WHAT'S ADVERTISING CONTENT WORTH? EVIDENCE FROM A CONSUMER CREDIT MARKETING FIELD EXPERIMENT[*]

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Firms spend billions of dollars developing advertising content, yet there is little field evidence on how much or how it affects demand. We analyze a direct mail field experiment in South Africa implemented by a consumer lender that randomized advertising content, loan price, and loan offer deadlines simultaneously. We find that advertising content significantly affects demand. Although it was difficult to predict *ex ante* which specific advertising features would matter most in this context, the features that do matter have large effects. Showing fewer example loans, not suggesting a particular use for the loan, or including a photo of an attractive woman increases loan demand by about as much as a 25% reduction in the interest rate. The evidence also suggests that advertising content persuades by appealing "peripherally" to intuition rather than reason. Although the advertising content effects point to an important role for persuasion and related psychology, our deadline results do not support the psychological prediction that shorter deadlines may help overcome time-management problems; instead, demand strongly increases with longer deadlines.

I. Introduction

Firms spend billions of dollars each year on advertising consumer products to influence demand. Economic theories emphasize the informational content of advertising: Stigler (1987, p. 243), for example, writes that "advertising may be defined as the provision of information about the availability and quality of a commodity." But advertisers also spend resources trying to

persuade consumers with "creative" content that does not appear to be informative in the Stiglerian sense.[1]

Although laboratory studies in marketing have shown that noninformative content may affect demand, and sophisticated firms use randomized experiments to optimize their advertising content strategy (Stone and Jacobs 2001; Day 2003; Agarwal and Ambrose 2007), academic researchers have rarely used field experiments to study advertising content effects.[2] Chandy et al. (2001) review evidence of advertising effects on consumer behavior and find that "research to date can be broadly classified into two streams: laboratory studies of the effects of ad cues on cognition, affect, or intentions and econometric observational field studies of the effects of advertising intensity on purchase behavior . . . each has focused on different variables and operated largely in isolation of the other" (p. 399).[3] Thus, although we know that attempts to persuade consumers with noninformative advertising are common, we know little about how, and how much, such advertising influences consumer choice in natural settings.

In this paper, we use a large-scale direct-mail field experiment to study the effects of advertising content on real decisions, involving nonnegligible sums, among experienced decision makers. A consumer lender in South Africa randomized advertising content and the interest rate in actual offers to 53,000 former clients (Figures I and II show example mailers).[4] The variation in advertising content comes from eight "features" that varied the presentation of the loan offer. We worked together with the lender to create six features relevant to the extensive literature (primarily from laboratory experiments in psychology and decision sciences) on how "frames" and "cues" may affect choices. Specifically,

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1. For example, see Mullainathan, Schwartzstein, and Shleifer (2008) for evidence on the prevalence of persuasive content in mutual fund advertisements.

2. Levitt and List (2007) discuss the importance of validating laboratory findings in the field.

3. Bagwell's (2007) extensive review of the economics of advertising covers both laboratory and field studies and cites only one randomized field experiment (Krishnamurthi and Raj 1985); only 5 of the 232 empirical papers cited in Bagwell's review address advertising content effects. DellaVigna (2009) reviews field studies in psychology and economics and does not cite any studies on advertising other than an earlier version of this paper. Simester (2004) laments the "striking absence" of randomized field experimentation in the marketing literature. For some exceptions see, for example, Ganzach and Karsahi (1995) and Anderson and Simester (2008), and the literature on direct mail charitable fundraising (e.g., List and Lucking-Reiley [2002]). Several other articles in the marketing literature call for greater reliance on field studies more generally: Stewart (1992), Wells (1993), Cook and Kover (1997), and Winer (1999).

4. The Online Appendix contains additional example mailers.

FIGURE I
Example Letter 1

mailers varied in whether they included a person's photograph on the letter, suggestions for how to use the loan proceeds, a large or small table of example loans, information about the interest rate as well as the monthly payments, a comparison to competitors' interest rates, and mention of a promotional raffle for a cell

FIGURE II
Example Letter 2

phone. Mailers also included two features that were the lender's choice, rather than motivated by a body of psychological evidence: reference to the interest rate as "special" or "low," and mention of speaking the local language. Our research design enables us to

estimate demand sensitivity to advertising content and to compare it directly to price sensitivity.[5]

An additional randomization of the offer expiration date also allows us to study demand sensitivity to deadlines. Our interest in deadline effects is motivated by the fact that firms often promote time-limited offers and by the theoretically ambiguous effect of such time limits on demand. Under neoclassical models, shorter deadlines shoul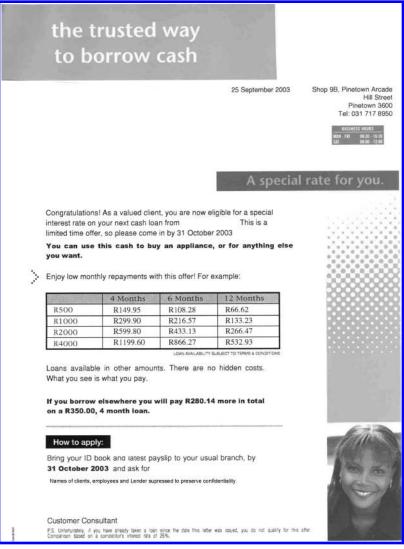d reduce demand, because longer deadlines provide more option value; in contrast, some behavioral models and findings suggest that shorter deadlines will increase demand by overcoming limited attention or procrastination.

Our analysis uncovers four main findings. First, we ask *whether* advertising content affects demand. We use joint *F*-tests across all eight content randomizations and find significant effects on loan takeup (the extensive margin) but not on loan amount (the intensive margin). We do not find any evidence that the extensive margin demand increase is driven by reductions in the likelihood of borrowing from other lenders, nor do we find evidence of adverse selection on the demand response to advertising content: repayment default is not significantly correlated with advertising content. This first finding suggests that traditional demand estimation, which focuses solely on price and ignores advertising content, may produce unstable estimates of demand.

Second, we ask *how much* advertising content affects demand, relative to price. As one would expect, demand is significantly decreasing in price; for example, each 100–basis point (13%) reduction in the interest rate increases loan takeup by 0.3 percentage points (4%). The statistically significant advertising content effects are large relative to this price effect. Showing one example of a possible loan (instead of four example loans) has the same

5. The existing field evidence on the effects of framing and cues does not simultaneously vary price. A large marketing literature using conjoint analysis does this comparison, but is essentially focused on hypothetical choices with no consumption consequences for the respondents; see Krieger, Green, and Wind (2004) for an overview of this literature. In a typical conjoint analysis, respondents are shown or described a set of alternative products and asked to rate, rank or select products from that set. Conjoint analysis is widely applied in marketing to develop and position new products and help with the pricing of products. As discussed in Rao (2008, p. 34), "an issue in the data collection in conjoint studies is whether respondents experience strong incentives to expend their cognitive resources (or devote adequate time and effort) in providing responses (ratings or choices) to hypothetical stimuli presented as profiles or in choice sets." Some recent conjoint analyses have tried to develop more incentive-aligned elicitation methods that provide better estimates of true consumer preferences; see, for example, Ding, Grewal, and Liechty (2005).

estimated effect as a 200–basis point reduction in the interest rate. This finding of a strong positive effect on demand of displaying *fewer* example loans provides novel evidence consistent with the hypothesis that presenting consumers with larger menus can trigger choice avoidance and/or deliberation that makes the advertised product less appealing. We also find that showing a female photo, or not suggesting a particular use for the loan, increases demand by about as much as a 200–basis point reduction in the interest rate.

Third, we provide suggestive evidence on the channels through which persuasive advertising content operates. We classify our content treatments into those that aim to trigger "peripheral" or "intuitive" responses (effortless, quick, and associative) along the lines of Kahneman's (2003) System I, and those that aim to trigger more "deliberative" responses (effortful, conscious, and reasoned) along the lines of Kahneman's (2003) System II. The System II content does not have jointly significant effects on takeup. The System I content does have jointly significant effects on loan takeup. Hence, in our context at least, advertising content appears to be more effective when it aims to trigger an intuitive rather than a deliberative response. However, because the classification of some of our treatments into System I or System II is open to debate, we view this evidence as more suggestive than definitive.

Finally, we report the effects of deadlines on demand. In contrast with the view that shorter deadlines help overcome limited attention or procrastination, we do not find any evidence that shorter deadlines increase demand; rather, we find that demand increases dramatically as deadlines randomly increase from two to six weeks. Nor do we find that shorter deadlines increase the probability of applying early, or that they increase the probability of applying after the deadline. So although our advertising content results point to an important role for persuasion and related psychology, our deadline results tell another story. The option value of longer deadlines seems to dominate in our setting: there is no evidence that shorter deadlines spur action by providing salience or commitment to overcome procrastination.

Overall, our results suggest that seemingly noninformative advertising may play a large role in real consumer decisions. Moreover, insights from controlled laboratory experiments in psychology and decision sciences on how frames and cues affect choice can

be leveraged to guide the design of effective advertising content. It is sobering, though, that we only had modest success predicting (based on the prior evidence) which *specific* content features would significantly impact demand. One interpretation of this failure is that we lacked the statistical power to identify anything other than large effects of any single content treatment, but it is also likely that some the findings generated in other contexts did not carry over to ours. This fits with a central premise of psychology—that context matters—and suggests that pinning down which effects matter most in particular market settings will require systematic field experimentation.

The paper proceeds as follows. Section II describes the market and our cooperating lender. Section III details the experimental and empirical strategies. Section IV provides a conceptual framework for interpreting the results. Section V presents the empirical results. Section VI concludes.

II. The Market Setting

Our cooperating consumer lender (the "Lender") had operated for over twenty years as one of the largest, most profitable lenders in South Africa.[6] The Lender competed in a "cash loan" market segment that offers small, high-interest, short-term, uncollateralized credit with fixed monthly repayment schedules to the working poor population. Aggregate outstanding loans in the cash loan market segment equal about 38% of nonmortgage consumer debt.[7] Estimates of the proportion of the South African working-age population currently borrowing in the cash loan market range from below 5% to around 10%.[8]

Cash loan borrowers generally lack the credit history and/or collateralizable wealth needed to borrow from traditional institutional sources such as commercial banks. Data on how borrowers use the loans are scarce, because lenders usually follow the "no questions asked" policy common to consumption loan markets. The available data suggest a range of consumption smoothing

6. The Lender was merged into a bank holding company in 2005 and no longer exists as a distinct entity.
7. Cash loan disbursements totaled approximately 2.6% of all household consumption and 4% of all household debt outstanding in 2005. (Sources: reports by the Department of Trade and Industry, Micro Finance Regulatory Council, and South African Reserve Bank.)
8. Sources: reports by Finscope South Africa and the Micro Finance Regulatory Council.

and investment uses, including food, clothing, transportation, education, housing, and paying off other debt.[9]

Cash loan sizes tend to be small relative to the fixed costs of underwriting and monitoring them, but substantial relative to a typical borrower's income. For example, the Lender's median loan size of 1,000 rand (about $150) was 32% of its median borrower's gross monthly income. Cash lenders focusing on the highest-risk market segment typically make one–month maturity loans at 30% interest *per month*. Informal sector moneylenders charge 30%–100% per month. Lenders targeting lower-risk segments charge as little as 3% per month, and offer longer maturities (twelve months or more).[10]

Our cooperating Lender's product offerings were somewhat differentiated from those of competitors. It had a "medium-maturity" product niche, with a 90% concentration of four- month loans, and longer loan terms of six, twelve, and eighteen months available to long-term clients with good repayment records. Most other cash lenders focus on one-month or twelve plus–month loans. The Lender's standard four-month rates, absent this experiment, ranged from 7.75% to 11.75% per *month* depending on assessed credit risk, with 75% of clients in the high-risk (11.75%) category. These are "add-on" rates, where interest is charged up front over the original principal balance, rather than over the declining balance. The implied annual percentage rate (APR) of the modal loan is about 200%. The Lender did not pursue collection or collateralization strategies such as direct debit from paychecks, or physically keeping bank books and ATM cards of clients, as is the policy of some other lenders in this market. The Lender's pricing was transparent, with no surcharges, application fees, or insurance premiums.

Per standard practice in the cash loan market, the Lender's underwriting and transactions were almost always conducted in person, in one of over 100 branches. Its risk assessment technology combined centralized credit scoring with decentralized loan officer discretion. Rejection was common for new applicants (50%) but less so for clients who had repaid successfully in the past (14%).

9. Sources: data from this experiment (survey administered to a sample of borrowers following finalization of the loan contract); household survey data from other studies on different samples of cash loan market borrowers (FinScope 2004; Karlan and Zinman forthcoming a).

10. There is essentially no difference between these nominal rates and corresponding real rates. For instance, South African inflation was 10.2% *per year* from March 2002 to March 2003 and 0.4% per year from March 2003 to March 2004.

Reasons for rejection include inability to document steady wage employment, suspicion of fraud, credit rating, and excessive debt burden.

Borrowers had several incentives to repay despite facing high interest rates. Carrots included decreasing prices and increasing future loan sizes following good repayment behavior. Sticks included reporting to credit bureaus, frequent phone calls from collection agents, court summons, and wage garnishments. Repeat borrowers had default rates of about 15%, and first-time borrowers defaulted twice as often.

Policymakers and regulators in South Africa encouraged the development of the cash loan market as a less expensive substitute for traditional "informal sector" moneylenders. Since deregulation of the usury ceiling in 1992, cash lenders have been regulated by the Micro Finance Regulatory Council. The regulation requires that monthly repayment not exceed a certain proportion of monthly income, but no interest rate ceilings existed at the time of this experiment.

III. Experimental Design, Implementation, and Empirical Strategy

III.A. Overview

We identify and price the effects of advertising content and deadlines, using randomly and independently assigned variation in the description and price of loan offers presented in direct mailers. The Lender sent direct mail solicitations to 53,194 former clients offering each a new loan, at a randomly assigned interest rate, with a randomly assigned deadline for taking up the offer. The offers were presented with randomly assigned variations on eight advertising content "features" detailed below and summarized in Table I.

III.B. Sample Frame Characteristics

The sample frame consisted entirely of experienced clients. Each of the 53,194 solicited clients had borrowed from the Lender within 24 months of the mailing date, but not within the previous 6 months.[11] The mean (median) number of prior loans from the

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11. This sample is slightly smaller than the samples analyzed in two companion papers because a subset of mailers did not include the advertising content treatments. See Appendix 1 of Karlan and Zinman (2008) for details.

TABLE I
EXPERIMENTAL SUMMARY

Creative content and its hypothesized effects on demand	Treatment value	Frequency	Sample frame/conditions
	Features 1–3: System I (intuitive processing) treatments		
Feature 1: Photo	No photo	0.20	All
	Black photo	0.48	Assigned conditional on client's race to produce the targeted ratio of client-photo matches
	Non-black photo:		
	Indian	0.13	
	White	0.12	
	Colored	0.07	
Match increases due to affinity/simliarity	Photo with race matched to client race	0.53	
	Photo with mismatched race	0.27	
Female increases due to affective response	Female photo	0.40	
	Male photo	0.40	
Match increases due to affinity/simliarity	Photo with gender matched to client gender	0.40	
	Photo with mismatched gender	0.40	
Feature 2: Number of example loans	One loan amount shown in example table	0.43	All
One loan increases: simplified choice avoids "choice overload" problem	Of low and medium risk clients	0.15	
	Of high risk clients	0.52	
	Four loan amounts shown in example table	0.57	
	Four loan amounts in table, one maturity (high risk clients)	0.48	

TABLE I
(CONTINUED)

Creative content and its hypothesized effects on demand	Treatment value	Frequency	Sample frame/conditions
	Four loan amounts in table, one maturity (low/med risk clients)	0.75	
	Four loan amounts in table, three maturities (low/med risk clients)	0.10	Only low and medium risk eligible for 4 amount, 3 maturity treatment
Feature 3: Interest rate shown in example(s)?	Interest rate shown (and monthly payments)	0.80	All
Indeterminate: several potentially counteracting channels (see Section III.F of text for details)	Interest rate not shown (just monthly payments)	0.20	
	Features 4–6: System II (deliberative processing) treatments		
Feature 4: Suggested loan uses	"You can use this loan for anything you want"	0.20	All
No suggested use maximizes demand, because suggesting particular uses triggers deliberation and reinforces the status quo (not borrowing)	"You can use this loan to X, or for anything else you want", where X is:		
	Pay off a more expensive debt	0.20	
	Buy an appliance	0.20	
	Pay for school	0.20	
	Repair your home	0.20	

TABLE I
(CONTINUED)

Creative content and its hypothesized effects on demand	Treatment value	Frequency	Sample frame/conditions
	Feature 5: Comparison to outside rate		
Comparison increases by inducing choice of dominating (Lender's) option	No comparison to competitor rates	0.20	All
	Gain frame	0.40	
Loss frame increases by triggering loss aversion	Loss frame	0.40	
	Feature 6: Cell phone raffle		
Indeterminate: mentioning increases if overestimate small probabilities, but decreases if reason-based choice and can't justify irrelevant good	Mentioned cell phone raffle	0.25	All
	Not mentioned cell phone raffle	0.75	
	Features 7 and 8: Lender-imposed treatments		
Feature 7: Client's language	No mention of language	0.37	
	"We speak [client's language]"	0.63	Eligible if non-English primary language (0.44 of full sample)
Feature 8: "A 'special' or 'low' rate for you"	Interest rate is labeled as "special" or "low"	0.75	All

TABLE I
(CONTINUED)

Creative content and its hypothesized effects on demand	Treatment value	Frequency	Sample frame/conditions
	No mention of "special" or "low"	0.25	
	Other treatments		
Interest rate	High risk: [3.25, 11.75] Medium risk: [3.25, 9.75] Low risk: [3.25, 7.75]		Monthly rates randomly assigned from a smooth distribution, conditional on risk
Deadline	Medium deadline (approx. 4 weeks)	0.78	1.0 of sample eligible for medium
	Long deadline (approx. 6 weeks)	0.14	0.79 of sample eligible for long (certain branches excluded by Lender)
	Short deadline (approx. 2 weeks)	0.03	0.14 of sample eligible for short (certain branches excluded by Lender, and all P.O. boxes excluded)
	Short deadline with option to extend 2 weeks by calling in	0.04	

TABLE II
SUMMARY STATISTICS (MEANS OR PROPORTIONS, WITH STANDARD DEVIATIONS
IN PARENTHESES)

	Full sample	Obtained a loan	Did not obtain a loan
Applied before deadline	0.085	1	0.01
Obtained a loan before deadline	0.074	1	0
Loan amount in rand	110	1,489	0
	(536)	(1,351)	(0)
Loan in default		0.12	
Got outside loan and did not apply with Lender	0.22	0.00	0.24
Maturity = 4 months		0.81	
Offer rate	7.93	7.23	7.98
Last loan amount in rand	1,118	1,158	1,115
	(829)	(835)	(828)
Last maturity = 4 months	0.93	0.91	0.93
Low risk	0.14	0.30	0.12
Medium risk	0.10	0.21	0.10
High risk	0.76	0.50	0.78
Female	0.48	0.49	0.48
Predicted education (years)	6.85	7.08	6.83
	(3.25)	(3.30)	(3.25)
Number previous loans with Lender	4.14	4.71	4.10
	(3.77)	(4.09)	(3.74)
Months since most recent loan with Lender	10.4	6.19	10.8
	(6.80)	(5.81)	(6.76)
Race = African	0.85	0.85	0.85
Race = Indian	0.03	0.03	0.03
Race = White	0.08	0.08	0.08
Race = Mixed ("Colored")	0.03	0.04	0.03
Gross monthly income in rand	3,416	3,424	3,416
	(19,657)	(2,134)	(20,420)
Number of observations	53,194	3,944	49,250

Lender was four (three). The mean and median time elapsed since
the most recent loan from the Lender was 10 months. Table II
presents additional descriptive statistics on the sample frame.

These clients had received mail and advertising solicitations
from the Lender in the past.[12] The Lender sent monthly state-
ments to clients and periodic reminder letters to former clients
who had not borrowed recently. But prior to our experiment none

12. Mail delivery is generally reliable and quick in South Africa. Two percent
of the mailers in our sample frame were returned as undeliverable.

of the solicitations had varied interest rates, systematically varied advertising content, or included any of the content or deadline features we tested other than the cell phone raffle.

III.C. Identification and Power

We estimate the impact of advertising content on client choice using empirical tests of the form

(1)
$$Y_i = f(r_i, c_i^1, c_i^2, \ldots, c_i^{13}, d_i, X_i),$$

where Y is a measure of client i's loan demand or repayment behavior, r is the client's randomly assigned interest rate, and $c^1, \ldots, c^{13}$ are categorical variables in the vector C_i of randomly assigned variations on the eight different content features displayed (or not) on the client's mailer (we need thirteen categorical variables to capture the eight features because several of the features are categorical, not binary). Most interest rate offers were discounted relative to standard rates, and clients were given a randomly assigned deadline d_i for taking up the offer. All randomizations were assigned independently, and hence orthogonal to each other by construction, after controlling for the vector of randomization conditions X_i.

We ignore interaction terms, given that we did not have any strong priors on the existence of interaction effects across treatments. Below, we motivate and detail our treatment design and priors on the main effects and groups of main effects.

Our inference is based on several different statistics obtained from estimating equation (1). Let β^r be the probit marginal effect or OLS coefficient for r, and $\beta^1, \ldots, \beta^{13}$ be the marginal effects or OLS coefficients on the advertising content variables from the same specification. We estimate *whether* content affects demand by testing whether the β^n's are jointly different from zero. We estimate the *magnitude* of content effects by scaling each β^n by the price effect β^r.

Our sample of 53,194 offers, which was constrained by the size of the Lender's pool of former clients, is sufficient to identify only economically large effects of individual pieces of advertising content on demand. To see this, note that each 100–basis point reduction in r (which represents a 13% reduction relative to the sample mean interest rate of 793 basis points) increased the client's application likelihood by 3/10 of a percentage point. The Lender's standard takeup rate following mailers to inactive former clients

was 0.07. Standard power calculations show that identifying a content feature effect that was equivalent to the effect of a 100–basis point price reduction (i.e., that increased takeup from 0.07 to 0.073) would require over 300,000 observations. So in fact we can only distinguish individual content effects from zero if they are equivalent to a price reduction of 200 to 300 basis points (i.e., a price reduction of 25% to 38%).

III.D. *Measuring Demand and Other Outcomes*

Clients revealed their demand by their *takeup* decision, that is, by whether they applied for a loan at their local branch before their deadlines. Loan applications were assessed and processed using the Lender's normal procedures. Clients were not required to bring the mailer with them when applying, and branch personnel were trained and monitored to ignore the mailers. To facilitate this, each client's randomly assigned interest rate was hard-coded *ex ante* into the computer system the Lender used to process applications. Alternative measures of demand include obtaining a loan and the amount borrowed. The solicitations were "pre-approved" based on the client's prior record with the Lender, and hence 87% of applications resulted in a loan.[13] Rejections were due to adverse changes in the client's work status, ease of contact by phone, or other indebtedness.

We consider two other outcomes. We measure outside borrowing using credit bureau data. We also examine loan repayment behavior by setting $Y = 1$ if the account was in *default* (i.e., in collection or written off as uncollectable as of the latest date for which we had repayment data), and $= 0$ otherwise. The motivating question for this outcome variable is whether any demand response to advertising content produces adverse selection by attracting clients who are induced to take loans they cannot afford. Note that we have less power for this outcome variable, because we only observe repayment behavior for the 4,000 or so individuals that obtained a loan.

III.E. *Interest Rate Variation*

The interest rate randomization was stratified by the client's preapproved risk category because risk determined the loan price

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13. All approved clients actually took loans. This is not surprising given the short application process (45 minutes or less), the favorable interest rates offered in the experiment, and the clients' prior experience and hence familiarity with the Lender.

under standard operations. The standard schedule for four-month loans was low-risk = 7.75% per *month*; medium-risk = 9.75%; high-risk = 11.75%. The randomization program established a target distribution of interest rates for four-month loans in each risk category and then randomly assigned each individual to a rate based on the target distribution for his or her category.[14,15] Rates varied from 3.25% per month to 11.75% per month, and the target distribution varied slightly across two "waves" (bunched for operational reasons) mailed September 29–30 and October 29–31, 2003. At the Lender's request, 97% of the offers were at lower-than-standard rates, with an average discount of 3.1 percentage points on the monthly rate (the average rate on prior loans was 11.0%). The remaining offers in this sample were at the standard rates.

III.F. Mailer Design: Content Treatments, Motivation, and Priors

Figures I and II show example mailers. The Lender designed the mailers in consultation with its South African–based marketing consulting firm and us. Each mailer contained some boilerplate content; for example, the Lender's logo, its slogan "the trusted way to borrow cash," instructions for how to apply, and branch hours. Each mailer also contained mail merge fields that were populated (or could be left blank in some cases) with randomized variations on the eight different advertising content features. Some randomizations were conditional on preapproved characteristics, and each of these conditions is included in the empirical models we estimate.

The content and variations for each of the features are summarized in Table I. We detail the features below along with some prior work and hypotheses underlying these treatments.

14. Rates on other maturities in these data were set with a fixed spread from the offer rate conditional on risk, so we focus exclusively on the four-month rate.

15. Actually *three* rates were assigned to each client: an "offer rate" included in the direct mail solicitation and noted above, a "contract rate" (r^c) that was weakly less than the offer rate and revealed only after the borrower had accepted the solicitation and applied for a loan, and a dynamic repayment incentive (D) that extended preferential contract rates for up to one year, conditional on good repayment performance, and was revealed only after all other loan terms had been finalized. This multitiered interest rate randomization was designed to identify specific information asymmetries (Karlan and Zinman forthcoming b). Because D and r^c were surprises to the client, and hence did not affect the decision to borrow, we exclude them from most analysis in this paper. In principle, r^c and D might affect the intensive margin of borrowing, but in practice adding these interest rates to our loan size demand specifications does not change the results. Mechanically what happened was that very few clients changed their loan amounts after learning that $r^c < r$.

We group the content treatments along two thematic lines. The first, and more important, thematic grouping is based on whether the content is more likely to trigger an intuitive or reasoned response. Such a distinction between intuitive and deliberative modes is common in much of the decision research on cognitive functions.[16] The deliberative or reasoning mode (Kahneman's [2003] System II) is what we do when we carry out a mathematical computation, or plan our travel to an upcoming conference. The peripheral or intuitive mode (Kahneman's [2003] System I) is at work when we smile at a picture of puppies playing, or recoil at the thought of eating a cockroach (Rozin and Nemeroff 2002). Intuition is relatively effortless and automatic, whereas reasoning requires greater processing capacity and attention. Research on persuasion suggests that the effect of content will depend on which System(s) the content triggers, and on the underlying intentions of the consumer (Petty and Cacioppo 1986; Petty and Wegener 1999). Content that triggers "central processing," or conscious deliberation, may be more effective when the product offer is consistent with the consumer's intentions; for example, a consumer who is actively shopping for a loan may be persuaded most by quantitative cost or location comparisons. Content that triggers "peripheral processing," or intuition, may be more effective when the offer is less aligned with intentions; for example, a consumer may be more persuaded to order a beer by a poster showing beautiful people sipping beer at sunset than by careful arguments about beer's merits. We group the content treatments below by whether they were more likely to trigger System I or System II responses, and highlight where our classification is debatable.

The second thematic grouping is based on whether the treatment was motivated more by a body of prior evidence (and hence the researchers' priors) or by the Lender's priors.

System I Treatments

Feature 1: photo. Visual (largely uninformative) images tend to be processed through intuitive cognitive systems. This may explain why visuals play such a large role in advertising. Mandel and Johnson (2002), for example, find that randomly manipulated background images affect hypothetical student choices in a

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16. See, for example, Chaiken and Trope (1999), Slovic et al. (2002), and Stanovich and West (2002). Kahneman (2003) refers to the intuitive and deliberative modes as System I and System II in his Nobel lecture.

simulated Internet shopping environment. Our mailers test the effectiveness of visual cues by featuring a photo of a smiling person in the bottom right-hand corner in 80% of the mailers. There was one photo subject for each combination of gender and race represented in our sample (for a total of eight different photos).[17] All subjects were deemed attractive and professional-looking by the marketing firm. The overall target frequency for each photo was determined by the racial and gender composition of the sample and the objective was to obtain a 2-to-1 ratio of photo race that matched the client's race and a 1-to-1 ratio of photo gender that matched the client's gender.[18]

Several prior studies suggested that matching photos to client race or gender would increase takeup by triggering intuitive affinity between the client and Lender. Evans (1963) finds that demographic similarity between client and salesperson can drive choice, and several studies find that similarity can outweigh even expertise or credibility (see, e.g., Lord [1996]; Cialdini [2001]; Mobius and Rosenblat [2006]).

We also predicted that a photo of an attractive woman would (weakly) increase takeup. This prior was based on casual empiricism (e.g., of beer and car ads) and a field experiment on door-to-door charitable fundraising in which attractive female solicitors secured significantly more donations (Landry et al. 2006).

Feature 2: number of example loans. The middle of each mailer prominently featured a table that was randomly assigned to display one or four example loans. Each example showed a loan amount and maturity based on the client's most recent loan and a monthly payment based on the assigned interest rate.[19] The rate itself was also displayed in randomly chosen mailers (see

17. For mailers with a photo, the employee named at the bottom of the mailer was that of an actual employee of the same race and gender featured in the photo. In cases where no employee in the client's branch had the matched race and gender, an employee from the regional office was listed instead.

18. If the client was assigned randomly to "match," then the race of the client matched that of the model on the photograph. For those assigned to mismatch, we randomly selected one of the other races. 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.

19. High risk clients were not eligible for six- or twelve-month loans, and hence their four-example tables featured four loan amounts based on small increments above the client's last loan amount. When the client was eligible for longer maturities we randomly assigned whether the four-example table featured different maturities. See Table II and Karlan and Zinman (2008) for additional details.

Feature 3). Small tables were nested in the large tables, to en-
sure that large tables contained more information. Every mailer
stated "Loans available in other amounts . . . " directly below the
example(s) table.

Our motivation for experimenting with a small vs. large table
of loans comes from psychology and marketing papers on "choice
overload." In strict neoclassical models demand is (weakly) *in-
creasing* in the number of choices. In contrast, the choice over-
load literature has found that demand can *decrease* with menu
size. Large menus can "demotivate" choice by creating feelings of
conflict and indecision that lead to procrastination or total inac-
tion (Shafir, Simonson, and Tversky 1993). Overload effects have
been found in field settings including physician prescriptions (Re-
delmeier and Shafir 1995) and 401k plans (Iyengar, Huberman,
and Jiang 2004). An influential field experiment shows that gro-
cery store shoppers who stopped to taste jam were much more
likely to purchase if there were 6 choices rather than 24 (Iyengar
and Lepper 2000).

Prior studies suggest that demotivation happens largely be-
yond conscious awareness, and hence largely through intuitive
processing. (In fact, the same people who are demotivated by
choice overload often state an *a priori* preference for larger choice
sets.) We therefore group our number of loans feature with Sys-
tem I. (There may be other contexts where menu size triggers
conscious deliberation, e.g., where a single loan may signal a cus-
tomized offer, or where multiple loans may signal full disclosure.
But this was unlikely to be the case here, given the sample's prior
experience with the Lender and common knowledge on the nature
and availability of different loan amounts.)

Feature 3: interest rate(s) shown in example(s)? Example
loan tables also randomly varied whether the interest rate was
shown.[20] In cases where the interest rate was suppressed, the
information presented in the table (loan amount, maturity, and
monthly payment) was sufficient for the client to impute the rate.
This point was emphasized with the statement below the table
that "There are no hidden costs. What you see is what you pay."

Displaying the interest rate has ambiguous effects on demand
in rich models of consumer choice. Displaying the rate may de-
press demand by overloading boundedly rational consumers (see

20. South African law did not require interest rate disclosure, in contrast to
the U.S. Truth-in-Lending Act.

Feature 2), or by debiasing consumers who tend to underestimate rates when inferring them from other loan terms (Stango and Zinman 2009). Displaying the rate may have no effect if consumers do not understand interest rates and use decision rules based on other loan terms (this was the Lender's prior). Finally, displaying the rate may induce demand by signaling that the Lender indeed has "no hidden costs," reducing computational burden, and/or clarifying that the rate is, indeed, low. Despite the potential for offsetting effects (and hence our lack of strong priors), we thought that testing this feature would be thought-provoking nonetheless, given policy focus on interest rate disclosure (Kroszner 2007).

Given the Lender's prior that interest rate disclosure would not affect demand, and its branding strategy as a "trusted" source for cash, it decided to err on the side of full disclosure and display the interest rate on the mailers with 80% probability. The interest rate feature is perhaps the most difficult one to categorize. Although it could trigger a "System II" type computation, the Lender's prior suggests that any effect would operate mostly as an associative or emotional signal of openness and trust. So we group rate disclosure with System I and also show below that the results are robust to dropping it from the System I grouping.

System II Treatments

Feature 4: suggested uses. After the salutation and deadline, the mailer said something about how the client could use the loan. This "suggested use" appeared in boldface type and was one of five variations on "You can use this loan to X, or for anything else you want." X was one of four common uses for cash loans indicated by market research and detailed in Table I. The most general phrase simply stated, "You can use this cash for anything you want." Each of the five variations was randomly assigned with equal probabilities.

We group this treatment with System II on the presumption that highlighting intended use would trigger client deliberation about potential uses and whether to take a loan. Because clients had revealed a preference for not taking up a loan in recent months, we presume that conscious deliberation would not likely change this preference. Hence we predicted that takeup would be maximized by *not* suggesting a particular use.[21]

21. We cannot rule out other cognitive mechanisms that could affect the response to suggested loan uses or the interpretation of an effect here. Suggesting a

Feature 5: comparison to outside rate. Randomly chosen mailers included a comparison of the offered interest rate to a higher outside market rate. When included, the comparison appeared in boldface in the field below "Loans available in other amounts. . . ." Half of the comparisons used a "gain frame"; for example, "If you borrow from us, you will pay R100 rand less each month on a four month loan." Half of the comparisons used a "loss frame"; for example, "If you borrow elsewhere, you will pay R100 rand more each month on a four month loan."[22]

Several papers have found that such frames can influence choice by manipulating "reference points" that enter decision rules or preferences. There is evidence that the presence of a dominated alternative can induce choice of the dominating option (Huber, Payne, and Puto 1982; Doyle et al. 1999). This suggests that mailers with our dominated comparison rate should produce (weakly) higher takeup rates than mailers without mention of a competitor's rate. Any dominance effect probably operates by inducing greater deliberation (Priester et al. 2004), and presenting a reason for choosing the dominating option (Shafir, Simonson, and Tversky 1993), particularly because the comparison is presented in text. Invoking potential losses may be a particularly powerful stimulus for demand if it triggers loss aversion (Kahneman and Tversky 1979; Tversky and Kahneman 1991), and indeed Ganzach and Karsahi (1995) find that a loss-framed message induced significantly higher credit card usage than a gain-framed message in a direct marketing field experiment in Israel. This suggests that the loss-framed comparison should produce (weakly) higher takeup rates than either the gain-frame or the no-comparison conditions.

Feature 6: cell phone raffle. Many firms, including the Lender and many of its competitors, use promotional giveaways as part

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particular use might make consumption salient and serve as a cue to take up the loan (although this sort of associative response may be difficult to achieve with text, which typically triggers more deliberative processing). Yet another possibility is that suggesting a particular use creates dissonance with the Lender's "no questions asked" policy regarding loan uses, a policy designed to counteract the stigma associated with high-interest borrowing. In any case, it is unlikely that suggesting a particular use provided *information* by (incorrectly) signaling a policy change regarding loan uses, because each variation ended with "or for anything else you want."

22. The mailers also randomized the unit of comparison (rand per month, rand per loan, percentage point differential per month, percentage point differential per loan), but the resulting cell sizes are too small to statistically distinguish any differential effects of units on demand.

of their marketing. Our experiment randomized whether a cell phone raffle was prominently featured in the bottom right margin of the mailer: "WIN 10 CELLPHONES UP FOR GRABS EACH MONTH!" Per common practice in the cash loan market, the mailers did not detail the odds of winning or the value of the prizes. In fact, the expected value of the raffle for any individual client was vanishingly small.[23] This implies that the raffle should not change the takeup decision based on strictly economic factors.[24]

Yet marketing practice suggests that promotional raffles may increase demand despite not providing any material increase in the expected value of taking up the offer. A possible channel is a tendency for individuals to overestimate the frequency of low-probability events. In contrast, several papers have reached the surprising conclusion that promotional giveaways can backfire and reduce demand. The channel seems to be "reason-based choice" (Shafir, Simonson, and Tversky 1993): many consumers feel the need to justify their choices and find it more difficult to do so when the core product comes with an added feature they do not value. This holds even when subjects understand that the added option comes at no extra pecuniary or time cost (Simonson, Carmon, and O'Curry 1994).

Given the conflicting prior evidence, we had no strong prior on whether promoting the cell phone raffle would affect demand. Because both postulated cognitive channels seem to operate through conscious (if faulty) reasoning, we classify the raffle as a System II treatment.

Lender-Imposed Treatments. Two additional treatments were motivated by the Lender's choices and the low-cost nature of content testing, rather than by a body of prior evidence on consumer decision making.

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23. The 10 cell phones were each purchased for R300 and randomly assigned within the pool of approximately 10,000 individuals who applied at the Lender's branches during the 3 months spanned by the experiment. The pool was much larger than the number of applicants who received a mailer featuring the raffle, because by law all applicants (including first-time applicants, and former clients excluded from our sample frame) were eligible for the raffle.

24. The raffle could be economically relevant if the Lender's market were perfectly competitive. In that case, and where raffles are part of the equilibrium offer, then *not* offering the small-value raffle could produce a sharp drop in demand (because potential clients would be indifferent on the margin between borrowing from the Lender or from competitors when offered the raffle, but would weakly prefer a competitor's offer when the Lender did not offer the raffle). But the cash loan market seems to be imperfectly competitive: see Section II, and the modest response to price reductions in Section V.A.

Feature 7: language affinity. Some mailers featured a blurb "We speak (client's language)" for a random subset of the clients who were not primarily English speakers (44% of the sample). When present, the matched language blurb was directly under the "business hours" box in the upper right of the mailer. The rest of the mailer was always in English. The Lender was particularly confident that the language affinity treatment would increase takeup and insisted that most eligible clients get it; hence the 63–37 split noted in Table I.

In contrast to matched photos, we did not think that the "language affinity" was well motivated by laboratory evidence or that it would increase takeup. The difference is one of medium. The language blurb was in text, and hence more likely to be processed through deliberative cognitive systems, where linguistic affinity was unlikely to prove particularly compelling. Photos are more likely to be processed through intuitive and emotional systems. The laboratory evidence suggests that affinities work through intuitive associations (System I) rather than through reasoning (System II).

Feature 8: "special" rate vs. "low" rate vs. no blurb. As discussed above, nearly all of the interest rate offers were at discounted rates, and the Lender had never offered anything other than its standard rates prior to the experiment. So the Lender decided to highlight the unusual nature of the promotion for a random subset of the clients: 50% of clients received the blurb "A special rate for you," and 25% of clients received "A low rate for you." The mail merge field was left blank for the remaining clients. When present, the blurb was inserted just below the field for the language match.

Our prior was that this treatment would not influence takeup, although there may be models with very boundedly rational consumers and credible signaling by firms where showing one of these blurbs would (weakly) increase takeup.

III.G. Deadlines

Each mailer also contained a randomly assigned deadline by which the client had to respond to obtain the offered interest rate. Deadlines ranged from "short" (approximately two weeks) to "long" (approximately six weeks). Short deadlines were assigned only among clients who lived in urban areas with a non–P.O. Box mailing address and hence were likely to receive their mail quickly

(see Table I for details). Some clients eligible for the short deadline were randomly assigned a blurb showing a phone number to call for an extension (to the medium deadline).

Our deadline randomization was motivated by advertising practices, which often promote limited-time offers, and by decision research on time management. Some behavioral models predict that shorter deadlines will boost demand by overcoming a tendency to procrastinate and postpone difficult decisions or tasks. Indeed, the findings in Ariely and Wertenbroch (2002), and introspection, suggest that many individuals choose to impose shorter deadlines on themselves even when longer ones are in the choice set. In contrast, standard economic models predict that consumers will always (weakly) prefer the longest available deadline, all else equal, due to the option value of waiting.

IV. Conceptual Framework: Interpreting the Effects of Advertising Content

As discussed above, the advertising content treatments in our experiment were motivated primarily by findings from psychology and marketing that are most closely related to theories of *persuasive* advertising. Here we formalize definitions of persuasion and other mechanisms through which advertising content might affect consumer choice. We also speculate on the likely relevance of these different mechanisms in our research context.

As a starting point, consider a simple decision rule where consumers purchase a product if and only if the marginal cost of the product is less than the expected marginal return (in utility terms) of consuming the product. A very simple way to formalize this is to note that the consumer purchases (loan) product (or consumption bundle) l iff

$$(2) \qquad u_i(l) - p_i > 0,$$

where u_i is the consumer's (discounted) utility gain from purchasing l and p is the price.[25] Advertising has no effect on either u or p and the model predicts that we will not reject the hypothesis

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25. In our context p is a summary statistic capturing the cost of borrowing. Without liquidity constraints the discounted sum of any fees + the periodic interest rate captures this cost. Under liquidity constraints, loan maturity affects the effective price as well (Karlan and Zinman 2008).

of null effects of advertising content on demand when estimating equation (1).

One might wonder whether a very slightly enriched model would predict that consumers who are just indifferent about borrowing (from the Lender) might be influenced by advertising content (say by changing the consumer from indifference to "go with the choice that has the attractive mailer"). This would be a more plausible interpretation in our setting if the experiment's prices were more uniform and standard, given that everyone in the sample had borrowed recently at the Lender's standard rates. But the experimental prices ranged widely, with a density almost entirely below the standard rates. Thus if consumers were indifferent on average in our sample then price reductions should have huge positive effects on takeup on average. This is not the case; Section V.A shows that takeup elasticities for the price reductions are substantially below one in absolute value.

Models in the "behavioral" decision-making and economics-of-advertising literatures enrich the simple decision rule in equation (2) and allow for the possibility that advertising affects consumer behavior, that is, for the possibility that the average effect of the advertising content variables in equation (1) is different from 0. Following Bagwell's (2007) taxonomy, we explore three distinct mechanisms.

A first possible mechanism is *informative* advertising content. Here the consumer has some uncertainty about the utility gain and/or price (which could be resolved by a consumer at a search and/or computational cost), and advertising operates through the consumer's *expectations* about utility and price. Now the consumer buys the product if

$$(3) \qquad E_t^u(\boldsymbol{C}_{it})[u_i(l)] - E_t^p(\boldsymbol{C}_{it})_t[p_i] > 0,$$

where expectations E at time t are influenced by the vector of advertising content $\boldsymbol{C}$ that consumer i receives.

In our setting, for example, announcing that the firm speaks Zulu might provide information. The content treatments might also affect expected utility through credible signaling. Seeing a photo on the mailer might increase the client's expectation of an enjoyable encounter with an attractive loan officer at the Lender's branch.

Our experimental *design* does not formally rule out these sorts of informative effects, but we do not find them especially

plausible in this particular implementation. Recall that the mailers were sent exclusively to clients who successfully repaid prior loans from the Lender. Most had been to a branch within the past year and hence were familiar with the loan product, the transaction process, the branch's staff and general environment, and the fact that loan uses are unrestricted.

A second possibility is that advertising is *complementary* to consumption: consumers have fixed preferences, and advertising makes the consumer "believe—correctly or incorrectly—that it [sic] gets a greater output of the commodity from a given input of the advertised product" (Stigler and Becker 1977). In reduced form, this means that advertising affects net utility by interacting with enjoyment of the product. So the consumer purchases if

$$(4) \qquad u_i(l, \, l^*\boldsymbol{C}_i) - p_i > 0.$$

Our design does not formally rule out complementary mechanisms, but their relevance might be limited in our particular implementation. Complementary models tend to be motivated by luxury or prestige goods (e.g., cool advertising content makes me enjoy wearing a Rolex more, all else equal), and the product here is an intermediate good that is used most commonly to pay for necessities. Moreover, the first-hand prior experience our sample frame had with consumer borrowing makes it unlikely that marketing content would change perceptions of the loan product in a complementary way.

Finally, a third mechanism is *persuasive* advertising content. A simple model of persuasion would be that the true utility of purchase is given by $u_i(l) - p_i$. But individuals decide to purchase or not based on

$$(5) \qquad D_i(u_i(l), \, \boldsymbol{C}_i) - p_i > 0,$$

where $D_i(u_i(l), \boldsymbol{C}_i)$ is the effective decision, rather than hedonic utility. Persuasion can operate directly on preferences by manipulating reference points, providing cues that increase the marginal utility of consumption, providing motivation to make (rather than procrastinate) choices, or simplifying the complexity of decision making. Other channels for persuasion arise if perceptions of key decision parameters are *biased* and can be manipulated by advertising content. As discussed above (in Section III.F), content may work through these channels by triggering intuitive and/or deliberative cognitive processes. Note that (5) does *not* allow content to

affect demand by affecting price sensitivity: $D_i(.)$ does not include p_i as an argument.

To clarify the distinction from the informative view, note that allowing for biased expectations or biased perceptions of choice parameters is equivalent to allowing a distinction between *hedonic utility* (i.e., true, experienced utility) and *choice utility* (perceived/expected utility at the time of the decision). Under a persuasive view of advertising, consumers decide based on choice utility. Finally, note that, as in the traditional model, price will continue to affect overall demand. In this sense, there may appear to be a stable demand curve. But the demand curve may shift as content C_i varies. Thus demand estimation that ignores persuasive content may produce a misleading view of underlying utility.

V. RESULTS

This section presents results from estimating equation (1) detailed in Section III.C.

V.A. *Interest Rates*

Consumer sensitivity to the price of the loan offer will provide a useful way to scale the magnitude of any advertising content effects. The first row of econometric results in Table III shows the estimated magnitude of loan demand price sensitivities in our sample.

Our main result on price is that the probability of applying before the deadline (8.5% in the full sample) rose 3/10 of a percentage point for every 100–basis point reduction in the monthly interest rate ((column (1)). This implies a 4% increase in takeup for every 13% decrease in the interest rate, and a takeup price elasticity of −0.28.[26] Column (4) shows a nearly identical result when the outcome is obtaining a loan instead of applying for a loan. Column (5) shows that the total loan amount borrowed (unconditional on borrowing) also responded negatively to price. The implied elasticity here is −0.34.[27] Column (6) shows that default rose with price;

26. Clients were far more elastic with respect to offers at rates greater than the Lender's standard ones (Karlan and Zinman 2008). This small subsample (632 offers) is excluded here because it was part of a pilot wave of mailers that did not include the content randomizations.

27. See Karlan and Zinman (2008) for additional results on price sensitivity on the intensive margin.

TABLE III
EFFECTS OF ADVERTISING CONTENT ON BORROWER BEHAVIOR

Dependent variable	Applied for loan before mailer deadline	Applied for loan before mailer deadline	Applied for loan before mailer deadline	Obtained loan before mailer deadline	Loan amount obtained before mailer deadline	Loan in collection status	Borrowed from other lender
Sample	Full	Males	Females	Full	Full	Obtained	Full
Estimator	Probit	Probit	Probit	Probit	OLS	Probit	Probit
Mean (dependent variable)	0.0850	0.0824	0.0879	0.0741	110.4363	0.1207	0.2183
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Monthly interest rate in percentage point units (e.g., 8.2)	−0.0029*** (0.0005)	−0.0025*** (0.0007)	−0.0034*** (0.0008)	−0.0026*** (0.0005)	−4.7712*** (0.8238)	0.0071*** (0.0022)	0.0009 (0.0008)
1 = no photo	0.0013 (0.0040)	−0.0050 (0.0048)	0.0021 (0.0055)	0.0029 (0.0037)	3.9316 (7.6763)	0.0013 (0.0166)	−0.0024 (0.0060)
1 = female photo (System I: affective response)	0.0057** (0.0026)	0.0079** (0.0034)	0.0032 (0.0038)	0.0056** (0.0024)	8.3292 (5.0897)	−0.0076 (0.0107)	−0.0047 (0.0040)
1 = photo gender matches client's (System I: affinity/similarity)	−0.0026 (0.0026)			−0.0033 (0.0024)	−7.1773 (5.0850)	−0.0059 (0.0107)	0.0041 (0.0040)
1 = photo race matches client's (System I: affinity/similarity)	−0.0056 (0.0048)	−0.0014 (0.0064)	−0.0099 (0.0070)	−0.0035 (0.0044)	9.0638 (10.4079)	0.0181 (0.0176)	−0.0018 (0.0072)
1 = one example loan shown (System I: avoid choice overload)	0.0068** (0.0028)	0.0099*** (0.0038)	0.0031 (0.0040)	0.0075*** (0.0026)	2.4394 (4.8383)	0.0073 (0.0117)	−0.0043 (0.0042)
1 = interest rate shown (System I: several, potentially offsetting, channels)	0.0025 (0.0030)	−0.0017 (0.0042)	0.0073 (0.0044)	0.0043 (0.0028)	2.8879 (6.7231)	0.0140 (0.0123)	0.0007 (0.0049)

TABLE III
(CONTINUED)

Dependent variable	Applied for loan before mailer deadline	Applied for loan before mailer deadline	Applied for loan before mailer deadline	Obtained loan before mailer deadline	Loan amount obtained before mailer deadline	Loan in collection status	Borrowed from other lender
Sample	Full	Males	Females	Full	Full		Full
Estimator	Probit	Probit	Probit	Probit	OLS	Obtained Probit	Probit
Mean (dependent variable)	0.0850	0.0824	0.0879	0.0741	110.4363	0.1207	0.2183
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1 = cell phone raffle mentioned (System II: overestimate small probabilities vs. conflict from reason-based choice)	−0.0023 (0.0026)	−0.0001 (0.0036)	−0.0049 (0.0039)	−0.0013 (0.0025)	−9.4384* (5.1200)	−0.0050 (0.0109)	−0.0015 (0.0041)
1 = no specific loan use mentioned (System II: mentioning specific use, via text, triggers deliberation)	0.0059** (0.0029)	0.0084** (0.0040)	0.0031 (0.0043)	0.0043 (0.0027)	4.0850 (5.6266)	0.0086 (0.0121)	−0.0033 (0.0045)
1 = comparison to competitor rate (System II: makes dominating option salient)	−0.0002 (0.0031)	−0.0012 (0.0043)	0.0010 (0.0046)	0.0012 (0.0029)	−2.6021 (6.2961)	−0.0027 (0.0133)	0.0054 (0.0049)
1 = loss frame comparison (System II: triggers loss aversion)	−0.0024 (0.0026)	−0.0018 (0.0035)	−0.0029 (0.0038)	−0.0021 (0.0024)	3.0925 (5.0678)	0.0032 (0.0108)	0.0027 (0.0040)
1 = we speak "your language" (Lender imposed)	−0.0043 (0.0036)	−0.0016 (0.0049)	−0.0073 (0.0053)	−0.0036 (0.0033)	−11.3556* (6.2935)	−0.0031 (0.0152)	0.0133** (0.0059)
1 = a "low" or "special" rate for you (Lender imposed)	0.0001 (0.0031)	−0.0022 (0.0043)	0.0027 (0.0045)	0.0010 (0.0028)	3.3864 (5.9209)	−0.0137 (0.0128)	−0.0002 (0.0047)
N	53,194	27,848	25,346	53,194	53,194	3,944	53,194

TABLE III
(CONTINUED)

Dependent variable	Applied for loan before mailer deadline	Applied for loan before mailer deadline	Applied for loan before mailer deadline	Obtained loan before mailer deadline	Loan amount obtained before mailer deadline	Loan in collection status	Borrowed from other lender
Sample	Full	Males	Females	Full	Full	Obtained	Full
Estimator	Probit	Probit	Probit	Probit	OLS	Probit	Probit
Mean (dependent variable)	0.0850	0.0824	0.0879	0.0741	110.4363	0.1207	0.2183
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(Pseudo-) r-squared	.0456	.0481	.0438	.0534	.0361	.0674	.0048
p-Value, F-test on all advertising content variables	.0729	.0623	.5354	.0431	.2483	.7485	.4866
Absolute value lower bound of range of joint content effect for which F-test rejects null	0.0010	0.0021		0.0026			
Absolute value upper bound of range of joint content effect for which F-test rejects null	0.0448	0.0388		0.0498			
p-Value, F-test on Lender-imposed content ("low" or "special"; language)	.5064	.8217	.3337	.5254	.1695	.5382	.0785
p-Value, F-test on psychology-motivated content (all other features)	.0522	.0300	.5541	.0286	.3420	.7262	.7583

TABLE III
(CONTINUED)

Dependent variable	Applied for loan before mailer deadline	Applied for loan before mailer deadline	Applied for loan before mailer deadline	Obtained loan before mailer deadline	Loan amount obtained before mailer deadline	Loan in collection status	Borrowed from other lender
Sample	Full	Males	Females	Full	Full	Obtained	Full
Estimator	Probit	Probit	Probit	Probit	OLS	Probit	Probit
Mean (dependent variable)	0.0850	0.0824	0.0879	0.0741	110.4363	0.1207	0.2183
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Split psychology-motivated content:							
p-Value, F-test on System II (reasoning) content (suggested use, comparison, cell)	.1946	.2643	.6200	.4499	.3399	.9360	.4947
p-Value, F-test on System I (intuitive) content (photo, # loans shown, rate shown)	.0598	.0211	.3929	.0127	.4362	.4346	.7675
p-Value, F-test on System I, dropping rate shown	.0355	.0288	.5130	.0072	.3288	.4196	.7169

Notes: Huber–White standard errors. Probit results are marginal effects. All models include controls for randomization conditions: risk, race, gender, language, and mailer wave (September or October). Treatment variable labels: parentheses contain summary description of our prior on why each ad content treatment would increase demand (or of reason(s) why we had no strong prior). Omitted categories: male photo, no photo gender match, no photo race match, four example loans shown, no interest rate shown, no cell phone raffle mentioned, specific loan use mentioned, no comparison to competitor rate, gain frame comparison, no mention of speaking local language, no mention of low or special rate.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

this result indicates adverse selection and/or moral hazard with respect to interest rates.[28] Column (7) shows that more expensive offers did not induce significantly more substitution to other formal sector lenders (as measured from credit bureau data). This result is a precisely estimated zero relative to a sample mean outside borrowing proportion of 0.22. The lack of substitution is consistent with the descriptive evidence discussed in Section II on the dearth of close substitutes for the Lender.

V.B. *Advertising Content Treatments*

Table III also presents the results on advertising content variations for the full sample.

The *F*-tests reported near the bottom of the table indicate *whether* the content features had an effect on demand that was jointly significantly different from zero.[29] The applied (or "takeup") model has a *p*-value of .07 (column (1)), and the "obtained a loan" model has a *p*-value of .04 (column (4)), implying that advertising content did influence the extensive margin of loan demand with at least 90% confidence. Column (5) shows that the joint effect of content on loan amount is insignificant (*p*-value = .25). Column (6) shows an insignificant effect on default; that is, we do not find evidence of adverse selection on response to content. Column (7) shows an insignificant effect on outside borrowing; that is, the positive effect on demand for credit from the Lender in columns (1) and (4) does not appear to be driven by balance-shifting from other lenders.

The results on the individual content variables give some insight into which features affected demand (although some inferential caution is warranted here, because with thirteen content variables we would expect one to be significant purely by chance). Three variables show significant increases in takeup: one example loan, no suggested loan use, and female photo.

The result on one example loan strikes us as noteworthy. It is a clear departure from strict neoclassical models, where more choice and more information weakly increase demand. It replicates prior findings and moreover suggests that choice

28. The finding here is reduced-form evidence of information asymmetries; see Karlan and Zinman (forthcoming b) for additional results that separately identify adverse selection and moral hazard effects.

29. Results are nearly identical if we omit the cell phone raffle from the joint test of content effects on the grounds that the raffle has some expected pecuniary value.

overload can matter even when the amount of content in the "more" condition is small: we test across two small menus, for a product that everyone in our sample has used before.

The effect of the female photo motivates consideration of whether advertising content effects differ by consumer gender; for example, in Landry et al. (2006), male charitable donor prospects respond more to female solicitor attractiveness than female prospects do.[30] Columns (2) and (3) of Table III show that male clients receiving the female photo took up significantly more, but female clients did not. In fact, female clients did not respond significantly to *any* of the content treatments. Males responded to example loans and loan uses, as well as to the female photo. Unsurprisingly, then, the joint F-test for all content variables is significant for male but not for female clients. Note that takeup rates and sample sizes are quite similar across client genders, so these findings are not driven purely by power issues. However, as with other results, the insignificant results for female clients are imprecise, and do not rule out economically large effects of advertising content.

Another notable finding on the individual content variables is the disjoint between our priors and findings. Several treatments we predicted would have significant effects did not (comparisons, and the other photo variables).

Results on the individual content feature variable conditions also provide some insight into how much advertising content affects demand, relative to price. For our preferred outcome (1 = applied), the statistically significant point estimates imply large magnitudes: a mailer with one example loan (or no suggested use, or a female photo) increased takeup by at least as much as a 200–basis point (25%) reduction in the interest rate. Table IV reports the results of this scaling calculation for each of the content point estimates in Table III; that is, it takes the point estimate on a content variable, divides it by the coefficient on the offer rate for that specification, and multiplies the result by 100 to get an estimate of the interest rate reduction needed to obtain the increase in demand implied by the point estimate on the content variable.

The bottom rows of Table III show results for our thematic groupings of content treatments. These results shed some light

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30. The Online Appendix presents results for subsamples split by income, education, and number of prior transactions with the Lender. We do not feature these results because we are underpowered even in the full sample and also lacked strong priors that treatment effects should vary with these other characteristics.

TABLE IV
EFFECTS OF ADVERTISING CONTENT ON BORROWER BEHAVIOR: POINT ESTIMATES IN
TABLE III, SCALED BY PRICE EFFECT

Dependent variable	Applied for loan before mailer deadline	Applied for loan before mailer deadline	Applied for loan before mailer deadline
Sample	Full	Males	Females
Mean (dependent variable)	0.0850	0.0824	0.0879
	(1)	(2)	(3)
1 = no photo	45	−200	62
1 = female photo (System I: affective response)	197	316	94
1 = photo gender matches client's (System I: affinity/similarity)	−90		
1 = photo race matches client's (System I: affinity/similarity)	−193	−56	−291
1 = one example loan shown (System I: avoid choice overload)	234	396	91
1 = interest rate shown (System I: several, potentially offsetting, channels)	86	−68	215
1 = cell phone raffle mentioned (System II: overestimate small probabilities vs. conflict from reason-based choice)	−79	−4	−144
1 = no specific loan use mentioned (System II: mentioning specific use, via text, triggers deliberation)	203	336	91
1 = comparison to competitor rate (System II: makes dominating option salient)	−7	−48	29
1 = loss frame comparison (System II: triggers loss aversion)	−83	−72	−85
1 = we speak "your language" (Lender imposed)	−148	−64	−215
1 = a "low" or "special" rate for you (Lender imposed)	3	−88	79

Notes: Cells divide the coefficent on the content variable from Table III by the offer rate (i.e., the price) coefficient, and multiply by −100, to estimate the interest rate drop (in basis points) that would be required to achieve the same effect on demand that was achieved by the content treatment. So negative numbers indicate the equivalent interest rate increase needed to generate the drop in demand implied by a negative point estimate on a content variable. Note that we calculate this for all content treatments here, including the ones that are not statistically significant in Table III. Treatment variable labels: parentheses contain summary descriptions of our prior on why each ad content treatment would increase demand (or of reason(s) that we had no strong priors).

on the *mechanisms* through which advertising content affects demand. *F*-tests show that the six content features motivated by prior evidence significantly affected takeup, whereas the two features imposed by the Lender did not. The last rows of *F*-tests show

that our grouping of System I (intuitive processing) treatments significantly affected takeup; in contrast, our System II (deliberative processing) treatments did not significantly affect takeup.

Hence, in this context, advertising content appears more effective when it is aimed at triggering an intuitive response rather than a deliberative response. There are, however, two important caveats that lead us to view this finding as mainly suggestive, and not definitive, evidence on the cognitive mechanisms through which advertising content affects demand. The first caveat is that our confidence intervals do not rule economically significant effects of System II content. The second caveat is that the classification of some of the treatments as System I or System II is debatable.

V.C. Deadlines

Recall that the mailers also included randomly assigned deadlines designed to test the relative importance of option value (longer deadlines make the offer more valuable and induce takeup) versus time management problems (longer deadlines induce procrastination and perhaps forgetting, and depress takeup). Table V presents results from estimating our usual specification with the deadline variables included.[31]

The results in Table V, Panel A, columns (1)–(3), suggest that option value dominates any time management problem in our context: takeup and loan amount increased dramatically with deadline length. Lengthening the deadline by approximately two weeks (i.e., moving from the omitted short deadline to the extension option or medium deadline, or from medium to long deadline) increases takeup by about three percentage points. This is a large effect relative to the mean takeup rate of 0.085, and enormous relative to the price effect. Shifting the deadline by two weeks had about the same effect as a 1,000–basis point reduction in the interest rate. This large effect could be due to time-varying costs of getting to the branch (e.g., transportation cost, opportunity cost of missing work) and/or to borrowing opportunities or needs that vary stochastically (e.g., bad shocks). Columns (4) and (5) show that we do not find any significant effects of deadline on default or on borrowing from other lenders.

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31. We omit the advertising content variables from the specification for expositional clarify in the table, but recall that all randomizations were done independently. So including the full set of treatments does not change the results.

TABLE V
EFFECTS OF DEADLINE ON BORROWER BEHAVIOR

Panel A: Predeadline demand

Dependent variable	Applied before own deadline	Obtained loan before own deadline	Loan amount obtained before own deadline	Loan obtained before own deadline in collection status	Borrowed from other lender	Applied within 2 weeks (short deadline length)
Sample	Full	Full	Full	Obtained	Full	Full
Estimator	Probit	Probit	OLS	Probit	Probit	Probit
Mean (dependent variable)	0.0850	0.0741	110.4363	0.1207	0.2183	0.0360
	(1)	(2)	(3)	(4)	(5)	(6)
Monthly interest rate in percentage point units (e.g., 8.2)	−0.0029***	−0.0026***	−4.7768***	0.0075***	0.0009	−0.0009***
	(0.0005)	(0.0005)	(0.8237)	(0.0023)	(0.0008)	(0.0003)
Short deadline, extended	0.0322***	0.0240**	31.1321*	0.0236	−0.0104	−0.0019
	(0.0118)	(0.0107)	(17.2858)	(0.0424)	(0.0131)	(0.0047)
Medium deadline	0.0300***	0.0270***	38.0335***	0.0205	−0.0065	−0.0046
	(0.0068)	(0.0061)	(13.8228)	(0.0300)	(0.0119)	(0.0047)
Long deadline	0.0603***	0.0563***	70.1119***	0.0138	−0.0054	−0.0055
	(0.0118)	(0.0112)	(15.0945)	(0.0363)	(0.0123)	(0.0042)
(Pseudo-)r-squared	.0461	.0538	.0351	.0597	.0007	.0471
N	53,194	53,194	53,194	3,944	53,194	53,194
F-test of joint significance of all deadlines	0.0000	0.0000	0.0000	0.8487	0.8813	0.6570

TABLE V
(CONTINUED)

Panel B: Postdeadline applications

Dependent variable = applied	After short deadline	After medium deadline	After long deadline
Sample	Full	Full	Full
Estimator	Probit	Probit	Probit
Mean (dependent variable)	0.1830	0.1477	0.1184
	(1)	(2)	(3)
Offer interest rate	0.0010	0.0005	0.0009
	(0.0008)	(0.0007)	(0.0006)
Short deadline, extended	−0.0224*	−0.0052	−0.0030
	(0.0117)	(0.0113)	(0.0102)
Medium deadline	−0.0058	−0.0035	−0.0047
	(0.0112)	(0.0102)	(0.0092)
Long deadline	−0.0089	0.0019	−0.0014
	(0.0114)	(0.0108)	(0.0095)
Pseudo r-squared	.0560	.0448	.0369
N	53,194	53,194	53,194
F-test of joint significance of all deadlines	0.2518	0.6332	0.8262

Notes: Huber–White standard errors. Probit results are marginal effects. All models include controls for randomization conditions: risk, mailer wave (September or October), and deadline eligibility. Short deadline is the omitted category; "short deadline, extended" gave customers a number to call and get an extension (to the medium deadline). Panel A, column (6): tests whether short deadlines spur action by inducing early applications. The dependent variable here is defined regardless of the individual's deadline length; that is, the dependent variable = 1 if the individual applied within two weeks of the mailer date, unconditional on her own deadline. Panel B: Testing three alternative measures of post-deadline takeup helps ensure that our results here are not driven by mechanical timing differences, because we have a finite number of postdeadline data (6 months). We measure postdeadline takeup using takeup after the short deadline (2 weeks), after the medium deadline (4 weeks), and after the long deadline (6 weeks). We define these outcomes for each member of the sample, regardless of their own deadline length, in order to ensure that everyone in the sample has the same takeup window. Otherwise those with the short deadline mechanically have a longer postdeadline window, and if there is a positive secular probability of hazard into takeup status within the range our deadlines produce (5 to 6 months), then this would mechanically push toward a decreasing relationship between deadline length and postdeadline takeup.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

It is theoretically possible that the strength of the longer-deadline effect may be due in part to the nature of direct mail. Although we took precautions to ensure that the mailers *arrived* well before the assigned deadline, it may be the case that clients did not *open* the mailer until after the deadline expired. For example, if clients only opened their mail every two weeks, then the short deadline would mechanically produce a very low takeup rate (in fact, the mean rate for those offered the short deadline was 0.057, versus 0.085 for the full sample). It is also theoretically possible that by capping the deadline variation at six weeks, we missed important nonlinearities over longer horizons. Note, however, that longer deadlines were arguably empirically irrelevant in our context, as the Lender deemed deadlines beyond six weeks operationally impractical.

Panel A, column (6), and Panel B explore whether the large increase in demand with deadline length obscures a smaller, partially offsetting time management effect, that is, whether there is a channel through which longer deadlines depress demand (by triggering procrastination and/or limited attention) that is swamped by the larger, positive effect of option value. Specifically, Panel A, column (6), tests whether short deadlines spur action by inducing early applications (here "applying within two weeks"—the short deadline length—is the dependent variable). The negative signs on the deadline coefficients are consistent with a time management effect, but the deadline variables are neither individually nor jointly significant, and the estimates are imprecise.

In Panel B, we test whether longer deadlines increase the likelihood of takeup *after* deadlines pass. Postdeadline takeup is an interesting outcome to study because the price of loans rose, substantially on the average, postdeadline. So postdeadline takeup could be an indicator of costly time management problems, and if short deadlines help consumers overcome such problems, we might expect postdeadline takeup to increase in deadline length. Panel B tests this hypothesis using three alternative measures of postdeadline takeup.[32] The deadline variables

32. Testing three alternative measures of postdeadline takeup helps ensure that our results here are not driven by mechanical timing differences, because we have a finite amount of postdeadline data (six months). We measure postdeadline takeup using takeup after the short deadline (two weeks), after the medium deadline (four weeks), and after the long deadline (six weeks). We define these outcomes for each member of the sample, regardless of their own deadline length, to ensure that everyone in the sample has the same takeup window. Otherwise those with the short deadline mechanically have a longer postdeadline window,

are not jointly significant for any of the three measures. Across all three specifications only one of the nine deadline variables is significant at the 90% level. So there is little support for the hypothesis that deadlines affect postdeadline takeup. Again, though, our confidence intervals do not rule out economically significant effects.

All in all, the results suggest that the demand-inducing option value of longer deadlines appears to dominate in this setting. But our design is not sharp enough to rule out economically meaningful time management effects.

VI. Conclusions

Theories of advertising, and prior studies on framing, cues, and product presentation, suggest that advertising content can have important effect on consumer choice. Yet there is remarkably little field evidence on how much and what types of advertising content affect demand.

We analyze a direct mail field experiment that simultaneously and independently randomized the advertising content, price, and deadline for actual loan offers made to former clients of a consumer lender in South Africa. We find that advertising content had statistically significant effects on takeup. There is some evidence that these content effects are economically large relative to price effects. Consumer response to advertising content does not seem to have been driven by substitution across lenders, and there is no evidence that it produced adverse selection. Deadline length trumped both advertising content and price in economic importance, and we found no systematic evidence of time management problems.

Our design and results leave many questions unanswered and suggest directions for future research. First, we found it difficult to predict *ex ante* which advertising content or deadline treatments would affect demand, and some prior findings did not carry over to the present context. This fits with a central premise of psychology—context matters—and suggests that pinning down the effects that will matter in various market contexts might

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and if there is a positive secular probability of hazard into takeup status within the range our deadlines produce (five to six months), then this would mechanically push toward a decreasing relationship between deadline length and postdeadline takeup.

require systematic field experimentation on a broad scale. But our paper also highlights a weakness of field experiments: real-world settings can mean low takeup rates, and hence a high cost for obtaining the statistical power needed to test some hypotheses of interest. Future advertising experiments should strive for larger sample sizes (as in Ausubel [1999]) and/or settings with higher takeup rates, and use the additional power to design tests for combinations of treatments, including interactions between advertising and price.

Another unresolved question is *why* advertising ("creative") content matters. In the taxonomy of the economics of advertising literature, the question is whether content is informative, complementary to preferences, and/or persuasive. A related question from psychology is how advertising affects consumers cognitively. In our setting, we speculate that advertising content operated via intuitive rather than deliberative processes. This fits with the nature of our advertised product (an intermediate good), the fact that little new information or novel arguments were likely in this context, and the experience level of consumers in the sample. But we emphasize that our design was not sufficiently rich to sharply identify the mechanisms underlying the content effects.

It also will be fruitful in the future to study consumer choice in conjunction with the strategies of firms that provide and frame choice sets. A literature on industrial organization with "behavioral" or "boundedly rational" consumers is just beginning to (re-)emerge (DellaVigna and Malmendier 2004; Ellison 2006; Gabaix and Laibson 2006; Barr, Mullainathan, and Shafir 2008), and there should be gains from trade between this literature and related ones on the economics of advertising and the psychology of consumer choice.

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