaged over each asset category's groups, weighting each group by its sampling weight times its size, so that they describe the average contract.

F The consórcio value

This appendix derives a closed form for the consórcio value V_i of Equation (1).

F.1 Setup

Consider a contract with credit P, a term of T months, and administration fee τ, awarded in month $t^A \leq T$, and let $n := T - t^A$. We drop the contract subscript i until it is needed. The participant pays $c^{pre} := (1+\tau)P/T$ in each month up to the award. A bid of $b(1+\tau)P$, with $0 \leq b \leq 1 - t^A/T$, is paid at the award, and the remaining balance, $(1+\tau)P\,(1 - t^A/T - b)$, is paid in n equal installments c^{post}. The payment stream is therefore

$$c_t = \begin{cases} c^{pre}, & t < t^A, \\ c^{pre} + b(1+\tau)P, & t = t^A, \\ c^{post}, & t > t^A, \end{cases}$$

and the credit P is received at t^A. Let r^S and r^B be the annual savings and borrowing rates, with $0 < r^S \leq r^B$, and $\rho^x := (1+r^x)^{1/12} - 1$ their monthly equivalents. For $x \in \{S, B\}$, $A^x(k) := [1 - (1+\rho^x)^{-k}]/\rho^x$ is the present value of BRL 1 a month for k months at ρ^x. Finally, $y^+ := \max\{y, 0\}$.

F.2 The value

The market replication deposits the contract's own payments up to the award, bid included, in a savings account. At the award, the fund holds

$$F := \sum_{t=1}^{t^A} c_t\,(1+\rho^S)^{t^A - t},$$

and the shortfall is $L := P - F$. If $L > 0$, the replication borrows L at r^B and repays it in n level payments $\mathrm{PMT} := L/A^B(n)$.[40] If $L \leq 0$, it keeps the excess $-L$, and $\mathrm{PMT} := 0$. Discounting both options at ρ^S,

$$\mathrm{NPV}^{\mathrm{con}} := P(1+\rho^S)^{-t^A} - \sum_{t=1}^{T} c_t(1+\rho^S)^{-t},$$

$$\mathrm{NPV}^{\mathrm{mkt}} := \big[P + (F-P)^+\big](1+\rho^S)^{-t^A} - \sum_{t=1}^{t^A} c_t(1+\rho^S)^{-t} - \sum_{t=t^A+1}^{T} \mathrm{PMT}\,(1+\rho^S)^{-t},$$

[40] $L > 0$ requires $n \geq 1$: at $t^A = T$ the fund holds at least the total payments, $(1+\tau)P \geq P$.

and $V := \text{NPV}^{\text{con}} - \text{NPV}^{\text{mkt}}$. The deposits are worth $F(1+\rho^S)^{-t^A}$, and the loan payments are worth PMT $A^S(n)$ at the award. Since $P + (F-P)^+ - F = L^+$, the market replication has NPV

$$\text{NPV}^{\text{mkt}} = \underbrace{-\left(\frac{A^S(n)}{A^B(n)} - 1\right)}_{\text{Additional borrowing cost per BRL borrowed}} L^+(1+\rho^S)^{-t^A},$$

and hence

$$V = \text{NPV}^{\text{con}} + \left(\frac{A^S(n)}{A^B(n)} - 1\right) L^+(1+\rho^S)^{-t^A}. \tag{F.1}$$

Therefore, V is the net present value of consórcios, plus any financial benefits of borrowing through a consórcio instead of the market. When $r^B = r^S$, there is no benefit of borrowing from the market at the opportunity cost r^S, and V is the NPV at r^S. Because r^B enters Equation (F.1) only through $A^B(n)$, which increases with r^B, V is strictly increasing in r^B when $L > 0$ and does not depend on r^B when $L \leq 0$. A break-even borrowing rate is therefore unique when it exists, and undefined for contracts whose savings cover the credit.

F.3 Implementation

For each contract, we compute V_i from Equation (F.1). It depends only on the contract's own terms (P_i, T_i, τ_i, b_i) and award date, not on other members, so it can be measured for early recipients in ongoing groups, whose awards are already observed. It is homogeneous of degree one in P_i, so we report it per BRL of credit.

G Treatment video and survey questions

G.1 Treatment videos

The information treatments are short videos with a Portuguese voice-over produced by the research team. Arms *D* and *DG* share a common set of scenes (approximately 90 seconds). The *DG* arm video adds extra scenes (approximately 30 seconds), for a total runtime of about two minutes. Each arm is tailored for asset categories (real estate, cars, and motorcycles) with identical scenes and narration except for the asset icon and the asset-specific statistics, all of which are computed from the Central Bank administrative data. Videos were rendered in a vertical format optimized for mobile screens and embedded directly in the endline survey.

Table G.1 lists the scenes. Below, we describe each scene's visuals and reproduce the narrated script verbatim (in Portuguese, with English translations). The narration quoted is from the motorcycle cut; where the cuts differ, the car and real-estate values are indicated.

Table G.1: Scenes of the treatment videos

Scene	Content	Arms
2	The common fund	D, DG
3	Two award mechanisms: lottery and bid	D, DG
4	Bid share of awards in the first half	D, DG
5	Bidding crowds the lottery	D, DG
6	The bid–lottery trade-off	D, DG
7	Timing of lottery awards	D, DG
8	Recap: think about your goal	D, DG
G1	Groups come in different sizes	DG
G2	One lottery per month: group size and bid share	DG
G3	Lottery chances by group size	DG
G4	Group sizes over time	DG

Base module (arms *D* and *DG*)

Scene 1. Disclaimer. An opening card shows a data icon next to the asset icon (house, car, or motorcycle). On-screen text only: *"As informações deste vídeo usam dados reais do Banco Central do Brasil."* ("The information in this video uses real data from the Central Bank of Brazil.")

Scene 2. The common pool. Twenty participant figures each send a coin, representing the monthly installment, into a money bag labeled *Fundo Comum* (common pool), under the caption *"Como funcionam as contemplações de crédito?"* ("How do awards work?").

"No consórcio, todo mês as parcelas dos participantes formam o fundo comum. Todas

as contemplações de crédito usam o dinheiro desse fundo."
("In a consórcio, every month the participants' installments form the common fund. All credit awards use the money from this fund.")

Scene 3. Two award mechanisms. The pool divides into two: a teal path leading to a *Sorteio* (lottery) badge and a gold path leading to a *Lance* (bid) badge. One figure is awarded through lottery and four through bids, reflecting true data.

"Tem dois jeitos de ser contemplado: por sorteio, ou por lance. A administradora decide a regra de contemplação. Sempre tem pelo menos um sorteio. E, normalmente, o restante é por lance."
("There are two ways to be awarded: by lottery or by bid. The administrator decides the award rule. There is always at least one lottery. And, normally, the rest is by bid.")

Scene 4. Bid share of awards in the first half. The five awarded figures regroup into a donut chart titled *"Lance ou sorteio: qual acontece mais?"* ("Bid or lottery: which happens more?") for the first half of the group, splitting awards into a gold bid slice and a teal lottery slice with asset-specific shares: 86%/14% (motorcycles), 82%/18% (cars), 76%/24% (real estate).

"Costuma ter muito mais contemplações por lance do que sorteio. Na primeira metade de um grupo de motos típico, quase 86% das contemplações são por lance, e só 14% são por sorteio."
("There are usually many more awards by bid than by lottery. In the first half of a typical motorcycle group, almost 86% of awards are by bid, and only 14% are by lottery.")

Scene 5. Bidding crowds the lottery. Two conveyor belts of participants move toward a *Crédito* (credit) badge: a gold bid lane designed to move four times faster than a teal lottery lane, illustrating that the queue for bid awards is faster. Figures from the bid lane periodically also move to the lottery award, showing that intending bidders compete in the lottery as well.

"Já que tem muito lance, o sorteio acaba demorando. Até porque quem pretende dar lance também compete no sorteio enquanto não for contemplado."
("Since there is a lot of bidding, the lottery ends up taking long — not least because those who intend to bid also compete in the lottery until they are awarded.")

Scene 6. The bid–lottery trade-off. Monthly award slots rebalance on screen from four bid slots and one lottery slot to two of each: with more lottery awards, the lottery queue absorbs waiting participants faster, but there is less room for bid awards.

> *"A proporção de contemplações por sorteio e por lance é importante. Se diminuem as contemplações por lance, e aumentam as por sorteio, a fila do sorteio anda mais rápido. Mas ao mesmo tempo, fica mais difícil contemplar por lance."*
> ("The proportion of awards by lottery and by bid matters. If bid awards fall and lottery awards increase, the lottery queue moves faster. But at the same time, it becomes harder to be awarded by bid.")

Scene 7. Timing of lottery awards. Twenty figures first spread out evenly along a timeline under the card *"Exemplo: 100% sorteios"* ("Example: 100% lotteries"). They are then pushed toward the end of the timeline into a pyramid concentrated near the final periods, under the card *"Média do mercado hoje"* ("Today's market average"). Two captions quantify the message with segment-specific values: the chance of an early lottery award (*"Chance de contemplar — 2 a 3 em cada 20"*; "Chance of being awarded — 2 to 3 in every 20"; 3 in 20 in the car and real-estate cuts) and, with a shaded region beyond the segment-specific median, *"Metade só perto do final."* ("Half only near the end.").

> *"Imagine o seguinte: se não tivesse lance, os sorteios seriam todos distribuídos igualmente ao longo do tempo. Agora, como os grupos contemplam muito por lance no começo, então a maioria das contemplações por sorteio acontecem na parte final do grupo. Por causa disso, contemplar por sorteio no começo do grupo é bem raro: a cada 20 pessoas, só 2 a 3 vão ser contempladas por sorteio na primeira metade do grupo. E quem depende de sorteio costuma esperar bastante: mais da metade das pessoas que dependem de sorteio só vão ganhar bem tarde."*
> ("Imagine the following: if there were no bids, the lotteries would all be distributed evenly over time. Now, since groups award a lot by bid at the beginning, most lottery awards happen in the final part of the group. Because of this, being awarded by lottery at the beginning of the group is quite rare: out of every 20 people, only 2 to 3 will be awarded by lottery in the first half of the group. And those who depend on the lottery usually wait a long time: more than half of the people who depend on the lottery will only win quite late.")

Scene 8. Recap: think about your goal. Two summary cards appear under the title *"Antes de começar o consórcio, pense no seu perfil."* ("Before starting a consórcio, think about your profile."): a *Lance* card ("more awards"; "brings the credit forward, but you need to pay") and a *Sorteio* card ("few awards at the beginning"; "can take quite long").

> *"Antes de entrar em um consórcio, pense no seu objetivo. O lance pode antecipar o crédito, mas precisa de dinheiro na mão ou usar parte do próprio crédito como lance. Quem depende só do sorteio não paga lance, mas pode esperar bastante."*
> ("Before joining a consórcio, think about your goal. A bid can bring the credit forward, but it requires money on hand or using part of the credit itself as the bid. Those who depend only on the lottery pay no bid, but may wait a long time.")

Scene 9. Attribution and closing disclaimer. On-screen text only, no narration. Five attribution lines appear in sequence: *"Dados obtidos junto ao Banco Central do Brasil. Estimativas preparadas pela equipe de pesquisa. Estimativas consideram sorteios apenas de cotas ativas. As estimativas exibidas podem variar por grupo e administradora. O Banco Central não é associado a essa pesquisa, apenas é a fonte dos dados."* ("Data obtained from the Central Bank of Brazil. Estimates prepared by the research team. Estimates consider lotteries of active contracts only. The estimates shown may vary by group and administrator. The Central Bank is not associated with this research; it is only the source of the data.") A final warning card closes the video: *"Sempre se informe e pense por conta própria antes de tomar uma decisão financeira."* ("Always inform yourself and think for yourself before making a financial decision.")

Group-size module (arm *DG* only)

Scene G1. Groups come in different sizes. Under the title *"O tamanho do grupo é importante."* ("Group size matters."), two groups appear side by side: a small row of participants labeled *Grupo Menor* (smaller group) and a large grid of participants labeled *Grupo Maior* (larger group).

> *"Para escolher melhor, é bom pensar no tamanho do grupo. Tem grupos de consórcios menores e grupos maiores."*
> ("To choose better, it is good to think about group size. There are smaller consórcio groups and larger ones.")

Scene G2. One lottery per month: group size and bid share. Month-level award slots are shown for each group: the small group has one lottery slot and two bid slots, while the large group has one lottery slot and nine bid slots — the single monthly lottery is diluted in larger groups.

> *"Quanto maior o grupo, maior costuma ser a proporção de lances, especialmente no começo. Isso porque os grupos costumam ter só um sorteio por mês."*
> ("The larger the group, the larger the share of bids tends to be, especially at the beginning. That is because groups usually have only one lottery per month.")

Scene G3. Lottery chances by group size. In each group, one participant is highlighted as the monthly lottery winner. Because both groups draw one winner, the smaller group's members face better odds, summarized on screen as *"3 vezes mais chance"* ("3 times higher chance") under the smaller group.

> *"Por causa disso, em grupos menores, a chance de ganhar por sorteio costuma ser maior."*
> ("Because of this, in smaller groups the chance of winning by lottery tends to be higher.")

Scene G4. Group sizes over time. A line chart titled *"Os grupos estão cada vez maiores"* ("Groups are getting bigger and bigger") traces the average group size of newly started groups in the viewer's segment from 2010 to 2024, computed from the Central Bank data. Value labels pop in at 2012, 2016, 2020, and 2024, showing that the average group size roughly doubled over the period.

> *"Nos últimos anos o tamanho médio dos grupos vem aumentando. Então, é cada vez mais importante pensar nisso."*
> ("In recent years, the average group size has been increasing. So it is increasingly important to think about this.")

Active control (arm C)

The control arm watched an existing informational video produced by the Central Bank of Brazil, in which a presenter explains basic institutional features of consórcios that we expect consumers to already be familiar with. The video is of comparable length to the treatment videos. It contains no information about award probabilities, bidding shares, lottery wait times, or group sizes, and we verify that it does not move beliefs by comparing Wave 1 and Wave 2 beliefs within the control group.

G.2 Belief questions

Figures G.1–G.3 reproduce the Wave 1 belief questions as they appear in the survey. The version below is a translated English version, as respondents answered in Portuguese. Each question refers to a scenario built from the respondent's earlier answers: the asset category and the contract duration are those the respondent chose, and the group size in the reference group is randomized across respondents as a multiple of the duration. The screenshots show one such scenario, a 60-month car contract in a group of 300.

Think of a **typical consórcio group** in the **Cars** market with a term of **60 months**.

Imagine that this group held its first meeting **6 months ago**.

What do you think is the **average group size**, that is, the **average number of contracts**?

Enter a whole number (this group needs at least 60 participants).

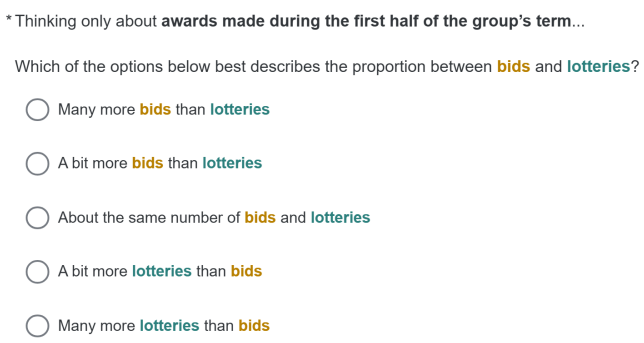
(a) Belief about the average group size

For all questions, use as **reference** a consórcio group with the following characteristics:

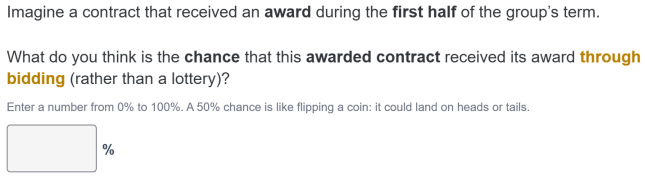
Segment	**Cars**
Group duration	**60 months**
Group size	**300 shares**

(b) Reference group for the bid-share questions

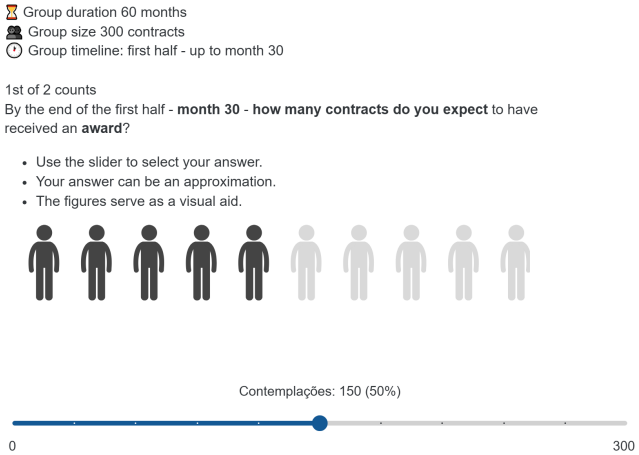
Figure G.1: Group-size question and reference group.



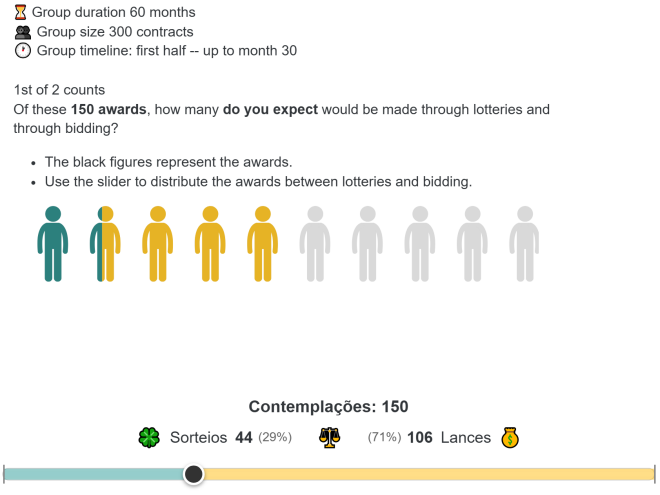
(a) Qualitative



(b) Probabilistic



(c) Quantitative, step 1: number of awards



(d) Quantitative, step 2: lotteries versus bids

Figure G.2: Bid-share questions. In the quantitative question, respondents first choose how many contracts in the reference group are awarded by the midpoint of the contract, then use a slider to split those awards between lotteries and bids.

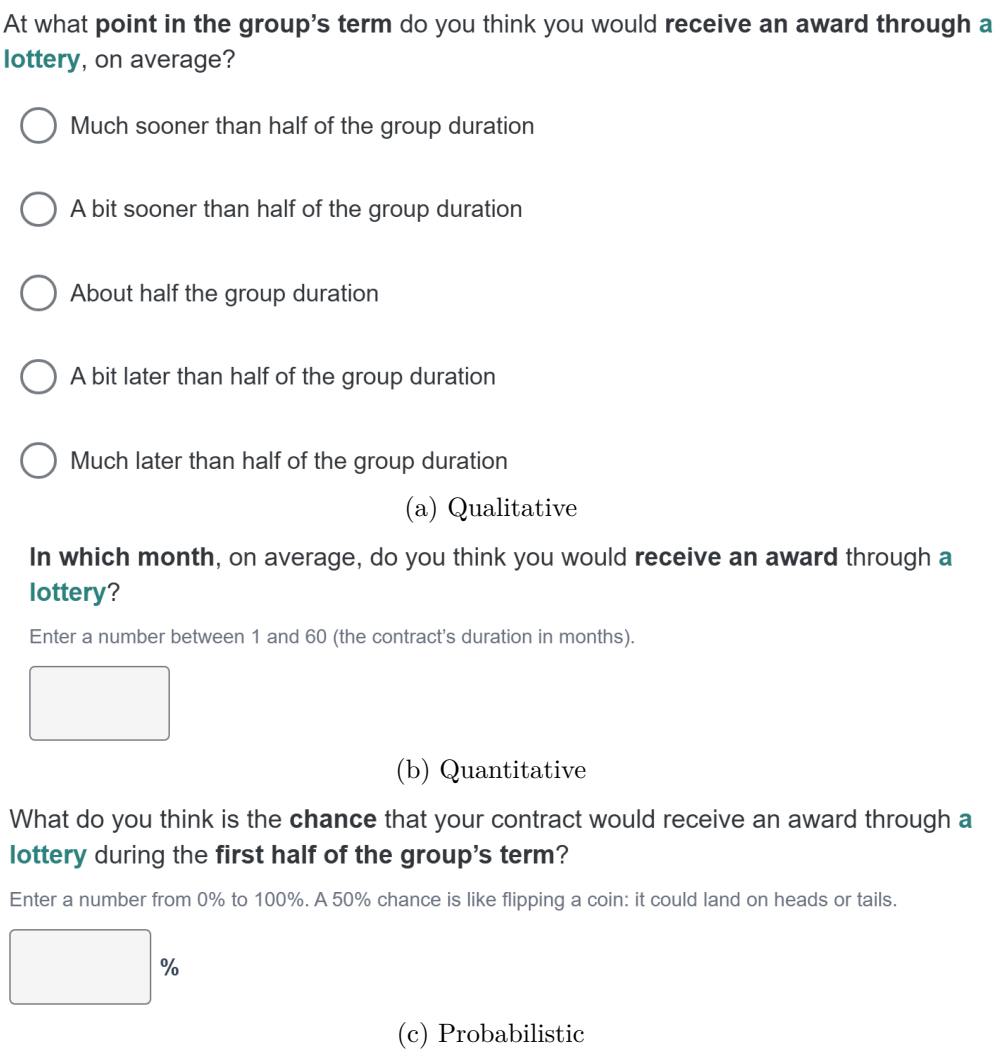


Figure G.3: Lottery-wait questions. Respondents answer assuming they never bid and wait for a lottery award.

H Proofs for Section 4

Section 4 has three periods and one consumer. Its propositions use a few things the text does not spell out, which we collect here.

The decision is the $t = 1$ self's: she joins by paying the first installment. There is no $t = 0$ self. A member not yet awarded is drawn with probability $p(\ell)$ at $t = 1$ and again at $t = 2$, and is awarded for sure at $t = 3$. We take the two draws to have the same odds; this is what lies behind the single anchor $\frac{1}{3}$ in the text. We assume the odds rise and are concave in ℓ. Installments are due at $t = 1, 2, 3$ whether or not she has been awarded. A member who cancels at $t = 2$ forfeits what she paid. She has no credit. She also holds no savings: with $r^S = 0$ and $\beta \leq 1$, saving a unit out of flat consumption changes a self's utility by $-v'(c) + \beta v'(c) \leq 0$, inside or outside the contract.

Write $\omega(\tau) \equiv v(y) - v(y - q(\tau))$ for the utility cost of one installment. We assume $\Delta > 2\omega$, so that the good is worth its last two installments to the selves who pay them, and $\omega(0) > p(1)\xi$, so that one lottery chance does not pay for an installment on its own. The upper bound $\bar{\beta}$ of Proposition 1 is the smallest β at which she buys the good without any contract; the bands are for consumers below it.

Lemma H1 (Thresholds)**.** *Let $\beta < \bar{\beta}$ and $p = p(\ell)$. The consumer joins (τ, ℓ) and completes it iff $\beta \geq \hat{\beta}(\tau, \ell) \equiv \max\{\hat{\beta}_1, \hat{\beta}_2\}$, where*

$$\hat{\beta}_1 = \frac{\omega - p\,\xi}{\Delta - 2\omega + \xi\, p\,(2 - p)}, \qquad \hat{\beta}_2 = \frac{\omega - p\,\xi}{\Delta - \omega}. \tag{H.1}$$

Both are positive, rise in τ and fall in ℓ.

Proof. Start with the $t = 2$ self. If she has not been awarded and cancels, she never buys the good, since $2y < 1$, and she consumes her income: this is worth $(1 + \beta)v(y)$. If she pays, she gets $v(y - q) + p\xi + \beta[v(y - q) + \Delta]$. So she pays iff

$$p\,\xi \;\geq\; \omega - \beta\,(\Delta - \omega), \tag{H.2}$$

which is $\beta \geq \hat{\beta}_2$.

Now the $t = 1$ self. Not joining is worth $(1 + 2\beta)v(y)$. If she joins and her $t = 2$ self completes, she consumes $y - q$ in every period, holds the good at $t = 3$, holds it at $t = 1$ with probability p and by $t = 2$ with probability $p(2 - p)$. This is worth $v(y - q) + p\xi + \beta[2v(y - q) + \Delta + p(2 - p)\xi]$. So she joins iff

$$p\,\xi\,[1 + \beta(2 - p)] \;\geq\; \omega - \beta\,(\Delta - 2\omega), \tag{H.3}$$

which is $\beta \geq \hat{\beta}_1$. There is one more case: she could join knowing that her $t = 2$ self will cancel,

hoping for an award at $t = 1$. Relative to not joining this is worth $-\omega + p[\xi + \beta(\xi + \Delta - 2\omega)]$. It is never positive when (H.2) fails. Failure of (H.2) needs $\omega > p\xi$ and gives $\beta < (\omega - p\xi)/(\Delta - \omega)$; positivity gives $\beta \geq (\omega - p\xi)/[p(\xi + \Delta - 2\omega)]$; together, $p(\xi + \Delta - 2\omega) > \Delta - \omega$. With $p \leq 1$ this needs $\xi > \omega$, and with $p < \omega/\xi$ it needs $\omega > \xi$. So she joins iff both (H.2) and (H.3) hold.

The numerators in (H.1) are at least $\omega(0) - p(1)\xi > 0$, and the denominators are positive because $\Delta > 2\omega$. Both thresholds rise with ω, hence with τ, and fall with p, hence with ℓ. ■

Proof of Proposition 1. Parts (i) and (ii) are Lemma H1 with $\underline{\beta}^R = \hat{\beta}(0, 1)$, $\underline{\beta}^C(\tau, \ell) = \hat{\beta}(\tau, \ell)$ and the upper bound $\bar{\beta}$. The containment says $\hat{\beta}(\tau, \ell) \geq \hat{\beta}(0, 1)$, which holds because $\tau \geq 0$ and $p(\ell) \leq p(1)$.

For (iii), $\underline{\beta}^R$ does not depend on ℓ, so we need $\hat{\beta}(\tau, \ell)$ to be decreasing and convex in ℓ. It is the larger of $\hat{\beta}_1$ and $\hat{\beta}_2$, and the maximum of two convex functions is convex, so we take them in turn. $\hat{\beta}_2$ is linear in p. For $\hat{\beta}_1$, write $m(p) \equiv p(2 - p)$, so that

$$\hat{\beta}_1 = \frac{\omega - p\,\xi}{\Delta - 2\omega + \xi\, m(p)}.$$

The numerator falls linearly in p. The denominator rises and is concave in p, so its reciprocal falls and is convex. The product of two positive, decreasing, convex functions is convex. Finally $p(\ell)$ rises and is concave, and a decreasing convex function of such a p is decreasing and convex in ℓ. ■

Proof of Proposition 2. With misperceived odds, $\hat{p}(\ell) = p(1) + \lambda[p(\ell) - p(1)]$ replaces $p(\ell)$ in (H.2) and (H.3). Nothing else changes, because the odds enter only through the flow terms and both selves hold the same beliefs. For $\ell < 1$, $\hat{p}$ falls as λ rises, and the thresholds fall in the odds. So the lower bound $\hat{\beta}(\tau, \ell; \lambda)$ rises with λ, from $\hat{\beta}(\tau, 1)$ at $\lambda = 0$ to $\hat{\beta}(\tau, \ell)$ at $\lambda = 1$.

(i) At $\lambda = 0$ she perceives (τ, ℓ) as $(\tau, 1)$: the fee is the same and $\hat{p}(\ell) = p(1)$ at both draws. Her outside option, saving alone or not buying, does not involve the odds, and the perceived odds enter the value of the contract continuously, including through the $t = 2$ self's decision to pay. So if she strictly prefers $(\tau, 1)$ to her outside option, she takes up (τ, ℓ) for all λ up to some $\bar{\lambda}(\tau, \ell) > 0$, at any β, including $\beta = 1$. For a consumer below $\bar{\beta}$ the condition reads $\beta > \hat{\beta}(\tau, 1) = \hat{\beta}(\tau, \ell; 0)$, and a higher β stays above the threshold for longer, so $\bar{\lambda}$ rises with β.

(ii) The lower bound rises with λ and the upper bound $\bar{\beta}$ does not move. The band widens as λ falls.

(iii) She pays fee τ iff $\beta \geq \hat{\beta}(\tau, \ell; \lambda)$, and the threshold rises with τ and with λ. As λ rises, the set of fees she accepts shrinks, so the highest one, $\bar{\tau}(\ell; \lambda)$, falls. ■