Unshrouding:
Evidence from Bank Overdrafts in Turkey

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Abstract

Lower prices produce higher demand… or do they? A bank's direct marketing to holders of "free" checking accounts show that a large discount on 60% APR overdrafts *reduces* overdraft usage, especially when bundled with a discount on debit card or auto-debit transactions. In contrast, messages mentioning overdraft availability without mentioning price *increase* usage. Neither change persists long after messages stop. These results do not square easily with classical models of consumer choice and firm competition. Instead they support behavioral models where consumers both underestimate and are inattentive to overdraft costs, and firms respond by shrouding overdraft prices in equilibrium.

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1. Introduction

Many business models in financial services and other industries rely on expensive add-ons that are tied to base goods. Examples include expensive overdraft credit (add-on) tied to a "free" checking account (base good), back-end management fees tied to "free" investment advice, printer cartridges and printers, luggage fees and airline tickets, and dealer-supplied maintenance and automobiles. A closely related practice is overage/penalty pricing. Businesses with such revenue models typically focus their marketing and competitive strategy on the base good, even though add-ons/overages can be critical revenue sources.

Retail banking provides a striking example: banks often market checking accounts as free, even though many consumers end up paying high fees for overdraft credit.[1] Government audits find that banks rarely market overdraft services at the customer acquisition stage and even actively discourage employees from providing information on overdraft terms (General Accounting Office 2008; Competition and Markets Authority 2014; Consumer Financial Protection Bureau v. TCF National Bank 2017). After acquiring customers, banks in some markets, including Turkey (the site of our experiment), have blurred the line between positive and negative balances by highlighting for customers an available-to-withdraw figure that adds the available overdraft credit line amount to the checking account balance, while making information on disaggregated balances and finance charges more difficult to find. Turkish banks during our study period basically only drew attention to overdrafts when promoting the feature to existing customers. Yet even those promotions did not explicitly mention the price of overdraft credit.

Why would a bank hide information on overdraft costs? After all, a classically rational consumer would simply infer that shrouded prices are high prices. But recent behavioral theories show that shrouded and high prices can persist if consumers tend to underestimate their add-on costs and firms cannot profit from de-biasing consumers with more transparent pricing or information about competitors' high add-on prices (Gabaix and Laibson 2006; Grubb 2015; Heidhues, Köszegi, and Murooka 2017).[2]

[1] An overdraft occurs if the checking account holder initiates a transaction that makes her balance negative, or more negative.

[2] For related evidence on consumer perceptions of overdraft costs see Armstrong and Vickers (2012) and Stango and Zinman (2014). For related models of limited and reactive consumer attention see, e.g., Gabaix (2014) and Bordalo et al (2015).

Bank regulators have responded by, for example, requiring upfront consumer opt-in for debit card and ATM overdrafts in the U.S., cautioning banks against relying too heavily on overdraft revenue in the U.S. and U.K., and capping overdraft prices in Turkey.

Yet empirical evidence is lacking on key questions raised by theory and policy. Do consumers actually underestimate costs of add-ons such as overdrafts? Do firms actually have incentives to shroud add-on prices instead of competing to de-bias consumers? How do consumers allocate attention to add-ons, and how quickly does consumer learning about add-ons break a shrouded equilibrium? In short, empirical evidence on what drives overdraft pricing, advertising, and usage is limited and largely descriptive.

These questions are central to overdraft markets. Beginning in 1990s, overdraft revenue replaced monthly subscription fees as banks' major source of explicit income from checking accounts, shifting the pricing equilibrium for retail banking in much of the world to "free if nonnegative balance, very expensive if in overdraft". In the US, banks collect more than $10 billion in overdraft revenue annually. In the UK, banks derive almost as much income from overdrafts as from re-investing checking account deposits (Competition and Markets Authority 2014). In Turkey, the site of our experiment, the announcement of a price ceiling on overdrafts was immediately followed by a 1.4% reduction in bank share prices, with a 2.1% drop for the most overdraft-reliant bank (Toksabay 2013).

We worked with Yapi Kredi (YK), one of the largest banks in Turkey, to design a randomized direct marketing experiment that distinguishes between classical and behavioral models of add-on pricing and advertising. YK sought to learn more about its optimal strategy for pricing and advertising its overdraft product. YK was particularly interested in understanding whether its past promotional pricing and advertising content tactics are effective in increasing demand, and if not why not. YK's interest rate (60% APR) and product design was in line with standard practices and regulations. As is common in overdraft markets, the product was priced expensively relative to seemingly close substitutes (like credit cards in Turkey), and disproportionally to credit risk (as found recently by Turkish authorities).

YK sought to target marginal overdrafters among its existing client base, and hence the experiment varies promotions YK sent via SMS from September-December 2012 to 108,000 existing checking account clients who had not overdrafted in the previous few months. These clients are likely representative of a substantial population of marginal overdrafters in

Turkey, and they share key characteristics with "banked" populations in both more- and less-developed countries.

The experimental design produces random variation, across clients and over time, in overdraft prices and in messaging content, frequency, and duration (see Figure 1). Our tests rely on overdraft-usage comparisons across groups receiving different promotions, since Turkish banks use SMS-based promotions frequently (a pure control group would be off-equilibrium). The key comparison is between overdraft-promoting messages that mention price and those that do not.

Our key hypothesis is that drawing attention to overdraft costs reduces demand for overdrafts. This can hold if consumers tend to underestimate overdraft costs and have limited/reactive attention. Mentioning price could then jog memory that overdrafts are costly, motivate individuals to learn the correct and higher price, and/or increase attention to avoiding overdrafts. In contrast, mentioning price will not reduce overdraft demand if consumers are classically rational and hence correctly perceive overdraft costs on average.

Our test of this hypothesis is one-sided in the sense that in our experiment YK only mentions price while also cutting it: the overdraft pricing arms of our experiment all offer 50% discounts on overdrafts. This stacks the deck against finding a negative effect of mentioning overdraft pricing, since even a behavioral consumer who is susceptible to shrouding presumably prefers lower prices. Yet we still find that mentioning overdraft price lowers overdraft demand. E.g., the likelihood of overdrafting during the experiment is 1.2 percentage points lower (se=0.4 pp) for those receiving the discount offer relative to those receiving a message that mentions overdraft without mentioning its price, on a baseline likelihood of 31%.

Another behavioral hypothesis is that bundling the overdraft discount with a discount on debit card or automated bill-payment usage will further depress demand. The idea is that mentioning overdrafts together with transactions that could trigger overdrafts could be a particularly powerful reminder to avoid such transactions, akin to the attention-by-association findings in Stango and Zinman (2014).[3] Again there are both classical and mechanical forces pushing against finding support for this hypothesis; in the absence of behavioral factors, the bundled discounts should generate weakly more outflows from the checking account,

[3] Stango and Zinman (2014) finds that drawing consumer attention to spending control, monitoring account balances, or other bank fees leads to overdraft reductions.

producing weakly more variance in the checking account balance and hence weakly more overdrafts. Nevertheless, we again find support for the behavioral hypothesis. Offering the overdraft discount alone reduces overdraft likelihood by only 0.7pp (se=0.5) relative to messages that mention overdraft without mentioning price, while the bundled reductions are 1.4pp for automated bill payment (se=0.5) and 1.7pp for debit card (se=0.5).

Importantly, discounts on the bundled products themselves do not backfire in the same way that overdraft discounts do: offering a discount for debit card usage, or automated bill-payment authorization, weakly increases demand for those services. This further highlights the distinction between advertising shrouded attributes (overdraft) vs. unshrouded ones (debit or autopay).

As noted above, our experimental design also includes an overdraft availability promotion that does *not* mention price, and we can identify its effects on demand by comparing it to messages that do not mention overdraft at all (that promote debit or automated bill payment transactions alone). The overdraft availability message *increases* overdraft likelihood, by about 0.9 pp (se=0.4). We do *not* find heterogeneous effects based on prior overdraft experience (although the confidence intervals do not rule out meaningful differences), suggesting that overdraft availability is not salient/top-of-mind even if the consumer is already informed about availability in some classical sense. This finding suggests that, after acquiring a customer, firms can promote non-price aspects of add-ons without drawing attention to costs.

Altogether our results are consistent with the models of shrouded equilibrium and limited/reactive consumer attention. In particular, they support: (1) the key modeling assumption that consumers tend to underestimate add-on costs (if consumers' estimates were unbiased then offering a discount would weakly increase demand); (2) the key assumption that firms lack incentives to unshroud prices; (3) a key prediction of reactive attention models that consumers respond differently when advertising highlights different add-on attributes (price or availability). Shifts from shrouded to unshrouded equilibria thus may be costly to generate and sustain. Section 5 discusses alternative, more-classical interpretations, and why they do not provide as complete an explanation of our setting and results.

Our paper informs several other literatures on limited attention, salience, and advertising. Our results provide insight into what comes to mind and what does not (Bordalo, Gennaioli, and Shleifer 2013; Eliaz and Spiegler 2011; Gabaix 2014; Hanna, Mullainathan, and

Schwartzstein 2014; Karlan et al. 2016). Our results also show that price promotions have attention effects that can backfire from the promoter's perspective, thereby adding evidence to literatures on the psychology of incentives (Gneezy and Rustichini 2000; Kamenica 2012) and price changes (Hastings and Shapiro 2013) that have not yet focused much on shrouded prices. Our results are consistent with results from other domains suggesting that consumers respond differently to base prices versus add-on prices (Anagol and Kim 2012; Brown, Hossain, and Morgan 2010; Chetty, Looney, and Kroft 2009).[4]

Relatedly, our findings contrast with those in Ferman (2016) and Elizondo and Seira (2017), both of which find little impact of messaging that makes the *base* price of high-interest credit cards more prominent in Brazil and Mexico, respectively Our results are broadly consistent with prior work finding that advertising content can have important and surprising effects on decisions about expensive debt (Bertrand et al. 2010), and that messaging from banks can change the behavior of existing customers (Cadena and Schoar 2011; Karlan, Morten, and Zinman 2015; Kast, Meier, and Pomeranz 2016). Our results on the long-run effects of short-run messaging complement the literature on the dynamics of learning and/or attention regarding add-on charges (Agarwal et al. 2013; Ater and Landsman 2013; Haselhuhn et al. 2012; Stango and Zinman 2014), may help explain why advertising is so prevalent (treatment effects dissipate quickly and hence repeated exposure matters), and suggest that short-run behavior changes do not necessarily induce learning or greater sophistication about attention (Schwartzstein 2014; Manoli and Turner 2015).[5]

2. Setting: The Overdraft Market in Turkey

[4] We do not actually observe price sensitivity to the base price in our setting. But given our result that cutting overdraft prices depresses overdraft demand, we can infer differential sensitivity to base and add-on prices simply by assuming that cutting the base price would not decrease demand for checking accounts.

[5] Stango and Zinman (2014) is probably the most closely related empirical paper to ours. SZ uses quasi-experimental variation in survey content, in a market research panel in the U.S., to identify effects and dynamics of attention to overdraft fees. Similar to here, SZ finds that an attention shock mentioning overdraft costs reduces overdraft usage, and that repeated attention shocks cumulate to some extent, although they depreciate more quickly in our setting. Aside from the obvious differences between the two study designs— market research surveys versus bank advertising as attention shocks, quasi-random versus random variation, US versus Turkey—there are at least two other key differences. First, we have randomly assigned price variation. Second, our treatments include some messages that mention the overdraft service but not its cost. These differences lead to the surprising new inferences that bringing overdrafts to mind increases demand, but bringing the price of overdrafts to mind, even if accompanied by a discount, lowers demand. See also Liu et al (2014) and Hunt et al (2015).

We discuss the economic importance of overdrafts above; here we focus on describing our partner bank, Yapi Kredi (YK), and the Turkish overdraft market. Our setting has many similarities to overdraft markets in other countries like the US and UK, with a key difference being substantially lower prices in Turkey (60% APR vs. $25-$40 per-transaction fees for very small and short-term loans).[6]

2.1 Retail and Mobile Banking in Turkey

Turkey's retail/consumer banking industry is concentrated. Only about 30 banks are licensed to take deposits, and the largest five banks have greater than 50% market share. YK is in the top five based on both total assets and number of branches, and is publicly traded. In recent years the Turkish market has become known for innovation in retail banking, with one bank rolling out the largest biometric ATM network in the region, and another bank becoming the first in the world to make money transfers possible on Facebook. 91% of Turkish adults have a cell phone, for one of the highest penetration rates in the world, and Turkey has the highest rate of mobile banking in Europe among internet users, at around 50%, according to a 2013 ING survey.

2.2 Overdraft Practices

The focus of our experiment is a checking account overdraft product, which YK brands the "Flexible Account", with features that were standard in Turkey (and throughout the world) during our study period. The product is an unsecured line of credit that allows qualifying customers to overdraw their account (i.e., to hold a negative balance), at a cost of 60% APR on outstanding credit (about 50% real after adjusting for inflation). Credit card APRs tend to be substantially lower. The bank approves about 55% of checking account customers for the overdraft feature, with credit limits that vary based on underwriting but are typically lower than other unsecured credit products. Although individuals can apply for the overdraft feature, typically (and in our study), the bank automatically chooses individuals to be given the feature, thus requiring individuals to opt-out if they do not want it rather than opt-in if they do. Customers then use the line, automatically, any time their checking account

[6] For details on overdraft markets in the US and Europe see, e.g., Stango and Zinman (2014), and various government reports (Bakker et al. 2014; Competition and Markets Authority 2014; Financial Conduct Authority 2014; General Accounting Office 2008). For policy developments in the US and EU, see, e.g., FDIC Supervisory Guidance warning banks about the risks of "excessive use" of overdrafts by customers and "maximizing fees" by banks, Dougherty (2014), and the European Parliament's Directive on Payment Accounts (issued April 2014).

falls (further) below zero. Negative balances begin accruing interest immediately. By law, any inflow to the checking account is automatically allocated first toward paying off overdraft credit. If inflows are not sufficient to clear the balance by the end of the statement date (four weeks), the bank sends a notice and gives the customer about two weeks to pay at least the accrued interest. If the customer fails to make the required payment after 60 days, the bank freezes the overdraft line.

Overdrafts have attracted regulatory scrutiny because they—and their prices—are rarely featured at the customer acquisition stage, and often not fully disclosed even post-acquisition. Pre-acquisition, we are not aware of any mass marketing campaigns promoting overdraft usage during our sample period in Turkey. Regulators found that when banks did communicate with (prospective) consumers, they often failed to provide mandated disclosures or did so only perfunctorily.

Communications with existing customers also indicate a (partially) shrouded equilibrium during our sample period. Customers lacked easy access to information on their own overdraft usage and charges. Banks did not routinely issue monthly statements, due to the unreliable postal system, and electronic notifications of overdraft usage did not mention the interest rate. Electronic banking was and is the dominant channel for customers getting information on their accounts, and online information on overdraft usage was folded into the customer's main transaction record rather than disaggregated. The transaction record would specify the amount charged in overdraft interest, but not the interest rate. Landing pages—the first screen the customer sees after logging in online, or at an ATM-- would obscure whether overdraft was being used, by highlighting the customer's balance based on "available-to-withdraw" (including credit line) instead of just the checking account balance. A customer seeking interest rate information would need to either navigate through several different screens or contact a customer service representative. Banks did promote overdraft usage to existing customers in direct messaging campaigns, but those campaigns did not mention the level of the interest rate even when offering discounts on that rate; e.g., an ad would say "half off your interest charges" instead of "half off your 60% interest rate".

2.3 Overdraft Users and Usage in Turkey

Who overdrafts? In Turkey there is not much data on the characteristics of overdrafters (e.g., our data lack information on education or income), but there are some clues. Over half of Turkey's population is unbanked, according to a 2012 World Bank report.

Many checking account holders are not approved for overdraft lines of credit due to credit risk that banks cannot price. These facts suggest that overdrafters are drawn from the upper half of the income distribution in Turkey, although not from the uppermost percentiles, who presumably have wealth and access to cheaper credit that would tend to render overdrafting unlikely and/or relatively unattractive.

Our sample overdrafts frequently in the ten months after our experiment started, despite having been selected for the experiment based on infrequent overdraft activity prior to the experiment (Section 3.2; Table 1). In any given month, 15-24% of our sample overdrafts. 45.8% of our sample overdrafts at least once between September 1, 2012-June 30, 2013, paying a mean finance charges of 30.82 TL (1 TL = $0.56 USD during our sample period) over the ten months. The 95th percentile is 228.08 TL.

The sharp increase in overdrafting from baseline to (post-) experiment is probably not due to mean reversion, as overdrafting tends to be strongly serially correlated month-to-month. Rather, discussions with YK and its regulators point to a market-wide increase that flowed from easy monetary policy, to widespread promotions by consumer lenders around the end of Ramadan, to overdrafts spurred by subsequent difficulties marginal borrowers had with managing their increased debt service. In any case, the sharp increase in overdrafting is evident in both aggregate data and YK's customer base. YK held back a no-message group of 39,000 from our experiment, and Figures 2a and 2b show similar trends in overdraft usage for this group compared to our experimental group of 108,000.[7]

2.4 Policy Postscript

As noted above, overdraft practices have been attracting legal scrutiny around the globe. In July 2013, seven months after our experiment ended, the Turkish Competition Authority found that banks were benefiting from substantial markups over risk-based prices and fined 12 banks for price-fixing on loans, including overdrafts. On May 27, 2013, the Turkish Central Bank imposed a binding price ceiling on overdraft APR. Turkish regulators

[7] YK applied somewhat different and not entirely reproducible filters in selecting the no-message comparison group, so we do not use this group as a pure control group for analysis purposes. Creating a pure, randomized control group was not a point of emphasis at the design and implementation stages of the experiment, since the equilibrium was one of extensive direct-messaging and hence the mapping of message vs. no-message comparisons to theory is less clear than comparisons across different promotions.

have also focused on marketing and communications since our experiment ended, pressuring and working with banks to make overdraft usage, pricing, and costs more transparent.

2.5 Ethics of Experimenting with High-Cost Credit

We are frequently asked in seminars whether researchers should partner with a lender that is seeking to sell more high-interest rate loans. We think yes, in this circumstance as in many related experiments on microcredit where take-up is an outcome of interest, for four key reasons. First, an ethical concern here presumes that high-cost consumer credit harms consumers. We emphasize the presumption; extensive research on this question suggests that a different assumption is warranted-- (weakly) beneficial impacts for consumers (Karlan and Zinman 2010; Zinman 2014; Banerjee, Karlan, and Zinman 2015). Second, YK's advertising was truthful and its terms were competitive. Thus, combining the first and second points, the experiment was not trying to convince consumers to accept a bad deal in either absolute terms or compared to market alternatives. Third, YK was going to promote overdraft usage among its existing customers with or without the participation of the research team; we helped convince bank management to feature prices despite its skepticism about the effectiveness of past overdraft price promotions. Fourth, YK and the research team contracted ex-ante that the academic co-authors would have unrestricted intellectual freedom to report the results and disseminate them publicly to benefit regulators and further scientific knowledge.

3. Experimental Design, Sample and Data

3.1 Experimental Design and Implementation

Figure 1 summarizes the experimental design and details the script of each message variant. The field experiment randomly assigns message content, frequency, and duration, as well as promotional offers on overdraft, debit card, and automated bill payment, to a sample of 108,000 checking account holders. YK did not send this sample any other promotional communications during this campaign. The only other communications YK sent to this sample were monthly account statements. YK sent the messages by SMS, which is the most common way banks communicate with their clients in Turkey.

YK began the experiment by sending half of the sample an "Overdraft Availability" message on August 30, 2012 that mentions the overdraft service and credit line but nothing

about its cost.[8] This first randomization is not crucial for testing our main hypotheses; it served primarily as a pilot for the subsequent randomizations and a test for a heterogeneous treatment effect suggested by some of the motivating theoretical models (Section 4.3).

YK continued the experiment on September 15, 2012 with a second, independent randomization, sending each person in the sample one of six randomly assigned promotions. Half of the sample received one of the three "Overdraft Interest Discount" messages detailed in Figure 1, with this group sub-divided into one-third getting only an overdraft discount, one-third getting an overdraft discount plus an automated bill payment discount, and one-third getting an overdraft discount plus a debit card discount. The other half of the sample did not get an overdraft interest discount, with this group sub-divided into one-third getting the automated bill payment discount only, one-third getting the debit card discount only, and one-third getting only the overdraft availability reminder message described above.

An independent frequency randomization determined whether YK resent the September 15 message frequently (every 10 days), less-frequently (every 20 days), or not again during the campaign period. A campaign duration randomization then determined whether the price promotion(s) or overdraft availability reminder, and any repeated messaging subsequent to September 15, lasted until November 15 or December 15.

Note that YK sent at least one message to everyone in our sample during the experiment. YK preferred this design feature because, like other banks, it often sends promotional and reminder messages to its customers and thought it would be counterproductive to scale back directed advertising to zero. The research team also preferred this design— placebo communications with respect to overdraft, rather than pure control— because any contact from YK could trigger the customer's attention and affect her usage of YK products.[9]

[8] "We remind you that, for your immediate cash needs, you have a Flexible Account at Yapi Kredi with [custom fill]TL limit. Have a nice day." One might think of this message as a "reminder" because the bank's policy and Turkish law require upfront disclosure of the overdraft features and pricing. However, given that the service is offered on an opt-out basis, and that our motivating questions concern shrouding prices, we allow for the possibility that this message provides new information rather than being a simple reminder. We explore this in Section 4.3.

[9] A closely related way of framing our interest in placebo communications is that frequent direct-messaging is the equilibrium, and we thought that the experiment would link more tightly to theory if the only deviations from equilibrium were around (partial) unshrouding.

3.2 Baseline Data on Sample Characteristics, and Balance Checks

YK sought to promote overdraft usage among existing clients who it deemed most likely to be close to the margin of overdrafting. To this end it selected customers for the experiment based on the following criteria: owned a YK checking account for at least a year, were in good standing in the account, and had a debit card linked to it; had an active cell phone; maintained an average total deposit account balance<5,000TL over the three months prior to the start of the experiment (May-July 2012); did not have more than three automated bill payment set up; and had the overdraft service in place already but had not used it during the prior three months. Many of these customers did have some experience with the product before the three month period prior to the experiment. Our pre-treatment data go back as far as September 2011, and from September 2011-April 2012 18.4% of our sample overdrafted at least once, with an average daily overdraft balance of 4.42TL (SD 23, Max 940) among these accountholders.

Table 1 summarizes the baseline data available to us (Column 1) and checks balance across treatment assignments (Columns 2 -10). In terms of demographics, we only have information on gender (29% female), the city of residence (28% Istanbul, 23% outside the four largest cities), and marital status (57% married). This information is collected by the bank at the account-opening stage and can be updated later by the client. Besides pre-treatment data on overdraft usage (described above and in Table 1), we also have data on the other behaviors targeted by the experiment: debit card usage and automatic debits for bill payments. We stratified on each of these baseline variables, and the last column confirms that none of them is correlated with treatment assignment by regressing each row variable on the treatment assignments indicated Columns 2-10.

3.3 Follow-up data

YK provided us with data on overdraft usage, debit card usage, automated bill payment authorizations, and deposit account balances, at the account-month level, from September 1, 2012 through the end of June 2013 (although we do not use the June 2013 due to the binding price cap imposed at the end of May 2013). In addition to the monthly data, YK provided us with daily data on overdraft usage for the experimental period: September 15-December 15, 2012. We use this data to construct outcome variables for estimating the short-run and longer-run treatment effects detailed below.

3.4 Hypothesis and Tests

Our key hypothesis is that drawing attention to the cost of overdrafting will depress demand for it. The motivation for this hypothesis comes from two key features of behavioral models of costly add-ons like overdrafts. First, consumers tend to underestimate, and have limited/reactive attention to, add-on costs.[10] Mentioning price could then jog memory that overdrafts are costly, motivate individuals to learn the correct and higher price, and/or increase attention to avoiding overdrafts. Any of these channels could lead the consumer to revise her cost estimate upward and thereby reduce her overdraft usage. In contrast, drawing attention to overdraft cost will not reduce demand if consumers are classically rational and hence correctly perceive overdraft costs on average. Second, and closely related to the demand response in the first feature, unshrouding costs (by, e.g., drawing consumer attention to them) may be unprofitable for the add-on supplier.

We test this hypothesis by comparing overdraft usage, during the experimental period, between customers sent an Overdraft Interest Discount message and customers sent an Overdraft Availability message. Both types of messages promote overdraft usage, but only the Discount messages say anything about the price. As noted in the Introduction, this test actually stacks the deck toward rejecting our key hypothesis because the Discount messages do not simply draw attention to overdraft costs, they also *cut* the price in half.[11] As such, accepting the hypothesis that Overdraft Interest Discount messages reduce demand—despite offering a much lower price-- would be an especially strong indication that add-on price advertising unshrouds add-on costs for consumers, leading consumers to sharply revise their

[10] Consumers could underestimate the price conditional on overdrafting (perhaps because they anchor on substantially lower prices for other bank services or sources of short-term credit), and/or underestimate the likelihood of overdrafting (perhaps they perceive it to be zero because they assume the bank will not let them overdraft, or perhaps they are overconfident about avoiding overdrafts). Grubb (2015) assumes that consumers perceive the price accurately but underestimate the likelihood of overdrafting, because they underestimate their cost of attention to balances. See also Grubb (2009). In terms of the other models most closely related to our setup, Gabaix and Laibson (2006), Heidhues, Köszegi, and Murooka (2017), and Bordalo, Gennaioli, and Shleifer (2015) assume that some consumers underestimate a reduced-form add-on cost that, in our setting, is the product of the price of overdrafting and the expected number of overdrafts (or, on the extensive margin, the product of the price of overdrafting and the likelihood of overdrafting.

[11] It would have been theoretically and statistically desirable to have experimental arms that simply mentioned the price of overdrafting without cutting it, and/or that mentioned the regular price (60% APR) while cutting it, but YK deemed such messaging too far off-equilibrium to be viable.

cost perceptions upward, depressing demand for the add-on and proving unprofitable for the supplier.

A second behavioral hypothesis is that bundling the overdraft discount with a discount on debit card or automated bill-payment usage will further depress demand. The idea is that mentioning overdrafts together with transactions that can trigger overdrafts could be a particularly powerful reminder to avoid overdrafts, akin to the findings in Stango and Zinman (2014) that drawing consumer attention to spending control, monitoring account balances, or other bank fees leads to overdraft reductions. Again there are both classical and mechanical forces pushing against finding support for the behavioral hypothesis: in the absence of inattention, the bundled discounts should generate weakly more outflows from the checking account, producing weakly more variance in the checking account balance and hence weakly more overdrafts.

We test this bundling hypothesis by comparing overdraft usage, during the experimental period, between customers sent an Overdraft Interest Discount only message and customers sent an Overdraft Interest Discount message that also includes a discount for automated bill payment or debit card use.[12]

Third, we hypothesize that promoting overdraft availability, without mentioning price, will change demand. Classically rational consumers will have accurate perceptions of overdraft availability, at least on average, and should not respond. In contrast, behavioral mechanisms could generate a negative response, if advertising availability brings overdraft costs to mind, or a positive response. One mechanism generating a positive response would be if the overdraft service generally, and not just its cost, is not at top of mind for consumers. If consumers have reactive attention, then drawing attention to a positive feature of the service—its availability when you are short on cash—can increase demand for it. Another potential mechanism is that the message operates on consumers who thought overdrafting was impossible: they perceive a zero likelihood and infinite price of overdrafting. In this case the availability message could increase demand if it reduces the perceived price by more than it increases the perceived likelihood. Note that this mechanism would still be consistent with

[12] Note that this is a conservative test of the hypothesized behavioral mechanism: the bundled messages are longer, and if longer messages tax limited attention we would expect them to push treatment effects on overdrafting toward zero instead of further depressing demand. Ignoring a message makes it akin to getting no message at all.

consumers underestimating overdraft *costs* at baseline, since impossibility implies zero expected cost.

We test whether and how promoting overdraft availability changes demand by comparing overdraft usage among customers sent the Overdraft Availability message to customers sent messages that promote only debit card or automated bill payment usage and do not mention overdraft at all.

Our final hypothesis focuses on understanding the dynamics of attention and overdraft behavior. The dynamics shed light on whether consumer learning and/or persistent attention to add-on costs will break a shrouded equilibrium. E.g., in Heidhues, Köszegi, and Murooka the profitability of high add-on prices is "limited by consumers' ex-post demand response to add-on prices" (p. 341), raising the possibility that, at the customer level, the firm wants to shroud at the customer acquisition stage but then unshroud while cutting the price of the add-on. In contrast, consumer forgetting and/or reactive attention to add-on costs increases the cost of de-biasing consumers, flattens or even reverses standard demand responses, and can make a shrouded equilibrium more durable.

To test the dynamics, we examine data from the post-campaign period (January-May 2013). Treatment effects will persist if consumer learning or attention re: add-ons is durable. Treatment effects will not persist if consumers quickly forget about add-ons or only attend to them when induced to by external stimuli like advertising.

4. Specifications and Results

We estimate OLS regressions at the level of YK's randomizations—the checking account, indexed by i— and a timeframe that corresponds to either during- or post-experimental advertising (indexed by t):

$$Y_{it} = T_i\beta + X_i\alpha + e_{it}$$

where Y is some measure of a behavior targeted by the marketing campaign: overdraft usage, debit card usage, or an automatic debit for bill payment. In Table 2 and Table 3 we measure outcomes over a time period designed to capture immediate/short-run treatment effects: t covers September 15-December 31, since the bank sent everyone at least one promotional message starting September 15 and sent the last promotional messages on December 15. Table 4 measures outcomes over the post-experiment period: January 2013-May 2013.

T is a vector of treatment assignments (see Figure 1), with β the vector of estimated coefficients on those treatment variables. *X* is a vector of the stratification variables used to block the randomization (see Table 1).

Our main tables define the treatment vector to test our key hypotheses with the simplest possible presentation; in particular, we make use of additive specifications that enable us to keep the omitted treatment category consistent across columns within each table, and to facilitate inference about comparisons of theoretical interest: overdraft promotions that mention price vs. those that do not (rows 4-7 in Table 2 and Table 4), and price promotions on other banking services that are bundled with overdraft discounts vs. those that are not (rows 2 and 4 in Table 3). The Appendix tables present a more comprehensive set of results.

4.1 Main Effects of Overdraft Promotions on Overdrafting, During Experiment

Table 2 presents the estimated effects of the different overdraft promotions, on three different measures of overdraft usage, during the experimental period (September 15-December 31, 2012).

Table 2 rows 1 and 3 estimate effects of the Overdraft Availability message that does not mention costs or offer a discount. This message *increases* demand for overdraft relative to not getting a message on August 30th (rows 1 and 2), and relative to messages that do not mention overdraft (row 3).[13] The magnitudes are on the order of 1 percentage point in 1/0 usage, 0.1 days with an overdraft balance, and 1TL in average overdraft balance. These results support the hypothesis that promoting overdrafts without reference to cost increases demand, the most straightforward interpretation of which is that that consumers have limited and reactive attention to all aspects of the overdraft services, not just to its cost.

Row 4 shows our estimates of effects of getting an overdraft interest promotion relative to getting the other overdraft-mentioning message—the availability message that does not mention price. Reading across columns one can see that this effect is negative—offering a lower price leads to *lower* demand— with estimates for two of the three demand measures having p-values < 0.01. The extensive margin falls by 1.2 percentage points (se=0.4), and days with a balance falls by 0.16 (se=0.05), for declines of 4 and 6% relative to

[13] One might wonder whether the "Have a nice day" portion of the availability message might be driving the effects rather than a reminder about availability per se, but if this were the case one might expect to see customers responding to the bank's niceness by using other banking services more. We do not find any evidence of that (Appendix Table 2 Row 3).

the sample means. These results support the hypothesis that drawing attention to overdraft costs reduced demand (even while offering a 50% discount!), with the key implications being that consumers tend to underestimate overdraft costs, and that banks lack incentives to unshroud or compete on overdraft prices because doing so backfires.[14]

Rows 5, 6, and 7 decompose the Overdraft Interest Discount effects into the overdraft discount only (row 5), and the overdraft discount bundled with discounts for auto-pay (row 6) or debit card use (row 7). These results suggest that the overdraft discount by itself does not depress demand to a statistically significant degree (although each of the point estimates is negative); rather, it is the bundled discounts that drive the backfiring effect (see also Table 3, Columns 3-5, Rows 2 and 4, which compare bundled discounts to the overdraft discount only). These results support the hypothesis that bundling overdraft discounts with other discounts is particularly demand-depressing. As discussed above, our favored interpretation is that attention to overdraft costs is reactive and associative; we consider alternative interpretations in Section 5.[15]

The treatment effects in Table 2 are likely economically as well as statistically significant, for several reasons. First, they suggest that drawing attention to overdraft costs induces upward-sloping demand, which is rarely seen and hence important qualitatively. Second, the messaging here does not mention the level of costs; instead, YK offers to give back "half of the interest charges". A pure de-biasing strategy likely would mention the price or cost level, along the lines of: "Beware of overdrafts at 60% APR!" Messaging that highlighted the 60% APR might depress demand even more, suggesting that we identify a lower bound on the effects of unshrouding overdraft costs. Third, messaging costs are low, and hence bank strategy is sensitive to small changes in demand. Fourth, our estimates identify the effects of *sending* messages rather than of consumers *attending* to them. Some recipients may have ignored the messages. This implies that our estimates capture lower bounds for the effects on consumers who actually paid attention to the messages.

[14] We confirm that lower demand likely maps into lower profitability for the bank in Appendix Table 1, which does not find any evidence that account balances increase to offset lost overdraft revenue with increased implicit interest. Stango and Zinman (2014) also infer that consumers do not engineer overdraft reductions with balance infusion.

[15] Another possible mechanism is directly testable: Appendix Table 1 Panel B does not find any evidence that the automated bill payment and debit card promotions lead to higher account balances, casting doubt on the possibility that these promotions motivate individuals to maintain larger bank account balances and thereby produce less overdrafting.

4.2 Do All Promotional Discounts Backfire? No.

Table 3 checks whether other promotional discounts backfire as well, by estimating treatment effects of the debit card and automated bill payment discount offers on their targeted behaviors during the experiment. The key results here are presented in Columns 1 and 2. In particular, Row 1-Column 1 estimates that offering the debit discount alone weakly increase debit card usage, by 0.5pp (se=0.4). And Row 3-Column 2 estimates that the auto-pay discount alone increases auto-pay signup (by 0.4pp, se=0.1). These effects are each scaled relative to the Overdraft Interest Discount Only message (n.b. the omitted category is different here than in Table 2, since here we are primarily interested in whether the other discounts affect demand for their services).[16] Row 2-Column 1 and Row 4-Column 2 test whether bundling a discount with the Overdraft Interest Discount performs differently, with respect to demand for debit cards and auto-pay, than offering a discount on debit or auto-pay alone. We do not find evidence of differential effects.

In sum, we find no evidence that offering discounts on other bank services backfires with respect to demand for those services, and some evidence that they increase demand as intended.

4.3 Do Treatment Effects Persist? Post-Experiment Effects of Overdraft Messaging

Table 4 re-estimates our main specifications from Table 2 over the post-promotional campaign period, for the two overdraft usage measures for which we have data from January-May 2013. (Recall that the most-intensively treated accountholders in our experiment received their last message on December 15, 2012.) We find no statistically significant evidence that treatment effects persist over the five-month post-experiment period: the overdraft discount effect is no longer demand-depressing, and the overdraft availability effect is no longer demand-increasing. Figures 3a and 3b plot the two treatment effects and their confidence intervals month-by-month, for our 1/0 measure of overdraft usage, and we see that both treatment effects trend toward zero over the post-campaign period and lose statistical significance about two months post-campaign (between February and March).

[16] Alternative comparisons, not shown in the table, produce similar inferences. If, e.g., we instead estimate effects relative to not getting a debit card discount, the Debit Card Discount Only coefficient for 1/0 debit card usage is 0.0067 (se=0.0039), and the Debit Card Discount + Overdraft Interest Discount coefficient is 0.0059 (se=0.0039).

Table 4's results suggest that the overdrafting changes induced by bank promotions are not "sticky": the changes do not persist long after the promotions stop. This could be related to the fact that consumers overdraft passively in the course of checking account usage (rather than with separate transactions where they explicitly draw from a line of credit): overdrafts are plausibly low "exposure" and hence relatively likely to be ignored or forgotten (Bordalo, Gennaioli, and Shleifer 2015). In any case, the lack of persistent effects suggests that consumer learning and/or attention concerning overdrafts depreciates quickly, and hence that advertising and de-biasing campaigns must persist to be effective.

4.4 Heterogeneous Effects of Overdraft Messaging?

Our motivating models predict that responses to add-on advertising will vary with how well-informed and/or attentive the consumer is. For example, a well-informed and attentive consumer should exhibit a standard demand response to the overdraft interest discount (increasing not decreasing demand), and respond weakly if at all to the overdraft availability reminder.

We construct two proxies for baseline exposure to the overdraft product and then test whether each proxy mediates our main treatment effects. The first proxy is recent overdraft use prior to the experiment. 18% of our sample overdrafted at some point during September 2011-August 2012. Table 5 interacts a prior use indicator with our main treatment variables and shows little evidence of heterogeneous treatment effects (p-values at the bottom of the table). The second proxy is generated by the August 30th overdraft availability message YK sent to half of the sample. This initial message may have provided some consumers with information and/or drawn their attention to the add-on. But Appendix Table 3 shows that while the point estimate for the interest discount is more negative for those who first received the August 30th overdraft availability message, the difference is not statistically significant (p-values at bottom of the table). We caution however that the confidence intervals for the heterogeneity results include both null and economically important point estimates.

We also examine heterogeneity with respect to baseline checking account balance. The intuition is that those with higher account balances may be less responsive to the messages (both availability and discount) as it is less relevant for them. As reported in Table 6, we do not find any evidence of heterogeneity in this regard. This could reflect that most accounts, even those with above-median balances at baseline, dip low enough that

overdrafting is viable and relevant. (Unfortunately we lack baseline data on *minimum* balances).

Another interesting margin of heterogeneity concerns the hypothesis that the most-intense overdraft users should be better informed about overdraft costs and more attuned to overdraft availability; consequently, both treatment effects should attenuate and even flip sign at high usage. We explore this hypothesis by estimating quantile treatment effects, and do see hints of the expected pattern (Appendix Figures 1a and 1b). However we caution that these quantile regressions provide only coarse tests of the relationship between overdraft intensity and demand responses —they are underpowered, and only identify the heterogeneity of interest with the additional assumption that ordinal position in the overdraft distribution is unaffected by the messages.

4.5 Other Treatment Variations: Messaging Frequency and Duration

Appendix Table 4 examines our messaging frequency treatments. We find that more-frequent messaging amplifies the demand-depressing effect of the Overdraft Interest Discount and the demand-increasing effect of the Overdraft Availability message.

The odd columns in Appendix Table 4 estimate the effect of one-shot versus repeated messaging for the September 15th message. In each treatment, the results are null for the one-shot, and strong when the messages are repeated—i.e., more negative for the discount, more positive for availability. Thus the Table 2 results are driven by the repeated messaging, not the one-shot message. Four of the six comparisons between one-shot and repeated messaging coefficients are statistically different from each other, and none of the six p-values is greater than 0.17. The even columns further break out repeated messaging into more- versus less-frequent (every 10 days versus every 20 days), and although the point estimates on more-frequent are indeed larger in absolute value in 5 of the 6 comparisons, no difference is statistically significant. The key takeaways from Appendix Table 4 are that one message is not enough to generate an effect, and that repeating messaging does influence demand, with diminishing marginal effects from messaging every 10 vs. 20 days.

Appendix Table 5 examines our other margin of messaging and promotional intensity—the "duration", or length of time over which the bank continued to send messages and offer discounts. Recall that short-duration campaigns lasted until November 15th, while long-duration campaigns continued until December 15th. We find little evidence that duration

alone affects demand, either for the overdraft interest discount (row 5 versus row 6), or for the overdraft availability message (row 2 versus row 3), with the lone exception being the availability messaging effect on overdraft average balance (column 6).

Promotional intensity depends on duration *and* frequency, and the even-numbered columns in Appendix Table 5 shed light on these interactions for the overdraft discount (rows 7-10) and the overdraft availability message (rows 2-4). Comparing, e.g., the most-intense price promotion (row 7: long duration + messages after Sept. 15) to the least-intense promotion (row 10: short duration, no messages after Sept. 15), we find lower demand for the most-intense promotion in all three cases, with p-values on the difference of 0.07, 0.08, and 0.002.[17] We see a similar pattern of results for overdraft availability marketing, with the point estimate on the most-intense promotion (row 2: long duration + messages after Sept. 15) substantially larger than that on the least-intense promotion (row 4: Sept. 15 message and no subsequent ones), with p-values of 0.07, 0.25, and 0.02.

All told, we infer that more-intense promotions amplify both the demand-increasing effect of the overdraft availability message and the demand-depressing effect of the overdraft interest discount. It could be that one message is sufficient to change behavior *if noticed*, but that any one message is noticed/encoded with low probability. Or it could be that one message is sufficient to trigger awareness (of a shrouded attribute) but that subsequent messages are required to trigger action. This also implies that demand responds more conventionally to less-intense overdraft price promotion.

5. Discussion

Altogether our results are consistent with models where consumers have limited and reactive attention to add-ons like overdrafts, and suppliers respond by shrouding add-on costs. Specifically, it seems that overdraft costs and availability are not at the top of mind for consumers, and even when brought closer to top of mind they do not stay there for long. As such recent behavioral models of add-on pricing, marketing, and usage do capture key aspects of reality with consumers that tend to underestimate add-on costs and react strongly but temporarily when their attention is drawn to add-on, and with firms that lack incentives to unshroud or compete on add-on costs.

[17] Also somewhat noteworthy is that the point estimates on one-shot messaging about the overdraft discount suggest weakly standard/downward-sloping price effects on demand (rows 1 and 5 in Appendix Table 4, and rows 9 and 10 here in Appendix Table 5).

Classically rational models can explain at most a subset of our results. In principle, one could rationalize the finding that a large price discount decreases overdraft demand with a wealth effect that is big enough to counteract the standard price effect. In practice, such a wealth effect seems implausible. First, the wealth effect would likely operate among those who actually overdraft and have the most wealth to gain, yet we find effects on the extensive margin. Second, the demand reduction does not persist after the promotions stop, implying that any wealth effect would have to be transient and begging the question of why, since the overdraft discount is conditional on usage and hence does not alleviate liquidity constraints. Third, a wealth effect should operate through non-overdraft discounts as well, yet we do not find any evidence that debit card or automated bill payment discounts alone reduce overdrafting. Fourth, a wealth effect does not explain why overdraft availability messaging also affects demand.

Another potential explanation for the overdraft price discount backfiring, particularly in light of Johnson et al (2015), is that consumers view the offer as "too good to be true": they mistrust YK. But mistrust would not readily explain our other key results on availability and lack of persistence; e.g., it is silent on why availability increases demand, and begs the question of why discount-driven mistrust would dissipate almost immediately after the campaign ends. Second, we wonder why consumers would mistrust the overdraft interest discount, but not other deep discounts that prevail in equilibrium, like "free" checking and teaser rates on credit cards. Third, a mistrust channel need not be distinct from the behavioral mechanisms described above; indeed, Johnson et al find that some of their "households expect there to be hidden fees and cumbersome processes that are not compensated by the attractiveness of the offer." Fourth, it is not clear why our consumers would respond by *decreasing* demand for overdrafts rather than simply ignoring the offer: do consumers assume that hidden costs exceed the value of the discount? Fifth, if YK's clients did think that YK was trying to trick them, we might expect them to reduce their demand for other YK services. Yet we find no evidence of this (Appendix Table 2), and the point estimates of the effect of the overdraft discount on the number of active YK accounts are actually positive rather than negative. Sixth, there are several institutional differences between our experiment and Johnson et al's that make mistrust more important in their context. The offer in Johnson et al *was* too good to be true in the sense of being government-subsidized. That offer was also made by a mortgage servicer at a time when the servicing industry was known to be mistreating and scamming customers (e.g., Consumer Financial Protection Bureau 2013).

Rational inattention, and/or high search costs a la Ellison (2005), could explain our results, under particular assumptions that strike us as antithetical to those sorts of models. E.g., instead of remembering new information consumers quickly forget it. Perhaps more critically, instead of mean-zero but noisy perceptions of costs and credit lines, a rational checking account holder in our setting would need to systematically underestimate them. But does it make sense to think of consumers as rational if they hold biased perceptions of contract terms in equilibrium? Much of behavioral economics answers this question with an emphatic no, indeed drawing the line between rational vs. behavioral based on a distinction between mean-zero vs. biased deviations from classical assumptions about decision making.

Relatedly, one could rationalize the bundled discount results with reduced attention costs more broadly, rather than increased attention to add-on costs themselves, as the key factor mediating consumer choices. Specifically, inducing use of auto-pay or debit cards could increase the customer's engagement with the bank, thereby lowering costs of monitoring her cash flows, thereby reducing overdrafts.[18] But messages that *only* discount the bundled products and do not mention overdraft do *not* reduce overdraft usage. Thus the mechanism seems unlikely to hinge *only* on a (rational) increase in attention to the checking account more broadly. Some heightened awareness of add-on costs is likely key.

Having said all that, we are not dismissing rational or near-rational explanations for our results. We are merely speculating that behavioral models of limited attention, memory, and shrouding have great potential to explain the full picture.

6. Conclusion

Working with a large Turkish bank to test SMS direct marketing promotions to 108,000 existing checking account holders, we find that messages promoting a 50% discount on the overdraft interest rate *reduce* overdraft usage. In contrast, messages that merely mention overdraft availability without mentioning price *increase* usage. Neither change persists long after messages stop. We also find some evidence that messaging intensity reinforces the main effects of overdraft discount and availability advertising—messaging more about the overdraft *discount* further reduces demand, while messaging more about overdraft *availability* further increases demand—and that messages offering discounts on debit card or auto-pay use along with overdraft backfire more than simply offering a discount on

[18] This is akin to a lower Gabaix and Laibson-type "substitution cost" of avoiding the add-on.

overdrafts. But not all discounts backfire; in fact, we find some evidence that debit card and auto-pay discounts increase usage of those features.

Practically speaking, our results suggest that competing on overdraft prices will *not* capture market share or increase usage, and thus will lower revenue. Although cutting overdraft prices could in principle generate more customer loyalty or reciprocity, the fact that induced overdraft behavior does not persist suggests these sorts of long-term benefits will not materialize for banks.

More subtly, our results should give pause to third parties seeking to improve overdraft markets with messages (like social marketing campaigns) that draw attention to overdraft costs. To fix ideas, imagine messaging around the theme of "Beware of big overdraft fees!", delivered by an entity that might actually benefit from unshrouding; e.g., a regulator, a firm with social objectives or a product-differentiation strategy, or a personal financial management service. Models of limited and reactive attention formalize the possibility that consumer responses to unshrouding are overreactions rather than optimal responses to new information, and our results are consistent with such models (e.g., Gabaix 2014; Bordalo, Gennaioli, and Shleifer 2015). Our results also suggest that unshrouding could be quite costly to sustain, since its effects do not persist. Moreover, our results suggest that incumbent suppliers could effectively counter unshrouding campaigns by advertising non-price attributes (like availability/credit lines in our case). Hence we are sympathetic to Heidhues, Köszegi, and Murooka's conjecture that third-parties, or deviating firms, will be outgunned in a messaging arms race with incumbent add-on suppliers.

Although our results support policymakers' increasing scrutiny of add-on features, pricing, and practices, we emphasize that we do not conduct the sort of welfare analysis that ought to motivate and guide policy interventions. One reason we stop short of welfare analysis is that we do not actually have the ability to sharply test existing models of shrouded equilibria, since our experiment considers the existing client base of a single firm rather than competition for customers across firms (Armstrong and Vickers 2012; Gabaix and Laibson 2006; Grubb 2015; Heidhues, Köszegi, and Murooka 2017).

Future work would do well to focus on welfare, although doing so may require far more household-level consumption and expenditure data than is typically available in studies that rely on administrative data alone. Refining our design could also help test across models, for instance by testing promotions that mention price without cutting it, and by mentioning

information on price levels as well as or instead of discounts. It would also be useful to examine consumer perceptions of add-on prices and expectations of usage more directly, given their centrality to theoretical assumptions and predictions.

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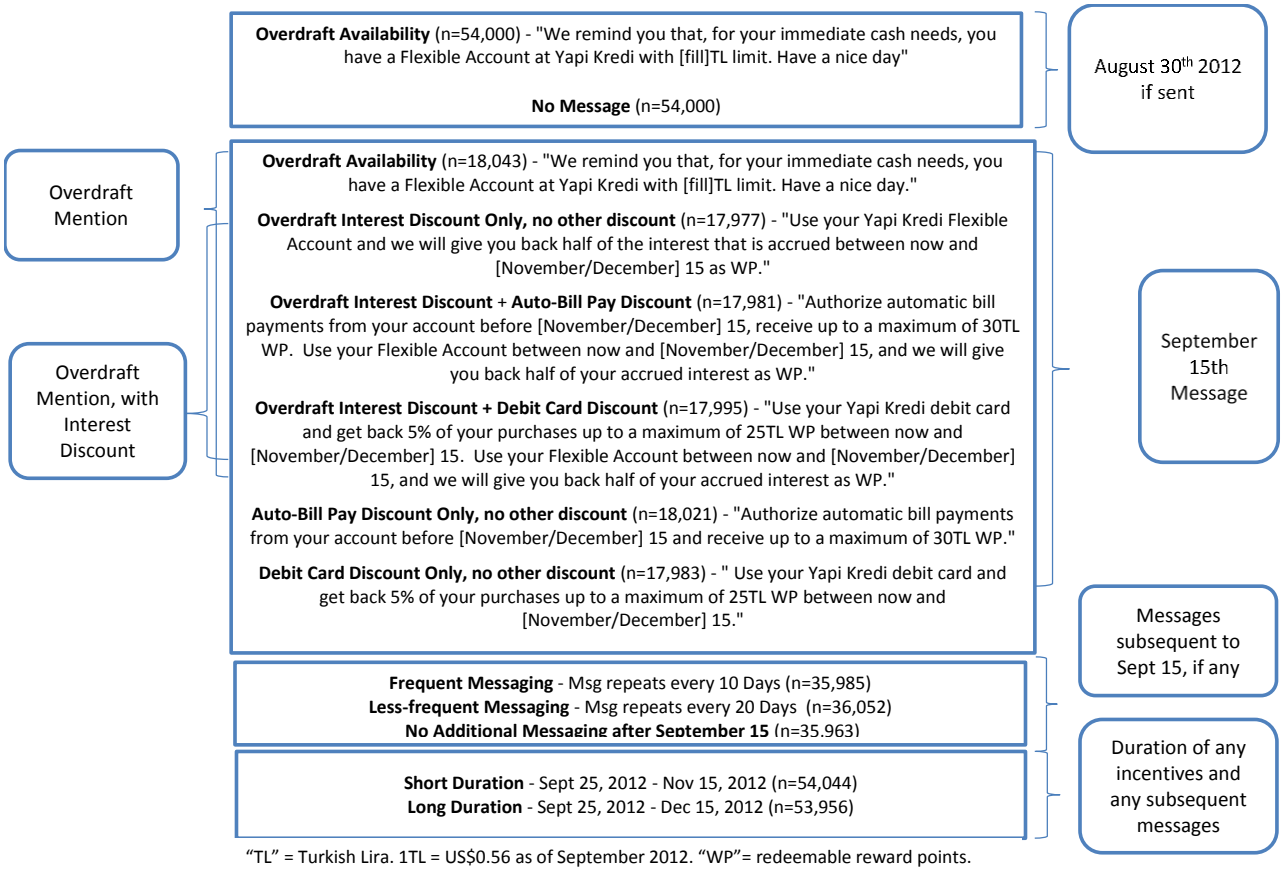
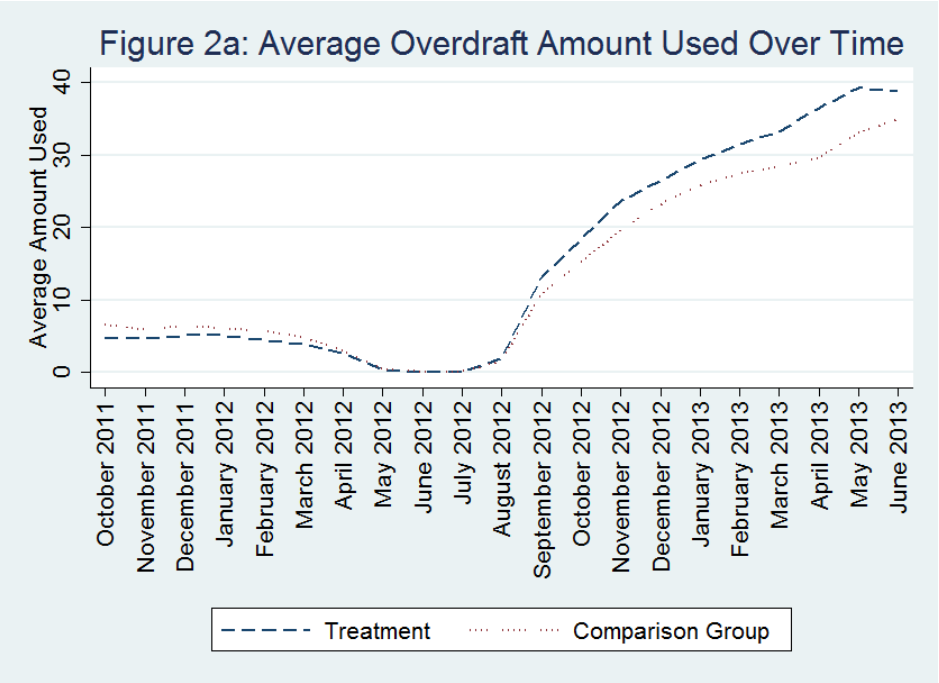
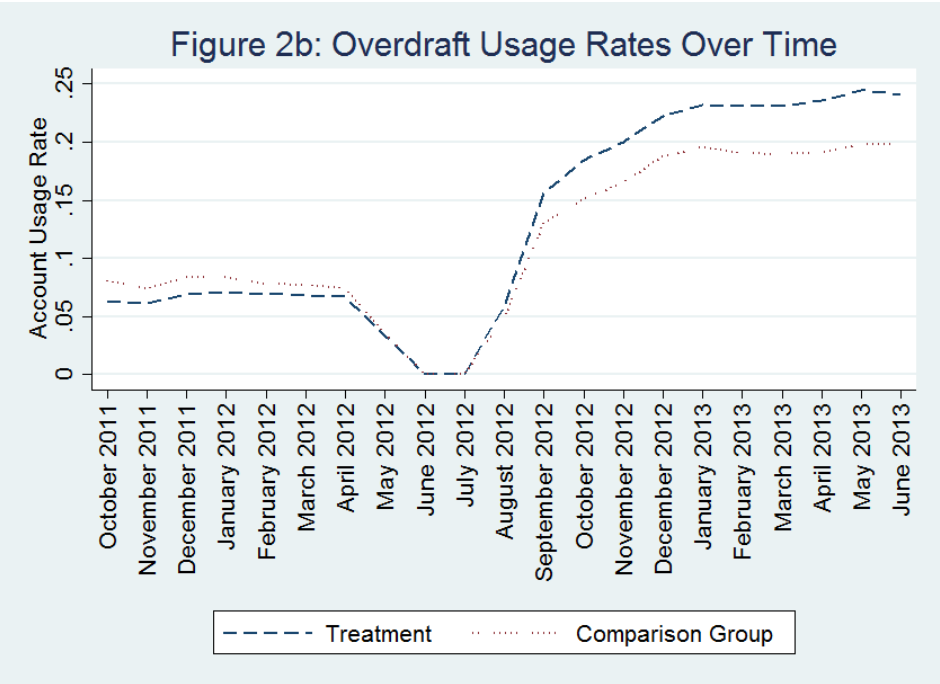
Figure 1. Experimental Design

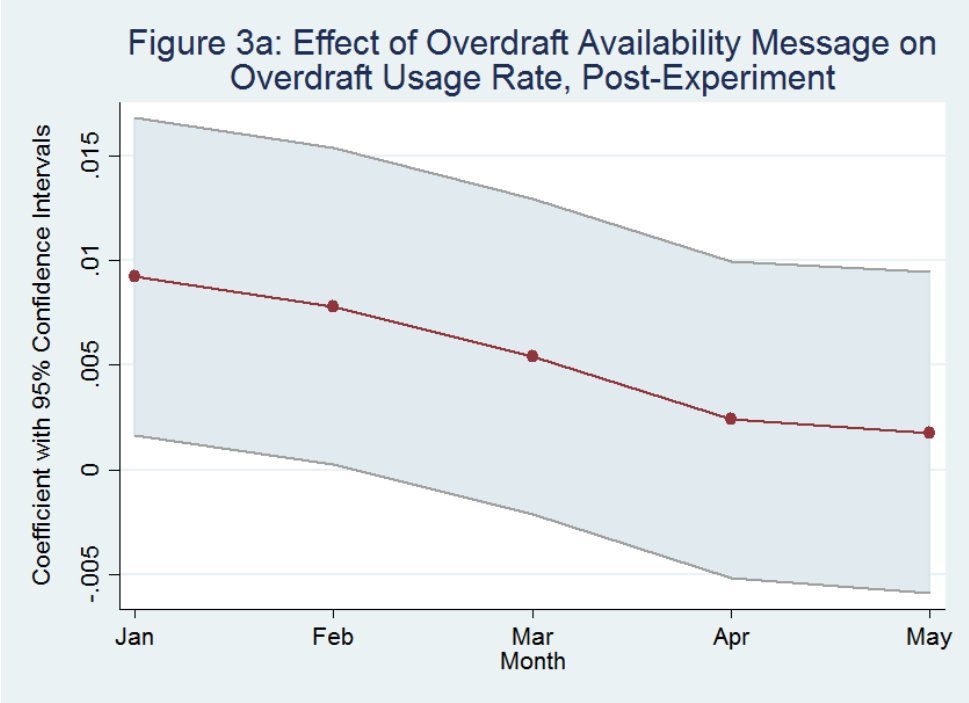


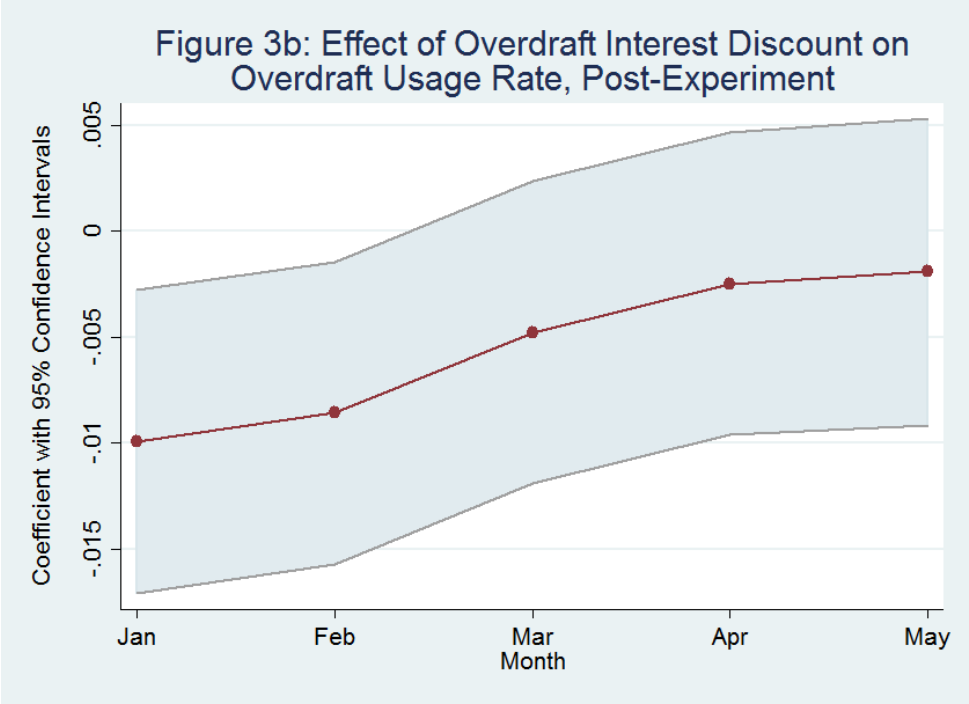
"TL" = Turkish Lira. 1TL = US$0.56 as of September 2012. "WP"= redeemable reward points. Yapi Kredi = the implementing bank. August 30 and other randomizations are independent. Each randomization is stratified on baseline measures of: gender, place of residence, marital status, and overdraft credit limit, and overdraft balance.





Notes: "Overdraft Usage" is indicated by any overdraft balance in a given month. "Treatment" group is the 108,000 accounts included in our experiment; "Comparison" group was selected by YK and not included in our experiment. The experimental period covers August 30, 2012 through December 31 2012. Dips just prior to the experimental period are due to the sample selection criterion of "no overdraft use in May, June and July 2012".





Notes: These are plots of month-by-month treatment effects and associated confidence bands covering the post-experiment period (Jan-May 2013). Coefficients are estimated using a monthly version of the specification in Table 4 Column 1.

Table 1. Baseline variables for sample description and orthogonality checks

			September 15 and Subsequent Messages								p-value
			Overdraft Interest Discount				Auto Bill-Pay Discount		Debit Card Discount Only		from regress
	Full Sample	Got Aug30 Msg	Any	Subsequent Msgs	/Auto Bill-Pa Discount	w/Debit Discount		Subsequent Msgs		Subsequent Msgs	row on columns
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Female	0.289	0.288	0.288	0.288	0.289	0.288	0.288	0.288	0.288	0.288	1.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	
Lives in Istanbul	0.275	0.275	0.275	0.275	0.275	0.274	0.275	0.274	0.276	0.276	1.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	
Lives in Ankara	0.090	0.090	0.090	0.091	0.090	0.090	0.091	0.090	0.089	0.089	1.000
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	
Lives in Izmir	0.050	0.050	0.050	0.050	0.050	0.050	0.050	0.050	0.050	0.051	1.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
Lives in Bursa	0.057	0.057	0.057	0.057	0.057	0.058	0.057	0.058	0.057	0.057	0.998
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	
Married	0.572	0.572	0.572	0.572	0.572	0.571	0.571	0.571	0.573	0.573	1.000
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	
Overdraft Limit Between	0.284	0.284	0.284	0.284	0.282	0.285	0.284	0.283	0.284	0.284	1.000
1/2 and 1 monthly min wage	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	
Overdraft Limit > monthly min wage	0.081	0.081	0.081	0.081	0.081	0.080	0.082	0.082	0.082	0.082	0.999
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	
Main Acct Bal: Balance below med	0.428	0.428	0.428	0.428	0.428	0.427	0.427	0.427	0.428	0.428	1.000
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	
Main Acct Bal: Balance above med	0.427	0.427	0.427	0.427	0.427	0.427	0.428	0.427	0.426	0.426	1.000
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	
Used Overdraft Sept 2011-Apr 2012	0.184	0.185	0.184	0.184	0.184	0.183	0.179	0.180	0.189	0.188	0.502
	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	
Auto Bill Pay Registered	0.028	0.028	0.027	0.026	0.026	0.026	0.029	0.028	0.030	0.031	0.193
any time Sept 2011-July 2012	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	
Any Debit Card POS Transactions	0.334	0.333	0.335	0.336	0.331	0.340	0.328	0.327	0.339	0.339	0.309
Sept 2011-July 2012 > 0	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	
Observations	108000	53974	53953	35989	17981	17995	18021	12026	17983	11985	

Columns 1-10 report proportions, with standard errors in parentheses. Column 11 reports the p-value from the test of thejoint hypothesis that the coefficients on the treatment indicators are zero. Variables measured in July 2012 unless noted otherwise. Figure 1 provides details on the treatment arms.

Table 2: Effects of Overdraft Marketing on Overdraft Usage During Experiment (September - December 2012)

LHS variable:	*Overdraft Account Used*		*Days with Overdraft Balance*		*Mean(Overdraft Account Balance)*	
August 30th Message:	(1)	(2)	(3)	(4)	(5)	(6)
(1) Overdraft Availability	0.0007	0.0007	0.0869**	0.0869**	1.0555**	1.0554**
	(0.0028)	(0.0028)	(0.0365)	(0.0365)	(0.4701)	(0.4701)
(2) No Message	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
September 15 Message:						
(3) Overdraft Mention: identifies effect of	0.0089**	0.0089**	0.1321**	0.1321**	0.9562	0.9562
Overdraft Availability message	(0.0042)	(0.0042)	(0.0553)	(0.0553)	(0.7029)	(0.7029)
(4) Overdraft Mention, with Interest Discount	-0.0124***		-0.1607***		-0.7427	
	(0.0040)		(0.0522)		(0.6718)	
(5) Overdraft Mention, with Interest Discount only		-0.0069		-0.0854		-0.3382
		(0.0049)		(0.0640)		(0.8108)
(6) Overdraft Mention, w/Interest + Auto Bill-Pay Discounts		-0.0137***		-0.1884***		-1.0909
		(0.0049)		(0.0635)		(0.8195)
(7) Overdraft Mention, w/Interest + Debit Card Discounts		-0.0167***		-0.2083***		-0.7989
		(0.0049)		(0.0632)		(0.8397)
(8) No Overdraft Mention =	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
Auto Bill-Pay Discount Only or Debit Card Discount Only						
Mean(LHS)	0.3077	0.3077	2.7676	2.7676	26.8511	26.8511
SD(LHS)	(0.46)	(0.46)	(6.05)	(6.05)	(77.97)	(77.97)
Observations	108000	108000	108000	108000	108000	108000

*** p<=0.01, ** p<=0.05, * p<=0.10. Each column presents treatment effect estimates from a single OLS regression, with Huber-White standard errors, of the usage measure in the column heading on the experimental variables in the rows. Variables in rows 4-7 identify the additive effect of one or more of the interest discount arms; i.e., they identify the difference in treatment effects between the Overdraft Availability message and the Overdraft Interest Discount. Usage measures cover September 15-December 31, 2012. Account balances are in Turkish Lira, 1TL=$0.56 at the start of the experiment. Figure 1 summarizes the experimental design and shows message scripts. Regressions also include controls for randomization strata. Unit of observation is the unit of randomization: a checking account.

Table 3. Effects of Auto Bill-Pay and Debit Card Marketing During Experiment (September-December 2012)

LHS variable:	*Any Debit Card POS Transactions*	*Any Auto Bill-Pay Registered*	*Overdraft Account Used*	*Days with Overdraft Balance*	*Avg Overdraft Account Balance (TL)*
September 15 Message:	(1)	(2)	(3)	(4)	(5)
(1) Debit Card Discount: includes both Debit Card Discount Only,	0.0053		0.0029	-0.0049	-0.3401
and Overdraft Interest Discount + Debit Discount	(0.0040)		(0.0049)	(0.0635)	(0.7896)
(2) Debit Card Discount + Overdraft Interest Discount	-0.0008		-0.0127***	-0.1180*	-0.1206
	(0.0049)		(0.0048)	(0.0627)	(0.8204)
(3) Auto Bill-Pay Discount (includes both Auto Bill-Pay Discount		0.0042***	-0.0070	-0.0883	-0.8953
Only, and Overdraft Interest Discount + Auto Bill-Pay Discount)		(0.0011)	(0.0048)	(0.0634)	(0.7969)
(4) Auto Bill-Pay Discount + Overdraft Interest Discount		-0.0018	0.0001	-0.0147	0.1426
		(0.0013)	(0.0048)	(0.0629)	(0.8060)
(5) Sep15 Message: Overdraft Interest Discount only	Omitted	Omitted	Omitted	Omitted	Omitted
Controls for Aug30 & Sep15 Overdraft Availability Only Msgs	yes	yes	yes	yes	yes
Mean(LHS)	0.3367	0.0134	0.3077	2.7676	26.8511
SD(LHS)	(0.47)	(0.11)	(0.46)	(6.05)	(77.97)
Observations	108000	108000	108000	108000	108000

Each column presents treatment effect estimates from a single OLS regression, with Huber-White standard errors, of the usage measure in the column heading on the experimental variables in the rows. Variables in rows 2 and 4 identify the additive effect of the overdraft interest discount; i.e., they identify the difference in treatment effects between the discount on some other service in row 1 or 3 and that discount bundled with the overdraft interest discount. Usage measures cover September 15-December 31, 2012. "POS" = point-of-sale. Account balances are in Turkish Lira, 1TL=$0.56 at the start of the experiment. Figure 1 summarizes the experimental design and shows message scripts. Regressions also include controls for randomization strata. Unit of observation is the unit of randomization: a checking account.

Table 4: Effects of Overdraft Marketing on Overdraft Usage After Experiment (January - May 2013)

LHS variable:	*Overdraft Account Used*		*Mean(Overdraft Account Balance)*	
August 30th Message:	(1)	(2)	(3)	(4)
(1) Overdraft Availability	-0.0004	-0.0004	0.7385	0.7383
	(0.0029)	(0.0029)	(0.6262)	(0.6262)
(2) No Message	Omitted	Omitted	Omitted	Omitted
September 15 Message:				
(3) Overdraft Mention: identifies effect of	0.0055	0.0055	0.0320	0.0320
Overdraft Availability message	(0.0044)	(0.0044)	(0.9344)	(0.9344)
(4) Overdraft Mention, with Interest Discount	-0.0050		0.1192	
	(0.0042)		(0.8916)	
(5) Overdraft Mention, with Interest Discount only		-0.0024		0.8686
		(0.0051)		(1.1030)
(6) Overdraft Mention, w/Interest + Auto Bill-Pay Discounts		-0.0067		-0.7063
		(0.0051)		(1.0724)
(7) Overdraft Mention, w/Interest + Debit Card Discounts		-0.0059		0.1954
		(0.0051)		(1.1058)
(8) No Overdraft Mention =	Omitted	Omitted	Omitted	Omitted
Auto Bill-Pay Discount Only or Debit Card Discount Only				
Mean(LHS)	0.3713	0.3713	34.0032	34.0032
SD(LHS)	(0.48)	(0.48)	(103.98)	(103.98)
Observations	108000	108000	108000	108000

*** p<=0.01, ** p<=0.05, * p<=0.10. Each column presents treatment effect estimates from a single OLS regression, with Huber-White standard errors, of the usage measure in the column heading on the experimental variables in the rows. Variables in rows 4-7 identify the additive effect of one or more of the interest discount arms; i.e., they identify the difference in treatment effects between the Overdraft Availability message and the Overdraft Interest Discount. Usage measures cover January 1-May 31, 2012. Account balances are in Turkish Lira, 1TL=$0.56 at the start of the experiment. Figure 1 summarizes the experimental design and shows message scripts. Regressions also include controls for randomization strata. Unit of observation is the unit of randomization: a checking account.

Table 5. Heterogenous Treatment Effects by Prior Use?

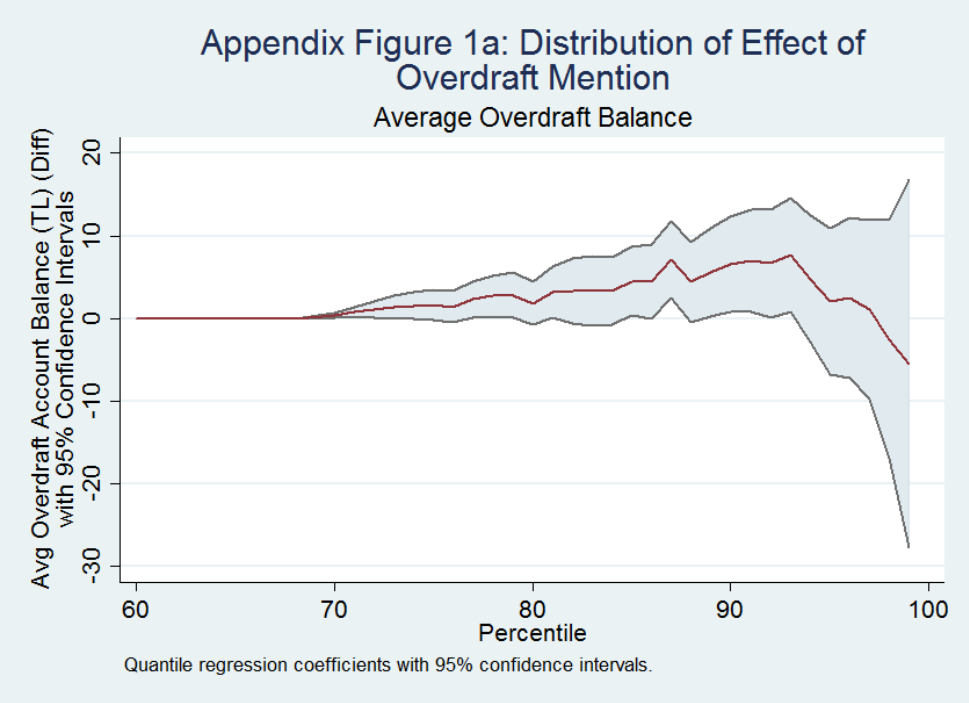
LHS variable:	*Overdraft*		*Days with*		*Mean(Overdraft*	
Measured during experiment: over September 15-December 31, 2012	*Account Used*		*Overdraft Balance*		*Account Balance)*	
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Prior Overdraft Use	0.3201***	0.3201***	4.0976***	4.0976***	41.6837***	41.6835***
	(0.0067)	(0.0067)	(0.1093)	(0.1093)	(1.5148)	(1.5148)
No Overdraft Use September 2011-April 2012	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
September 15th Message:						
(2) Overdraft Mention * Prior Overdraft Use	0.0193*	0.0193*	0.2546	0.2546	0.5915	0.5920
	(0.0107)	(0.0107)	(0.1807)	(0.1807)	(2.5326)	(2.5326)
(3) Overdraft Mention * No Prior Overdraft Use	0.0055	0.0055	0.0912*	0.0912*	0.8841	0.8843
	(0.0044)	(0.0044)	(0.0521)	(0.0521)	(0.6385)	(0.6385)
(4) Overdraft Mention, with Interest Discount * Prior Overdraft Use	-0.0205**		-0.2640		0.4073	
	(0.0101)		(0.1703)		(2.3965)	
(5) Overdraft Mention, with Interest Discount * No Prior Overdraft Use	-0.0094**		-0.1223**		-0.8171	
	(0.0041)		(0.0492)		(0.6130)	
(6) Overdraft Interest Discount, No Other Discount * Prior Overdraft Use		-0.0176		-0.1611		1.1800
		(0.0124)		(0.2097)		(2.9235)
(7) Overdraft Interest Discount, No Other Discount * No Prior Overdraft Use		-0.0036		-0.0559		-0.5237
		(0.0051)		(0.0603)		(0.7335)
(8) Overdraft Interest Discount + Auto Bill-Pay Discount * Prior Overdraft Use		-0.0204		-0.3532*		-1.6837
		(0.0124)		(0.2093)		(2.9142)
(9) Overdraft Interest Discount + Auto Bill-Pay Discount * No Prior Overdraft Use		-0.0109**		-0.1362**		-0.7935
		(0.0050)		(0.0598)		(0.7550)
(10) Overdraft Interest Discount + Debit Card Discount * Prior Overdraft Use		-0.0236*		-0.2784		1.7192
		(0.0124)		(0.2093)		(3.0806)
(11) Overdraft Interest Discount + Debit Card Discount * No Prior Overdraft Use		-0.0137***		-0.1745***		-1.1335
		(0.0050)		(0.0592)		(0.7528)
No Overdraft Mention = Auto Bill-Pay Discount Only or Debit Card Discount Only	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
p-value on F-test of equality between rows (2) and (3)	0.2348	0.2348	0.3852	0.3851	0.9108	0.9109
p-value on F-test of equality between rows (4) and (5)	0.3068		0.4239		0.6203	
p-value on F-test of equality between rows (6) and (7)		0.2952		0.6297		0.5716
p-value on F-test of equality between rows (8) and (9)		0.4793		0.3190		0.7675
p-value on F-test of equality between rows (10) and (11)		0.4578		0.6330		0.3678
Mean(LHS)	0.3077	0.3077	2.7676	2.7676	26.8511	26.8511
SD(LHS)	(0.46)	(0.46)	(6.05)	(6.05)	(77.97)	(77.97)
Observations	108000	108000	108000	108000	108000	108000

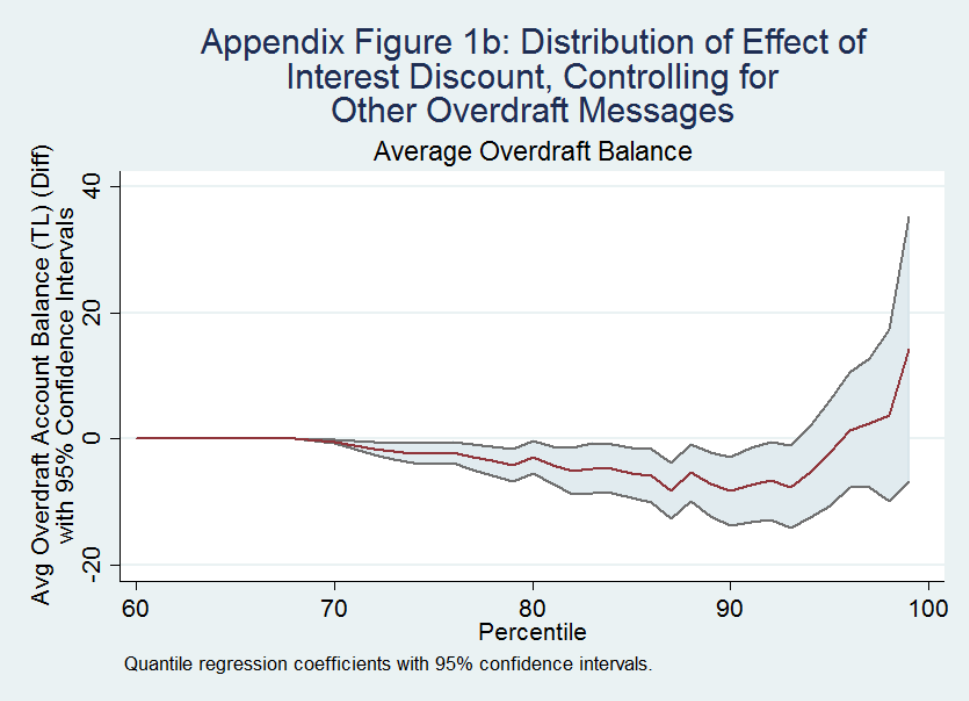
*** p<=0.01, ** p<=0.05, * p<=0.10. Each column presents treatment effect estimates from a single OLS regression, with Huber-White standard errors, of the usage measure in the column heading on the experimental variables in the rows. Variables in rows 4-11 identify the additive effect of one or more of the interest discount arms; i.e., they identify the difference in treatment effects between the Overdraft Availability message and the Overdraft Interest Discount. Overdraft usage measures cover September 15-December 31, 2012, except for prior use, which is measured 1/0 pre-experiment, over September 2011-April 2012. Account balances are in Turkish Lira, 1TL=$0.56 at the start of the experiment. Figure 1 summarizes the experimental design and shows message scripts. Regressions also include controls for randomization strata. Unit of observation is the unit of randomization: a checking account.

Table 6. Heterogenous Treatment Effects by Baseline Account Balance

LHS variable:	*Overdraft Account Used*		*Days with Overdraft Balance*		*Account Balance (TL)*	
Overdraft Account Used	(1)	(2)	(3)	(4)	(5)	(6)
(1) Baseline Account Balance Above Median	0.0112	0.0113	-0.2678***	-0.2674***	-1.6892	-1.6870
	(0.0071)	(0.0071)	(0.0926)	(0.0926)	(1.1565)	(1.1565)
(2) Sep15 Message: Overdraft Mention * Baseline Acct Bal Above Median	0.0107*	0.0107*	0.1244*	0.1244*	0.8753	0.8752
	(0.0059)	(0.0059)	(0.0692)	(0.0692)	(0.9368)	(0.9368)
(3) Sep15 Message: Overdraft Mention * Baseline Acct Bal Below Median	0.0082	0.0082	0.1573*	0.1573*	0.9519	0.9519
	(0.0060)	(0.0060)	(0.0861)	(0.0861)	(1.0263)	(1.0263)
(4) Sep15 Message: Overdraft Mention, with Interest Discount * Baseline Acct Bal Above Median	-0.0138**		-0.1219*		-0.6248	
	(0.0056)		(0.0655)		(0.8936)	
(5) Sep15 Message: Overdraft Mention, with Interest Discount * Baseline Acct Bal Below Median	-0.0117**		-0.2070**		-0.6918	
	(0.0057)		(0.0812)		(0.9825)	
(6) Sep15 Message: Overdraft Interest Discount; No Other Discount * Baseline Acct Bal Above Median		-0.0095		-0.0695		-0.3711
		(0.0069)		(0.0803)		(1.0767)
(7) Sep15 Message: Overdraft Interest Discount; No Other Discount * Baseline Acct Bal Below Median		-0.0053		-0.1199		-0.3425
		(0.0070)		(0.0996)		(1.1917)
(8) Sep15 Message: Overdraft Interest Discount; Auto Debit Discount * Baseline Acct Bal Above Median		-0.0129*		-0.1216		-0.5927
		(0.0069)		(0.0800)		(1.0773)
(9) Sep15 Message: Overdraft Interest Discount; Auto Debit Discount * Baseline Acct Bal Below Median		-0.0151**		-0.2608***		-1.2366
		(0.0069)		(0.0985)		(1.2193)
(10) Sep15 Message: Overdraft Interest Discount; Debit Card Discount * Baseline Acct Bal Above Median		-0.0189***		-0.1742**		-0.9077
		(0.0068)		(0.0791)		(1.1189)
(11) Sep15 Message: Overdraft Interest Discount; Debit Card Discount * Baseline Acct Bal Below Median		-0.0145**		-0.2401**		-0.4921
		(0.0070)		(0.0986)		(1.2285)
p-value on F-test of equality between rows (2) and (3)	0.7714	0.7714	0.7661	0.7661	0.9561	0.9560
p-value on F-test of equality between rows (4) and (5)	0.7897		0.4151		0.9598	
p-value on F-test of equality between rows (6) and (7)		0.6683		0.6939		0.9858
p-value on F-test of equality between rows (8) and (9)		0.8172		0.2727		0.6923
p-value on F-test of equality between rows (10) and (11)		0.6509		0.6024		0.8026
Mean(LHS) Sept-Nov	0.3077	0.3077	2.7676	2.7676	26.8511	26.8511
std dev	(0.46)	(0.46)	(6.05)	(6.05)	(77.97)	(77.97)
Observations	107337	107337	107337	107337	107337	107337

*** p<=0.01, ** p<=0.05, * p<=0.10. Each column presents treatment effect estimates from a single OLS regression, with Huber-White standard errors, of the usage measure in the column heading on the experimental variables in the rows. Variables in rows 2-11 identify the effect of one of the interest discount arm interacted with baseline account balance. Usage measures cover September 15-December 31, 2012. Account balances are in Turkish Lira, 1TL=$0.56 at the start of the experiment. Figure 1 summarizes the experimental design and shows message scripts. Regressions also include controls for randomization strata. Unit of observation is the unit of randomization: a checking account.





Appendix Figure 1a plots the marginal effects of a message mentioning the overdraft account on each quantile of overdraft account balance. Appendix Figure 1b plots the average marginal effects of an interest discount on each quantile of overdraft account balance.

Appendix Table 1. Effects of Marketing on Average Monthly Checking + Savings Balances During Experiment (September-December 2012)

LHS variable:	*Average Balances*		*Top 1% Winsorized*		*Top 1% Dropped*		*Log*	
Panel A. Overdraft Marketing	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
August 30th Message:								
(1) Overdraft Availability	-8.8782	-8.8742	-8.5687*	-8.5665*	-7.3419*	-7.3425*	-0.0118	-0.0118
	(9.7697)	(9.7695)	(4.9102)	(4.9100)	(3.7965)	(3.7965)	(0.0115)	(0.0115)
(2) No Message	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
September 15 Message:								
(3) Overdraft Mention: identifies effect of	-3.5914	-3.5934	2.5982	2.5969	3.9231	3.9228	-0.0082	-0.0082
Overdraft Availability message	(13.9764)	(13.9766)	(7.3735)	(7.3736)	(5.7159)	(5.7159)	(0.0171)	(0.0171)
(4) Overdraft Mention, with Interest Discount	11.4448		3.0329		1.9943		0.0108	
	(12.2665)		(6.9651)		(5.4186)		(0.0162)	
(5) Overdraft Mention, with Interest Discount only		24.3365		12.3639		8.0409		0.0237
		(15.6249)		(8.6572)		(6.7305)		(0.0199)
(6) Overdraft Mention, w/Interest + Auto Bill-Pay Discounts		19.7938		7.1275		0.7018		0.0047
		(15.7186)		(8.5911)		(6.5985)		(0.0199)
(7) Overdraft Mention, w/Interest + Debit Card Discounts		-9.7688		-10.3752		-2.7430		0.0042
		(14.9912)		(8.3311)		(6.5513)		(0.0197)
(8) No Overdraft Mention =	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
Auto Bill-Pay Discount Only or Debit Card Discount Only								
Mean(LHS)	415.3350	415.3350	359.4495	359.4495	305.0098	305.0098	4.2853	4.2853
SD(LHS)	(1631.25)	(1631.25)	(847.06)	(847.06)	(654.51)	(654.51)	(2.11)	(2.11)
Observations	108000	108000	108000	108000	106920	106920	106020	106020
Panel B. Auto Bill-Pay and Debit Card Marketing								
September 15 Message:								
(1) Debit Card Discount (includes both Debit Card Discount Only,	-13.0974		-10.2527		-12.8017**		-0.0213	
and Overdraft Interest Discount + Debit Discount)	(13.5978)		(7.4184)		(5.6403)		(0.0172)	
(2) Debit Card Discount + Overdraft Interest Discount	-8.8192		-6.2956		6.0471		0.0137	
	(15.5592)		(8.3217)		(6.4248)		(0.0198)	
(3) Auto Bill-Pay Discount (includes both Auto Bill-Pay Discount	-4.0234		-7.2920		-3.0861		0.0140	
Only, and Overdraft Interest Discount + Auto Bill-Pay Discount)	(17.0081)		(7.3615)		(5.7486)		(0.0172)	
(4) Auto Bill-Pay Discount + Overdraft Interest Discount	11.6737		8.2507		-0.2214		-0.0211	
	(19.2085)		(8.5362)		(6.5709)		(0.0199)	
(5) Overdraft Interest Discount only	Omitted		Omitted		Omitted		Omitted	
Controls for Aug30 & Sep15 Overdraft Availability Only Messages	yes		yes		yes		yes	
Mean(LHS)	415.3350		359.4495		305.0098		4.2853	
SD(LHS)	(1631.25)		(847.06)		(654.51)		(2.11)	
Observations	108000		108000		106920		106020	

*** p<=0.01, ** p<=0.05, * p<=0.10. Each column-panel presents treatment effect estimates from a single OLS regression, with Huber-White standard errors, of the average balances measure in the column heading on the experimental variables in the rows. Variables in Panel A rows 4-7 identify the additive effect of one or more of the interest discount arms; i.e., they identify the difference in treatment effects between the Overdraft Availability message and the Overdraft Interest Discount. Variables in Panel B rows 2 and 4 identify the additive effect of the overdraft interest discount; i.e., they identify the difference in treatment effects between the discount on some other service in row 1 or 3 and that discount bundled with the overdraft interest discount. Balance measures cover September 15-December 31, 2012. Account balances are in Turkish Lira, 1TL=$0.56 at the start of the experiment. Figure 1 summarizes the experimental design and shows message scripts. Regressions also include controls for randomization strata. Unit of observation is the unit of randomization: a checking account.

Appendix Table 2: Effects of Overdraft Marketing on Number of Active Products During Experiment

LHS variable:	*Count*		*Log*	
August 30th Message:	(1)	(2)	(3)	(4)
(1) Overdraft Availability	0.0131	0.0131	0.0028	0.0028
	(0.0136)	(0.0136)	(0.0047)	(0.0047)
(2) No Message	Omitted	Omitted	Omitted	Omitted
September 15 Message:				
(3) Overdraft Mention: identifies effect of	-0.0002	-0.0002	-0.0005	-0.0005
Overdraft Availability message	(0.0204)	(0.0204)	(0.0070)	(0.0070)
(4) Overdraft Mention, with Interest Discount	0.0155		0.0093	
	(0.0192)		(0.0066)	
(5) Overdraft Mention, with Interest Discount only		0.0145		0.0074
		(0.0235)		(0.0081)
(6) Overdraft Mention, w/Interest + Auto Bill-Pay Discounts		-0.0073		0.0027
		(0.0234)		(0.0081)
(7) Overdraft Mention, w/Interest + Debit Card Discounts		0.0393*		0.0177**
		(0.0236)		(0.0081)
(8) No Overdraft Mention =	Omitted	Omitted	Omitted	Omitted
Auto Bill-Pay Discount Only or Debit Card Discount Only				
Mean(LHS)	2.6238	2.6238	0.6348	0.6348
SD(LHS)	(2.27)	(2.27)	(0.78)	(0.78)
Observations	108000	108000	108000	108000

*** p<=0.01, ** p<=0.05, * p<=0.10. Each column presents treatment effect estimates from a single OLS regression, with Huber-White standard errors, of the measure of the account holder's active products at the experimenting bank (including the checking account that is our unit of observation) in the column heading on the experimental variables in the rows. Variables in rows 4-7 identify the additive effect of one or more of the interest discount arms; i.e., they identify the difference in treatment effects between the Overdraft Availability message and the Overdraft Interest Discount. Figure 1 summarizes the experimental design and shows message scripts. Regressions also include controls for randomization strata. Unit of observation is the unit of randomization: a checking account.

Appendix Table 3. Does the August 30 Message Mediate Treatment Effects of Later Messages?

LHS variable:	*Overdraft*		*Days with*		*Mean(Overdraft*	
Measured during experiment: over September 15-December 31, 2012	*Account Used*		*Overdraft Balance*		*Account Balance)*	
August 30th Message:	(1)	(2)	(3)	(4)	(5)	(6)
(1) Overdraft Availability	-0.0001	-0.0001	0.0477	0.0476	1.1445	1.1444
	(0.0049)	(0.0049)	(0.0631)	(0.0631)	(0.7875)	(0.7875)
No message	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
September 15 Message:						
(2) Overdraft Mention with Aug 30 Msg	0.0114*	0.0114*	0.2021**	0.2021**	0.7911	0.7912
	(0.0060)	(0.0060)	(0.0797)	(0.0797)	(1.0046)	(1.0046)
(3) Overdraft Mention without Aug 30 Msg	0.0063	0.0063	0.0622	0.0622	1.1209	1.1212
	(0.0060)	(0.0060)	(0.0767)	(0.0767)	(0.9814)	(0.9815)
(4) Overdraft Mention, with Interest Discount, with Aug 30 Msg	-0.0151***		-0.2149***		-0.6116	
	(0.0056)		(0.0755)		(0.9576)	
(5) Overdraft Mention, with Interest Discount, w/o Aug 30 Msg	-0.0098*		-0.1067		-0.8735	
	(0.0056)		(0.0722)		(0.9420)	
(6) Overdraft Interest Discount, No Other Discount, with Aug 30 Msg		-0.0082		-0.1819**		-0.9928
		(0.0069)		(0.0916)		(1.1290)
(7) Overdraft Interest Discount, No Other Discount, w/o Aug 30 Msg		-0.0056		0.0109		0.3160
		(0.0069)		(0.0895)		(1.1630)
(8) Overdraft Interest Discount + Auto Bill-Pay Discount, w/Aug 30 Msg		-0.0135*		-0.1894**		0.0209
		(0.0069)		(0.0922)		(1.1848)
(9) Overdraft Interest Discount + Auto Bill-Pay Discount, w/o Aug 30 Msg		-0.0139**		-0.1875**		-2.2005*
		(0.0069)		(0.0875)		(1.1330)
(10) Overdraft Interest Discount + Debit Card Discount, with Aug 30 Msg		-0.0236***		-0.2734***		-0.8622
		(0.0069)		(0.0911)		(1.2162)
(11) Overdraft Interest Discount + Debit Card Discount, w/o Aug 30 Msg		-0.0098		-0.1434		-0.7351
		(0.0069)		(0.0877)		(1.1541)
No Overdraft Mention = Auto Bill-Pay Discount Only or Debit Card Discount Only	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
p-value on F-test of equality between rows (2) and (3)	0.5475	0.5477	0.2058	0.2058	0.8142	0.8140
p-value on F-test of equality between rows (4) and (5)	0.5031		0.2999		0.8454	
p-value on F-test of equality between rows (6) and (7)		0.7870		0.1323		0.4192
p-value on F-test of equality between rows (8) and (9)		0.9644		0.9875		0.1755
p-value on F-test of equality between rows (10) and (11)		0.1556		0.3038		0.9395
Mean(LHS)	0.3077	0.3077	2.7676	2.7676	26.8511	26.8511
SD(LHS)	(0.46)	(0.46)	(6.05)	(6.05)	(77.97)	(77.97)
Observations	108000	108000	108000	108000	108000	108000

*** p<=0.01, ** p<=0.05, * p<=0.10. Each column presents treatment effect estimates from a single OLS regression, with Huber-White standard errors, of the usage measure in the column heading on the experimental variables in the rows. Variables in rows 4-11 identify the additive effect of one or more of the interest discount arms; i.e., they identify the difference in treatment effects between the Overdraft Availability message and the Overdraft Interest Discount. Overdraft usage measures cover September 15-December 31, 2012. Account balances are in Turkish Lira, 1TL=$0.56 at the start of the experiment. Figure 1 summarizes the experimental design and shows message scripts. Regressions also include controls for randomization strata. Unit of observation is the unit of randomization: a checking account.

Appendix Table 4. Effects of Overdraft Marketing Frequency on Overdraft Usage During Experiment (September - December 2012)

LHS variable:	*Overdraft Account Used*		*Days with Overdraft Balance*		*Mean(Overdraft Account Balance)*	
September 15 Message:	(1)	(2)	(3)	(4)	(5)	(6)
(1) Overdraft Mention No Subsequent Messages	0.0003	0.0003	0.0429	0.0429	-0.3108	-0.3108
	(0.0064)	(0.0064)	(0.0835)	(0.0835)	(1.0420)	(1.0420)
(2) Overdraft Mention + Subsequent Messages	0.0132***		0.1765***		1.5872*	
	(0.0049)		(0.0644)		(0.8283)	
(3) Overdraft Mention + Frequent Messages		0.0136**		0.2119**		1.4980
		(0.0065)		(0.0854)		(1.0996)
(4) Overdraft Mention + Infrequent Messages		0.0127**		0.1411*		1.6763
		(0.0065)		(0.0855)		(1.1017)
(5) Overdraft Mention, with Interest Discount, No Subsequent Messages	-0.0017	-0.0017	0.0386	0.0385	1.8416	1.8416
	(0.0069)	(0.0069)	(0.0898)	(0.0898)	(1.1400)	(1.1400)
(6) Overdraft Mention, with Interest Discount + Subsequent Messages	-0.0178***		-0.2602***		-2.0319**	
	(0.0049)		(0.0642)		(0.8315)	
(7) Overdraft Mention, with Interest Discount + Frequent Messages		-0.0207***		-0.3368***		-2.0617*
		(0.0069)		(0.0906)		(1.1763)
(8) Overdraft Mention, with Interest Discount + Infrequent Messages		-0.0149**		-0.1836**		-2.0021*
		(0.0069)		(0.0909)		(1.1706)
(9) No Overdraft Mention = Auto Bill-Pay Discount Only or Debit Card Discount Onl	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
Mean(LHS)	0.3077	0.3077	2.7676	2.7676	26.8511	26.8511
SD(LHS)	(0.46)	(0.46)	(6.05)	(6.05)	(77.97)	(77.97)
Observations	108000	108000	108000	108000	108000	108000

*** p<=0.01, ** p<=0.05, * p<=0.10. Each column presents treatment effect estimates from a single OLS regression, with Huber-White standard errors, of the usage measure in the column heading on the experimental variables in the rows. Variables in rows 5-8 identify the additive effect of one or more of the interest discount arms; i.e., they identify the difference in treatment effects between the Overdraft Availability message and the Overdraft Interest Discount. Usage measures cover September 15-December 31, 2012. Account balances are in Turkish Lira, 1TL=$0.56 at the start of the experiment. Figure 1 summarizes the experimental design and shows message scripts. Regressions also include controls for randomization strata. Unit of observation is the unit of randomization: a checking account.

Appendix Table 5. Effects of Overdraft Message and Discount Duration on Overdraft Usage During Experiment (September -December 2012)

LHS variable:	*Overdraft Account Used*		*Days with Overdraft Balance*		*Mean(Overdraft Account Balance)*	
September 15 Message:	(1)	(2)	(3)	(4)	(5)	(6)
(1) Overdraft Mention: identifies effect of Overdraft Availability Message	0.0089**		0.1320**		0.9556	
	(0.0042)		(0.0553)		(0.7029)	
(2) Overdraft Mention, long duration, with Subsequent Messages		0.0154**		0.1718**		3.1076***
		(0.0065)		(0.0857)		(1.1831)
(3) Overdraft Mention, short duration, with Subsequent Messages		0.0109*		0.1811**		0.0711
		(0.0065)		(0.0853)		(1.0163)
(4) Overdraft Mention No Subsequent Messages		0.0003		0.0429		-0.3108
		(0.0064)		(0.0835)		(1.0420)
(5) Overdraft Mention, with Interest Discount, long duration	-0.0134***		-0.1467**		-1.0662	
	(0.0044)		(0.0583)		(0.7510)	
(6) Overdraft Mention, with Interest Discount, short duration	-0.0114**		-0.1747***		-0.4189	
	(0.0044)		(0.0580)		(0.7516)	
(7) Overdraft Mention, with Interest Discount, long duration, with Subsequent Messages		-0.0227***		-0.2671***		-4.2866***
		(0.0069)		(0.0910)		(1.2475)
(8) Overdraft Mention, with Interest Discount, short duration, with Subsequent Messages		-0.0129*		-0.2533***		0.2155
		(0.0069)		(0.0905)		(1.0995)
(9) Overdraft Mention, with Interest Discount, long duration, w/o Subsequent Messages		0.0006		0.1033		2.3327*
		(0.0077)		(0.1014)		(1.3053)
(10) Overdraft Mention, with Interest Discount, short duration, w/o Subsequent Messages		-0.0040		-0.0263		1.3480
		(0.0077)		(0.1001)		(1.2777)
(11) No Overdraft Mention = Auto Bill-Pay Discount Only or Debit Card Discount Only	Omitted	Omitted	Omitted	Omitted	Omitted	Omitted
p-value on F-test of equality between rows (2) and (3)		0.5931		0.9344		0.0371
p-value on F-test of equality between rows (2) and (4)		0.0738		0.2454		0.0206
p-value on F-test of equality between rows (3) and (4)		0.2094		0.2118		0.7764
p-value on F-test of equality between rows (5) and (6)	0.6183		0.5858		0.3359	
p-value on F-test of equality between rows (7) and (8)		0.3159		0.9141		0.0068
p-value on F-test of equality between rows (7) and (9)		0.0244		0.0065		0.0002
p-value on F-test of equality between rows (7) and (10)		0.0711		0.0750		0.0016
p-value on F-test of equality between rows (8) and (9)		0.1919		0.0087		0.2145
p-value on F-test of equality between rows (8) and (10)		0.3910		0.0926		0.5018
p-value on F-test of equality between rows (9) and (10)		0.5010		0.1555		0.4174
Observations	108000	108000	108000	108000	108000	108000

*** p<=0.01, ** p<=0.05, * p<=0.10. Each column presents treatment effect estimates from a single OLS regression, with Huber-White standard errors, of the usage measure in the column heading on the experimental variables in the rows. Variables in rows 5-10 identify the additive effect of one or more of the interest discount arms; i.e., they identify the difference in treatment effects between the Overdraft Availability message and the Overdraft Interest Discount. Usage measures cover September 15-December 31, 2012. Account balances are in Turkish Lira, 1TL=$0.56 at the start of the experiment. Figure 1 summarizes the experimental design and shows message scripts. Regressions also include controls for randomization strata. Unit of observation is the unit of randomization: a checking account.