# How Much Does Psychology Matter? A Field Experiment in the Consumer Credit Market

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**Abstract**

Numerous laboratory experiments document behavior inconsistent with economic models of rational maximization. How much do these inconsistencies matter in natural settings, when consumers make large, real decisions and have the opportunity to learn from experiences? We report on a field experiment designed to address this question. Incumbent clients of a lender in South Africa were sent letters offering them large, short-term loans at randomly chosen interest rates. Psychological "cues" on the letter, which did not affect offer terms or economic content, were also independently randomized. Consistent with standard economics, the interest rate significantly affected loan take-up. Inconsistent with standard economics, though, the psychological cues also significantly affected take-up. The two independent randomizations allow us to quantify the relative importance of psychological cues and prices. Our core finding is the sheer magnitude of the psychological effects. Any *one* manipulation has the same effect as a one to two percentage *point* change in the monthly interest rate. Interestingly, psychological factors appear more important for less advantageous offers. Moreover, psychological cues do not appear to draw in marginally worse clients, nor does the magnitude of the psychological effects vary systematically with income or education. In short, even in a market setting with large stakes and experienced customers, subtle psychological cues appear to be extremely powerful drivers of behavior.

# 1 Introduction

A growing body of laboratory evidence suggests that people do not make rational decisions. Many economists however remain skeptical about the relevance of this laboratory evidence for the natural settings such as markets.[1] They question the external validity of these experiments, doubting whether hypothetical choices in "artificial" laboratory settings generalize to real world situations. Indeed, people in natural settings will have heightened motivation and chances for learning that could push towards more rational choices. Moreover, the laboratory evidence offers little guidance as to the empirical magnitude of these psychological effects. In natural settings, these effects may be small in size compared to that of economic factors such as price. Since little testing of deviations from the rational choice model has taken place outside of the laboratory, it has remained difficult to directly address these criticisms.[2]

This paper reports on the results of a large-scale field experiment involving large stakes and real decisions. A lender in South Africa mailed out more than 50,000 letters to incumbent clients offering them short-term loans at a specific, randomly chosen interest rate. Several psychological "features" of the offer letter were also independently randomized (including how the offer was described in the letter, whether the letter displayed the photo of a male of female, whether a promotional giveaway was incorporated in the offer and how long of a deadline customers had to take-up on the offer). This field experiment has two advantages over prior work. First, it takes place in an ideal market context for a conservative test of the economic relevance of psychological factors. Consumers in this market are quite motivated because of the rather large stakes–the median loan is about a third of the borrowers' gross monthly income. They are also experienced with the decision at hand since they have borrowed extensively from this lender in the past—the median client has had roughly 4 loans with this lender. Second, the independent randomization of both the interest rate and the psychological cues allows for a precise quantification of the monetary importance of psychological factors. Indeed, we can scale the impact of a given psychological cue on take-up by the impact of the interest rate on take-up and hence "price" the importance of that psychological cue. Specifically, suppose that a cue increases take up by $x$ and a one point decrease in interest rate raises take up by $y$. Then the ratio $\frac{x}{y}$ measures the market importance of this psychological cue: how large a

[1]See Camerer, Loewenstein and Rabin (2003) for an overview of the experimental evidence.

[2]A few existing field experiments include List (2003), Ashraf, Karlan and Yin (2004), Field (2004), Miravete (2003), and Fehr and Lorenz (2002).

change in interest rate is needed to produce the same size effect.[3]

The psychological cues incorporated in the letter were chosen based on ease of implementation. For example, the Lender varied the description of the offer, either showing the monthly payment for one typical loan or for a variety of loan terms and sizes.[4] This particular manipulation aims at contrasting the economic perspective that more potential choices can only help against the psychological perspective that greater choice can overload decision-makers. Other randomizations include whether and how the offered interest rate is compared to a "market" benchmark, race and gender cues through the inclusion of a photo in the corner of the letter, the expiration date of the offer, whether the offer is combined with a promotional giveaway and whether the offer letter mentions suggested uses for the loan. Also, the lender also performed several phone-calls to either remind consumers of the offer or to subconsciously prime consumers through suggestion effects. Using administrative data from the lender, we can measure how actual take-up of the loan responds to the interest rate as well as to the psychological factors.

As economic models predict, the interest rate strongly affects take-up. There appears to be a robust, negatively sloping, demand curve in this market. Yet, some of the psychological cues also strongly affect demand in ways that are difficult to reconcile with the rational choice model. For example, take-up rises with the simplicity of the presentation of the offer: consumers are more likely to take-up if repayment under only one example loan term and size is described in the offer letter than if many examples are provided. Or take-up rises among male customers with the inclusion of the photo of a woman in a corner of the offer letter. While not all of the psychological manipulations have a significant effect on take-up, many do. Moreover, their impact is quite large. Any *one* "positive" cue increases take-up by almost as much as a one to two percentage point drop in the *monthly* interest rate.[5]

[3]In this way, the paper also differs from most field experiments in marketing, which rarely benchmark magnitude of effects against price. For example, Ganzach and Karashi (1995) examines the impact of gain-loss framing on credit card utilization. Wansink, Kent and Hoch (1998) examine the impact of subtle anchors on purchase quantity. Neither has any price variation. Dhar and Hoch (1996) who examine in-store coupons versus direct discounts come, closest since they have variation in price, though it is non-random and they do not interpret their results in this way. A more explicit measure of "value" of manipluations could be had in a few specific contexts. For example, supermarkets routinely charge producers for the benefit of having their products placed at eye level on the shelf.

[4]In all cases, it was specified that this was only a sample term and loan size and that all other terms and loans were available.

[5]These large results also raise the interesting possibility that the firm was not profit-maximizing in its choice of cues prior to experimentation. This could be due to several reasons. First, the introduction of techniques could respresent a true innovation that the firm was not aware of before. Second, the costs of experimentation might be large. Third, and related, experimentation may be a public good. In our context, we cannot conclusively separate out these three

We also report on four additional findings that speak to how our main results may play themselves out in general equilibrium. First, positive psychological cues appear relatively more effective at inducing take-up when the interest rate is high. In other words, the psychological factors matter more for the less attractive offers.[6] Second, there is no discernible difference in the take-up impact of the psychological cues across income or education groups. Third, the increase in take-up due to psychological factors do not draw in marginally worse clients: default rates are not statistically higher for the marginal borrowers brought in via the psychological manipulations. This contrasts with the adverse selection observed on price in this market.[7]

Finally, the increased take-up of loans induced by the psychological cues appears to be net new borrowing rather than market substitution.

As a whole, our results suggest an important role for psychology in market contexts. At the individual level, psychological factors appear to be as important as price in determining demand. Our results also hint at the possibility that these psychological factors may affect the aggregate equilibrium. By competing on psychological factors (or marketing), firms seem able to raise aggregate demand without suffering from adverse selection, all the while dulling the incentives for price competition.

## 2 Background: The South African Credit Market

### 2.1 The Market

The consumer credit market in South Africa is distinct from most other developing countries in that there is a large, for-profit industry segment extending "cash loans" to individuals with verifiable employment. These lenders offer small, high-interest, short-term credit with fixed repayment schedules to a "working poor" population estimated to comprise anywhere from 2.5 million to 6.6 million people. Cash lenders arose to substitute for traditional "informal sector" moneylenders following deregulation of the usury ceiling in 1992, and they are regulated by the Micro Finance

explanations. Irrespective of the interpretation of profits, the results are quite interesting for undrestanding individual behavior.

[6]Though since our range of interest rate variation primarily cover "good" offers compared to the market benchmark, we do not know whether positive cues could also be used to induce take-up of very bad offers.

[7]Karlan and Zinman (2005) more thoroughly examine the impact of the interest rate in this experiment on adverse selection and moral hazard. See Ausubel (2003) for an experimental study of adverse selection with United States credit card data.

Regulatory Council (MFRC). The MFRC estimates that 65% of consumer credit in South Africa is delivered by such lenders or by retail stores. Only 3% of credit to individuals is provided by NGOs, the "typical" governance structure for microfinance in other developing countries (Porteous, 2003), with the remaining 31% of the South African market delivered by banks or their subsidiaries.

The working poor population lacks the credit history and/or collateralizable wealth needed to borrow from traditional institutional sources such as commercial banks. Loan sizes tend to be small relative to the fixed costs of underwriting and monitoring them, but substantial relative to borrower income; e.g., our cooperating Lender's median loan size of R1000 ($150) is 33% of its median borrower's gross monthly income. Not surprisingly, credit card and mortgage markets are extremely thin in South Africa (and other developing countries) compared to the U.S.

Cash loans are very short-term and expensive relative to credit card or mortgage rates in industrialized nations, although their terms compare favorably to informal sector substitutes in South Africa and elsewhere. Cash lenders focusing on the observably high-risk market segment typically make one-month term loans at 30% interest per month. Lenders targeting observably lower risk segments may charge as little as 3% per month. Note there is essentially no difference between these nominal rates and corresponding real rates, since inflation continues to be quite small relative to these rates (e.g., 10.2% from March 2002- March 2003 and 10.4% from March 2003-March 2004). The Lender rejects 50% of new loan applicants.[8]

## 2.2 The Lender

The Lender has been in business for over 20 years and is one of the largest micro-lenders in South Africa, with over 150 branches throughout the country. Our experiment took place in a mix of 86 urban and rural branches throughout the provinces of Kwazulu-Natal, Eastern Cape, Western Cape, and Gauteng. All loan underwriting and transactions are conducted face-to-face in the branch network, with the risk assessment technology combining centralized credit scoring with decentralized loan officer discretion. The Lender's product offerings are somewhat differentiated from competitors. Unlike many cash lenders, it does not pursue collection or collateralization strategies such as direct debit from paychecks, or physically keeping bank books and ATM cards

[8]It is unclear whether these rates correspond to abnormal profits or not, given the difficulty of screening for new clients, and the fixed costs of delivering the loans. It is important to keep this in mind since our sample is a highly pre-screened group of borrowers, having borrowed extensively from the Lender in the past.

of clients. The Lender is also unusually transparent in its pricing, with no surcharges, application fees, insurance premiums, etc., added to the cost of the loan. The Lender also has an unusual "medium-term" product niche, with a large concentration of 4-month loans (85%). Most other cash lenders focus on 1-month or 18-month loans.[9] The Lender's standard 4-month rates, absent this experiment, range from 7.75% to 11.75% per month, depending on credit history and prior transaction frequency with the Lender. The Lender places no restriction on the use of proceeds from the loan and there is limited evidence as to what the funds borrowed are typically used for.

## 3 Experimental Design

The Lender sent direct mail solicitations to 53,194 former clients offering them a new loan at randomly different interest rates. The solicitations were sent in two groups, one on September 29-30 and the other on October 29-31.[10] The rates ranged from 3.25% to 11.75% per month. Each letter also contained several marketing manipulations, each randomized independently of the interest rate randomization. Credit approval (i.e., the Lender's decision on whether to offer a loan after updating the client's information) and maximum loan size were orthogonal to the experimental interest rates and marketing manipulations. Since all clients had a prior record with the Lender, 87% of the applications were accepted, with rejection occuring mostly because of a change in work status or other indebtedness.[11]

Receiving mail from the Lender is common for clients. The Lender sends monthly statements to clients via mail, as well as reminder letters to former clients who have not borrowed recently. In the past, these letters have never offered any special deals, interest rates, or marketing tests. The Lender does not update its mailing address database; so, former clients who have moved remain in the database.

[9]The Lender does also have 1, 6, 12, and 18-month products, with the longer terms offered at lower rates and restricted to the most observably creditworthy customers.

[10]A small pilot to test feasibility was also conducted in July which included a small subset of these manipulations.

[11]In the results below, we use loan take-up as the outcome variable. We find very similar results if we use loan application as an alternative left-hand side variable.

### 3.1 The Sample

The sample frame consisted of all individuals from 86 branches who have borrowed in the past twenty four months, but who did not have a loan outstanding in the thirty days prior to the mailer.[12] The Lender categorized the sample into three different risk categories, based on the frequency and quality of their prior borrowing history. In the normal course of operations, this risk category determines a borrower's interest rate and loan term options. All clients are eligible for 4-month loans, but only the "medium" and "low" risk clients are eligible for 6 and 12 month loans. The randomizations were conditional on these risk categories. Because the interest rates used in the experiment are equal to or less than the normal rate, the range of rates for the lower risk clients is smaller than the range for the higher risk clients.

In the analysis below, we will breakdown the full sample into two subgroups based on the number of past loans a given individual received from the lender in the past and on how recently this last loan was received. Specifically, we isolate a subgroup of customers that have borrowed at least twice from the Lender in the past and at least once in the last eight months from those that have not. Such a breakdown is relevant for our analysis in at least two regards. First, because the Lender does not update its mailing database, we expect the addresses where the offer letters were sent to be more outdated for those individuals that had not borrowed from the Lender in a quite a while.[13] Second, it is reasonable to suspect that lower frequency borrowers and those that did not take-up a loan from the Lender in a while may not systematically read mail they receive from the Lender. Based on this last point, we will refer to the individuals that have borrowed more often and more recently from the Lender as the "higher attention" group; the remaining individuals will be referred to as the "lower attention" group.[14]

Table 1 reports summary characteristics for the full sample as well as for the sub-samples of individuals that did and did not take-up on the loan offer.

[12] This was done because many clients take a new loan out immediately after repaying the prior. The Lender did not want to crowd-out this business they would receive regardless of the offer.

[13] The postal system returns undeliverable mail, and the return rate was 1.51% for the low risk clients, 2.05% for the medium risk and 2.68% the high risk clients.

[14] We have attempted other cuts of the data based on frequency and recency of borrowing, all of which qualitatively produce similar results. We chose this cut because it most closely resembles the Lender's own internal "risk categories" which summarize the riskiness of the borrower. We chose a cut on recency and frequency so that the mean differences matched the difference in recency and frequency between the high and other risk groups.

### 3.2 The Randomizations

Two independent sets of randomizations were conducted. The first set involved the interest rate. Each client was randomly assigned an offer interest rate, and a contract interest rate which was equal to or lower than the offer interest rate and revealed to the client after they agreed to borrow at the offered interest rate. The contract interest rate is important for a related paper on identifying adverse selection and moral hazard (Karlan and Zinman, 2005). For this analysis, we will focus strictly on the offer interest rate, since this is the interest rate that clients responded to when they decided to borrow. As mentioned before, interest rates varied from 3.25% per month to 11.75 % per month.[15] Following the randomization, we verified that the assigned rates were uncorrelated with other known information, such as credit report score (Karlan and Zinman 2005).

The second set of randomizations involved the marketing manipulations. We conducted five broad categories of psychological treatments: the description of the offer, the comparison of the offer to competitor rates, subtle cues, time management, and suggestion effects.[16] Sample offer letters incorporating different subsets of these manipulations are shown in the Appendix. Appendix Table 2 reports on the frequency of each marketing manipulation.

#### 3.2.1 Describing the Offer

We varied the description of the offer provided in the letter. For some borrowers, we displayed on the letter only one example of repayment for a given loan term and size while for others we displayed on the letter examples of repayment under multiple possible terms and/or sizes. In both cases, we *explicitly* stated on the offer letter that other loan sizes and terms were available. Under the economic model, the simple presentation of multiple examples should have no effect on take-up, or may possibly raise take-up if these multiple examples appear to provide more "choices" to the individual or reduce on the transaction cost associated with computing these repayment rates.

In contrast, psychological models suggest that the presentation of more choices may be a bad. More choices may produce decisional conflict and reduce take-up (cite?). Psychological studies

[15] Note these are "add-on" rates, where interest is charged upfront over the original principal balance, rather than over the declining balance. Such "add-on" rates are conventional in the cash loan market.

[16] We exclude for the discussion altogether two manipulations that were performed at the request of the Lender. One was to include a "We Speak Zulu" in the letter and the other was to describe the rate as "special." Neither produced any effect. We exclude these manipulations from the discussion below as they are of limited academic interest.

suggest that people often are tempted to defer decision when a compelling reason for choice is not readily available and the decision feels hard, compared to when there is a compelling rationale and the decision is easy (Shafir, Simonson, and Tversky, 1993). This can have non-obvious implications. For example, when presented with a larger choice set, people may choose none of the items, simply because choosing between them is harder. Such a tendency advantages the status-quo, which also benefits from loss aversion, and from the fact that departures from the status-quo typically require more justification than its retention.

In one study, for example, physicians had to decide what medication to prescribe to a patient with osteoarthritis. They were more likely to decline prescribing a new medication when they had to choose between two new medications than when only one new medication was available (Redelmeier and Shafir, 1995). Apparently, the difficulty in deciding between the two medications led some physicians to prescribing either. A similar pattern was documented with shoppers in an upscale grocery store, where tasting booths offered the opportunity to taste any of 6 jams in one condition, or any of 24 jams in the second. Of those who stopped to taste, 30% proceeded to purchase a jam in the 6-jams condition, whereas only 3% purchased a jam in the 24-jam condition (Iyengar and Lepper, 2000). Similar experiments have been conducted for a variety of other choices.

Specifically, we varied the form of a "table" included in the letter that described the offer.[17] We used three different table formats:

1. Big table with 4 different loan amounts, one loan term, 4 monthly repayments and one interest rate. Every client was eligible for this table and 38% of the entire sample received it.

2. Big table with 4 different loan amounts, 3 loan terms, 4 monthly repayments and 3 interest rates based on the term of the loan (all clients had a fixed yield curve). Only "low" and "medium" risk clients were eligible for this table (since only they can receive loans longer than 4 months) and 17% received it.

3. Small table with one loan size, one loan term, one monthly repayment and one interest rate. Every client was eligible for this table and 44% received it.

It is important to stress again that all offer letters (i.e. independently of the description frame

[17]We also varied for some of the letters whether the interest rate was explicitly shown. Twenty percent of the clients (3% in condition 2 and 17% in condition 3 above) were simply shown their installment payment and not the interest rate explicitly.

applied) explicitly mentioned that "Loans were available in other sizes and terms." In other words, we only manipulated here the description of the offer, not its intrinsic content. In practice, we will contrast take-up under a presentation where a single sample loan is displayed in a small "table" (number 3 above), versus presentations where multiple alternative sample loans are displayed (numbers 1 and 2).

#### 3.2.2 Comparison of Offered Interest Rate to Competitor Rates

In a subset of the offer letters, we also included a comparison of the offered interest rate to an outside market rate. In a standard economic model, such comparisons should have little effect since the borrower is supposed to be informed about market conditions and, maybe most importantly, since the Lender is not a credible source for the outside market rate. In addition, exactly how the comparison to the outside market rate is framed (e.g. "save if you borrow from us" or "lose if you borrow elsewhere") should not matter for take-up.

Psychologically, however, one could imagine people taking such comparisons at face value, for example because the addition of a dominated alternative has been shown to increase the market share of the dominating option (Huber, Payne and Puto, 1982). Moreover, the exact nature of the comparison could matter. Losses and gains often generate discrepant attitudes towards risk, with greater risk aversion in the context of gains compared with frequent risk-seeking in choice between losses, which can lead to "framing effects" (Kahneman and Tversky, 1979).

In practice, we attempted three categories of randomization under the comparison umbrella. First, some letters were assigned randomly to the "comparison" group, and received a simple statement comparing the offer interest rate to a generic (unstated) competitor. Others rceived no comparison. In formulating these comparisons, we use a 20% interest rate per month as the competitor's offer. Second, the comparison was either phrased in a positive or negative frame. Third, the units were randomized, such that letters said how much the client would save (lose) in either Rand per month, Rand per loan, percentage point differential per month or total percentage point differential per loan.

Some examples follow. The positive/negative frame: "If you borrow elsewhere (from us), you will pay R100 Rand more (less) each month on a four month loan." The monthly saving/total saving frame: "If you borrow from us, you will pay R100 (R400) Rand less each month (in total) on a four month loan." The percentage points/ total percent frame: "If you borrow from us, your interest rate will be 4.00% lower!," versus "If you borrow from us, you will pay 32% less each month on a four month loan."

#### 3.2.3 Demographic Cues

We also experimented with adding a photo in the corner of a random subset of the offer letters. In the standard economic model, such photos should have no effect on take-up. Psychologically, however, such subtle cues could have a large effect. A large social psychology literature suggests that persuasive communication can be influenced by source attractiveness and by similarity. Research has shown that we automatically assign to attractive individuals favorable traits such as talent, intelligence, and honesty. Closely related to the notion of attractiveness is that of sexual arousal and its possible impact on behavior. For example, Frederick (2003) finds that seeing a female picture makes men more impatient and cites sexual arousal as one possible source. Persuasive communication also tends to have more impact when it comes from people similar to the self. One study, for example, examined the sales records of insurance companies and found that customers were more likely to buy insurance when the salesperson was like them in age, religion, politics, etc. (Evans, 1963). In fact, a study that pitted source similarity against expertise found that similarity is, in some cases, more important than credibility (see, e.g., Lord, 1997; Cialdini, 2001, and references therein).

These suggested two manipulations with the photo: race and gender. For race, letters with photos were randomly assigned to "match" or "mismatch."[18] If the client was assigned randomly to "match," then the race of the client matched the race of the model on the photograph. For those assigned to mismatch, we randomized which mismatched race they received. In order to determine a client's race, we used the race most commonly associated with his/her last name (as determined by employees of the Lender). The gender of the photo was then randomized unconditionally at the individual level. Hence, among the clients that received an offer letter with a photo, half received a photo of the same gender, and half received a photo of the opposite gender.

Ultimately, clients received one of nine variations: no photo (20%), black male (24.5%), black female (24.5%), coloured male (3.5%), coloured female (3.5%), Indian male (6.0%) or Indian female

[18]The photos used were either photos that the marketing firm that helped design the letters already had in stock or photos that were commissioned by them for this project.

(6.0%).[19]

Additionally, the race and gender of the person on the photo (if a photo was included) were also matched to the race and gender of the employee name that appeared at the bottom of every letter. Specifically, this name appeared under a section entitled "How to Apply" that told clients to "Bring your ID book and latest pay slip to your usual branch by XX, 2003 and ask for Mr. (Mrs.) XXX," as well as in the signature line. The name used was that of an actual employee from the client's branch.[20]

### 3.3 Promotional Giveaway

Some companies use promotional giveaways as part of their marketing, including the Lender. What is the effect of these giveaways on demand? In principle, under the economic model, these should have a small positive or no effect on demand, depending on the magnitude of the prize. In contrast, the psychological evidence suggests that these giveaways could backfire and in fact end up reducing demand. Indeed, studies have shown that endowing an option with a feature that is intended to be positive but in fact has no value for the decision maker, can reduce the tendency to choose that option, even when it is understood that the added feature comes at no extra cost (Simonson, Carmon, and O'Curry, 1994). For example, an offer to purchase a Collector's Plate – that most did not want – when buying a particular brand of cake mix, was shown to lower the tendency to buy that particular brand relative to a second, comparable cake mix brand. Choosing brands that offer worthless bonuses was deemed more difficult to justify and more susceptible to criticism, with a majority of those who failed to select the bonus option explicitly mentioning not needing the added feature. It should be noted that such sale promotions are widely used and there is no evidence that they lead to inferences about the quality of the promoted product (see Shafir, Simonson, and Tversky, 1993, for further discussion.)

To contrast these two views, we randomly included in 25% of the letters the following small announcement: "WIN 10 CELLPHONES UP FOR GRABS EACH MONTH!" Most competitors,

[19] Coloured are modern-day descendants of slaves from India, Indonesia, Madagascar and Mozambique imported into South Africa by Dutch settlers. Over time they have mixed with Dutch settlers, black South African and the indigenous Khoi and Bushmen. They are found predominately in the Western Cape and this is the only area where photos of a coloured model were included.

[20] In order to apply for a loan, it was not necessary for the client to actually ask for and speak to this person. In cases where no employee in that branch was of the assigned race, then a name from the regional office was used.

as well as this Lender, offer such promotions, be it monthly or at some other regular interval. Like our promotion, competitors' promotions do not detail the odds of winning or the value of the prize (in this case, cell-phones).

### 3.4 Time Management

In the standard economic model, people have no trouble following through on the tasks they set for themselves. If they decide when reading the loan offer that they want to take-up the offer, they will follow through on this decision. Psychological evidence suggests however that several factors such as poor planning, impulsivity and forgetting may intercede with this process. Intertemporal choices have been shown to exhibit a number of systematic anomalies including discount rates that decline sharply with the length of time to be waited and with the size of the reward (Loewenstein and Thaler, 1992; Loewenstein, Read, and Baumeister, 2003).[21] In addition, people have been shown to exhibit systematic over-optimism in their estimates of the time required for the completion of various tasks (Buehler, Griffin, and Ross, 1994; Griffin and Buehler, 1999). This bias, called the "planning fallacy," is exhibited for all manner of projects, including the preparation of lab reports, apartment cleaning, or finishing tax returns, and is most pronounced when participants are motivated to complete the task quickly (Buehler et al., 1994). Both hyperbolic discounting and the planning fallacy can contribute to what amounts to as procrastination. Relatedly, people may simply forget that they intended to undertake a given action.[22]

These considerations suggest an interesting role for deadlines. Consider the impact of a short deadline. On the one hand, a short deadline may lead to people irrationally miss the deadline.[23] As in the planning fallacy, people may budget too many other activities and never get around to taking out the action. On the other hand, a shorter deadline may reduce procrastination (Ariely and Wertenbroch 2002). By providing a specific date by which the action must be taken, more people may eventually take out the action.

The implicit comparison here is to a no deadline case. Practical difficulties obviously prevented us from implementing this sort of comparison (short deadline versus no deadline). We were able,

[21] See Loewenstein and Prelec (1998), Laibson (2001) and Rabin and Odonaghue (2003) for theoretical models.
[22] See Dow 1993, Mullainathan 2002 and Wilson 2003 for theoretical models that incorporate forgetting.
[23] An option value argument or simple cost of time argument might also generate this effect in a rational model. Its magnitude should be bounded though by the opportunity cost of time.

however, to implement a roughly similar comparison: shorter deadline versus longer deadline. In practice, each offer was randomly allocated one of 3 deadlines: short, medium or long. The short deadline, 2 weeks, was only given to those clients whose address was not a PO Box, who lived in a city and who worked in this same city.[24] This was to avoid offering short deadlines to clients who do not check their mail regularly. In the first mailing, 3% of the clients were given the short deadline, while 19% received the short deadline in the second mailing.[25] The medium deadline had no restrictions in its application and was given to 87% of the people in first mailing and only 9% in the second mailing. The long deadline was different for the two mailings: 8 weeks for the first mailing (10%) and 6 weeks for the second mailing (72%).[26]

Half of the letters assigned the short deadline (3.5% of the entire sample) contained an additional note stating the client could extend his/her deadline by calling a given number. When he/she actually called the number, a customer service representative would tell him that he now had an additional two weeks to take-up the offer.

A second set of manipulations attempted to directly test for time mismanagement through the use of a reminder phone call. A small random subset of clients were selected ex ante to receive a reminder phone call a few days before the expiration of the offer. Only clients whose offer letter was due to expire on October 31, 2003 were eligible for a reminder phone call. This corresponded to those with the medium deadline in first wave of mailers. The reminder phone calls were made in a three day window starting on October 20, 2003. The call was simple: a customer service representative phoned the client to remind them of the letter offer. First, the representative asked whether they had read the letter. Then, the representative asked whether the client was "interested but just hasn't found the time to come in and apply?"

The mechanics of implementation however corrupted the randomized nature of this design. The call center did not follow the random list we had originally created but instead called another subset of clients. When analyzing this part of data, it should therefore be kept in mind that these results

[24]It is very common in South Africa for people to have their mail send to a PO box, which they check only weekly or bi-monthly.

[25]In practice, the short deadline was never enforced. Instead, the client was actually able to qualify for the project rate until the medium deadline. Clients, however, were not informed of this.

[26]The long deadline was shorter for the second (October) mailing due to the holiday season; the deadline was Dec. 15 and any later deadline would have interfered with loan operations during that time of the year. Since borrowing in December may be particularly related to Christmas, we have also examined the deadline effects for the first mailing and found similar patterns.

are no longer coming from a true randomized design.

### 3.4.1 Suggestion Effects

A final set of manipulations was motivated by a pscyhological literature on the power of suggestion effects. For example, several studies have documented the effects of hypothetical questions on respondents' subsequent decisions. One line of investigation has shown that people's prediction of their future behavior, although inaccurate, can affect their actual behavior. In one experiment (Sherman, 1980), college students were asked to write counter-attitudinal essays. In a prior, seemingly unrelated survey, half the students were asked to predict whether they would comply with such a request, and many predicted they would not. The eventual rate of compliance among these subjects was much lower than among those who had not made an earlier prediction. Subjects had thus mis-predicted their own behavior (since many would have written the essay had they not been asked to predict). Nonetheless, the actual rate of compliance was very close to that predicted. In effect, people went on to behave in a manner consistent with their own mis-predictions. Related research has shown that such self-erasing errors may be used to increase voter turnout simply by asking people to predict whether they will vote (Greenwald, Carnot, Beach, and Young, 1987).

Even when faced with questions that are purely hypothetical, respondents are unable to prevent a substantial biasing effect on their behavior, particularly when the questions are highly relevant (Fitzsimons and Shiv, 2001). For example, Morwitz et al. (1993) found that merely asking consumers whether they intended to purchase an automobile or a personal computer in a survey increased their subsequent purchase rate of those goods. Follow-up interviews suggest that the effects of hypothetical questions on choice occur beyond awareness and, as a result, are quite difficult to counteract.

We attempted to test for suggestion effects in this credit market context. A subset of clients from the second mailing wave were chosen randomly (across all risk categories) to receive a phone call from a market research firm in the week prior to the mailing of the offer letters. The individual then asked two questions: "Would you mind telling us if you anticipate making large purchases in the next few months, things like home repairs, school fees appliances, ceremonies (weddings etc) or even paying off expensive debt?" and "Have you considered taking out a cash loan in the coming months?" As with the reminder phone call, however, the randomization was not properly implemented. Because of clerical error, the call center did not follow the random list of names we had created but instead called an arbitrary set of clients. Consequently, these results should be interpreted more carefully as they may not be causal.

Somewhat different in nature, a second suggestion manipulation was aimed at influencing the usage clients had in my mind when taking up on the loan offer. Every letter was randomly assigned one of five "loan usage" phrases. The phrases were equally divided amongst the letters (i.e. each phrase was given to 20% of the clients). The most general phrase simply stated: "You can use this cash for anything you want." The other four phrases also contained this text, but *in addition* listed a more specific goal (pay off a more expensive debt, repair your home, buy an appliance, or pay for school fees). These were the most common uses identified by the Lender in prior market research. We were specifically interested in whether a given phrase increased the proportion of clients who planned to use the loan for the stated purpose.

# 4 Basic Results

## 4.1 Overview

We first present each manipulation individually for simplicity. For each manipulation $Z$ we run a probit regression of the type:

$$Pr(T = 1) = \Phi(a + b * Z + c * r + d * X)$$

where $T$ is a dummy indicating loan take-up, $r$ is the offered rate and $X$ is a vector of indicator variables for risk category and experimental wave.[27] If the randomization is conditional on variables other than risk category and experimental wave, these will also be included in the $X$ vector. We also estimate this regression separately for the low and high attention categories. In each Table, we report marginal effects and standard errors. All reported estimated coefficients have been multiplied by 100. So, for example, a coefficient of 0.7 on a dummy variable indicates that turning that dummy variable on increases take-up by 0.7 percentage points. Also, for each psychological manipulation, we present the "interest rate equivalent" of that manipulation. It is under brackets

[27] As noted earlier, looking at take-up includes endogenous loan approval. Using application as the outcome variable produces the same results for all the manipulations. In unreported regressions we also estimated the impact of the psychological manipulations on loan size, conditional on take up, and found no effect. Thus the impact on the take-up decision summarizes the overall impact on demand.

under the relevant standard error. It is computed as the ratio of the estimated coefficient on the psychological manipulation to the estimated coefficient on the interest rate in that regression ($\frac{b}{c}$). As noted earlier, this quantifies how large of a change in the interest rate is needed to achieve the same effect on take-up as the psychological manipulation under study.

Two features of take-up are worth pointing out. First, there is much lower take-up among the low frequency borrowers. While about 1 out of 5 individuals in the high and medium frequency groups took up on the offer, the take-up rate is close to ten percentage points lower (i.e. more than 50 percent lower) in the low frequency group. As we discussed above, this likely corresponds to the combination of two factors. First, individuals in the low frequency group have had less interaction with the Lender and unlike the higher frequency borrowers may not always systematically read any mailing received from the Lender. Second, the lack of update of the mailing database by the Lender implies that a higher fraction of offer letters in that group were sent to outdated addresses and therefore never actually received. We are unable to partial out the relative importance of these two explanations. Second, across the full sample there is a negative and significant impact of the interest rate on take-up. The magnitude indicates roughly that a 1 percentage point drop in the offer interest rate increases take-up by about .26 percentage points (see column 1 of Table 3). Since the average take-up rate is about 8 percent, this implies that a one percentage point increase in take-up leads to a 3 percent rise in take-up.

## 4.2 The Description of the Offer

Table 3 reports the impact of displaying on the offer letter a table with many choices compared to a table with only one choice. How is the sensitivity of take-up affected by this description of the offer? In column 1, the estimated coefficient on the "small table" dummy is positive and statistically significant. Everything else equal, offer letters displaying a small table generate a .60 percentage point higher take-up than offer letters displaying a large table. Under brackets in column 1, we quantify this effect in interest rate terms. Given an estimated coefficient of $-.26$ on the interest rate for the full sample, our findings suggest that using a simple description for the offer has roughly the same effect on take-up as dropping the interest rate by 2.3 percentage points.

Separate analyses by borrowing frequency categories reveal some differences in point estimates across these groups, though standard errors do not allow us to reject the null of no differences. In both groups, though, we find a positive effect of the small table description on take-up. In interest rate terms, the estimated effect ranges between 3.5 (for the high frequency group) and 1.9 (for the low frequency group).

Our finding that more simplicity in the description of the offer increases take-up is, we find, very hard to rationalize. Under the view that consumers have to pay some costs to analyze the value of different potential loans and are trading off the value of their time with the expected value of the loan, one would, if anything, predict a higher take-up under the richer description of the offer, as (part of) this (possibly costly) computational work has already been done for the consumers in that case.

### 4.3 Comparison of Offer to Competitor Rates

Our findings on the comparison frame manipulations are reported in Table 4 where we regress take-up on two indicator variables: whether there was any comparison to the competitor's rate and whether this comparison was expressed as a gain or a loss. The addition of a comparison has no statistically significant effect on the take-up decision. Despite the fact that, for nearly all the observations, the comparison made the offer look attractive relative to the market benchmark, individuals appear to ignore it. When we breakdown the full sample into borrowing frequency categories, we find a similar lack of statistically significant effect across categories.

Given this non-response to the comparison, it is not so surprising that the framing of the comparison does not matter either. The impact of the gain comparison on take-up is insignificant and even varies in sign across the borrower frequency groups. One possible explanation is that insufficient attention was paid to the comparison because it was placed late in the letter. Alternatively, customers may have rationally discounted the comparison as being worthless as it conveyed no information.

### 4.4 Race and Gender Cues

Table 5 reports the effect of the race of the person on the photo added to some of the offer letters. As is clear from that table, we find no systematic effect of the race on the photo, and no systematic effect of a match between the race of the client and the race on the photo. This lack of a significant effect suggests that either racial cues are unimportant in this context or that our standard errors are too large to discern any behavioral response. This is surprising since the racially charged environment of South Africa might have led one to believe that racial cues would be particularly powerful.

Table 6 reports on the effect of the gender of the person on the photo added to some of the offer letters. In Panel A, we examine the effect for male and female clients of seeing either the photo of a person of the opposite gender (odd columns) or the photo of a woman (even columns) on the offer letter; we also include a dummy variable for whether a photo was included on the letter.

Both the "opposite gender" dummy and the "female photo" dummies produce quite large effects on take-up, ranging between 1.3 and 2.5 percentage points in interest rate terms. But the effect of the "opposite gender" dummy is insignificant (relative to the omitted "same gender" category), while the effect of the "female photo" dummy is statistically significant (relative to the "male photo" category) in most specifications. In fact, the "no photo" dummy is positive and significant in 2 of the 3 even column regressions, suggesting that perhaps the largest effect is a negative effect on take-up of including a male photo on the offer letter.

In Panel B, we focus on male customers only. For these male customers, replacing the photo of a male with a photo of female on the offer letter increases take-up by about as much as dropping the interest rate by 4.5 percentage points. For that group of customers, there is no statistically significant difference between the "no photo" treatment and the "male photo" treatment; however, the point estimates indicate a positive effect of "no photo" relative to '‘male photo." Overall, these results show a very powerful effect of seeing a female photo on the offer letter for male customers. Standard errors however do not allow us to isolate one specific mechanism for this effect. One possibility is that the inclusion of the photo of a female serves as a sexual trigger for male customers. Another possibility is that the main take-up effect is the negative influence of the inclusion of the photo of a man.

### 4.5 Promotional Giveaway

Table 7 describes how take-up varies based on whether or not the offer letter mentioned the promotional competition. In the pooled sample (column 1), we find a negative effect of the give-away on take-up though this effect is not statistically significant. But when we break down the sample into borrowing categories, we see that this effect is very large and statistically significant among the more frequent borrowers. For this group of customers, introducing this promotional feature, which represents a real cost for the Lender, is equivalent to raising the interest rate by nearly 4 percentage points. Hence, consistent with the literature findings described above, the addition of this intended-to-be-positive feature in fact reduces the likelihood of loan take-up. The nonnegative effect among the lower frequency borrowers (column 3) may indicate that this negative choice effect of the promotional lottery may be offset in this case by an attention-getting effect, which one would expect to be most important for that group of customers.

### 4.6 Time Management

Our study of time management issues in this context evolved around a randomization of deadlines assigned to each offer letter as well as the use of reminder phone calls a few days before the offer expiration date.

Our findings with regard to the deadline effects are reported in Table 8. Column 1 of Table 8 shows very clearly that shorter deadlines did not spur greater take-up. In contrast, the longer the deadline (from short to medium to long), the greater the take-up before the deadline. This first finding clearly rules out in this context the psychologically motivated hypothesis that shorter deadline may help people in managing their time better. This is in contrast with previous fieldwork on coupons that finds shorter deadlines increase usage (Dhar, LeClerc and Little forthcoming). Such a difference may reflect that people may have less need for such time management "help" because procrastination is less of an issue in this higher stake environment (we come back to this hypothesis in detail below). "An alternative explanation is that our results on the short deadline are driven by our operational constraints than any underlying consumer choice. The psychological literature has focused on very short and salient deadlines, but this was impractical in our case due to uncertainty about when clients would receive their mail. This problem would likely be important in many other contexts where mailers are being sent. It may be less important for coupons appearing in the newspaper on particular day (e.g. Thursday).

While finding a higher take-up rate on the shorter deadline would have been impossible to rationalize, we are left with findings that can a priori be reconciled with both a psychological model and a rational model. On the one hand, these results might reflect irrational procrastination that leads people to let short deadline expire without being able to take advantage of them (even though they would have wanted to "get to it."). Alternatively, it could be totally consistent with the rational model. If someone has only a week to take-up on an offer, they may decide that the opportunity cost of their time is too high. Individuals facing shorter deadline may rationally forego taking up the loan because other (higher benefit) activities arise in the interim. Also, more opportunities for usage of the loan are more likely to arise in a 2-months window than in a 2-weeks window.

Further investigation of these deadline effects lead us to lean somewhat in favor a procrastination interpretation for the findings in column 1 of Table 8. Our first argument relates to the extremely large magnitude of the deadline effects in contrast with reasonable measures of the opportunity cost of time. By benchmarking against the interest rate one can therefore loosely "calibrate" these findings. Comparing the short to the medium deadline effect in column 1 suggests that the a slightly longer deadline (2 weeks) leads to an increase in take-up that is equivalent to a ten percentage points drop in the monthly interest rate. A move from the medium to the long deadline generates a similar size effect. Under the rational interpretation of the deadline effect, this would imply a ludicrously high opportunity cost of time. While not impossible, this however warrants considering time mismanagement as a reasonable alternative interpretation.

Further evidence against a purely rational interpretation is provided in the remaining columns of Table 8. For column 4 , we construct a new take-up variable that is equal to 1 if the customer took up a loan from the Lender before the long deadline expiration date, *whatever deadline was assigned to that client's offer letter.* This reveals a very striking fact that is very hard to reconcile with the rational model. Clients that were assigned the short deadline (and to a lesser degree the medium deadline) are *more* likely to have taken up a loan from the Lender over that time period than clients that were assigned the long deadline. Not surprisingly, as columns 5 and 6 indicate, this simply indicates that clients that were assigned the short deadline (and to a lesser degree the medium deadline) are more likely to have taken-up a loan from the Lender *after the expiration* of their offer letter (whether as stated on by the printed deadline or as enforced by the Lender). These findings are very difficult to reconcile with an opportunity cost of time interpretation or arrival of new spending opportunities interpretation. Indeed, because the offer letter had significantly better deals than those typically proposed by the Lender, this indicates that the short deadline clients (and to a lesser degree the medium deadline clients) on average borrowed more and at a worse interest rate than the long deadline clients.[28]

Additional test of the procrastination hypothesis could be provided by an analysis of the effect of a reminder phone call on the likelihood of take-up. We report on the result of this test in Table 9. Before discussing these results, it is important to remember that they do not fall under the randomized design that has been followed throughout the paper so far. As we already indicated, the call center at the Lender decided to generate its own list of clients "to be reminded." In addition, only a very small fraction of clients of those clients that were called were eventually reached. All of this indicates that these results should be treated with much more caution as those described above, and certainly cannot carry the same purely causal interpretation.

With this very important caveat in mind, we report on three different empirical approaches. Under all approaches, we limit the sample to those individuals that received an October 31 deadline on their offer letter (the only group that was eligible for a reminder phone call) and had not taken up a loan prior to October 20, 2003. First, we simply compare take-up among the treated (those who actually received the call) to the untreated (those who did not get a call, either because they were not called or because they were unreachable). Our findings in Table 9 shows a very strong association between receiving a reminder phone call and the likelihood of take-up, corresponding to roughly a doubling of take-up based on receiving such a call. Of course, this strategy is poorly identified if, amongst the eligible, those who were unreachable are very different than those who were reachable. The effect is especially large (and only statistically significant) for the "high attention" group.

Of course, since these are not true causal estimates one should be concerned about omitted variables. We therefore also report on estimated effects on the treated after controlling for a battery of individual characteristics: credit score, income, predicted education, residence dummies, language spoken, number of dependents and indicators for whether they have a cell phone. The addition of these controls virtually leaves the estimated effect of receiving a reminder phone call on take-up unaffected. While this obviously leaves open the possibility that other unobservable customer characteristics are driving this correlation, it is quite striking that none of the abovementioned observable characteristics (which are likely correlated with the unobservable characteristics) alter the estimated effect.

[28]One percent of the sample did get a worse offer in the letter than they would have otherwise gotten. Excluding these does not change the result.

Finally, we also report on IV estimates where we instrument the "treated" dummy with a dummy for whether or not the call center attempted to reach a given individual. We present these IV results only for the full sample and both with and without controls for observable client characteristics listed above. These IV estimates remain positive and large (in price terms) but become statistically insignificant. However, these IV estimates remain significant when we focus on the "high attention group" (not reported in the table).

In summary, but keeping in mind the important caveat raised above, the combined findings in this section suggest that forgetting may play a role in explaining take-up.[29]

### 4.7 Suggestion Effects

As discussed above, we performed two different "suggestion" randomizations: a suggestion phone call prior to the mailing of the offer letter and the use of different "suggested loan usage" phrases in the offer letter. We report on each of these interventions.

First, a market research firm randomly called a subset of customers prior to their receipt of the letter. In the phone call, they were asked several market research questions such as whether they wanted to borrow in the future. As for the reminder phone call, there was a failure of randomization in that the call center decided to come up with its own list of people "to be suggested." In addition, only a small fraction of those that the call center attempted to call were eventually reached. We therefore have to raise the same strong interpretation caveat here as for the reminder phone call results. Given this caveat, we again present results under 3 different empirical approaches: treatment on the treated, treatment on the treated conditioning on a battery of client characteristics and IV effects (where we instrument the treated dummy with a dummy for whether the call center attempted to reach a given client).

The findings are reported in Table 10. As for the reminder phone call, we find extremely large positive effects of the suggestion phone call on take-up. In addition, the OLS estimates are again remarkably robust to adding the vector of controls for observable client characteristics. The magnitude of effects is also again larger among the "high attention" group, even though the relative pattern of statistical significance is the opposite. Finally, the IV estimates are comparable

[29]An alternative interpretation is that the reminder call merely encouraged people to actually read the letter. We find this implausible because the reminder call came nearly a month after the initial letter. It is almost certain that those who hadn't read the letter at that point no longer had it.

in magnitude and statistical significance.

We next assess whether the suggested loan usage phrases randomly assigned to the offer letters had any impact on the actual usage customers had for the loans they took up. For example, we ask whether clients that were assigned "school fees" as a suggested usage are more likely to plan to use the loan for school-related expenditures. In order to measure customer-specific loan usage, managers at the Lender's branches were required to ask loan applicants what they were going to use the loan for.[30] While branch managers were supposed to ask this question to all loan applicants, there was substantial non-compliance in practice, so that we have answers to this usage question for only about a third of all taken-up loans. About 19 percent of all surveyed clients reported planning to use the loan for school-related expenditures, 11 percent planned to use it to repay other "accounts" and 11 percent for home-related expenditures. The two next largest usage categories were "personal usage" (17 percent) "unknown usage" (10 percent).[31]

In Table 11 we examine whether there is a relation between suggested use and actual use. For the set of customers for which we have data, we pool customers into categories based on actual loan usage. Each column reports the proportion breakdown by treatment for each loan usage category. For example, in column (1), we focus on those 154 customers who used the house for house related expenditures. Since 21.02% of the customers were in the treatment that had suggested a house related use, we would expect $21.02 * 154$ of these customers to come from this treatment category under the null of no suggestion effect. Similarly since 18.63% received an educational suggestion, we would expect $0.1863 * 154$ of the customers in column 1 to come from this treatment category.

In bold in each cell, we report the percentage deviation from these expected numbers. For example, among those customers that receive the "house usage" suggestion, there were 3% more customers who used the loan for their house related experiences than would have been expected under the null of no suggestion effect. Similarly, of the 161 customers who used the loan to pay off debt, 3.6% percentage points more came from the "pay off debt" suggestion treatment than would have been expected under the null of no suggestion effect. As one can see from Table 11, there is a positive excess for each of the suggested specific usage categories. A binomial test of these four excesses produces a p-value of .0587, suggesting that these results may not be due to chance alone.

[30] This question was asked after the loan had been approved but prior to the physical handing of cash. Because of the time, answers to the question could not affect approval, though we cannot rule out that customers felt this way.

[31] Very few clients (less than 2 percent) reported planning to use the loan to buy appliances.

In other words, there is statistically significant evidence of a powerful effect of suggested usage on actual usage.

## 5 Pooling the Manipulations

We have reported so far on our findings for each of the marketing manipulations separately. To address a set of additional questions, it will be useful to try to pool of these manipulations into a single treatment intensity variable. To do so, we label each of the individual manipulations as either a positive or a negative. For each offer letter, we then add the number of positive interventions and subtract the number of negative interventions, thereby computing a total number of net positive interventions. We code as a positive intervention whether the offer is described with a small simpler table, whether the offered interest rate is being compared to the outside rate, and whether the offer letter include the photo of a person of the same race as the client. We code the inclusion of a promotional lottery on the offer letter as a negative.[32]

There is more subjectivity with the coding of the remaining manipulations. We therefore try different approaches and report on all of them. [33] First, with respect to the coding of gender on the photo, we code as a positive intervention either whether the photo is that of a female, or whether the photo is that of someone of the opposite gender of that of the client, or whether the photo is that of a female and the letter was sent to a male. Most tricky given our discussion above is the coding of the deadline. We therefore present aggregation results that either code the short deadline as a positive, or code the short deadline as a negative or simply ignore the deadline manipulation altogether.

Note that we ignore the reminder phone call and suggestion phone call interventions in the construction of this treatment intensity variable because, as discussed before, these do not fall under the same strict randomization design.[34] We also ignore the suggested usage manipulation as this manipulation does not relate to influencing the take-up level.

Finally, it will also be relevant for some of the analysis that follows (such as studying the type

[32]Such codings were made on the basis of prior beliefs so some of these do not in practice actually have a positive impact.

[33]Since we mainly use this pooling approach to examine broader questions, such as how the psychological manipulations interact with the interest rate, the impact of any remaining subjectivity is hopefully minimal.

[34]All findings in the tables that follow are qualitatively unchanged if we include these 2 manipulations, coding them as both positive interventions.

of selection operating on the psychological margin) to focus exclusively on those manipulations that "worked," i.e. induced a significant effect on take-up. We therefore also construct a version of the treatment intensity variable that count as zeros those interventions that led to no statistically significant effect on take-up.

### 5.1 Basic Results

Table 12 reports on the effect of these various treatment intensity variables on take-up. Let $P$ be the treatment intensity measure and $T$ denote take-up. We then estimate a probit model of the form:

$$Pr(T = 1) = \Phi(a + b * P + c * r + d * X)$$

where $r$ is the interest rate and $X$ is a vector of controls, including dummies for experimental wave as well as all variables conditional on which the randomization of any of the manipulations in the intensity variable took place (see section 3.2 for details).

Each cell in Table 12 corresponds to a separate probit model corresponding to the version of the treatment intensity variable defined by that row and column. Reported in each cell is the estimated marginal effect of that treatment intensity variable on take-up, the standard error on this estimated effect (in parentheses) and the quantification of this effect in interest rate terms (in brackets).

The first three columns focus on the treatment intensity variables that include all interventions and either exclude the deadline manipulation, count the short deadline as a positive or count the short deadline as a negative. The last two columns focus on the interventions that produced statistically significant effects, either excluding the deadline manipulation or counting a short deadline as a positive intervention. The 3 rows of Table 12 correspond to the three different coding of the "photo gender" manipulation, as described above.

When looking across all interventions (first 3 columns of Table 12), we find that every additional positive psychological manipulation corresponds to a drop in the monthly interest rate of between .3 to 1 percentage point. Not surprisingly, we find the lowest (and statistically insignificant) effects are associated with the coding of the short deadline as a positive intervention (column 2). Similarly, the largest (and most significant) effects are associated with the coding of the short deadline as a negative intervention. However, the effect of the treatment intensity variable remains mostly statistically significant (2 out of 3 cases) and economically large (between .55 and .77 percentage point) even when we exclude the deadline manipulation (column 1).

When we focus on the significant manipulations only (last 2 columns of Table 12), we find marginal effects on the treatment intensity variable that correspond to between a 1.2 to 2.2 percentage point drops in the monthly interest rate and are, by construction, highly statistically significant.[35].

### 5.2 Nonlinearity of Results

The first additional question we address with this treatment intensity variable relates to how the various psychological manipulations interact with each other in their effect on take-up. Are they substitutes so that having two positive interventions is not twice as strong as having one? Or are they complements, with a given additional intervention reinforcing the effect of the other one? To address this question, we use the versions of the treatment intensity variable that focus on the significant interventions only, either excluding the deadline manipulation or coding the short deadline as a positive intervention.

In the first two columns of Table 13, we simply turn the linear treatment intensity variable into a set of dummies that correspond to each separate number of net positive interventions. These individual dummies are estimated with a great deal of noise, preventing us from making any strong inference. However, the pattern of estimated coefficients *per se* does not indicate a great deal of nonlinearity. In the next 2 columns, we instead simply allow for a non-linear effect by including in the take-up model a quadratic term for the net number of positive intervention. The point estimate on that quadratic term is negative (thus indicating some concavity), but small in magnitude and statistically very insignificant. Finally, in the last 2 columns of Table 13, we adopt a slightly different approach to address this same question. Instead of giving each intervention a $+1$ or $-1$ in the construction of the treatment intensity variable, we give it a weight equal to its marginal effect as estimated in the single probit regressions above (Tables 3 to 8). We then add up these coefficients. This is designed so that a regression of take-up on this new variable should produce a coefficient of 1. We then assess whether this weighted treatment intensity variable has a nonlinear effect by

[35] Again, as we discuss earlier, these last two columns are not meant to be interpreted as representative of the average psychological manipulation as we by definition condition here on selecting only those manipulations that produced a significant effect. Instead, these versions of the treatment intensity variables will be most useful in answering further questions about *how* the psychological interventions do in fact affect take-up

"splining" it at its median value. The estimated coefficients on the 2 splines are consistent with some concavity. However, marginal interventions appear to still affect take-up past the median. As a whole, our findings in Table 13 suggest there might be some concavity in the combined effect of the psychological interventions. However, additional interventions still appear to affect take-up even for those letters that are already heavily loaded on the psychological features.

### 5.3 Interaction of Psychology with the Interest Rate

Do the effects of psychology vary with the interest rate? Do they especially help in generating take-up in case of good deal? Or do they mitigate the benefit of offering a good deal? In Table 14 we show simple probit models of take-up on the treatment intensity variables. We also include in these probit models a dummy variable for whether the offered interest rate is high, which is defined to be 1 if the rate is higher than the median offered *for that risk category* and an interaction between this interest dummy and the treatment intensity variable. Obviously, all models also include a vector controls for all the variables conditional on which the randomizations were done.[36] Irrespective of the treatment intensity variable used to estimate this model, the results in Table 14 show a very clear pattern. The psychological interventions matter far more when the interest rate is high.[37] In other words, the psychological manipulations appear to weaken the price sensitivity of demand.

### 5.4 Which Clients Respond More to the Psychological Manipulations?

Do the psychological interventions influence take-up more for less educated or the poor? We examine this question in this Table 15. In that table, we allow for the effect of the psychological treatment intensity variables on take-up to vary based on whether a given client falls above or below the sample median in terms of predicted education or income.[38] In all regressions reported in Table 15, we also include the direct effect of education (or income), as well as an interaction between the interest rate and education (or income). .

[36]We find qualitatively similar results if we include the continuous interest rate variable instead. The dummy specification simply allows us to more easily factor in the fact that interest rates were assigned conditional on the risk categories.

[37]Recall that nearly all the deals offered here were favorable so "high" means "less favorable".

[38]Education was predicted based the client's occupation (as available in the Lender's records). The occupation variable was recoded to match that in the South African Living Standards Measurement Survey (LSMS). The LSMS was then used to predict years of education associated with a given occupation code.

As is clear from Table 15, we find no evidence that it is those less educated clients or those with lower income levels that are particularly sensitive to the psychological manipulations. In fact, all but one of the estimated coefficients on the interaction term between treatment intensity and education or income are positive, even though not statistically significant. In regressions not reported here, we also considered how the sensitivity to the psychological interventions varied based on the level of past experience a given client had with dealing the Lender (which we measured by the number of past loans the client had with the Lender). Again, we found no evidence that increased experience reduced the sensitivity to the psychological manipulations. In summary, there is no systematic evidence that the responsiveness to the psychological cues is dampened by superior cognitive skills (as proxied for with education or income) or longer experience (as proxied for with past borrowing activity with this Lender).

In Table 16, we examine whether the psychological manipulations induce selection in some other margins by looking at repayment rates on the taken-up loans. Specifically, we construct a new dependent variable that measures the amount past due on the loan as a percentage of the total loan amount. We then ask whether the various psychological treatment intensity variables systematically relate to greater amount past due. Included in all regressions are also the offered interest rate and the vector of controls conditional on which the randomizations were done. See Karlan and Zinman (2005) for greater detail.

Column 1 of Table 16 simply focus on the offered interest rate effect on repayment rate. The estimated coefficient on the interest rate variable is positive and statistically significant, indicating that those clients that took up a loan at higher interest rate are more likely to be late on their repayment. In contrast, the remaining columns of Table 16 show that there is no systematic evidence of adverse selection on the psychological manipulations margin. In fact, all the estimated coefficients on the treatment intensity variables, while noisy, are negative. In other words, the point estimates indicate an increase in the number of positive interventions, *decreases* the percentage amount of the loan that is past due.

There is thus a very sharp difference between the interest rate and the psychological manipulations when regarded as two different instruments firms can use to increase their profit. A hike in the interest rate will only increase profit if this pure price effect is not offset by the lower take-up rate and the negative adverse selection it will induce. In contrast, the use of more positive psychological cues appears to have an unambiguous positive effect as it increases take-up (at a given interest rate) without adversely affecting the pool of borrowers.

### 5.5 Crowd Out

A final question of importance is whether the psychological manipulations generate new borrowing or simply draw clients to the firm who would have borrowed elsewhere or at a different point in time from the Lender. To answer this question, we collected for all individuals in the sample credit report information on their borrowing with other formal institution over a six-months period after the mailing of the offer letter. The credit report aggregates loans taken from all other sources reporting to the credit bureau. Thus it is a somewhat accurate snap shot of formal sector borrowing, but not of borrowing from the informal sector (such as money lenders, family or friends). We also collected for all individuals in the sample information on their borrowing from the Lender over a six-months period after the mailing of the offer letter (excluding any loan taken out in response to the offer letter). We then constructed based on this information two variables: whether the individual took up any loan from any of these sources, and how much in total the individual borrowed. We then regressed these two variables on the various versions of the treatment intensity variable.

The results of this exercise are reported in Table 17. The dependent variable in the first 3 columns is a dummy variable that equals one for any borrowing; the dependent variable in the last 3 columns is total amount borrowed. As one can see, we find no statistically significant evidence of a crowd-out effect. Even though most of the point estimates are negative, they are very noisily estimated.

## 6 Potential Reconciliation with Rational Choice Models

Can our findings in this paper be reconciled with a rational choice model? One possible argument might be that while some psychological interventions indeed appear to affect demand, others have been shown to be ineffective. Should we regard this instability across manipulations as a sign of failure for a more behavioral model of choice? We think not. In fact, this variability in effectiveness is already central in the psychological literature, which places great emphasis on contextual specificity.[39] In addition, as we saw in Table 14, context specificity does not appear to be restricted to the psychological model but may also be intrinsic to the rational choice model. In that table, we showed significant interactions between the psychological variables and the price variable. Put another way, had we run a pure interest rate experiment to measure the elasticity of demand, our findings in Table 14 show that the results might have been very different based on numerous (rationally) irrelevant features of the offer letter.

Another attempt at reconciliation would be to argue that the clients in our experiment were relatively indifferent about whether to get a loan or not. Under this view, the psychological factors have such a large effect only because they "push in" the many people who stand on the margin of whether or not to take a loan. This view, however, is inconsistent with our price sensitivity benchmarking exercise. If clients are rational and indifferent between taking a loan and not, small variation in prices ought to have very large effects on take-up.[40] This in turn would mute the relative importance of psychological manipulations. In other words, by scaling the psychological effects in interest rate terms, we adjust for the intensity of preference in price terms.

Another direction for discussion is that perhaps some of the psychological manipulations we have performed provide informative signals to the client. For example, a female photo on the offer letter may signal a friendlier bank. Or the addition of a promotional giveaway may signal a lower quality lender or a worse deal. This type of argument is, we believe, qualitatively implausible in many cases. First, the clients in our sample have already accumulated a lot of past experience with the Lender, so that many of the things that could be signaled have already been learned.[41] Also, when it comes to signaling the attractiveness of a given loan (taking the lender quality as given), a rational individual should have enough information to assess whether the offer is attractive or not (given the market rate). Finally, it is unclear how a signaling argument could be a rational equilibrium here. For signals to have such large effects, these signals should be costly to provide. In contrast, most of the manipulations in question (for example adding a female photo to the letter) are essentially costless..

[39] Contextual specificity could also help to explain why prior field studies, which typically focus on one single manipulations, themselves differ in whether they uncover psychological effects or not. Because our study examines numerous psychological manipulations at once, it makes the variability in effectiveness more transparent.

[40] Unless clients are indifferent about everything altogether, which would be a rather vacuous model.

[41] As we discussed earlier, we find no evidence that greater past experience (measured in terms of number of past loans) dampen the sensitivity of demand to the psychological manipulations.

A different confounding factor in interpreting our results is the specificity of the South African context. How comparable would we expect the results of a similar experiment to be in another country? It is impossible to tell. We can only argue for the fact that the individuals in our sample are experienced users in this credit market and are familiar with the product and terms. However, even among these experienced customers, one might still argue that perhaps they had only limited exposure to advertising in the past and that greater exposure to advertising may reduce the response to the psychological manipulations. Advertising is very common in South Africa, though direct mail solicitations are not as common as in the United States. We also find no evidence of weaker sensitivity among the most educated or higher income clients, who likely have had more exposure to other forms of advertising. In short, as with any study of one context we cannot rule out that our results are context-specific. Interestingly, though, the psychological research which motived our interventions were largely conducted on U.S. subjects in the lab who showed very similar behavior. Moreover, it is worth noting though that there is nothing *a priori* in economic theory that would predict that this particular context should show such anomalous behavior.

# 7 Conclusion

In the context of a field experiment in the consumer credit market in South Africa, we have shown that a firm in this market can exploit consumers' psychological biases to increase demand without lowering prices. Three key features of our findings are worth stressing in these concluding remarks. First, while several of the psychological manipulations we attempted affected demand, several did not. This suggests, as already stressed in the psychological literature, that psychological effects are very context sensitive and may require experimentation to pin down. To a certain degree, this is not unlike the experimentation firms may have to engage in to pin down the "optimal price." Second, the magnitude of these psychological effects is large, with each "binding" manipulation mapping into drops in the monthly interest rate ranging from 1 percentage point (most often) to sometime as much as 4 percentage points. Finally, our combined findings regarding the apparent lack of a crowd-out effect, the absence of adverse selection on the psychological margin and the weakened price sensitivity associated with the psychologically more loaded offer letters suggest that psychology may impact the market equilibrium. By competing on psychological factors (or marketing), firms seem able to raise aggregate demand without suffering from adverse selection, all the while dulling the incentives for price competition.

These results have two broad sets of implications. First, it casts some light on different views of advertising (Bagwell 2004). Few, if any, of our manipulations could be perceived as informative, especially not for the magnitudes we find, nor for consumers with this level of experience with the Lender. In this sense, these findings appear hard to reconcile with informational views of advertising (Ozga 1960, Stigler 1961 and Nelson 1974). Instead, they seem more consistent with "persuasive" views of advertising, in which advertising expenditures generate demand (Braithwaite 1928, Kaldor 1950). They highlight however the need and directions for greater micro-foundation of persuasive models. In this particular case, the psychological cues are not particularly expensive advertising expenditures. Instead, they are fairly inexpensive manipulations that sometimes appear to raise demand. Moreover, the fact that some of the manipulations do not work suggest the "search" process firms may need to engage in.[42] The results also suggest the greater need to understand how these phenomena aggregate to produce market equilibria (Russell and Thaler, Haltiwanger and Waldmann, Gabaix and Laibson).

These results are also informative about the importance of psychology in driving economic decisions. They suggest that even in natural contexts, psychological forces can potentially be quite large. But they also clearly indicate that the incorporation of these drivers in our models will not be a simple task. Instead, it will require a much deeper understanding of the specific contexts in which a particular psychological driver is likely to be relevant and the specific contexts in which it is not. The economic magnitude of our findings above, however, suggests that the development of such richer models may be necessary in order to reach an accurate description of economic behavior.

[42]The fact that we are finding such large effects at the margin also suggests that firms may not yet be optimally using these techniques. Though we cannot tell that whether this is because search costs are large, this particular context is an aberration or whether in general firms are not at the profit maximizing frontier on this dimension.

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## Table I
## Demographic Summary of Bank Customers in Sample[a]

| Demographic Variable | Full Sample | Customers Who Took Up the Loan Offer | Customers Who Did Not Take Up the Loan Offer |
|---|---|---|---|
| Male | 0.524 | 0.507 | 0.525 |
| | (0.50) | (0.50) | (0.50) |
| Black | 0.850 | 0.846 | 0.850 |
| | (0.36) | (0.36) | (0.36) |
| Coloured | 0.035 | 0.040 | 0.034 |
| | (0.18) | (0.20) | (0.18) |
| Indian | 0.032 | 0.029 | 0.032 |
| | (0.17) | (0.17) | (0.18) |
| White | 0.084 | 0.085 | 0.084 |
| | (0.28) | (0.28) | (0.28) |
| Low risk | 0.103 | 0.206 | 0.095 |
| borrower | (0.30) | (0.40) | (0.29) |
| Medium risk | 0.135 | 0.299 | 0.122 |
| borrower | (0.34) | (0.46) | (0.33) |
| High risk | 0.761 | 0.495 | 0.783 |
| borrower | (0.43) | (0.50) | (0.41) |
| Months since | 10.42 | 6.19 | 10.76 |
| last CI loan | (6.80) | (5.81) | (6.76) |
| Previous number | 4.14 | 4.71 | 4.10 |
| of CI loans | (3.77) | (4.09) | (3.74) |
| Low frequency | 0.678 | 0.419 | 0.699 |
| borrower | (0.47) | (0.49) | (0.46) |
| High frequency | 0.322 | 0.581 | 0.301 |
| borrower | (0.47) | (0.49) | (0.46) |
| Speaks English? | 0.480 | 0.488 | 0.479 |
| | (0.50) | (0.50) | (0.50) |
| Gross monthly | 3416.446 | 3424.354 | 3415.812 |
| Income (Rand) | (19657.41) | (2133.56) | (20420.44) |
| Predicted Years | 6.85 | 7.08 | 6.83 |
| of Education | (3.25) | (3.30) | (3.25) |
| Loan Amount | – | 1489.49 | – |
| | – | (1351.46) | – |
| Past Due Amount as a | – | .12 | – |
| Percent of Total Debt | – | (.28) | – |
| Total Debt Taken out from | 4052.57 | 4586.71 | 4009.80 |
| Other Banks After Letter | (21935.53) | (21854.36) | (21903.2) |
| Sample | 53194 | 3944 | 49250 |

[a]Notes:

1. Loan take up is defined as having taken up the loan offer by the letter-specific stated deadline.
2. Borrower risk category is a customer classification variable used by the bank and based on internal records on customers' credit history.
3. Borrower frequency category is a variable used by this study and constructed from variables on customers' loan history with the bank. Specifically, high frequency customers are those who (prior to the offer letter) had borrowed with the bank at least twice before and at least once within the last eight months
4. Predicted Years of Education is an education variable based on bank customers' occupation. Occupation was coded according to the South African Living Standards Measurement Survey (LSMS), and then the LSMS was used to predict the years of education associated with particular occupations.

**Table II**
**Summary of Randomized Intervention Assignment in Sample[a]**

| Randomized Intervention | Full Sample | Customers Who Took Up the Loan Offer | Customers Who Did Not Take Up the Loan Offer |
|---|---|---|---|
| September wave | 0.395 | 0.401 | 0.394 |
| of project | (0.49) | (0.49) | (0.49) |
| October wave | 0.605 | 0.599 | 0.606 |
| of project | (0.49) | (0.49) | (0.49) |
| Small option table? | 0.432 | 0.349 | 0.438 |
| (Y=1) | (0.50) | (0.48) | (0.50) |
| No comparison to competitor? | 0.200 | 0.200 | 0.200 |
| | (0.40) | (0.40) | (0.40) |
| Comparison expressed as gain? | 0.401 | 0.408 | 0.400 |
| | (0.49) | (0.49) | (0.49) |
| No photo on mailing | 0.202 | 0.206 | 0.202 |
| | (0.40) | (0.40) | (0.40) |
| Mailing on photo is black | 0.477 | 0.476 | 0.477 |
| | (0.50) | (0.50) | (0.50) |
| ...Coloured | 0.071 | 0.071 | 0.071 |
| | (0.26) | (0.26) | (0.26) |
| ...Indian | 0.125 | 0.122 | 0.125 |
| | (0.33) | (0.33) | (0.33) |
| ...White | 0.124 | 0.125 | 0.124 |
| | (0.33) | (0.33) | (0.33) |
| ...Female | 0.399 | 0.411 | 0.398 |
| | (0.49) | (0.49) | (0.49) |
| ...Male | 0.399 | 0.383 | 0.400 |
| | (0.49) | (0.49) | (0.49) |
| Photo's race matches | 0.534 | 0.531 | 0.535 |
| customer's race? | (0.50) | (0.50) | (0.50) |
| Photo's gender matches | 0.401 | 0.388 | 0.402 |
| Customer's gender? | (0.49) | (0.49) | (0.49) |
| Mailing includes | 0.250 | 0.246 | 0.251 |
| promotional lotter | (0.43) | (0.43) | (0.43) |
| Has a short deadline | 0.035 | 0.024 | 0.036 |
| with no extension | (0.18) | (0.15) | (0.19) |
| Has a short deadline | 0.036 | 0.034 | 0.036 |
| with extension | (0.19) | (0.18) | (0.19) |
| Has a medium deadline | 0.785 | 0.768 | 0.786 |
| | (0.41) | (0.42) | (0.41) |
| Has a long deadline | 0.145 | 0.174 | 0.142 |
| | (0.35) | (0.38) | (0.35) |
| Received a reminder call | 0.002 | 0.003 | 0.002 |
| to apply | (0.05) | (0.05) | (0.05) |
| Received a suggestion | 0.003 | 0.005 | 0.003 |
| call to apply | (0.05) | (0.07) | (0.05) |
| Sample | 53194 | 3944 | 49250 |

[a]Notes:

1. Loan take up is defined as having taken up the loan offer by the letter-specific stated deadline.
2. See text for detailed descriptions of the randomized interventions.

**Table III Effect of Simplicity of Offer Description on Take-Up**[a]

Dependent Variable: Take-Up Dummy

| | Transaction Frequency | | |
|---|---|---|---|
| **Sample:** | **All** | **High** | **Low** |
| Small option table? | 0.603 | 1.146 | 0.407 |
| | (0.239) | (0.674) | (0.219) |
| $\Delta$ interest rate equivalent | [2.337] | [3.570] | [1.887] |
| Interest rate | -0.258 | -0.321 | -0.215 |
| | (0.049) | (0.145) | (0.044) |
| Risk Category F.E.? | yes | yes | yes |
| Experimental Wave F.E.? | yes | yes | yes |
| Sample size | 53194 | 17108 | 36086 |

[a]Notes:

1. Sample is the entire set of customers that were mailed the experimental loan offer letter, excluding those for which the offer letter was returned to the Lender.
2. Dependent variable is a dummy variable that equals 1 if the customer tookup at least one loan in response to the offer letter by the stated deadline, 0 otherwise. “Transaction frequency category” refers to the classification of customers as a high or low frequency borrower based on the frequency and most recent date of their past loans. “Small option table?” is a dummy variable that equals 1 if the offer letter sent to the customer displayed only one example of a loan, 0 if it displayed multiple examples. See text for details.
3. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated.
4. Bracketed values, the ”$\Delta$ interest rate equivalent”, gives the ratio of the intervention’s effect on takeup to the measured effect of a change in interest rates on loan takeup.
5. All regressions are estimated using a Probit model. Marginal effects are reported in the table.

## Table IV
## Effect of Gain Comparison Frame on Take-Up[a]

Dependent Variable: Take-Up Dummy

| | | Transaction Frequency | |
|---|---|---|---|
| **Sample:** | **All** | **High** | **Low** |
| No comparison to competitor? | 0.330 | 0.432 | 0.315 |
| (Y=1) | (0.458) | (1.090) | (0.474) |
| Δ interest rate equivalent | [1.282] | [1.349] | [1.462] |
| Comparison expressed as gain? | 0.225 | 0.645 | 0.076 |
| (Y=1) | (0.239) | (0.572) | (0.245) |
| Δ interest rate equivalent | [0.874] | [2.011] | [0.351] |
| Interest rate | -0.258 | -0.321 | -0.215 |
| | (0.049) | (0.145) | (0.044) |
| Risk Category F.E.? | yes | yes | yes |
| Experimental Wave F.E.? | yes | yes | yes |
| Sample size | 53194 | 17108 | 36086 |

[a]Notes:

1. Sample is the entire set of customers that were mailed the experimental loan offer letter, excluding those for which the offer letter was returned to the Lender.
2. Dependent variable is a dummy variable that equals 1 if the customer tookup at least one loan in response to the offer letter by the stated deadline, 0 otherwise. "Transaction frequency category" refers to the classification of customers as a high or low frequency borrower based on the frequency and most recent date of their past loans. "No comparison to competitor?" is a dummy variable that equals 1 if the offer letter did not include a comparison of the Lender's rates to competitor's rates, 0 if it made the comparison. "Comparison expressed as a gain?" is a dummy variable that equals 1 if the comparison of the Lender's rates to the competitor's rate was shown so that the customer saw the Lender's lower rates as a "gain", 0 if the comparison was such that the customer saw the competitor's higher rates as a "loss". See text for details.
3. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated.
4. Bracketed values, the "Δ interest rate equivalent", gives the ratio of the intervention's effect on takeup to the measured effect of a change in interest rates on loan takeup.
5. All regressions are estimated using a Probit model. Marginal effects are reported in the table.

**Table V**
**Effect of Photo Race on Take- Up[a]**

Dependent Variable: Take-Up Dummy

| | Transaction Frequency | | |
|---|---|---|---|
| **Sample:** | **All** | **High** | **Low** |
| No photo | 0.049 | 0.809 | -0.237 |
| | (0.414) | (1.036) | (0.403) |
| | [0.191] | [3.149] | [0.924] |
| Black photo | 0.239 | 0.745 | -0.025 |
| | (0.483) | (1.219) | (0.474) |
| | [0.931] | [2.898] | [0.098] |
| Coloured photo | -0.179 | 0.743 | -0.568 |
| | (0.517) | (1.325) | (0.487) |
| | [0.695] | [2.891] | [2.209] |
| Indian photo | -0.212 | 0.872 | -0.611 |
| | (0.445) | (1.142) | (0.420) |
| | [0.825] | [3.393] | [2.376] |
| White photo | – | – | – |
| Race Match? | -0.391 | 0.289 | -0.614 |
| (Y=1) | (0.437) | (1.103) | (0.432) |
| | [1.520] | [1.123] | [2.388] |
| Interest rate | -0.257 | -0.322 | -0.213 |
| | (0.049) | (0.145) | (0.044) |
| | 53194 | 17108 | 36086 |
| Risk Category F.E.? | yes | yes | yes |
| Experimental Wave F.E.? | yes | yes | yes |
| Race Dummies | yes | yes | yes |
| Sample size | 53194 | 17108 | 36086 |

[a]Notes:

1. Dependent Variable is take-up of loan offer in a linear probability model.
2. Sample is the entire set of customers that were mailed the experimental loan offer letter, excluding those for which the offer letter was returned to the Lender.
3. Dependent variable is a dummy variable that equals 1 if the customer tookup at least one loan in response to the offer letter by the stated deadline, 0 otherwise. "Transaction frequency category" refers to the classification of customers as a high or low frequency borrower based on the frequency and most recent date of their past loans. "Race match?" is a dummy variable that equals 1 if the Lender employee's photograph on the offer letter match the race of the customer , 0 otherwise. See text for details.
4. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated.
5. Bracketed values, the "$\Delta$ interest rate equivalent", gives the ratio of the intervention's effect on takeup to the measured effect of a change in interest rates on loan takeup.
6. All regressions are estimated using a Probit model. Marginal effects are reported in the table.

**Table VI**
**Effect of Photo Gender on Take-Up**[a]

Dependent Variable: Take-Up Dummy

Panel A: Both Genders

| | Transaction Frequency | | | | | |
|---|---|---|---|---|---|---|
| **Sample:** | **All** | | **High** | | **Low** | |
| Opposite Gender | 0.346 | | 0.765 | | 0.187 | |
| | (0.241) | | (0.577) | | (0.248) | |
| | [1.341] | | [2.368] | | [0.869] | |
| Female Photo | | 0.571 | | 0.786 | | 0.483 |
| | | (0.243) | | (0.577) | | (0.251) |
| | | [2.223] | | [2.456] | | [2.245] |
| No Photo | 0.460 | 0.579 | 0.434 | 0.443 | 0.479 | 0.639 |
| | (0.300) | (0.303) | (0.715) | (0.714) | (0.310) | (0.316) |
| | [1.785] | [2.252] | [1.345] | [1.384] | [2.225] | [2.975] |
| Interest Rate | -0.258 | -0.257 | -0.323 | -0.320 | -0.215 | -0.215 |
| | (0.049) | (0.049) | (0.145) | (0.145) | (0.044) | (0.044) |
| Customer gender F.E.? | yes | yes | yes | yes | yes | yes |
| Risk Category F.E.? | yes | yes | yes | yes | yes | yes |
| Experimental wave F.E.? | yes | yes | yes | yes | yes | yes |
| Sample size | 53194 | 53194 | 17108 | 17108 | 36086 | 36086 |

Panel B: Male Customers

| | Transaction Frequency | | |
|---|---|---|---|
| **Sample:** | **All** | **High** | **Low** |
| Opposite Gender | 0.871 | 1.486 | 0.635 |
| | (0.332) | (0.794) | (0.343) |
| | [4.521] | [5.515] | [4.080] |
| No Photo | 0.580 | 0.653 | 0.573 |
| | (0.414) | (0.983) | (0.432) |
| | [3.011] | [2.425] | [3.684] |
| Interest Rate | -0.193 | -0.269 | -0.156 |
| | (0.067) | (0.199) | (0.061) |
| Risk Cateogry F.E.? | yes | yes | yes |
| Experimental wave F.E.? | yes | yes | yes |
| Sample size | 27848 | 8903 | 18945 |

[a]Notes:

1. Sample in Panel A is the entire set of customer that were mailed the experimental loan offer letter, excluding those for which the offer letter was returned to the Lender. Sample in Panel B is the subset of male customers within that set.
2. Dependent variable is a dummy variable that equals 1 if the customer tookup at least one loan in response to the offer letter by the stated deadline, 0 otherwise. "Transaction frequency category" refers to the classification of customers as a high or low frequency borrower based on the frequency and most recent date of their past loans. "Female photo" is a dummy variable that equals 1 if the photo of a female was displayed on the offer letter, 0 otherwise. "No photo" is a dummy variable that equals 1 if no photo was displayed on the offer letter, 0 otherwise. See text for details. "Opposite Gender" is a dummy that equals 1 if the photo gender is the opposite of the customer's gender.
3. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated.
4. Bracketed values, the "$\Delta$ interest rate equivalent", gives the ratio of the intervention's effect on takeup to the measured effect of a change in interest rates on loan takeup.
5. All regressions are estimated using a Probit model. Marginal effects are reported in the table.

**Table VII**
**Effect of Promotional Lottery on Take-Up**[a]

| Dependent Variable: | Take-Up Dummy | | |
|---|---|---|---|
| | Transaction Frequency | | |
| **Sample:** | **All** | **High** | **Low** |
| Promotional lottery | -0.133 | -1.162 | 0.290 |
| | (0.245) | (0.579) | (0.256) |
| | [0.517] | [3.602] | [1.349] |
| Interest rate | -0.258 | -0.323 | -0.215 |
| | (0.049) | (0.145) | (0.044) |
| Risk category F.E.? | yes | yes | yes |
| Experimental wave F.E.? | yes | yes | yes |
| Sample size | 53194 | 17108 | 36086 |

[a]Notes:

1. Sample is the set of customers that were mailed the experimental loan offer letter, excluding those for which the offer letter was returned to the Lender.
2. Dependent variable is a dummy variable that equals 1 if the customer tookup at least one loan in response to the offer letter by the stated deadline, 0 otherwise. "Transaction frequency category" refers to the classification of customers as a high or low frequency borrower based on the frequency and most recent date of their past loans. "Promotional lottery" is a dummy variable that equals 1 if the offer letter mentions a promotional lottery, 0 otherwise. See text for details.
3. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated.
4. Bracketed values, the "$\Delta$ interest rate equivalent", gives the ratio of the intervention's effect on takeup to the measured effect of a change in interest rates on loan takeup.
5. All regressions are estimated using a Probit model. Marginal effects are reported in the table.

**Table IX**
**Effect of Reminder Phone Call on Take-Up**[a]

Dependent Variable: Take-Up Dummy

| | Transaction Frequency | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Sample:** | **All** | | | | **High** | | **Low** | |
| Specification | OLS | OLS | IV | IV | OLS | OLS | OLS | OLS |
| Reminder call (treated) | 4.04 | 3.89 | 1.13 | 1.74 | 9.11 | 9.00 | 1.66 | 1.64 |
| | (1.53) | (1.54) | (3.05) | (3.05) | (3.80) | (3.87) | (1.40) | (1.39) |
| | [24.22] | [23.48] | [6.81] | [10.57] | [45.37] | [39.57] | [10.30] | [10.61] |
| Interest rate | -0.17 | -0.17 | -0.17 | -0.16 | -0.20 | -0.23 | -0.16 | -0.15 |
| | (0.06) | (0.06) | (0.06) | (0.06) | (0.18) | (0.19) | (0.05) | (0.05) |
| Control for customer char. | no | yes | no | yes | no | yes | no | yes |
| Frequency F.E. | yes | yes | yes | yes | yes | yes | yes | yes |
| Risk Category F.E. | yes | yes | yes | yes | yes | yes | yes | yes |
| Sample size | 17850 | 17057 | 17850 | 17057 | 5656 | 5373 | 12194 | 11684 |

[a]Notes:

1. Sample is the set of customer that were mailed the experimental loan offer letter in the first experimental wave, received an October 31, 2003 deadline and did not borrow prior to October 20, 2003;
2. Dependent variable is a dummy variable that equals 1 if the customer tookup at least one loan in response to the offer letter by the stated deadline, 0 otherwise. "Transaction frequency category" refers to the classification of customers as a high or low frequency borrower based on the frequency and most recent date of their past loans. "Reminder call (treated)" is a dummy variable that equals 1 is the customer actually received a reminder phone call, 0 otherwise. See text for details.
3. In the IV regressions, we instrument the "Reminder call (treated)" with "Reminder call (attempted)"
4. Included as controls for customer characteristics in columns 2 and 4 are: dummy variables for the number of months the client's account at the Lender has been dormant, the logarithm of the number of months the client has been employed at his or her current employer, the logarithm of the client's gross monthly income, the client's ITC score (and a dummy variable for the ITC score zero being zero), a gender dummy, a dummy variable for the client having a high (and not low) education background, dummy variables for the client's province of residence, dummy variables for the client's first language, the client's number of dependents (and a dummy for the client having no dependents), and a dummy variable for clients whose cellular and home phone numbers are both invalid.
5. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated.
6. Bracketed values, the "$\Delta$ interest rate equivalent", gives the ratio of the intervention's effect on takeup to the measured effect of a change in interest rates on loan takeup.
7. All regressions are estimated using a linear probability model.

## Table X
## Effect of Suggestion Phone Call on Take-Up[a]

Dependent Variable: Take-Up Dummy

| **Sample:** | **All** | | | | **Transaction Frequency High** | | **Transaction Frequency Low** | |
|---|---|---|---|---|---|---|---|---|
| Specification | OLS | OLS | IV | IV | OLS | OLS | OLS | OLS |
| Suggestion call (treated) | 5.00 | 5.25 | 7.68 | 7.55 | 6.41 | 6.56 | 4.06 | 4.42 |
| | (2.12) | (2.12) | (3.55) | (3.54) | (4.70) | (4.74) | (2.14) | (2.13) |
| | [21.50] | [21.53] | [32.99] | [31.46] | [14.61] | [14.06] | [23.06] | [24.42] |
| Interest rate | -0.23 | -0.24 | -0.23 | -0.24 | -0.44 | -0.47 | -0.18 | -0.18 |
| | (0.07) | (0.07) | (0.07) | (0.07) | (0.19) | (0.19) | (0.06) | (0.06) |
| Control for customer char. | no | yes | no | yes | no | yes | no | yes |
| Frequency F.E. | yes | yes | yes | yes | yes | yes | yes | yes |
| Risk Category F.E. | yes | yes | yes | yes | yes | yes | yes | yes |
| Sample size | 28713 | 28353 | 28713 | 28353 | 9254 | 9171 | 19459 | 19182 |

[a]Notes:

1. Sample is the set of customers that were mailed the experimental loan offer letter, excluding those for which the offer letter was returned to the Lender
2. Dependent variable is a dummy variable that equals 1 if the customer tookup at least one loan in response to the offer letter by the stated deadline, 0 otherwise. “Transaction frequency category” refers to the classification of customers as a high or low frequency borrower based on the frequency and most recent date of their past loans. “Reminder call (treated)” is a dummy variable that equals 1 is the customer actually received a reminder phone call, 0 otherwise. See text for details.
3. In the IV regressions, we instrument the ”Reminder call (treated)” with ”Reminder call (attempted)”
4. Included as controls for customer characteristics in columns 2 and 4 are: dummy variables for the number of months the client’s account at the Lender has been dormant, the logarithm of the number of months the client has been employed at his or her current employer, the logarithm of the client’s gross monthly income, the client’s ITC score (and a dummy variable for the ITC score zero being zero), a gender dummy, a dummy variable for the client having a high (and not low) education background, dummy variables for the client’s province of residence, dummy variables for the client’s first language, the client’s number of dependents (and a dummy for the client having no dependents), and a dummy variable for clients whose cellular and home phone numbers are both invalid.
5. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated.
6. Bracketed values, the ”$\Delta$ interest rate equivalent”, gives the ratio of the intervention’s effect on takeup to the measured effect of a change in interest rates on loan takeup.
7. All regressions are estimated using a linear probability model.

**Table XI**
**Pooled Effect of Suggested Loan Usage on Reported Usage**[a]

| **Full Sample** | Loan to Be Used for: | | | | | All Loans |
|---|---|---|---|---|---|---|
| | House | School | Debt | Appliances | Other | Expected Distribution |
| Suggested Money Usage is: | | | | | | |
| House | 24.03% | 21.69% | 21.12% | 20.83% | 20.26% | **21.02%** |
| | **+3.00%** | +0.67% | +0.09% | -0.19% | -0.77% | |
| Education | 19.48% | 21.69% | 17.39% | 20.83% | 17.68% | **18.63%** |
| | +0.85% | **+3.06%** | -1.24% | +2.20% | -0.95% | |
| Pay Off Debt | 16.88% | 17.28% | 22.98% | 16.67% | 19.91% | **19.39%** |
| | -2.50% | -2.11% | **+3.60%** | -2.72% | +0.52% | |
| Appliance | 16.23% | 18.75% | 21.74% | 20.83% | 21.31% | **20.34%** |
| | -4.11% | -1.59% | +1.40% | **+0.49%** | +0.97% | |
| Generic | 23.38% | 20.59% | 16.77% | 20.83% | 20.84% | **20.61%** |
| | +2.76% | -0.03% | -3.84% | +0.22% | +0.23% | |
| Sample Size | 154 | 272 | 161 | 24 | 854 | 1465 |
| Joint P value: | 0.0587 | | | | | |

[a]Notes:

1. Sample is the subset of 1,465 customers randomly selected to receive an offer letter including a suggested loan usage. The suggestions included using the loan to pay for a house or home improvements, to pay for education, to pay off debt, to buy appliances, or a generic statement that included all the aforementioned usages. The five different suggestion options were randomly assigned to the subsample. This actual distribution of possible loan use suggestions for this subsample are listed in bold in the column "Expected Distribution".
2. The subsample (n=1,465) is further broken down into five subgroups based on customers' responses to the question of loan use. Under the null, the suggested loan usages should have no effect on the actual loan usages. This should be reflected in the distribution of suggested loan usage for each of the subgroups of actual loan matching the expected distribution based on the random assignment in the full sample. For example, for the 154 customers who used their loan to pay for a house or home improvements, we would expect, under the null, that 21.0using the money for a house, 18.6% for education, 19.4% for debt, etc. Listed in column 1 of the table are the actual distributions of suggested loan usage for the subgroup of the subsample which used the loan for a house as well as the differentials from the expected distributions. Of particular note is the diagonal (differentials in bold) where loan use and suggested use match. Also reported in the table is the joint P value of the significance of these four actual distributions differing from the expected distributions.

**Table XI (continued)**
**Pooled Effect of Suggested Loan Usage on Reported Usage by Borrower Frequency Category**[a]

| **Low Frequency use** | Loan to Be Used for: | | | | | All Loans |
|---|---|---|---|---|---|---|
| | House | School | Debt | Appliances | Other | Expected Distribution |
| Suggested Money Usage is: | | | | | | |
| House | 24.32% | 23.31% | 23.08% | 18.18% | 18.93% | **20.67%** |
| | **+3.65%** | +2.64% | +2.41% | -2.49% | -1.74% | |
| Education | 25.68% | 20.30% | 15.38% | 36.36% | 17.59% | **18.93%** |
| | +6.75% | **+1.37%** | -3.54% | +17.44% | -1.33% | |
| Pay Off Debt | 16.22% | 15.04% | 26.92% | 18.18% | 19.82% | **19.33%** |
| | -3.11% | -4.29% | **+7.59%** | -1.15% | +0.49% | |
| Appliance | 12.16% | 18.05% | 20.51% | 9.09% | 23.83% | **21.07%** |
| | -8.91% | -3.03% | -0.56% | **-11.98%** | +2.76% | |
| Generic | 21.62% | 23.31% | 14.10% | 18.18% | 19.82% | **20.00%** |
| | +1.62% | +3.31% | -5.90% | -1.82% | -0.18% | |
| Sample Size | 74 | 133 | 78 | 11 | 449 | 745 |
| Joint P value: | 0.187 | | | | | |

| **High Frequency use** | Loan to Be Used for: | | | | | All Loans |
|---|---|---|---|---|---|---|
| | House | School | Debt | Appliances | Other | Expected Distribution |
| Suggested Money Usage is: | | | | | | |
| House | 23.75% | 20.14% | 19.28% | 23.08% | 21.73% | **21.39%** |
| | **+2.36%** | -1.25% | -2.11% | +1.69% | +0.34% | |
| Education | 13.75% | 23.02% | 19.28% | 7.69% | 17.78% | **18.33%** |
| | -4.58% | **+4.69%** | +0.94% | -10.64% | -0.56% | |
| Pay Off Debt | 17.50% | 19.42% | 19.28% | 15.38% | 20.00% | **19.44%** |
| | -1.94% | -0.02% | **-0.17%** | -4.06% | +0.56% | |
| Appliance | 20.00% | 19.42% | 22.89% | 30.77% | 18.52% | **19.58%** |
| | +0.42% | -0.16% | +3.31% | **+11.19%** | -1.06% | |
| Generic | 25.00% | 17.99% | 19.28% | 23.08% | 21.98% | **21.25%** |
| | +3.75% | -3.26% | -1.97% | +1.83% | +0.73% | |
| Sample Size | 80 | 139 | 83 | 13 | 405 | 720 |
| Joint P value: | 0.177 | | | | | |

[a]Notes:

1. The top panel is done only for the subsample "low frequency customers" and the bottom panel, for "high frequency customers", referring to the classification of customers as a high or low frequency borrower based on the frequency and most recent date of their past loans. See notes on previous page

## Table XII
## Additive Effects of All Marketing Interventions on Loan Takeup[a]

Dependent Variable: Take-Up Dummy

| FULL SAMPLE | All Interventions | | | Sig. Interventions | |
|---|---|---|---|---|---|
| | no deadline | short deadline is positive | short deadline is negative | no deadline | short deadline is negative |
| Female Photo | 0.177 | 0.116 | 0.230 | 0.381 | 0.472 |
| | (.100) | (.098) | (.100) | (.134) | (.132) |
| Δ interest rate equivalent | [0.68] | [0.45] | [0.88] | [1.47] | [1.82] |
| Opposite Gender Photo | 0.141 | 0.080 | 0.198 | 0.318 | 0.412 |
| | (.100) | (.099) | (.099) | (.135) | (.133) |
| Δ interest rate equivalent | [0.54] | [0.31] | [0.76] | [1.22] | [1.58] |
| Interacted Female Photo | 0.201 | 0.129 | 0.266 | 0.457 | 0.564 |
| | (0.108) | (0.107) | (0.107) | (0.149) | (0.146) |
| Δ interest rate equivalent | [0.77] | [0.49] | [1.02] | [1.76] | [2.17] |

[a]Notes:

1. The dependent variable is a dummy variable for loan takeup and is equal to 1 for those bank customers who signed up for a loan after receiving the offer letter but before the deadline.
2. "Net Positive Interventions" is a discrete variable that adds up the number of positive interventions that each letter contained. Each intervention is a dummy variable for whether a customer did or did not have a particular variation on their letter. The interventions for prize and no offer comparison are negative and the interventions for simple table, race matching photo, gender-matching photo, and long or medium deadline are positive. This additive effects variable has integer values from -2 to 4. Coefficient values are shown in terms of their percentage point effect on takeup, i.e., an additional marketing intervention will increase takeup by .177 percentage points.
3. "All interventions" (columns 1-3) include simple (letter has a simple offer table), femalephoto (the picture of the bank rep. on the letter is that of a woman), prize (the letter included offers of a prize), photorace (a dummy for the picture of the bank rep. on the letter having the same race as the customer) and nocomp (a dummy for the offer letter having no comparison to other banks' terms of loan). "Significant interventions" (col. 4-5) includes simple, femalephoto, and prize only. Deadline variation as a component of "net positive interventions" changes with each column as labelled.
4. The Additive effects variable also differ by the definition of the photo gender intervention variable. Row 1 has a dummy for the photo being female, row 2 has a dummy for the photo being of the opposite gender of the customer, and row 3 has a dummy for the photo being female*the customer being male.
5. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated. Controls include race and gender dummies.
6. The line Δ interest rate equivalent compares the marketing effects to the interest rate effects by reporting the absolute value of the ratio of the two coefficients. A standard -0.26 percentage point effect is used for this ratio for the interest rate effects. This value come from a base regression that regresses takeup on the interest rate and risk and wave controls only. Higher values of the ratio (greater than 1) imply that adding each additional intervention has a larger impact on loan takeup than a 1 percentage point decrease.
7. All regressions are estimated using a Probit model. Marginal effects are reported in the table.

**Table XIIIIndividual and Marginal Additive Effects of Marketing Interventions on Loan Takeup**[a]

Dependent Variable: Take-Up Dummy

| | Significant Interventions Only | | | | | |
|---|---|---|---|---|---|---|
| | No Deadlines | Including Deadlines | No Deadlines | Including Deadlines | No Deadlines | Including Deadlines |
| Weighted Number of Interventions | | | 1.102 | 1.401 | | |
| | | | (.577) | (.383) | | |
| Weighted Number of Interventions$^2$*100 | | | -0.185 | -0.090 | | |
| | | | (.467) | (.0455) | | |
| Spline 1 of Weighted Number of Interventions | | | | | 1.064 | 1.052 |
| | | | | | (.520) | (.232) |
| Spline 2 of Weighted Number of Interventions | | | | | 0.741 | 0.529 |
| | | | | | (.500) | (.115) |
| Number of net positive Interventions = -1 | dropped | dropped | | | | |
| Number of net positive Interventions = 0 | 0.622 | 1.823 | | | | |
| | (.351) | (2.210) | | | | |
| Number of net positive Interventions = 1 | 1.078 | 2.636 | | | | |
| | (.382) | (1.967) | | | | |
| Number of net positive Interventions = 2 | 1.375 | 3.163 | | | | |
| | (.635) | (2.116) | | | | |
| Number of net positive Interventions = 3 | | 3.966 | | | | |
| | | (2.682) | | | | |
| Interest rate | -0.258 | -0.258 | -0.258 | -0.258 | -0.258 | -0.257 |
| | (.049) | (.049) | (.049) | (.049) | (.049) | (.049) |
| Risk Category F.E. | yes | yes | yes | yes | yes | yes |
| Experimental Wave F.E. | yes | yes | yes | yes | yes | yes |
| Controls | yes | yes | yes | yes | yes | yes |
| Deadline controls | no | yes | no | yes | no | yes |

[a]Notes:

1. The dependent variable is a dummy variable for loan takeup and is equal to 1 for those bank customers who signed up for a loan after receiving the offer letter but before the deadline.
2. "Net Positive Interventions" is a discrete variable that adds up the number of positive interventions that each letter contained. Each intervention is a dummy variable for whether a customer did or did not have a particular variation on their letter. The interventions for prize and no offer comparison are negative and the interventions for simple table, race matching photo, gender-matching photo, and long or medium deadline are positive. This additive effects variable has integer values from -2 to 4. Coefficient values are shown in terms of their percentage point effect on takeup, i.e., an additional marketing intervention will increase takeup by .177 percentage points.
3. "Significant interventions" refers to the inclusion of only a particular set of interventions in the "net positive interventions" count. These include simple (letter has a simple offer table), femalephoto (the picture of the bank rep. on the letter is that of a woman), and prize (the letter included offers of a prize). All columns used the gender interacted (male==1) version of female photo, meaning the female photo intervention counts as a net positive intervention only for male customers. Deadline variation as a component of "net positive interventions" changes with each column as labelled with "including deadlines" indicating that non-short deadlines are positive.
4. Columns 1 and 2 show the marginal effect of each additional net positive intervention. Columns 3 and 4 model the effect of net positive interventions with a quadratic model. Columns 5 and 6 use a spline model.
5. Columns 3-6 use a weighted version of "net positive interventions" where each intervention is weighted by its coefficient obtained from regressing takeup on the intervention's dummy and controls (see tables 3-8). Specifically, someone with the simple table as their only intervention will have a value of +.603 instead of +1.
6. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated. Deadline controls control for customers' eligibility for the long and short deadline options.

**Table XIV**
**Interest Rate Interaction with Additive Effects of Marketing Interventions on Loan Takeup** [a]

Dependent Variable: Take-Up Dummy

| | All Interventions | | Core Interventions | |
|---|---|---|---|---|
| | Excluding Deadlines | Including Deadlines | Excluding Deadlines | Including Deadlines |
| Net Positive | -0.007 | 0.045 | 0.249 | 0.334 |
| Interventions | (.144) | (.142) | (.196) | (.192) |
| Interventions*High Rate | 0.454 | 0.480 | 0.454 | 0.499 |
| | (.209) | (.206) | (.279) | (.272) |
| High Rate | -1.307 | -1.789 | -1.157 | -1.655 |
| | (.256) | (.400) | (.235) | (.417) |
| Frequency F.E. | yes | yes | yes | yes |
| Experimental Wave F.E. | yes | yes | yes | yes |
| Controls | yes | yes | yes | yes |
| Deadline controls | no | yes | no | yes |

[a] Notes:

1. The dependent variable is a dummy variable for loan takeup and is equal to 1 for those bank customers who signed up for a loan after receiving the offer letter but before the deadline.
2. "Net Positive Interventions" is a discrete variable that adds up the number of positive interventions that each letter contained. Each intervention is a dummy variable for whether a customer did or did not have a particular variation on their letter. The interventions for prize and no offer comparison are negative and the interventions for simple table, race matching photo, gender-matching photo, and long or medium deadline are positive. This additive effects variable has integer values from -2 to 4. Coefficient values are shown in terms of their percentage point effect on takeup, i.e., an additional marketing intervention will increase takeup by .045 percentage points.
3. "All interventions" (columns 1-3) include simple (letter has a simple offer table), femalephoto (the picture of the bank rep. on the letter is that of a woman), prize (the letter included offers of a prize), photorace (a dummy for the picture of the bank rep. on the letter having the same race as the customer) and nocomp (a dummy for the offer letter having no comparison to other banks' terms of loan). "Significant interventions" (col. 4-5) includes simple, femalephoto, and prize only. Deadline variation as a component of "net positive interventions" changes with each column as labelled with "including deadlines" indicating that non-short deadlines are positive.
4. The Additive effects variable uses a gender-interacted version of femalephoto, specifically the dummy =1 if the photo is female and the customer is male.
5. "High Rate" is a dummy that =1 if the interest rate from the offer letter was greater than the median offer rate. This dummy is defined differently across the risk categories. Customers were eligible for different ranges of interest rates based on their risk category, so the median interest rate used to define high rate varies across the three risk cateogries.
6. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated. Controls include race and gender dummies. Deadline Controls indicates dummies for whether the customer was eligible to receive a long or short deadline.
7. All regressions are estimated using a Probit model. Marginal effects are reported in the table.

**Table XV**
**Interest Rate Interaction with Additive Effects of Marketing Interventions on Loan Takeup by Customer Characteristics**[a]

Dependent Variable: Take-Up Dummy

| | All Interventions Excluding Deadlines | | Core Interventions Excluding Deadlines | | Core Interventions Including Deadlines | |
|---|---|---|---|---|---|---|
| Net Positive | 0.237 | 0.182 | 0.358 | 0.299 | 0.430 | 0.447 |
| Interventions | (.162) | (.153) | (.221) | (.212) | (.216) | (.206) |
| Interventions*Education | -0.070 | | 0.210 | | 0.272 | |
| | (.217) | | (.300) | | (.293) | |
| Interventions*Income | | 0.036 | | 0.355 | | 0.262 |
| | | (.216) | | (.299) | | (.292) |
| Interest Rate | -0.212 | -0.246 | -0.210 | -0.241 | -0.210 | -0.240 |
| | (.075) | (.069) | (.075) | (.069) | (.075) | (.069) |
| Frequency F.E. | yes | yes | yes | yes | yes | yes |
| Experimental Wave F.E. | yes | yes | yes | yes | yes | yes |
| Controls | yes | yes | yes | yes | yes | yes |
| Deadline controls | no | no | no | no | yes | yes |
| Observations | 53194 | 53194 | 53194 | 53194 | 53194 | 53194 |

[a]Notes:

1. The dependent variable is a dummy variable for loan takeup and is equal to 1 for those bank customers who signed up for a loan after receiving the offer letter but before the deadline.
2. "Net Positive Interventions" is a discrete variable that adds up the number of positive interventions that each letter contained. Each intervention is a dummy variable for whether a customer did or did not have a particular variation on their letter. The interventions for prize and no offer comparison are negative and the interventions for simple table, race matching photo, gender-matching photo, and long or medium deadline are positive. This additive effects variable has integer values from -2 to 4. Coefficient values are shown in terms of their percentage point effect on takeup, i.e., an additional marketing intervention will increase takeup by .045 percentage points.
3. "All interventions" (columns 1-3) include simple (letter has a simple offer table), femalephoto (the picture of the bank rep. on the letter is that of a woman), prize (the letter included offers of a prize), photorace (a dummy for the picture of the bank rep. on the letter having the same race as the customer) and nocomp (a dummy for the offer letter having no comparison to other banks' terms of loan). "Significant interventions" (col. 4-5) includes simple, femalephoto, and prize only. Deadline variation as a component of "net positive interventions" changes with each column as labelled with "including deadlines" indicating that non-short deadlines are positive.
4. The Additive effects variable uses a gender-interacted version of femalephoto, specifically the dummy =1 if the photo is female and the customer is male.
5. Odd columns regress take up on "net positive interaction", the interest rate, controls, and all of the preceding's interaction with a dummy for having years of education above the median for the full sample. Also included in the regression is the direct effect from years of education. Even columns run a similar regression using the interaction of the covariates and a dummy for being above the median monthly gross income of the full sample.
6. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated. Controls include race and gender dummies. Deadline Controls indicates dummies for whether the customer was eligible to receive a long or short deadline.
7. All regressions are estimated using a Probit model. Marginal effects are reported in the table.

**Table XVI**
**Additive Effects of All Marketing Interventions on Loan Repayment[a]**

Dependent Variable: Past Due Amount as a Percent of Total Loan Amount

| | Baseline | All Interventions Excluding Deadlines | Sig. Interventions Excluding Deadlines | Sig. Interventions Including Deadlines |
|---|---|---|---|---|
| Net Positive Interventions | | -0.291 | -0.738 | -0.902 |
| | | (.752) | (1.047) | (1.023) |
| Interest Rate | 1.060 | 1.214 | 1.219 | 1.223 |
| | (.329) | (.351) | (.352) | (.352) |
| Frequency F.E. | yes | yes | yes | yes |
| Experimental Wave F.E. | yes | yes | yes | yes |
| Controls | no | yes | yes | yes |
| Deadline controls | no | no | no | yes |
| Sample Size | 4381 | 3944 | 3944 | 3944 |

[a]Notes:

1. The dependent variable is the average amount of the loan past due as a percent of the total amount of the loan taken out by the customer from the Lender under the special offer letter rate.
2. "Net Positive Interventions" is a discrete variable that adds up the number of positive interventions that each letter contained. Each intervention is a dummy variable for whether a customer did or did not have a particular variation on their letter. The interventions for prize and no offer comparison are negative and the interventions for simple table, race matching photo, gender-matching photo, and long or medium deadline are positive. This additive effects variable has integer values from -2 to 4. Coefficient values are shown in terms of their percentage point effect on takeup, i.e., an additional marketing intervention will decrease past due amount as a percent of the total loan by .29 percentage points.
3. "All interventions" (columns 2) include simple (letter has a simple offer table), femalephoto (the picture of the bank rep. on the letter is that of a woman), prize (the letter included offers of a prize), photorace (a dummy for the picture of the bank rep. on the letter having the same race as the customer) and nocomp (a dummy for the offer letter having no comparison to other banks' terms of loan). "Significant interventions" (col.3-4) includes simple, femalephoto, and prize only. Deadline variation as a component of "net positive interventions" changes with each column as labelled with "including deadlines" indicating that non-short deadlines are positive. The Additive effects variable uses a gender-interacted version of femalephoto, specifically the dummy =1 if the photo is female and the customer is male.
4. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated. Controls include race and gender dummies. Deadline Controls indicates dummies for whether the customer was eligible to receive a long or short deadline.
5. The regressions use a tobit model.

**Table XVII**
**Additive Effects of All Marketing Interventions on Other Bank Borrowing[a]**

| | Dependent Variable: Total Debt Taken Out After Offer Letter (excluding CI special offer) Amount | | | Dummy | | |
|---|---|---|---|---|---|---|
| | All Interventions | Sig. Interventions | | All Interventions | Sig. Interventions | |
| | Excluding Deadlines | Excluding Deadlines | Including Deadlines | Excluding Deadlines | Excluding Deadlines | Including Deadlines |
| Net Positive | 49.26 | -272.97 | -405.95 | 0.03 | -0.13 | -0.27 |
| Interventions | (225.85) | (309.02) | (301.65) | (0.21) | (0.28) | (0.28) |
| Interest Rate | 111.27 | 111.40 | 112.10 | 0.03 | 0.03 | 0.03 |
| | (101.38) | (101.38) | (101.38) | (0.09) | (0.09) | (0.09) |
| Frequency F.E. | yes | yes | yes | yes | yes | yes |
| Experimental Wave F.E. | yes | yes | yes | yes | yes | yes |
| Controls | yes | yes | yes | yes | yes | yes |
| Deadline controls | no | no | yes | no | no | yes |
| Sample Size | 53194 | 53194 | 53194 | 53194 | 53194 | 53194 |

[a]Notes:

1. The dependent variable for columns 1-3 is the total debt taken out after the letter from other lenders and the Lender if taken out after the deadline. The dependent variable in columns 4-6 is a dummy for having taken out any additional debt after the offer letter was sent out (including debt from the Lender after the deadline date of the offer letter).
2. “Net Positive Interventions” is a discrete variable that adds up the number of positive interventions that each letter contained. Each intervention is a dummy variable for whether a customer did or did not have a particular variation on their letter. The interventions for prize and no offer comparison are negative and the interventions for simple table, race matching photo, gender-matching photo, and long or medium deadline are positive. This additive effects variable has integer values from -2 to 4. Coefficient values are shown in terms of their percentage point effect on takeup.
3. “All interventions” (columns 1, 4) include simple (letter has a simple offer table), femalephoto (the picture of the bank rep. on the letter is that of a woman), prize (the letter included offers of a prize), photorace (a dummy for the picture of the bank rep. on the letter having the same race as the customer) and nocomp (a dummy for the offer letter having no comparison to other banks’ terms of loan). ”Significant interventions” (col.2-3,5-6) includes simple, femalephoto, and prize only. Deadline variation as a component of ”net positive interventions” changes with each column as labelled with ”including deadlines” indicating that non-short deadlines are positive. The Additive effects variable uses a gender-interacted version of femalephoto, specifically the dummy =1 if the photo is female and the customer is male.
4. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated. Controls include race and gender dummies. Deadline Controls indicates dummies for whether the customer was eligible to receive a long or short deadline.
5. Columns 1-3 use a tobit model; Columns 4-6, a probit model with marginal effects reported in the table.

## Table VIII Effect of Deadlines Take-Up[a]

| Dependent Variable | Takeup by own Deadline | Takeup by Short Deadline | Takeup by Medium Deadline | Takeup by Long Deadline | Takeup after Stated Deadline | Takeup after Enforced Deadline |
|---|---|---|---|---|---|---|
| Short Deadline | – | 0.390 | – | – | – | – |
| | | (0.447) | | | | |
| | | [4.26] | | | | |
| Short Deadline | 2.403 | – | – | -1.934 | -3.706 | -0.507 |
| Extension | (1.074) | | | (0.785) | (0.894) | (1.098) |
| | [9.33] | | | [6.71] | | |
| Medium Deadline | 2.700 | – | – | -0.668 | -3.900 | -0.126 |
| | (0.616) | | | (0.821) | (1.055) | (0.989) |
| | [10.48] | | | [2.32] | | |
| Long Deadline | 5.628 | – | -0.996 | -1.419 | -5.450 | -2.405 |
| | (1.132) | | (0.290) | (0.778) | (0.805) | (0.955) |
| | [21.85] | | [4.14] | [4.92] | | |
| Interest rate | -0.258 | -0.091 | -0.240 | -0.288 | 0.058 | 0.052 |
| | (0.049) | (0.034) | (0.050) | (0.057) | (0.069) | (0.068) |
| Risk Category F.E.? | yes | yes | yes | yes | yes | yes |
| Experimental Wave F.E.? | yes | yes | yes | yes | yes | yes |
| Deadline controls | yes | yes | yes | yes | yes | yes |
| Sample size | 53194 | 53194 | 49448 | 53194 | 53194 | 53194 |

[a]Notes:

1. Sample is the set of customers that were mailed the experimental loan offer letter, excluding those for which the offer letter was returned to the Lender.
2. In column 1, the dependent variable is a dummy variable that equals 1 if the customer tookup at least one loan in response to the offer letter by the stated deadline, 0 otherwise. In column 2-4, the dependent variable equals 1 if the customer tookup a loan by the column-specific date. Note that the exact number of days varies by wave, so the date is specified according to the wave's relevant short, medium, and long deadline dates. Column 5 uses a dummy that equals 1 if the customer tookup a loan from the Lender after the date specified in the offer letter (and therefore went into the bank under the impression that he/she was ineligible for the special offer rates.) Column 6 is a dummy that equals 1 if the customer tookup a loan from the Lender after the enforced deadline (i.e. short deadline customers who applied for a loan after the short deadline but before the medium deadline were, in practice, still offered the special offer).
3. In columns 1 and 4-6, the regression compares the 4 different deadline types, short with no extension, short with extension, medium, and long deadline. In column 2, the regression uses a dummy that equals 1 if the customer had short deadline with no extension, 0 otherwise. In this case, all non "short deadlines with extension" are lumped into one category. In column 3, the regression compares only medium and long deadline recipients. All customers who received short deadline with no extension or with extension are excluded from the regression sample.
4. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated. "Deadline Controls" indicates dummies for whether the customer was eligible to receive a long or short deadline and the time of the mailer (September or October).
5. Bracketed values, the "$\Delta$ interest rate equivalent", gives the ratio of the intervention's effect on takeup to the measured effect of a change in interest rates on loan takeup.
6. All regressions are estimated using a Probit model. Marginal effects are reported in the table.

## Appendix Table I Additive Effects of All Marketing Interventions on Size of Loan for Full Sample[a]

Dependent Variable: CI Loan Size (Taken Out After Offer Letter)

| | Baseline | All Interventions Excluding Deadlines | Core Interventions Excluding Deadlines | Core Interventions Including Deadlines | All Interventions Excluding Deadlines | Core Interventions Excluding Deadlines | Core Interventions Including Deadlines |
|---|---|---|---|---|---|---|---|
| Net Positive Interventions | | 52.062 | 109.127 | 134.602 | 11.609 | 65.049 | 86.443 |
| | | (26.030) | (36.008) | (35.200) | (34.641) | (47.206) | (46.197) |
| Interest Rate | -69.719 | -67.413 | -67.514 | -67.612 | | | |
| | (10.980) | (11.959) | (11.959) | (11.956) | | | |
| High Rate | | | | | -314.481 | -293.150 | -397.518 |
| | | | | | (62.106) | (56.776) | (101.000) |
| Interventions*High Rate | | | | | 88.621 | 95.930 | 104.077 |
| | | | | | (58.365) | (67.320) | (65.658) |
| Frequency F.E. | yes | yes | yes | yes | yes | yes | yes |
| Experimental Wave F.E. | yes | yes | yes | yes | yes | yes | yes |
| Controls | no | yes | yes | yes | yes | yes | yes |
| Deadline controls | no | no | no | yes | no | no | yes |
| Sample Size | 53194 | 53194 | 53194 | 53194 | 53194 | 53194 | 531944 |

[a]Notes:

1. The dependent variable is the average size of the loan taken out by the customer from the Lender under the special offer letter rate. The dependent variable has imputed zeroes for those customers who did not take out a loan.
2. "Net Positive Interventions" is a discrete variable that adds up the number of positive interventions that each letter contained. Each intervention is a dummy variable for whether a customer did or did not have a particular variation on their letter. The interventions for prize and no offer comparison are negative and the interventions for simple table, race matching photo, gender-matching photo, and long or medium deadline are positive. This additive effects variable has integer values from -2 to 4. Coefficient values are shown in terms of their percentage point effect on takeup, i.e., an additional marketing intervention will increase takeup by .045 percentage points.
3. "All interventions" (columns 1-3) include simple (letter has a simple offer table), femalephoto (the picture of the bank rep. on the letter is that of a woman), prize (the letter included offers of a prize), photorace (a dummy for the picture of the bank rep. on the letter having the same race as the customer) and nocomp (a dummy for the offer letter having no comparison to other banks' terms of loan). "Significant interventions" (col. 4-5) includes simple, femalephoto, and prize only. Deadline variation as a component of "net positive interventions" changes with each column as labelled. The Additive effects variable uses a gender-interacted version of femalephoto, specifically the dummy =1 if the photo is female and the customer is male.
4. "High Rate" is a dummy that =1 if the interest rate from the offer letter was greater than the median offer rate. This dummy is defined differently across the risk categories. Customers were eligible for different ranges of interest rates based on their risk category, so the median interest rate used to define high rate varies across the three risk cateogries.
5. Risk Category F.E. control for the type of borrower. Experimental Wave F.E. control for the wave (sept. and oct.) of the project the bank customer participated. Controls include race and gender dummies. Deadline Controls indicates dummies for whether the customer was eligible to receive a long or short deadline.
6. The regressions use a tobit model.

**Appendix Table I (continued)**
**Additive Effects of All Marketing Interventions on Size of Loan for Respondents to Letter [a]**

Dependent Variable: CI Loan Size (Taken Out After Offer Letter)

| | Baseline | All Interventions Excluding Deadlines | Core Interventions Excluding Deadlines | Core Interventions Including Deadlines | All Interventions Excluding Deadlines | Core Interventions Excluding Deadlines | Core Interventions Including Deadlines |
|---|---|---|---|---|---|---|---|
| Net Positive Interventions | | 16.695 | -0.709 | -2.267 | 52.180 | 22.340 | 25.496 |
| | | (21.200) | (29.607) | (28.941) | (28.105) | (38.728) | (38.016) |
| Interest Rate | -26.172 | -26.047 | -25.906 | -25.704 | | | |
| | (9.180) | (10.005) | (10.006) | (9.988) | | | |
| High Rate | | | | | -45.587 | -79.205 | -16.784 |
| | | | | | (49.528) | (45.271) | (81.403) |
| Interventions*High Rate | | | | | -78.793 | -52.448 | -62.479 |
| | | | | | (40.982) | (55.163) | (53.990) |
| Frequency F.E. | yes | yes | yes | yes | yes | yes | yes |
| Experimental Wave F.E. | yes | yes | yes | yes | yes | yes | yes |
| Controls | no | yes | yes | yes | yes | yes | yes |
| Deadline controls | no | no | no | yes | no | no | yes |
| Sample Size | 4381 | 3944 | 3944 | 3944 | 3944 | 3944 | 3944 |

[a]Notes:

1. The dependent variable is the average size of the loan taken out by the customer from the Lender under the special offer letter rate. The sample excludes those customers who did not take out a loan.
2. See notes on previous page.