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LIMITED AND VARYING CONSUMER ATTENTION:
EVIDENCE FROM SHOCKS TO THE SALIENCE OF BANK OVERDRAFT FEES

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Limited and Varying Consumer Attention: Evidence from Shocks to the Salience of Bank
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ABSTRACT

We explore dynamics of limited attention in the $35 billion market for checking overdrafts, using survey content as shocks to the salience of overdraft fees. Conditional on selection into surveys, individuals who face overdraft-related questions are less likely to incur a fee in the survey month. Taking multiple overdraft surveys builds a "stock" of attention that reduces overdrafts for up to two years. The effects are significant among consumers with lower education and financial literacy. Individuals avoid overdrafts by making fewer low-balance debit transactions and cancelling automatic recurring withdrawals. The results raise new questions about consumer financial protection policy.

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I. Introduction

Does limited attention hinder individuals from acquiring and using readily available information when making financial decisions? If yes, how does consumer attention respond to shocks and evolve over time? Answering these questions is critical for understanding how people make financial tradeoffs and for designing sound public policy in retail financial markets.

We explore the role and dynamics of limited consumer attention in the payment of checking account overdraft fees, broadly defining limited attention as incomplete consideration of elements and/or prices in one's choice set. Over the past decade overdraft fees have become an increasingly important dimension of bank pricing, while generating considerable controversy and policy scrutiny. Limited customer attention is often put forth as an explanation for overdrafts, but little is known about the drivers of overdrafts generally or the importance of limited attention specifically.

To shed light on these issues we examine data from thousands of individuals' checking accounts, covering up to three years of complete account activity. The data are collected by a market research firm that pays panelists for permission to access their accounts. For every account in the data we observe the full set of transactions, account balances, and all fees incurred on the account, including overdraft fees.

We then ask how overdrafts are affected by shocks to checking account holders' attention. Our measure of attention comes from online survey questions answered by panelists. The market research firm invites all panelists to take surveys periodically. All of the surveys have something to do with household financial services, but specific topics and questions vary from survey to survey. Topics/questions are not announced before a consumer chooses to take a survey. Most surveys do not contain overdraft-related questions (we call these "generic" surveys). But a

handful of surveys plausibly draw consumers' attention to overdraft fees by asking questions about overdraft fee payment, use of overdraft protection programs, or (dis)satisfaction with overdraft fees. We label these surveys "overdraft-related." Most of the overdraft-related surveys convey relatively little information in the traditional sense: the questions do not describe specific account terms faced by panelists, and, with one exception, overdraft-related questions represent less than 5% of the total survey. The exception is an "overdraft-focused" survey that asks a series of overdraft-related questions and little else, but even that survey provides no consumer-specific information about account terms. All surveys are administered online, so we observe questions exactly as panelists observe them. Conditional on selection into surveys, panelist fixed effects, and time-varying sample-wide shifts in the propensity to incur overdraft charges, we find that taking an overdraft-related survey has a substantial immediate effect on behavior: within the month of the survey, taking an overdraft-related survey reduces the probability of incurring any overdraft fee by an estimated 3.7 percentage points on a base of 30%. There is weak evidence that overdrafts are positively related to selection into survey-taking overall (see Section IV-B for details), meaning that selection into surveys may attenuate the effect of taking an overdraft-related survey.

To the extent that individuals may start an overdraft survey but not complete it – and hence face no exposure to the overdraft questions – we may underestimate the effect of exposure *per se*. But the magnitude of the difference between this treatment-on-the-treated effect and our intention-to-treat estimates is almost certainly small, given that item non-response to the overdraft questions is only about 5%.

We also find evidence of a strong stock effect: *each* overdraft-related survey taken within a two-year period reduces the probability of paying an overdraft fee by 1.7 percentage points.

Again, this is a within-individual effect and conditional on the timing and number of surveys taken. Selection into survey-taking also attenuates this effect: there is a positive relationship between the number of generic surveys taken and incurring overdraft fees (see Section IV-B).

These full sample results mask important heterogeneity: various results suggest that there are more- and less-marginal panelists who are more- and less-impacted by attention shocks. E.g., the immediate and stock effects of overdraft-related surveys are strong for those with lower education, lower financial literacy, and lower income, and are not statistically significant for those with higher education or higher financial literacy.[1]

Additional evidence on the mechanics and dynamics of attention comes from more detailed exploration of relationships between survey content and overdrafts. The stock effect of taking the "overdraft-focused" survey is significantly greater than the stock effect of taking the more subtle "overdraft-mentioning" surveys. We also find significant effects on overdrafts of non-overdraft questions about spending control, monitoring account balances, and other bank fees. We find no such relationships for a set of "placebo" questions about auto loans, gift cards, or contactless RFID cards.

Panelists who take overdraft-related surveys engineer reductions in overdraft fees by reducing spending transactions. There is some evidence they do this both at high and low frequencies: we see significant reductions in both debit card transactions and recurring "autodebit" transactions. That pattern suggests that reductions occur on transactions where the balance-at-clearing is difficult to monitor/forecast. There is little evidence that panelists avoid overdrafts by maintaining higher account balances. The results are consistent with consumers responding to attention shocks by paying greater attention to account balances.

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[1] We measure financial literacy using responses to a question from a survey consumers complete when they are registering their accounts in the panel: "I know more than most people when it comes to managing my money and investments [agree/disagree/neutral]."

In all, our results suggest that consumers have a limited, time-varying, associative and malleable stock of attention paid to day-to-day household finance. Surveys can operate as shocks to attention, changing behavior and outcomes. Repeated shocks to attention cause sustained changes in behavior that depreciate fairly slowly. Our estimated magnitudes, even at their low end, are economically meaningful. Our combined immediate and stock effects could, if aggregated up fairly conservatively, imply billions of dollars annually in avoided overdraft fees.[2] In light of the fact that individual-level overdraft behavior is "sticky" (strongly positively correlated over time), these are meaningful results.

The results here provide the first evidence from a U.S. setting that being surveyed *per se* changes subjects' subsequent behavior, and they are consistent with other work on survey-taking and decision-making.[3] Zwane et al. (2011) show that being surveyed about health and household finances increases the subsequent purchase of insurance and use of water purification in three developing country settings. This follows a large literature showing that asking subjects more directly about their intent or likelihood of engaging in a targeted behavior (as opposed to being surveyed *per se*: being asked questions that are related to the behavior but do not elicit a forecast) changes that behavior (Dholakia 2010). A literature on priming and other context effects suggests that small changes in economically irrelevant stimuli can have substantial, and

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[2] Even if one assumes that the panelists who avoid overdrafts are those closest to the extensive margin (i.e., those with one or two overdrafts in a month rather than many), applying the treatment effect of six overdraft-related surveys in a two-year period generates a 19% reduction in the total dollar amount of overdraft fees paid in our sample. That percentage multiplied by the dollar amount of overdraft fees paid annually ($35 billion) implies a roughly $7 billion reduction in overdraft fees paid. This figure is illustrative only: one should be cautious about extrapolating our results to the entire U.S. population of deposit account-holders.

[3] If surveys change behavior, this raises ethical and methodological issues about relying on surveys to measure behavior and elicit behavioral parameters. We distinguish between being surveyed *per se*, about topics *related* to the target behaviors of interest, and being asked more directly about the target behavior of interest, in the sense of questions that are administered by a party with a vested interest in the target behavior (e.g., a firm selling the product under study), and/or questions that ask respondents about their intent or likelihood of engaging in the target behavior. See Zwane et al. (2011) for discussions of the evidence on how surveys affect behavior.

long-lasting, effects on consumer choice, at least in the lab (Chartrand et al. 2008; Sela and Shiv 2009; LeBoeuf, Shafir, and Belyavsky Bayuk 2010).[4]

We also add to a body of evidence pointing to an important role for limited attention in (household) finance and other domains (see DellaVigna 2009 for a review).[5] Related theories of attention and salience include Fishman and Hagerty (2003), Hirshleifer and Teoh (2003), Gabaix and Laibson (2006), Peng and Xiong (2006), and Grubb (2012). Empirical work on publicly-traded equities finds that limited attention affects how investors incorporate news into equity prices (Barber and Odean 2008; DellaVigna and Pollet 2009; Hirshleifer, Lim, and Teoh 2009; Hirshleifer, Lim, and Teoh 2011). One can view our paper as studying how investment— and borrowing— in checking accounts responds to *uninformative* attention shocks; Karlan et al (2012) finds that uninformative reminders affect deposit behavior in savings accounts. Our focus on the dynamics of penalty fee payments complements Haselhun et al. (2012) and Agarwal et al. (2011), with those papers estimating direct links between incurring fees now and incurring them later that could come from learning. Our work shows that fee-paying is affected by shocks to attention that contain no information about recent behavior or the structure of fees for a particular individual.

II. Checking Overdraft Fees: Background and Public Policy

A. Overdrafts and Bank Policies

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[4] Persuasive advertising is an important application of priming effects; e.g., Bertrand et al. (2010) find that small changes in uninformative ad content have large effects on the take-up of expensive consumer loans.

[5] See also and Lee and Malmendier (2011) for theory and empirics on limited attention in auctions; Lacetera et al.(2012) on limited attention in the car market; and Alba, Hutchinson, and Lynch (1991), Eliaz and Spiegler (2011), and Koszegi and Szeidl (2012) on consideration sets.

An overdraft occurs when a checking account owner initiates a transaction that would bring the account balance below zero. Any account debit can trigger an overdraft: an ATM withdrawal, check, debit card transaction, Automated Clearing House (ACH) debit, etc.[6]

When an overdraft occurs, a bank can pay or decline the transaction according to the overdraft policy in the customer's checking account agreement. The agreement specifies both the manner in which overdrafts are paid and any overdraft or nonsufficient fund (NSF) fees that an overdraft will incur. Banks sometimes call this policy "bounce protection" because by paying the transaction, the bank helps the customer avoid bounced check fees charged by merchants; we will follow more recent convention (and the Fed) and refer to this as "standard overdraft service." Standard overdraft service policy generally grants banks complete discretion over whether to pay or decline a particular overdraft. Banks typically will pay overdraft transactions up to an internally specified limit on the account balance and decline any transactions exceeding the limit; the limit protects the bank against default by customers who never bring balances back into positive territory.[7]

The most common standard overdraft service pays overdrafts up to the internal limit and charges a fixed per-transaction overdraft fee ranging from $20-35. Some banks charge NSF or "return item" fees for transactions they decline to pay. Some also charge daily "negative balance fees" of $2-5 while the account balance remains negative and/or charge interest on the overdrawn amount.

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[6] A Federal Deposit Insurance Corporation (FDIC) report (Burrhouse et al. 2008) provides an extensive description of bank overdraft pricing and policies during our sample period, and we draw heavily on that report here. For additional institutional details on the U.S and U.K. see General Accounting Office (2008) and Armstrong and Vickers (2012).

[7] The internal limit varies across banks, across customers within banks, and even for a given customer over time. Most large banks use fraud/default protection software similar to that used by credit card companies; if a particular account displays suspicious behavior, or behavior suggesting a high likelihood that the customer plans to default on the negative balance, the bank will start to decline overdraft transactions.

Another type of overdraft policy is "overdraft protection," which links the customer's checking account to another account or source of liquidity such as a credit card. When the customer overdraws the account, the bank charges the overdraft amount to the linked account or credit card and also often charges a transfer fee and/or interest on the overdrawn balance. Transfer fees are typically $5 or $10, but the fee may depend on the amount overdrawn.

Most banks post overdraft fees and account balances daily and show them as debits on checking account statements. Many banks mail (or e-mail) customers notification about overdrafts; those typically reach customers within a few days after the overdraft. More recently, some banks (and third parties) have begun marketing balance-monitoring and other messaging services that, e.g., send an account holder a text message and/or e-mail when the account balance falls below a pre-specified threshold.

Consumers choose an overdraft program when opening an account, and generally can change their overdraft program at any time, or choose to not have overdrafts paid by the bank at all. Most deposit account agreements during our sample period "default in" account owners to standard overdraft service (Burrhouse et al. 2008; Center for Responsible Lending 2010). National surveys produce a broad range of estimates of the prevalence of accounts with overdraft protection: from 16 percent (Burrhouse et al 2008) to roughly 40 percent (Parrish 2008; Center for Responsible Lending 2009).

B. *Overdraft Prevalence and Importance*

Overdraft fees have become common in recent years as banks shifted from a pricing model based primarily on monthly fees (see, e.g., Stavins (1999)) to one based primarily on fees for service (ATM fees, overdraft fees). The FDIC (Burrhouse et al. 2008) and industry analysts such

as Moebs document this transition and find that overdraft revenue rose to about 75% of explicit deposit account revenue and 6% of *total net* operating revenue in recent years. That explicit revenue has become more important to bank income statements in recent years.

Industry reports estimate that annual overdraft fees paid are roughly $30-40 billion. Roughly speaking, this averages $150 per year per checking account. There are limited customer-level data on overdrafts, and most are self-reported (American Bankers Association 2007; Burrhouse et al. 2008; Parrish 2008; Center for Responsible Lending 2009). It is possible that customers under-report their own overdrafts; such under-reporting has been documented for other types of unsecured borrowing (e.g., see Zinman (2009) on credit card debt). Thus, examining administrative data, as we do, is important for understanding how consumers behave.

C. Controversy and Public Policy Regarding Overdrafts

In recent years bank overdraft practices have become controversial, along some of the same lines as mutual fund fees. Consumer groups and other watchdogs allege that many banks fail to effectively disclose both the terms of their overdraft programs and the choices that customers face (General Accounting Office 2008); in mutual funds, Khorana, Servaes, and Tufano (2009) take the additional step of showing that fund fees are lower in countries with stronger investor protection. Complaints also note that many fees seem disproportionate to the value generated for the user or the cost incurred by the supplier; if a $4 cup of coffee generates a $35 overdraft fee, the argument goes, then the customer has been exploited (see Malkiel (2013) for a similar argument re: asset management fees). Consumer advocates moreover argue that overdraft fees disproportionately affect the poor.[8] Bank settlement practices have also attracted scrutiny; e.g.,

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[8]See, e.g., http://www.responsiblelending.org/overdraft-loans/research-analysis/quick-facts-on-overdraft-loans.html.

batch-processing transactions daily, clearing and settling them in order from highest to lowest dollar amount, increases the total number of overdraft fees assessed.

The Federal Reserve Board recently required banks to secure affirmative consumer permission (i.e., an active opt-in) to pay overdrafts and charge accompanying fees on most debit card and ATM transactions. Another recent regulation requires banks to more prominently disclose overdraft fees that a customer has paid in any periodic statements issued to that customer. Some recent proposals would require banks to notify customers at the point of sale when an overdraft is about to occur. The Consumer Financial Protection Bureau has also raised concerns about bank overdraft practices (Consumer Financial Protection Bureau 2013).

We do not take a stance on these policy issues, except to note that they highlight the need to understand more about the reasons that consumers overdraw their accounts. The merits of more complete up-front disclosure, for example, are greater if poor up-front disclosure generates overdrafts, but they may be negligible if poor up-front disclosure is not a driving force behind overdrafts. More generally, it is difficult to assess the merits of any particular policy given what we currently know. Our paper is a step toward filling that void.

D. Limited Attention and Overdrafts

A variety of economic and psychological models can generate overdrafts.

In a purely neoclassical, full-information model, a checking account holder will pay an overdraft fee due to some combination of motives to ease liquidity constraints and/or economize on transaction costs. It can be perfectly rational to pay a $34 fee to ensure that a $20 transaction can be completed, if the marginal utility from that transaction is worth at least $54 in utility terms, and cheaper sources of liquidity are not available (where "cheap" includes transaction

costs, broadly defined). We do not attempt to rule out this sort of traditionally rational explanation for overdrafts; indeed, the overdraft-focused survey in this panel finds that a substantial fraction of customers express a willingness to pay a $35 overdraft fee even on very small transactions.

Limited attention also might play a role in overdrafts. Our view of what limited attention means is quite general: we take it to mean incomplete consideration of information that would inform choices, whether that information is about account terms or available balances. Our definition encompasses that of Grubb (2012), who takes inattention to mean consumers are unaware of their own past account usage (implying uncertainty about available balances in our setting). In both Grubb's definition and ours, a consumer with limited attention will be uncertain about the *marginal* price associated with a transaction that might overdraw the account. Occasional overdrafts would be a natural consequence of such uncertainty.

Survey evidence suggests that limited attention is a plausible explanation for overdrafts. In one survey question answered by panelists taking the "overdraft-focused" survey studied below (see Section III-E), 60% of respondents report overdrafts when they "thought there was enough money in my account." Most of the remainder report that "the money I deposited was not yet available." Both reasons are consistent with limited attention to checking account balances. There is also evidence of limited attention to account terms. In that same survey, 24 percent of checking account holders did not know/remember whether the bank described different overdraft coverage options at the time of account opening. In two other surveys, 12 and 13% of checking account holders report not knowing whether they *currently have* overdraft protection on their checking account (Parrish 2008; Center for Responsible Lending 2009).

Descriptive evidence from administrative data is also consistent with limited attention as a reason for overdrafts. Consumers could avoid many overdraft fees by tapping readily available sources of liquidity (Stango and Zinman 2009). One conservative measure classifies a fee as avoidable if the overdraft amount is exceeded by the *minimum* available liquidity in another bank account or credit card during the month of that overdraft. Under this measure, among consumers with both a checking account and a credit card, over 50% of overdraft fees are avoidable.

Finally, many panelists frequently come close to the margin of an overdraft but do not go "over the edge": among panelists who ever overdrew an account in our sample period, an overdraft occurs in 31% of panelist-months, but available balances fall below $100 in *83%* of all panelist-months. Whether an overdraft is incurred in these panelist-months, even conditional on being "close to the edge," is highly panelist-specific (i.e., well-explained by panelist fixed effects). These descriptive statistics are consistent with-- though not proof of - an important link between attention to account balances and overdrafts.

In DellaVigna's taxonomy (2009 p. 349), overdraft-related surveys may increase the *salience* of overdraft fees and may therefore affect consumers' effort to acquire and/or use overdraft-related information. For example, getting asked a question like "Do you have overdraft protection?" could remind someone about the opportunity to enroll in such a program. Alternatively, that or another overdraft-related question could induce the customer to monitor balances more closely, to keep a higher buffer stock of balances in the account, to cut back on spending generally, or to do several of these things simultaneously.

In short, limited attention is a plausible explanation for overdrafts, and it is also plausible that surveys could increase attention and affect the frequency with which people overdraw their accounts.

III. Data

A. Overview of Panel Creation and Data Content

Our data come from Lightspeed Research (formerly Forrester Research). Panelists in our sample are members of the "Ultimate Consumer Panel," which is one of many such panels maintained by Forrester/Lightspeed.[9]

Panelists enter the Ultimate sample by providing Lightspeed with access to at least two online bank accounts (checking, credit card, savings, loan or time deposit) held within the household. Panelists have typically participated in other Forrester/Lightspeed panels; the incremental payment for enrolling in the Ultimate panel averages $20. After initial enrollment panelists need take no action to maintain membership in the panel, and a panelist may request to leave the panel at any time. Enrollment of new panelists occurs consistently throughout our sample period, as Lightspeed attempts to keep panel size constant by balancing enrollment against attrition.

In addition to the account data, Lightspeed collects survey data on Ultimate panelists. All panelists complete a short online *registration survey* when they sign-up for the panel; this gives us some baseline information on demographics, financial characteristics, and respondent-assessed financial literacy. Once in the panel, panelists are then invited to take online surveys that are offered periodically. Survey topics are not preannounced, and we use variation in survey content to identify attention shocks.

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[9] Other Forrester/Lightspeed panels track consumer behavior of interest to market researchers, such as the use and purchases of new technology. Those panels are widely used by industry researchers and academics; see, e..g, Goolsbee (2000; 2001), Kolko (2010), and Prince (2008).

B. Details on Account/Transaction Data

The checking account data collected by Lightspeed have two main components. The first component is transaction-level and comes from monthly checking (and credit card) account statements. The statements contain every accounting debit and credit to the account: check deposits and withdrawals, debit card purchases, ATM deposits/withdrawals, automated clearinghouse (ACH) debits and credits such as bill payments and PayPal transactions, transfers to/from other accounts, and so on. Every fee on the account is also recorded as an accounting debit (or credit, if the fee is refunded). For every transaction we observe whether it is a debit or credit, the transaction date, and the transaction amount. Other critical information comes from a set of text strings that identify the bank ("BANK OF AMERICA ALL EXCEPT CA, WA & ID), account name (e.g., "MYACCESS CHECKING5266") and transaction description. The last string is most important for our purposes, as it nearly always contains information about the payment medium, payee/payer, merchant, location, and so on. We use that text string to identify transaction types, fees in general and overdraft fees in particular. An example of a debit card transaction description is this: "CHECKCARD 0607 QUIZNO'S SUB #6431 Q54 HARRISONVILLEMO 2432…" The string therefore indicates payment method, merchant, store ID (#6341) and location (Harrisonville, MO). That is in addition to transaction amount, bank, account name, and date. A typical overdraft fee transaction description looks like this: "WITHDRAWALS/D OVERDRAFT FEE, 25.00." A fee associated with a linked account might look like this: "OVERDRAFT XFER FROM CREDIT CARD OR LINE." We use a text substring identification routine to classify transaction types and fees. Although we can generally distinguish between standard overdraft fees and fees related to overdraft protection, we cannot

distinguish between accounts with overdraft protection and accounts with standard overdraft service. We discuss the implication of this limitation below.[10]

A second component of account data is a near-daily running "available" and "current" account balance.[11] Lightspeed obtains these data by logging in and "scraping" the consumer's account between three and seven times per week. These running balances provide a relatively complete picture of the funds available in the account on a day-to-day basis.

C. Panelists and Overdrafts in Our Sample

The sub-sample of panelists we examine here are those with at least one "active" bank checking account. An active account is one with at least one transaction that could generate an overdraft fee (i.e., an accounting debit) during our sample period. For most of our panelists, the checking account in the Lightspeed data seems to be the only one held by the panelist: roughly 95% of our panelists report holding only one open checking account in their registration survey.

An observation in the raw administrative data is a panelist-transaction, but for this paper we aggregate to the panelist-month. We do this because although we observe transactions and fees on a day-by-day basis, our identifying variation in shocks to attention exists only at the monthly level (we know only the month, not the day/time, that someone took a survey). Table 1 shows summary data on panelists in our sample. The administrative data cover 2006-2008; we report on

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[10] The central problem is that we observe fees only when they are incurred. Thus, an account with no fees (or a period without fees) may have either standard overdraft service or overdraft protection. It is also difficult to use fees paid to infer what overdraft service a customer has. For example, the majority of accounts in which an overdraft protection fee has been incurred *later* incur a standard overdraft fee – presumably because the linked account has been depleted or hit its limit.

[11] The available balance is what the consumer may withdraw before an overdraft. The current balance is the available balance plus holds on the account and netting out pending debits/credits.

the month-years Feb-06 to Dec-08, inclusive.[12] We observe a total of 7,448 panelists with an active checking account, and 102,334 panelist-months of data. Section IV-F discusses how we control for changes in sample composition over time.

Of our active *panelists*, roughly half incur at least one overdraft fee during the sample (Table 1). We define an overdraft fee as any explicit pecuniary cost associated with a negative account balance. Thus, this definition includes the $20-35 fee associated with standard overdraft service. It also includes fees associated with returned items, linked accounts, and so on. In our data, the most common fee by far is the standard overdraft fee, but we include others for completeness.

Across all panelist-*months*, the share with at least one overdraft fee is 16%. That number is naturally higher (31%) among panelists who pay at least one overdraft fee in our data. Overdrafts are not uncommon, and being close to the margin of an overdraft is extremely common: 72% of all panelist-months show at least one available balance below $100, and that figure is higher (83%) among panelists with at least one in-sample overdraft fee. It is worth noting that even panelists who never overdraw in-sample often have low balances: among those with no in-sample overdrafts, balances fall below $100 in a full 56% of all panelist-months. This is *prima facie* evidence of systematic variation across customers in attention paid to balances; it is *not* simply the case that those who never overdraw their accounts never face that prospect.

Roughly 15% of all panelists, and 28% of those with at least one overdraft, have at least one "snowball" month with five or more overdraft fees. Three percent of all panelist-months are snowball months, and that number rises to 6% among those who have ever overdrawn an account.

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[12] Table A1 provides more information on the time series of participation in the panel month-by-month. We observe transactions in January 2006, but we do not use those data in the empirical work because we often condition on lagged monthly information – which is first available as of Feb-06.

There is substantial heterogeneity across panelists and months in the frequency of overdrafts and the dollar amount of fees paid (Figures A1 and A2). Although roughly half of all panelists never overdraw in our data, a substantial share of panelists overdraw serially Two percent of panelists who overdraw do so at least once in *every* month. Dollar amounts of overdraft fees charged are similarly skewed. There are modes at levels representing integer multiples of common overdraft fees ($25-30, $50-60, $80-100, and so on). But in roughly 8% of cases with at least one overdraft fee, the total dollar value of fees exceeds $250, and in numerous panelist-months total overdraft fees exceed $500.

There is also a very strong month-to-month correlation in overdraft fees, within-panelist. The last three rows of Table 1 illustrate this. Fifty-four percent of panelists who incur overdraft fees in a given month do so again in the next month. Eighty-one percent of panelists who incur 5+ fees incur at least one in the following month.

D. Details on Registration Survey

Along with collecting the administrative data (which is done passively from the panelist's perspective), Lightspeed also actively solicits survey information from its panelists. All panelists complete a "registration survey" at the time of enrollment covering demographics, household financial assets/liabilities (such as stock market participation, the number of credit cards held, etc.), and attitudes in household finance and other domains. In the empirics below we use data on three panelist characteristics collected at registration: education, income, and financial literacy. All are self-reported and categorical. We do not observe these variables for every panelist, because panelists register at different times and registration survey questions change over time. We measure literacy using the response to: "I know more than most people when it comes to

managing my money and investments," using "agree" as an indicator of high self-assessed financial literacy, and "neutral" and "disagree" as indicators of medium/low literacy.[13] The correlation between "disagree" (i.e., rating oneself below average) and credit score is -0.15, in the subsample of panelists for whom we also observe credit scores. The correlation between rating oneself above average and credit score is 0.17.

Table A2 shows simple relationships between education/literacy/income and overdrafts.. At the panelist level, those with higher education and financial literacy incur overdraft fees less often; the relationship between income and incurring fees is weaker.

E. Details on Periodic Surveys

Lightspeed also periodically gives panelists opportunities to complete additional surveys on financial product use and satisfaction. Invitations are sent via e-mail to all panelists, and survey respondents are compensated by being entered into a prize lottery. Figure 1 shows the click-through screen that a panelist sees before taking a survey; for our purposes it is important to note that the click-through screen reveals nothing about survey content (overdraft-related vs. non-overdraft-related). Lightspeed classifies any respondent who clicks through as having taken the

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[13] We note that this type of self-report has two issues that warrant mention, drawing heavily on Hastings, Madrian, and Skimmyhorn (2012). First, individual self-reports and actual financial decisions do not always correlate strongly (Karlan and Zinman 2008; Collins et al. 2009; Hastings and Mitchell 2011) Second, consumers may be overconfident about their skill level (Agnew and Szykman 2005; OECD), although this is not evident in our data, where self-assessed financial literacy is centered on average. Despite these issues, prior work has found that self-assessed financial capabilities and more objective measures of financial literacy are indeed positively correlated (e.g., Lusardi and Mitchell 2009; Parker et al. 2012), and that self-reported financial literacy or confidence often have independent predictive power for financial outcomes relative to more objective test-based measures of financial literacy (e.g., Van Rooij, Lusardi, and Alessie 2011; Allgood and Walstad 2012). We suspect that any bias introduced by the self-report works against finding a significant difference between the responses of low- vs. high-financial literacy consumers to attention shocks, given that prior findings on over-confidence suggest that most misclassification would be true-low literacy consumers classifying themselves as self-reported higher-literacy consumers.

survey, and we too classify someone as "taking" a survey if they click through. This is a bit conservative, given 5% item-nonresponse, as we discuss further in Section IV-A.

We have complete data on survey taking, content and responses for our panelists starting in August 2004 (the date of the first survey) and continuing to the end of 2008. The survey data predate our administrative data, so that even in the first month of administrative data we have comprehensive information for each panelist on past surveys administered and taken. We observe survey content and responses for 21 surveys overall. We observe the estimated month in which the survey is administered, but not the precise date of administration; to the extent that there is measurement error in the survey month, our estimated survey effects, particularly the "immediate" effect, will be biased toward zero.[14] Table 2 shows each month in which a periodic survey was offered and also shows information about survey content. Response rates are typically in the 20-30% range for the 2006-2008 surveys.[15] We cannot measure response rates for the pre-2006 surveys, because we lack the administrative data that would give us the total number of panelists.

Content of the periodic surveys varies and provides the identifying variation in attention that we exploit in our empirical work. Each survey contains roughly 60 multiple choice/response questions. Many of the questions appear on nearly every survey and ask panelists to report the types of bank accounts they hold (credit cards, checking, savings etc.), and the bank(s) and/or

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[14] There is some ambiguity on Lightspeed's/Forrester's part about whether their recorded survey dates reflect dates by which surveys were administered (emailed to panelists), dates by which surveys were completed by panelists, or dates by which Lightspeed received the completed survey data. The survey months we assign reflect our best attempts, in concert with Lightspeed, to pin down the date by which the panelist completed the survey. See below for a discussion of how we explored and dealt with this uncertainty empirically.

[15] Response rates across surveys vary for several reasons. The set of panelists varies over time and may introduce differences in average panelist-level propensity for taking surveys. Some surveys are left "open" online longer than others. And there are small variations from survey to survey in prizes/compensation for participating.

credit card companies with which they do business. Many surveys also ask questions about usage of credit cards vs. debit cards. Some surveys collect attitudinal information about borrowing and saving. Some are more narrowly focused (say, on different loans held by the panelist, including amounts and repayment behavior). Some focus on panelists' use of or willingness to adopt new financial products (such as "contactless cards" that use RFID technology).

Most important for our purposes, six of the surveys ask "overdraft-related" questions. Table 2 summarizes which surveys asked overdraft-related questions, and Table A3 shows the overdraft-related questions themselves. Some surveys ask whether panelists have overdraft protection. Some ask whether overdraft fees are a source of "dissatisfaction" with the panelist's current bank. We classify any survey containing at least one question referring to overdraft fees or protection as "overdraft-related."

In five out of the six overdraft surveys, the overdraft-related questions make up a small fraction of total questions on the survey: no larger than 5%. None of the questions on these surveys mentions specific fee amounts or defines overdraft protection (i.e., outside options to standard overdraft services are not described in detail). We label these surveys as "overdraft-mentioning" surveys.

The other overdraft survey is what we call "overdraft-focused." The October 2006 survey was commissioned by the Center for Responsible Lending (CRL), a consumer advocacy group that lobbies on overdraft policy, and 12 of the 15 questions mention overdrafts, asking both about actual behavior and about consumers' preferences (e.g., to have banks pay or decline overdrafts by default). Although this survey does provide some information on prices and outside options, none of that information is customer-specific.

For estimating the relationship between overdrafts and having taken overdraft-related surveys, the empirical ideal would be random assignment into overdraft-related surveys. Our identifying assumption, given that panelists consciously decide whether to take a survey when confronted with the click-through screen, is that *conditional on a panelist's decision to take a survey, the survey content is random* (see Section IV-A for more details on our empirical strategy, including a more precise statement of our key identifying assumption).

Support for our identifying assumption comes from the click-through screen (Figure 1), which contains no information about survey content. Coupled with the irregular intervals at which surveys mention overdrafts (see Table 2), it is unlikely that panelists are able to consciously select into overdraft-related surveys. We do allow for selection into *surveys overall* in the empirical work and find some evidence that this pushes against (i.e., attenuates) our estimated treatment effects. Further supporting our identifying assumption is the finding that consumer-level characteristics are uncorrelated with taking overdraft-specific surveys once we condition on the total number of surveys taken. (Table A4). We estimate a series of OLS regressions where the dependent variable is the count of overdraft-related surveys taken and the RHS variables are panelist demographics and variables measuring how long a panelist is in the panel. Without controlling for the total number of surveys taken, the panelist demographics are significant in explaining variation in overdraft surveys (p-value=0.00), but when we condition on the total number of surveys taken, that correlation disappears (p-value=0.69). Thus, we cannot reject the hypothesis that customer characteristics are unrelated to selection into overdraft surveys conditional on the propensity to take any survey.

Table 2 also classifies surveys based on other content that is plausibly related to overdrafts, particularly if attention is driven in part by salience that works in an associative way. Some

surveys ask about spending control, monitoring checking balances or other non-overdraft fees. We explore the role of this survey content in the empirical work below.[16] The table also classifies surveys based on other content that appears at similar frequencies to overdraft questions but is plausibly *unrelated* to overdrafts: contactless (RFID) cards, gift cards and auto loans. Below we use these variables for a robustness check and a form of placebo test on our main results.

Table 3 tabulates panelist-level data summarizing total and overdraft-related surveys taken. Only 30% of panelists never take any survey. Another 30% take one or two surveys. Twenty-seven percent of panelists take at least one overdraft-related survey, and 16% take more than one. The data therefore display substantial variation across and within panelists in both total and overdraft surveys taken; the within-panelist variation in surveys taken and survey content drives our empirics.

F. Surveys and Overdrafts: Descriptive Evidence

Figure 2 shows an event-time analysis illustrating the raw variation in the data. The figure shows changes in the frequency of overdrafts following overdraft surveys, where "t=0" is the month in which we classify panelists as having taken an overdraft-related survey.[17] For comparison, the figure also shows changes in the frequency of overdrafts for all other panelist-

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[16] Example of spending control question: "I like being in control of my spending [agree/disagree scale]." Example of balance-monitoring question: "How often do you log into your [Q1 BANK] banking website specifically to look at your account balance?" Example of other fee question: "Debit cards have too many fees associated with them [agree/disagree scale]."

[17] The "OD Survey" group includes all OD-related surveys, including the overdraft-focused survey.

months in which t=0 was a survey month, but no overdraft survey was taken.[18] The post-event analysis measures changes in the frequency of overdrafts relative to the average incidence in the four months preceding the survey; the pre-event average includes all panelists with an opportunity to take any survey in month t=0, including both those taking overdraft-related surveys and those not taking overdraft-related surveys.

Figure 2 shows a clear decline in the frequency of overdrafts for the 12-month period following taking an overdraft-related survey. There is no visible decline for those who do not take overdraft-related surveys. Although the difference between overdraft survey takers and non-takers is quite clear, the figure is best viewed as suggestive, because it does not control for a variety of confounding factors. One such factor is the clustering of overdraft-related surveys in time, which might explain why the "effect" appears to increase over time, at least initially, rather than decay: someone who took an overdraft-related survey in any particular month often also took another overdraft-related survey, or perhaps two, in other recent months. The post-event pattern in Figure 2 is consistent with the "stock" effect we estimate below. Another noteworthy pattern is that the onset of the "effect" appears less than perfectly discrete, and in the raw data the (t-1) "lead effect" is as large as the (t=0) effect, although neither of these point estimates are significant in the raw data. We show below that the leads are insignificant, and that the onset (the "immediate", t=0) effect becomes significant once we estimate dynamics in a more fully specified and flexible econometric model. We turn to those models after providing some necessary caveats about our data and sample.

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[18] The "No OD survey" data therefore average across panelist-months in which an overdraft survey was offered but the panelist did not take the survey, and panelist-months in which a non-overdraft survey was offered – whether the panelist took that survey or not. All of these observations contribute to the identifying variation in our data. We include all eligible observations in the figure, meaning that the panel is unbalanced (as it is in the econometrics below).

G. Advantages and Limitations of the Data

The Lightspeed data are unique (to our knowledge) in three important respects. First, the account and transaction data span multiple banks. Some recent work in household finance employs administrative transaction-level data, but typically only from a single provider.[19] Second, our data link administrative measures of actual behavior to a rich set of panelist characteristics collected in the registration survey. Third, Lightspeed matches its administrative data with periodic surveys regarding consumer behavior, preferences and other attitudes.

The main disadvantage of the Lightspeed data is that they are not nationally representative, although the incidence of overdrafts compares reasonably well to that in the U.S. population. For example, if one aggregates our data up (based simply on the number of accounts we observe compared to the number of accounts at the national level), the dollar level of implied overdraft fees is roughly comparable to the $30 billion per year range reported in other sources.[20] But the Lightspeed data looks less representative along other dimensions. The requirement that panelists register accounts online selects for people who manage their finances online, are relatively educated, and higher-income (Table A2). Panelists are likely fairly comfortable sharing possibly sensitive financial information (in exchange for the compensation they get for participating), although nearly all household surveys on consumer finance face a similar selection issue. The distribution of our self-reported financial literacy measure may also be unusual: it is roughly

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[19] The study by Agarwal et al. (2009) is a nice exception, drawing on administrative data from multiple financial institutions.

[20] In terms of overdraft frequency, it is difficult to compare our administrative data to self-reported overdraft behavior. In our data 16% of panelist-months have one or more overdrafts, and roughly half of panelists have at least one overdraft over the three-year sample period. The FINRA National Financial Capability Survey reports that 23% of respondents self-declare as incurring overdrafts "occasionally." See http://www.usfinancialcapability.org/downloads/NFCS_2009_Natl_Full_Report.pdf .

centered on average, and even tilted a bit toward below-average. Other evidence finds that people tend to self-assess (too) favorably in financial knowledge (Lusardi and Tufano 2009)[21] and related domains such as stock-picking (Glaser, Weber, and Langer 2010).

These representativeness issues are worth noting, but it is equally important to note that they predict nothing about the central representativeness question here, which relates to possible heterogeneity in comparative statics: do our panelists respond more or less to shocks that might affect attention paid to daily household finance? We are not aware of any evidence, theoretical or empirical, that produces sharp priors.

IV. Empirical Strategy and Results

A. Model and Identification

Our main research question is whether shocks to attention affect overdrafts, in both the short run and over sustained periods. Our empirical model approaches that question by estimating the relationship between overdrafts and within-panelist variation in overdraft surveys taken. The unit of observation is a panelist-month, because we observe only the month in which a survey was administered.

The primary estimating equation is:[22]

$$
\begin{aligned}
AnyOD_{it} = {} & \beta_1 TookOD_{it} + \beta_2 ODSurveys\_Last2yrs_{it} \\
& + \beta_3 TookAny_{it} + \gamma \cdot AnySurveys_{it} \\
& + \beta_4 AnyOD_{i,t-1} + \beta_5 Snowball_{i,t-1} + \delta_i + \lambda_t + \varepsilon_{it}
\end{aligned}
$$

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[21] Lusardi and Tufano's Table 4 suggests that only 13% of the U.S. population rate themselves as having below-average overall financial knowledge.

[22] Conditional (fixed effect) logit models yield nearly identical results, and are in most specifications more precisely estimated (meaning that the coefficients of interest are statistically significant at levels greater than what we discuss below). The model also includes a dummy equal to one if the month is the panelist's last in the sample. We typically observe transactions for only part of the month in such instances, meaning that overdrafts are less common.

We estimate the model with OLS, and cluster standard errors on panelist.[23] The dependent variable is an indicator equal to one if panelist i incurs at least one overdraft fee in month t. This specification models attention as operating on the extensive margin ("does the panelist overdraw?"). We also consider more continuous measures of overdrafts below.

The model estimates both short- and long-run relationships between overdrafts and having taken overdraft-related surveys. The contemporaneous variable $TookOD_{it}$ equals one if the panelist clicked-through to[24] an overdraft survey in the current month and measures the "immediate" effect.[25] The variable $ODSurveys\_Last2yrs_{it}$ measures the total number of overdraft-related surveys taken in the last two years. This variable increases with survey-taking and falls over time as overdraft-related surveys recede into the past for a panelist. It specifies what one can think of as a "stock" of attention that builds with survey-taking and decays over time. Below we present results from models with shorter- and longer-horizon specifications of the stock effect and more flexible functional forms on the relationship between the stock variable and overdrafts. The survey stock variable increments by one in the month after taking a survey, in order to cleanly distinguish the stock effects from the immediate effects.

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[23] Within survey months there is variation in survey-taking; in one month, we have variation in survey-taking and survey content, because both an overdraft-related and a generic survey were offered. Clustering on survey-month is not recommended because we have fewer than 50 clusters on that dimension.

[24] 95% of those who click through to an overdraft-related survey answer at least one overdraft-related question. The remaining 5% may have actually been exposed to the questions in the sense of seeing them, but not responding. To the extent that some survey-"takers" are not actually exposed to the survey content of interest, our estimates are intent-to-treat effects, and lower bounds on treatment-on-the-treated effects, under standard assumptions.

[25] We have estimated alternate specifications of the "immediate" effect, motivated by the fact that we do not observe the survey date precisely. If we include the lagged value of $TookOD$, that coefficient is positive but not statistically significant (see Figure 3). It is possible that panelists avoid overdrafts in the near term by pushing expenses forward into the next month, which is what one might expect if expenses are less discretionary in the short run – see Table 7 for evidence on this point.

We control for selection into surveys generally by including a contemporaneous indicator $TookAny_{it}$ equal to one if the panelist took *any* survey, including an overdraft-related survey, in the current month. Thus, the coefficient on $TookAny_{it}$ measures the relationship between selection into surveys and overdrafts (along with any causal effect of survey-taking generally), and the coefficient on $TookOD_{it}$ measures the incremental effect of taking an overdraft-related survey relative to taking any survey. $AnySurveys_{it}$ is a set of indicators for the total number of surveys taken. These control flexibly for longer-run selection into surveys: secular trends or dynamics in the dependent variable that are associated with taking surveys or with taking multiple surveys.

The other controls are fixed month/year effects and fixed panelist effects.[26] Some models also include a lagged dependent variable $AnyOD_{i,t-1}$ measuring overdrafts in the previous month, and a variable $Snowball_{i,t-1}$ equal to one if the panelist incurred five or more overdraft fees in the previous month. Both variables are intended to capture the autocorrelation in overdrafts summarized in Table 1, although their inclusion does not change the results.[27] The inclusion of a lagged dependent variable in models with fixed effects can introduce a positive bias on the lagged dependent variable coefficient(s). In our case, this problem appears to be minor, given the small estimated coefficient on the lagged dependent variable, but it is worth noting when interpreting that coefficient.[28]

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[26] We have also estimated specifications including leads of overdraft-survey and any-survey taking; these variables tend to be insignificant and do not change the results. We include panelists who never take any survey, despite having panelist fixed effects, to help identify the estimates on the non-survey variables.

[27] We have also estimated specifications that control (flexibly) for the number of months a panelist has appeared in the panel. These variables are not significant correlates of overdrafting and tend to reduce the precision of the estimates on the variables of interest. The "months-in-sample" variables are included (and significant) in some of the specifications we discuss later re: Tables 7 and 8.

[28] In unreported results we implement various corrections for the lagged dependent variable bias (Anderson-Hsiao- and Arellano-Bond-type instruments). Doing so leaves the primary results qualitatively unaffected.

Because we rely on within-panelist variation in survey-taking to identify the model, our primary econometric analysis excludes panelists with no variation in the dependent variable. We exclude the 48% of panelists who never overdraw their accounts, and the 1% of panelists who overdraw every month. It is technically possible to include the "never" and "always" overdraft panelists in the analysis, but doing so makes an assumption that the data-generating process for those panelists is not different from the data-generating process for those whose overdraft behavior varies over time. That assumption need not hold, and we feel more comfortable estimating the results conditional on the latter sample – which is, arguably, the sample of greatest interest.

Our identifying assumption is that,conditional on the right-hand-side variables, there are no differential unobserved secular dynamics in the dependent variable across those who take overdraft-related surveys and those who do not take overdraft-related surveys. Under that assumption our estimate of the causal stock effect of taking overdraft-related surveys is the $ODSurveys\_Last2yrs_{it}$ coefficient, and the immediate causal effect is the $TookOD_{it}$ coefficient.[29] The identifying assumption seems reasonable given that survey topics are not pre-announced and that we control for selection into survey-taking through the "any survey" variables.

B. Main Results

Table 4 presents results from our primary specifications. We build from simpler to richer specifications reading across columns to highlight sources of identifying variation in the data and illustrate (and test) the implications of some functional form choices. The first column includes

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[29] Mechanically, the immediate effect is the sum of the coefficients on the two immediate survey variables, subtracting any bias from selection into surveys.

only the overdraft survey variables. The next adds the "any survey" variables, including $AnySurveys_{it}$ linearly. Tests of the linearity restriction for both the overdraft and any survey stock variables do not reject linearity on the $ODSurveys\_Last2yrs_{it}$ variable, but do reject linearity on $AnySurveys_{it}$ (column 3). The "any survey" stock variables are jointly significant and generally indicate that selection into survey-taking is positively correlated with incurring overdraft fees.[30] Column 4 adds controls for other survey content (gift card, contactless card and auto loan questions). The coefficients on these variables are not significant and can be interpreted as a form of placebo test. Column 5 adds measures of lagged overdrafts—the $AnyOD_{i,t-1}$ and $Snowball_{i.t-1}$ variables. Overall, the results are robust to variations in specification. The specification in column 5 is our preferred one for the rest of the analyses because it contains the richest set of controls.

We find significant immediate effects of taking an overdraft-related survey. In our preferred specification (column 5), the point estimate on the $TookOD_{it}$ coefficient is -0.037 (p-value 0.06) on a sample mean of 0.26, for an estimated reduction in the likelihood of an overdraft of 14% from its mean level. The point estimate on the immediate "any survey" coefficient is positive, with a p-value of 0.09. The lagged overdraft coefficients are what one would expect given the very strong unconditional positive serial correlation documented in Table 1.

We also find statistically significant (p-value=0.04) and economically meaningful stock effects of taking multiple overdraft-related surveys. The point estimate in column 5 suggests that each overdraft survey taken in the last two years reduces the probability of an overdraft by 0.017.

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[30] The relationship between generic survey-taking and overdrafts is non-linear: there is little relationship until roughly the fifth survey, and then the likelihood of an overdraft rises. For 7-12 generic surveys taken, the likelihood of an overdraft averages 5% higher than baseline. Results beyond that (13+ surveys) are noisy.

For a panelist taking three surveys, the cumulative effect is -0.051 on a mean of 0.26, which is a 20% decline.

The full sample likely includes many consumers and panelist-months that are infra-marginal—i.e., in which an overdraft is highly unlikely under any circumstances, independent of attention paid—which introduces noise and reduces the precision of the estimates. Our first attempt to improve precision is to discard panelist-months in which the account balance does not approach the "edge" (fall below $100). In this sub-sample (column 6) the results are both larger in point terms and stronger statistically: the p-values are 0.012 and 0.024.[31] We also show below (in Table 5) that the results are stronger in two other sub-samples of consumers who are plausibly closer to the margin of an overdraft.

Auxiliary results provide more detail on the specification and dynamics of the stock effect. (Table A5). In linear specifications of the stock effect with faster decay ("OD surveys taken, last year") and no decay ("OD surveys taken, ever"), the coefficients are smaller and not statistically significant, but not significantly different from the stock variables in our preferred specification. In specifications that include all three variables the two-year stock effect variable is significant, while the others are not. We see a similar pattern when we replace the linear stock variable with a set of indicators for number of surveys taken and also vary the stock horizon: larger and more-significant point estimates using the two-year horizon, but no significant differences between the two-year horizon and alternative horizons. The linear specification also seems to be a good approximation for the survey-by-survey stock effects: one cannot reject the linear functional form restriction in any model.

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[31] One could view Table 4 Column 6 as the reduced form of a two-stage model in which the first stage explains whether the account comes "close to the edge," and the second stage examines overdrafts conditional on being close to the edge. We considered estimating such a model, but have no strong candidates for an exclusion restriction that would identify the model. We do examine whether taking overdraft-related surveys affects how often accounts come close to the edge, in Table 7 below.

The fact that we estimate stronger two-year than one-year effects, in point terms at least, may seem at odds with an intuitive pattern of onset and decay. Recalling Figure 2, we are also interested in exploring more fully the question of whether there are significant pre-trends. To shed light on these issues, we show in Figure 3 the coefficients from a distributed lag model regressing overdrafts on a full set of leads and lags extending from (t+3) – up to a three-month lead – to (t-23), which is two years after the overdraft survey date:

$$AnyOD_{it} = \sum_{t-23}^{t+3} \beta_t TookOD_{it} + \beta_3 TookAny_{it} + \gamma \cdot AnySurveys_{it}$$
$$+ \beta_4 AnyOD_{i,t-1} + \beta_5 Snowball_{i.t-1} + \delta_i + \lambda_t + \varepsilon_{it}$$

The model is identical to that in Table 4, but for the fully flexible specification of the overdraft survey-related dynamics. We have estimated other similar models (e.g., specifying the "any survey" effects using the distributed lag specification, and/or omitting the lagged dependent variable), with similar results.

Figure 3 shows a much more intuitive pattern of onset and decay than in Figure 2 – albeit a slightly noisy one. The largest effects in point terms are those in months 0, 9, 7, 2 and 13 in descending order; those coefficients are significant at 10% or better (indicated by darker shading of the bars on Figure 3). Ten of twelve coefficients during the first year after the survey are negative. Turning to the second year, there seems to be a noisy but fairly intuitive pattern of decay, with ten of twelve coefficients remaining negative. The one counterintuitive coefficient is that in t+23, which seems to belie the general pattern of decay, but that coefficient is not significantly different from zero, and in any case one expects a false positive or two when estimating 27 coefficients.

Although intuitive, the pattern in Figure 3 does not provide a clear explanation for any two-year vs. one-year difference in the stock effects. (There is no statistically significant difference

between the one-year and two-year stock coefficients, so we are speculating about an "eyeball" rather than a statistical difference.) It may be that a one-year horizon is too short to appropriately capture decay. Also, the two-year horizon includes a different/broader set of surveys for many panelists, and, e.g., the overdraft-mentioning vs. overdraft-focused surveys may have had heterogeneous effects (Table 6). In all, the results thus far suggest that taking surveys with overdraft-related questions meaningfully reduces the likelihood of overdrafts in the short term, and also leads to persistent but not permanent cumulative effects of having taken many surveys. Attention builds with survey-taking and decays over time. The precise time horizon over which the stock effect operates is not something we can pin down completely.

C. Sample Splits by Education, Income, and Financial Literacy

Table 5 presents results from our preferred specification (Table 4, column 5), split across three panelist characteristics measured in the registration survey: education, financial literacy and income. Because of the noise that arises when splitting the sample too finely, we break the sample into two groups for each of the three baseline variables of interest here. The top row of the table reports sample means for the dependent variable for each group. Sample sizes vary across groups in this table, both because of the category break-points and because of missing data for some panelists (the financial literacy question, in particular, was asked only in a subset of registration surveys).

The results in Table 5 show that the point estimates of treatment effect(s) are uniformly larger among those with less education (relative to more education), low/medium self-assessed financial literacy (relative to high), and lower income (relative to higher income).[32] The lower

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[32] The results are similar, quantitatively and qualitatively, if we split the sample along similar lines using any of the other specifications in the latter columns of Table 4.

groups do overdraw accounts more often on average, though not dramatically so, meaning that the *proportional* effects implied by the point estimates are also larger for these groups. For example, in the low-education subsample, the point estimates suggest that a panelist who has taken two surveys has the probability of overdraft lowered by 0.044 on a base of 0.290, a 15% reduction.

Evidence on whether the treatment effects differ significantly across the groups is weak. T-tests reject equality only for the stock effect in the education split. Generally, the coefficient estimates are not precise enough for us to make sharper statements about how demographics are correlated with the effects of survey-taking.

We can say with some conviction, however, that the treatment effects are substantial in the low-education, low-literacy and low-income subsamples.[33] This is informative in itself, in that consumer advocates and some policymakers tend to focus on bank fees paid by those groups (see, e.g., http://www.overdrawnmovie.net/).

D. Effects Beyond the Extensive Margin?

Table A6 specifies the dependent variable in two alternative ways: as the number of overdraft fees incurred, and as the level of overdraft fees incurred in dollars. These results combine the extensive and intensive margins. The results here are noisier than when we examine the extensive margin alone, although the point estimates remain negative and economically large. We do find some statistically significant temporary reductions in two of the three more-marginal sub-samples ("close to edge," and low-education). It may be that the effects of survey-taking

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[33] We have also estimated pooled specifications in which we include income, education and literacy interacted with the survey variables. Doing so reduces efficiency because income, education and literacy are highly correlated, but we find similar results: there are strong effects in the low-income/education/literacy group, with mixed results regarding whether the higher-income/education/literacy groups display statistically different effects.

operate more strongly on the extensive margin, or that effects on the intensive margin are substantive but not cleanly identified: our confidence intervals do not rule out the latter.

E. More Treatment Effects: Other Useful Variation in Survey Content

Survey content varies in two other useful ways. As we mention above, one survey focuses entirely on overdrafts and potentially provides some information on prices and outside options. One might expect the "overdraft-focused" survey to have different effects than surveys that are merely "overdraft-mentioning." Other useful variation in survey content comes from questions that are not precisely about overdrafts but which seem plausibly related in an associative way. We identify three categories of such questions (see footnote 30 for examples of question wording). One is spending control, since reducing spending is one way people can engineer overdraft reductions (see the next subsection). Another is monitoring of available checking account balances, given that closer monitoring of balances may be key to overdraft avoidance (see the next subsection). The third category is other bank fees, since overdraft fees represent the majority of bank fees paid by our panelists, raising the possibility that mentioning "bank fee" may trigger the thought of "overdraft fee" in the panelist's mind.[34]

Table 6 presents results from models that allow for differences between overdraft-mentioning surveys and the overdraft-focused survey, and that allow balance-, fee-, or spending-mentioning surveys to affect overdrafts too. Column (1) adds the balance/fee/spending survey variables to our main specification (from Table 4, column 5). The immediate effect is not significant, but the stock effect is significant and comparable in point terms to the effect of overdraft-related surveys

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[34] We have attempted to estimate whether questions about credit card fees are associated with changes in overdrafts, but lack the power to identify an effect (no credit card fee questions appear on surveys without questions about overdraft fees). We have also estimated whether even more tangential questions – about, for example, loan payments – are associated with changes in overdrafts, but find no effects.

(compare to Table 4, column 5 and/or to the overdraft stock variables in this table). Column (2) relaxes the restriction that overdraft-mentioning and overdraft-focused surveys have identical effects. Their immediate effects are quite similar in point terms, but the stock effect of the overdraft-focused survey is much larger, though only significantly different from the overdraft-mentioning stock effect at a relatively weak level (p=0.12). Column (3) refines the specification by restricting the "overdraft-mentioning" and balance/fee/spending surveys to have identical *stock* effects; here the overdraft-focused survey stock effect is statistically different from the effect of other surveys (p=0.03). Column (4) maintains that restriction and also restricts the overdraft-mentioning and overdraft-focused surveys to have identical *immediate* effects, since these restrictions cannot be rejected.

The upshot of this table is that variation in survey content seems to matter in an intuitive and associative way. The survey focused on overdrafts has a greater effect, at least in stock terms. Surveys about topics related to overdrafts also have meaningful stock effects. This result suggests that shocks to attention work in an associative way, cognitively speaking.

F. Mechanisms: How People Engineer Overdraft Reductions

If taking overdraft-related surveys induces less frequent overdrafts, how are panelists engineering those reductions? We address this question in Tables 7 and 8 by analyzing other measures of checking account usage as dependent variables, using our main specification for RHS variables except for changing the lagged dependent variable in accordance with the new LHS variables used here. We also control for tenure in the sample here by including a variable measuring "months in sample" and interacting that variable with the time period during which the panelist entered.

The first dependent variable in Table 7 is the total number of monthly checking account spending transactions (column 1). The immediate (temporary) effect on this variable of taking an overdraft-related survey is negative (implying a 2% reduction) but insignificant. The stock (cumulative) effect is negative and implies a 4% reduction in the transaction count for each overdraft-mentioning survey taken within the last two years. Column 2 shows that we do not find a significant reduction in checking account spending in dollar terms (we use the log of spending to deal with skewness), although the confidence intervals do include substantial changes.

Columns 3 and 4 show that the stock (cumulative) effect on spending transactions operates both when balances are quite close to the overdraft threshold (balance <$100) and when balances are higher (>=$100): panelists reduce transactions in both states. (Note from column 7 that crossing the low-balance threshold is a common occurrence: 83% of panelist-months dip below a $100 balance at some point.) We get similar results using other cutoffs such as $50 and $20. The point estimate on low-balance transactions is larger in proportional terms, implying a 5% reduction (0.376/7.0) for each overdraft-mentioning survey taken, vs. a 3% reduction (1.428/41.5) for higher-balance transactions.

Balances can be a choice variable too, and so we examine whether panelists take actions that are consistent with a strategy of increasing balances (e.g., maintaining a buffer stock) to avoid overdrafts. Column 5 shows no effects on the log of the dollar value of account credits (deposits and transfers into the account). Columns 6 and 7 show that the likelihood of reaching a low-balance threshold (<$100) does not change following overdraft-related survey-taking.

In all, Table 7 suggests that panelists engineer overdraft reductions by reducing spending transactions (i.e., by managing outflows) but not by increasing inflows.

Table 8 looks at the composition of spending transactions. Columns 1 and 3 show significant and negative stock effects on both debit card and autodebit (ACH) transactions. These results seem intuitive given that balances-at-clearing are relatively hard to monitor and manage for both types of transactions (although the same could be said of checks, and we do not find a significant result there). These results also suggest that consumers may use a mix (within and/or across people) of high-frequency and low-frequency vigilance to avoid overdrafts: reducing debit card use may well involve a series of decisions to not pull out the card, whereas an automatic debit can be cancelled (and the bill switched to manual pay) with a single phone call or web log-in. We do not find significant effects on checks, cash withdrawals, or paydown of credit card balances (for credit cards we use dollar value rather than transaction count, since credit cards require at least one payment per month).

In all, Table 8 suggests that people respond to overdraft surveys by managing spending transactions more intensively on multiple margins. Whether this heterogeneity in response to attention shocks occurs within-person (i.e., a given person implements multiple strategies) or across-person (different people respond differently) is not something we can pin down. It is interesting that we find relatively strong stock (cumulative) effects and weaker immediate (temporary) effects. One explanation is that adjusting spending patterns takes time. Another explanation is that immediate effects may just be more difficult to detect, statistically; given the multiplicity of strategies for implementing overdraft reductions, identifying prevalent strategies may only be possible as their use accumulates over time.

V. Conclusion

Overdraft fees became an increasingly important source of consumer outlays and bank profits throughout the past decade. Our results suggest that limited consumer attention plays an important role in explaining overdrafts.

We find that individuals are substantially less likely to pay an overdraft fee after taking surveys that mention overdrafts or topics that consumers might associate with overdrafts: spending control, balance-monitoring, or other bank fees. Plausibly unrelated survey content (on auto loans, gift cards, and contactless cards) does not affect overdrafts. The overdraft surveys have an immediate (same-month) effect and a stock (long-run) effect: attention is built via survey-taking and decays slowly over time. The stock effect is stronger from the one overdraft-focused survey than from the other surveys that merely mention overdrafts. These effects exist among the less well-educated and financially literate, an important finding if one wants to target those groups with an attention-based treatment. Consumers implement overdraft fee reductions by reducing spending transactions on debit cards and autodebits (ACH), suggesting a combination of high-frequency and low-frequency vigilance on transactions where the balance at the time of transaction clearing is difficult to monitor/forecast. We do not find evidence that people respond to the shocks by increasing balances in their checking accounts.

Our results suggest an important role in household finance for limited consumer attention that is dynamic, associative, and malleable. The surveys we examine do not provide any direct and panelist-specific information about account terms, meaning that the effects we observe are, in principle, distinct from those one would observe following classic disclosure of account terms.

Some caveats are in order. The external validity of our findings is uncertain; e.g., would they hold in populations less-educated and/or less-online than our sample? Our results on lower-education and lower-financial literacy sub-samples suggest that attention shocks may have

stronger effects on more "down-market" populations, but some of this could be mechanical (overdrafts are higher in these sub-samples), so more work is needed to explore whether the pattern is robust and why it exists. We also caution against taking welfare implications from our results. We cannot say, for example, whether consumers pay too little or too much attention in the absence of shocks, or whether attention to other matters falls when attention to overdrafts rises. In all, it would be imprudent to draw conclusions about the welfare effects of public policy, overdraft-related or other, from our findings.

It would be interesting to examine how the attention effects we observe compare to the direct learning effects – that paying a fee reduces the incidence of future fees – inferred by Haselhuhn et al. (2012) and Agarwal et al. (2011). In our data, paying a fee last month is *positively*, and quite strongly, correlated with paying a fee this month. We hope to explore those dynamics more fully in later work.

Another area for future work involves examining whether increased attention on one margin (e.g., bank account balances) has a costly downside in the form of reduced attention on other margins (e.g., credit card balances). Work on limited attention in public equities suggests that such an idea is plausible: limited attention seems to affect how prices incorporate news, and the relative strength of different claims on investor attention seems to affect the allocation of attention to those claims. Going forward it will be interesting to explore whether third parties (e.g., SigFig, or more traditional brokers or advisors who are seeking to differentiate) might improve decision and market efficiency by filtering news. A related consideration in debt markets is how innovations that reduce attention costs for consumers (e.g., low-balance alerts provided by banks on an opt-out basis, or by 3rd-party automated personal financial management services like Mint or HelloWallet) would affect equilibrium contracts and pricing.

Further inquiry on these lines should improve understanding of how limited attention affects financial decisions. That understanding is a necessary step toward designing sound public policy in household finance and related domains. Much of the recent policy discussion regarding overdrafts, mutual funds, and consumer financial protection more generally revolves around mandated disclosure of account terms that firms might "shroud" absent regulation. The central issue there is whether providing consumers with *more information* helps them to make better decisions. A model of limited attention suggests a different question: can reminders or other shocks to attention, even if they are *uninformative* in the classic sense, help people make better decisions? Mandated disclosure also typically focuses on the one-time, up-front provision of information, and our results suggest that this approach may have only a temporary effect on consumer behavior.[35] Might ongoing reminders be more effective than (or a useful complement to) one-shot, upfront disclosure? If so, who should and will provide such messaging? These questions merely skim the surface of the possibilities for further research on limited attention, household finance, and consumer financial protection.

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[35] Stango and Zinman (2011) highlight enforcement challenges involved in mandated disclosure in consumer lending. Barr, Mullainathan, and Shafir (2008) make a similar point regarding mandated default options; the lender "moves last" and has incentives to get the consumer to undo the default (e.g., "just initial here" to opt in to standard overdraft service).

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consumer say.com

$panelPersonal.getFirstName(),

You are invited to participate in a new ConsumerSay survey!!

Name: Consumer Opinion Survey #13
Time: 10 minutes
Reward: An entry in a drawing to win one of twenty $25 Amazon©
Gift Codes

Just sign in to your Members Page and access the link to the survey.

SIGN IN

Your Username: $panel.emailAddress
Your ConsumerSay.com Password: $panel.password

If you cannot view the button above, please copy the link below and
paste it in your browser:
http://www.consumersay.com

If you have any problems or questions, please respond to this email.
Enter the name of the survey in the subject line.

We look forward to your participation!

ConsumerSay

Figure 1. Periodic survey click-through screen.

Overdrafts and Overdraft-related Surveys
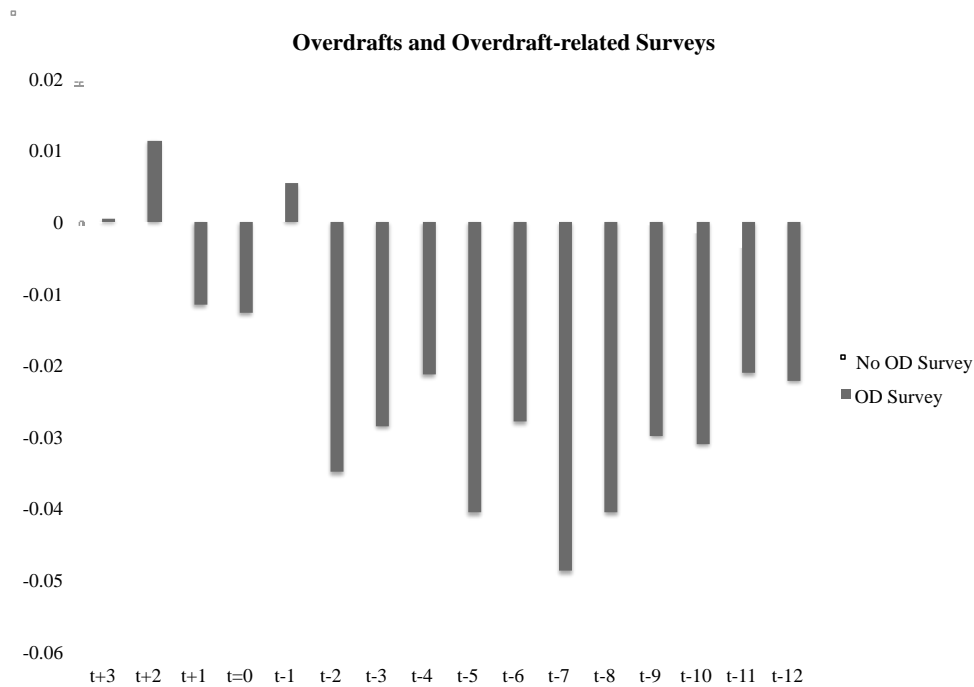
Figure 2. Overdraft survey-taking and overdrafts in event-time. Positive numbers relative to "t" indicate leads, and negative numbers indicate lags. Time "t=0" is estimated month in which survey is completed. "No OD survey" group averages across those who do not take a survey, and those who take a non-overdraft survey, in all months in which any survey was offered.

Lead/lag Coefficients From Distributed Lag Specification
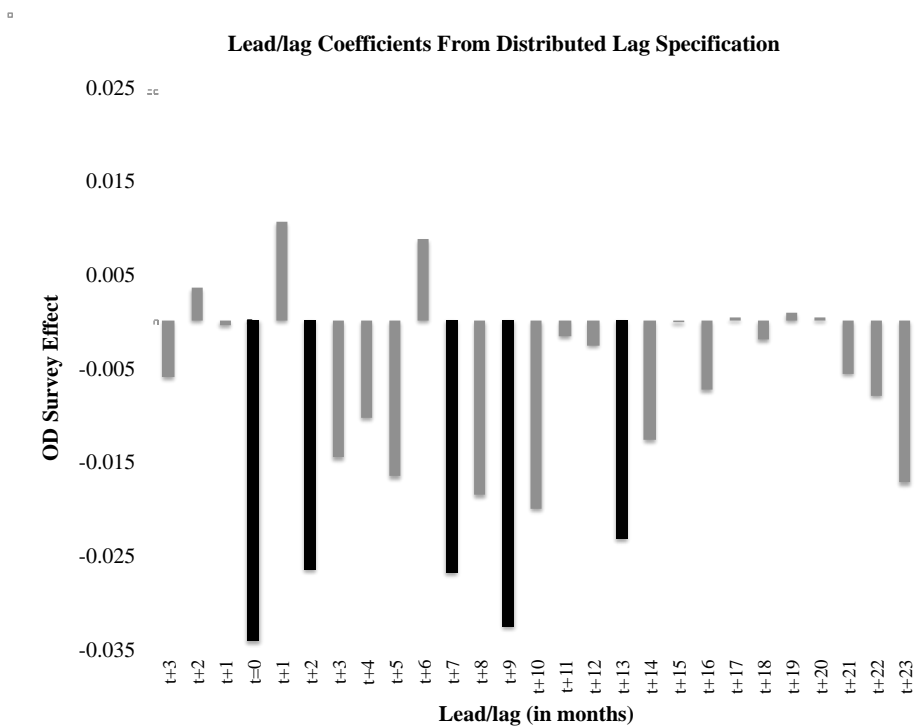
Figure 3. Coefficients from distributed lag specification described on page 26. Black (dark shaded) columns indicate statistical significance at 10% or better.
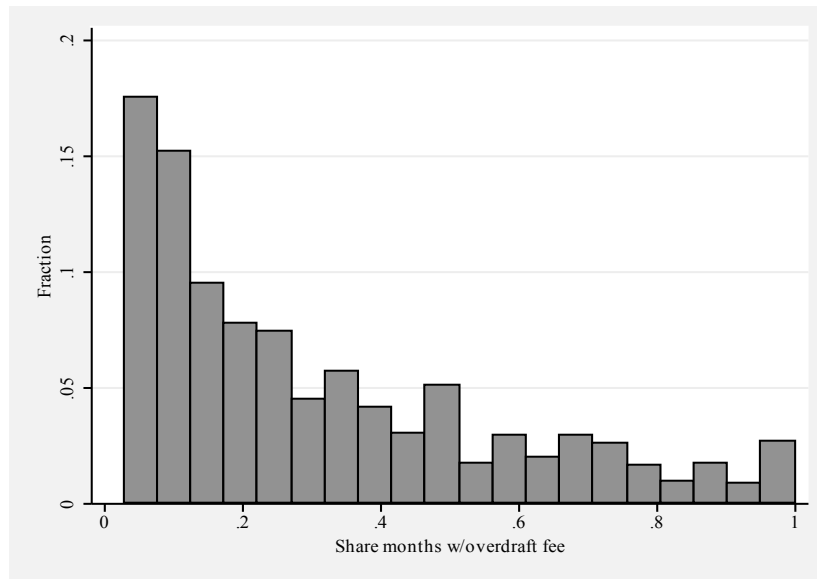
Figure A1. Share of months with at least one
Overdraft fee, panelist-level.
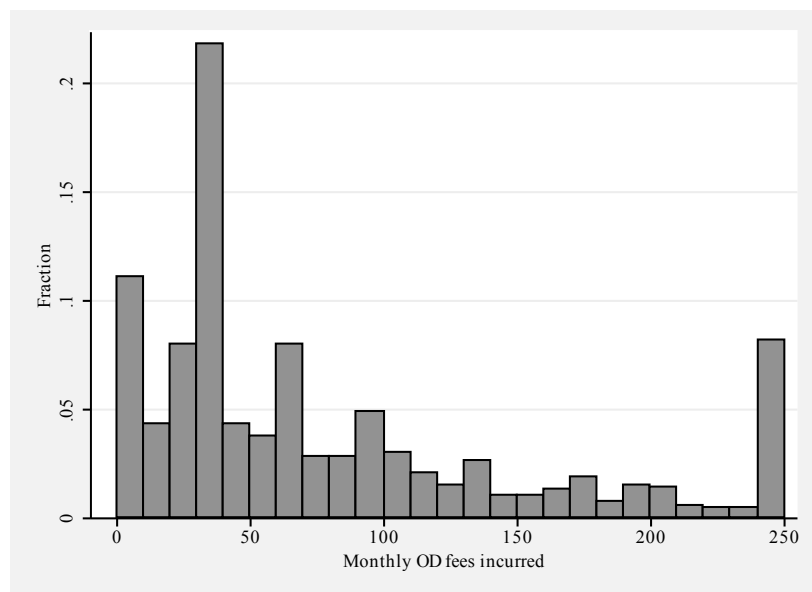
Figure A2. Distribution of monthly overdraft fees incurred, in panelist-months
with at least one overdraft fee.

Table 1. Sample and overdraft fee frequency

	All Active	Ever paid OD fee
Panelists	7448	3860
Panelist-months	102334	60096
Median months per panelist	16	19
Share panelists with any overdraft fee ever	0.52	1.00
Share months with OD fee	0.16	0.31
Share months with balance <$100	0.72	0.83
Any "snowball" month with 5+ OD fees?	0.15	0.28
Share "snowball" months with 5+ OD fees	0.03	0.06
Prob. of OD fee given 1+ OD fee last month	0.54	
Prob. of OD fee given 5+ OD fees last month	0.81	

Notes: Sample includes "active" accounts with at least one spending (check, bill, debit card, ATM) transaction within the sample. Sample includes only panelist-months for which a lagged value of overdraft fees is observed (zero or otherwise), for consistency with the sample used in our empirical model of Table 4 and beyond. "Overdraft fee" includes any account charge posted as a result of a negative account balance. "Share months..." variables are averaged across panelists. See Table A1 for details on the time series of panelists and overdraft fees.

Table 2. Survey timing and content.

Survey month/year	Overdraft fees or protection	Monitoring balances	Other bank fees	Spending control	Clicked Through to Survey
Aug04				yes	379
Oct04			yes		571
Jan05			yes		591
Mar05	yes		yes		631
Jun05					935
Aug05	yes				926
Sep05			yes		1317
Dec05	yes	yes	yes		1184
Mar06			yes	yes	1455
Jul06				yes	781
Aug06 (OD)	yes			yes	843
Aug06 (non-OD)					505
Oct06	yes				828
Nov06	yes	yes	yes	yes	686
q12007*					2008
Apr07		yes			1406
Aug07			yes		1502
Feb08		yes	yes		1010
Jul08			yes		1399
Oct08					961
Dec08				yes	1113

Notes: Surveys typically contain 60 questions. Questions about one of the topics above generally make up 2-3 of the questions. All panelists are invited to take every survey. October 2006 survey is denoted "overdraft-focused." Q12007 survey was administered over six months, on rolling basis; note its higher response rate.

Table 3. Panelist counts by surveys taken

Total surveys	Overdraft surveys							
	0	1	2	3	4	5	6	row total
0	2,204	0	0	0	0	0	0	2,204
1	1,409	168	0	0	0	0	0	1,577
2	823	246	26	0	0	0	0	1,095
3	491	228	86	8	0	0	0	813
4	193	84	101	31	1	0	0	410
5	140	41	109	59	9	0	0	358
6	118	15	48	78	19	2	0	280
7	51	7	29	59	52	12	0	210
8	0	3	16	43	46	15	1	124
9	1	1	4	26	48	27	6	113
10	0	0	3	8	32	31	5	79
11	0	0	0	4	5	26	13	48
12	0	0	0	2	10	14	20	46
13	0	0	0	1	9	17	13	40
14	0	0	0	0	4	8	12	24
15	0	0	0	0	1	4	7	12
16	0	0	0	0	2	4	1	7
17+	0	0	0	0	1	1	6	8
column total	5,430	793	422	319	239	161	84	7,448

Sample includes active panelists. Sample period contains 21 total surveys, and 6 overdraft-related surveys.

Table 4. Immediate and stock effects of taking overdraft surveys.

	Dependent variable: incurred at least one overdraft fee in month					
Mean (LHS)	0.26	0.26	0.26	0.26	0.26	0.35
	(1)	(2)	(3)	(4)	(5)	(6)
Immediate (Temporary) Effect: Took overdraft survey this month	-0.025 (0.016)	-0.037** (0.017)	-0.033* (0.017)	-0.037* (0.020)	-0.037* (0.020)	-0.069** (0.027)
Stock (Cumulative) Effect: OD surveys taken, last two years	-0.008* (0.005)	-0.012** (0.006)	-0.014** (0.006)	-0.017* (0.009)	-0.017** (0.008)	-0.026** (0.012)
Took any survey this month		0.017** (0.007)	0.013* (0.008)	0.020 (0.012)	0.021* (0.012)	0.054*** (0.017)
Any surveys taken		0.006 (0.004)				
Incurred overdraft fee last month					0.074*** (0.006)	0.055*** (0.007)
Incurred >5 overdraft fees last month					0.154*** (0.010)	0.132*** (0.011)
Includes "any surveys taken" dummies? (p-value)	no	no	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)
Includes controls for other survey content? (p-value)	no	no	no	yes (0.90)	yes (0.86)	yes (0.90)
p-value, linear restriction on "OD surveys"	0.98	0.99	0.95	0.99	0.99	0.44
p-value, linear restriction on "Any surveys"	n/a	0.00	n/a	n/a	n/a	n/a
N	59564	59564	59564	59564	59564	37249

* $p<0.10$ ** $p<0.05$ *** $p<0.01$

Notes: Unit of observation is panelist-month. Dependent variable is an indicator for "incurred at least one overdraft fee" in the panelist-month. Sample period is February 2006-December 2008. Relative to the full sample of 102,334, regressions in cols. (1)-(5) do not use 42,228 observations of panelists with no overdrafts during the entire sample, and 534 observations of panelists who overdraw in every month. Column (6) includes only panelist-months in which current balances in the account fell below $100 on at least one day. All models are OLS, with standard errors (clustered on panelist) in parentheses. Overdraft survey variables and "any survey" variables are not mutually exclusive. All models also include fixed panelist effects, fixed month/year effects, and a dummy for "panelist's last month in sample." The linear functional form restriction for "OD surveys taken" is not rejected, while the linear functional form restriction for "any surveys taken" is rejected. "Other survey content" controls include "took this month" and "surveys taken, last two years" for contactless card, gift card and auto loan questions.

Table 5. Effects of taking overdraft surveys by education/financial literacy/income

	LHS: Incurred any OD fee in month					
Mean of LHS:	0.29	0.22	0.29	0.22	0.27	0.26
Sample:	No college	College+	Low/Med. Lit.	High Lit.	<=$45,000	>$45,000
	(1)	(2)	(3)	(4)	(5)	(6)
Immediate (Temporary) Effect:	-0.065***	-0.021	-0.130***	-0.040	-0.073**	-0.037*
Took overdraft survey this month	(0.025)	(0.025)	(0.036)	(0.047)	(0.030)	(0.021)
Stock (Cumulative) Effect:	-0.022***	-0.002	-0.023**	0.004	-0.025**	-0.010
OD surveys taken last two years	(0.008)	(0.008)	(0.011)	(0.020)	(0.010)	(0.006)
Took any survey this month	0.028***	-0.003	0.046***	0.001	0.003	0.019**
	(0.011)	(0.011)	(0.013)	(0.017)	(0.014)	(0.009)
Incurred overdraft fee last month	0.072***	0.072***	0.063***	0.058***	0.065***	0.077***
	(0.008)	(0.009)	(0.009)	(0.015)	(0.010)	(0.007)
Incurred >5 overdraft fees last month	0.159***	0.140***	0.153***	0.176***	0.146***	0.157***
	(0.013)	(0.019)	(0.015)	(0.037)	(0.019)	(0.013)
Immediate OD effects diff., p-value		0.20		0.12		0.34
Stock OD survey effects diff., p-value		0.06		0.23		0.21
N	33094	24587	21369	7799	18484	38535

* p<0.10   ** p<0.05   *** p<0.01

Notes: Specification is identical to that in Table 4, column 5. Sample splits are based on income, education and financial literacy as self-reported in the registration (baseline) survey. Sample sizes are smaller than in Table 4 due to missing values for education/literacy/income. Literacy question asks "I know more than most people when it comes to managing my money and investments." Possible responses and our classification of literacy include "agree" (high), "neutral" (medium) and "disagree" (low). Total sample size varies across income, education and literacy splits due to category breaks and changes in registration survey content.

Table 6. Other types of survey content and overdraft fees.

	LHS: Incurred any OD fee in month (mean = 0.26)			
	(1)	(2)	(3)	(4)
Took overdraft-focused survey this month	-0.034	-0.029	-0.030	
	(0.024)	(0.034)	(0.033)	
				-0.034
Took OD-mentioning survey this month	-0.034	-0.035	-0.036	(0.024)
	(0.024)	(0.028)	(0.027)	
Took balance-/fee-/spending-mentioning survey this month	0.004	0.007	0.007	0.006
	(0.020)	(0.021)	(0.021)	(0.020)
Took overdraft-focused survey in last two years	-0.033***	-0.053***	-0.055***	-0.055***
	(0.010)	(0.018)	(0.017)	(0.017)
Overdraft-mentioning surveys taken last two years	-0.033***	-0.018	-0.019**	
	(0.010)	(0.015)	(0.008)	
				-0.019**
Bal.-/fee-/spending-mentioning surveys taken last two years	-0.021***	-0.018**	-0.019**	(0.008)
	(0.008)	(0.008)	(0.008)	
Took any survey this month	0.013	0.010	0.010	0.016
	(0.021)	(0.023)	(0.023)	(0.012)
Incurred overdraft fee last month	0.073***	0.073***	0.073***	0.073***
	(0.006)	(0.006)	(0.006)	(0.006)
Incurred >5 overdraft fees last month	0.154***	0.154***	0.154***	0.154***
	(0.011)	(0.011)	(0.011)	(0.011)
N	59564	59564	59564	59564

* $p<0.10$ ** $p<0.05$ *** $p<0.01$

Notes: Specifications here are identical to those in Table 4, column 5, except for the new RHS variables above, which are defined based on survey content. See Table 2 for details on overlap of different survey content variables. Column (1) restricts OD-mentioning and OD-focused surveys to have identical immediate and stock effects. Column (2) does not impose any coefficient equality restrictions. Column (3) restricts the overdraft-mentioning and balance/fee-/spending-mentioning coefficients to have identical stock effects. Column (4) restricts the overdraft-mentioning and balance/fee-/spending-mentioning coefficients to have identical stock effects, and restricts the OD-mentioning and OD-focused coefficients to have identical immediate effects.

Table 7. Checking account spending and balances, and survey-taking

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LHS:	Spending txns	ln(spending) in dollars	Spending txns, balances <$100	Spending txns, balances >$100	ln(account credits) in dollars	Days with balances <$100	Month with balances <$100
mean of LHS:	48.5	8.1	7.0	41.5	8.0	6.5	0.83
Immediate (Temporary) Effect: Took overdraft survey this month	-0.905 (0.959)	0.015 (0.034)	-0.166 (0.430)	-0.724 (0.922)	-0.035 (0.048)	0.015 (0.191)	-0.007 (0.014)
Stock (Cumulative) Effect: OD surveys taken last two years	-1.738*** (0.403)	-0.019 (0.013)	-0.376** (0.180)	-1.428*** (0.380)	-0.019 (0.015)	0.054 (0.071)	-0.002 (0.005)
Took any survey this month	0.247 (0.501)	-0.022 (0.018)	0.173 (0.194)	0.043 (0.477)	-0.025 (0.023)	0.034 (0.095)	0.007 (0.006)
Lagged dependent variable	0.485*** (0.010)	0.352*** (0.014)	0.306*** (0.014)	0.445*** (0.010)	0.226*** (0.021)	0.363*** (0.008)	0.214*** (0.009)
N	59564	59564	59564	59564	59564	59564	59564

* p<0.10   ** p<0.05   *** p<0.01

Notes: Sample and RHS specification identical to Table 4, column 5, except for the change in the lagged dependent variable and the addition of controls for changes in sample composition: months-in-panel and an interaction between that linear variable and an indicator for whether the panelist enrolled in the sample prior to January 2007. "Spending txns" measures the count of monthly account debits not including transfers and fees. Account credits include deposits and transfers from other accounts. "Days with..." measures the count of days in a month with lowest available balance below $100. "Month with balances..." take a value of one if daily balances fell below $100 on any day within the month.

Table 8. Spending composition and survey-taking

	(1)	(2)	(3)	(4)	(5)
LHS:	Debit card txns	Check txns	Autodebit txns	Cash withdrawal txns	Credit card payments ($)
mean of LHS:	13.7	4.7	1.0	3.1	757
Immedate (Temporary) Effect: Took overdraft survey this month	-0.291 (0.428)	-0.029 (0.167)	-0.101 (0.066)	-0.021 (0.156)	105.025 (73.645)
Stock (Cumulative) Effect: OD surveys taken last two years	-0.382** (0.189)	-0.090 (0.068)	-0.072*** (0.025)	0.006 (0.078)	-48.317 (32.425)
Took any survey this month	-0.060 (0.221)	0.053 (0.081)	0.012 (0.034)	-0.006 (0.069)	22.458 (32.654)
Lagged dependent variable	0.481*** (0.018)	0.408*** (0.022)	0.452*** (0.035)	0.318*** (0.019)	0.096*** (0.017)
N	59564	59564	59564	59564	59564

* p<0.10   ** p<0.05   *** p<0.01

Notes: Sample and RHS specification identical to Table 7. "Check txns" measures the count of monthly checks written."Autodebit txns" variable measures count of automatic deductions from account. "Credit card payments" is dollar amount of payments to credit card companies.

Table A1. Panelists and overdraft fees by month/year.

Month/year	Active		Any OD fee in sample	
	Panelists	Share w/overdraft	Panelists	Share w/overdraft
Feb-06	2,860	0.17	1,662	0.29
Mar-06	2,913	0.18	1,718	0.30
Apr-06	2,904	0.17	1,743	0.29
May-06	2,838	0.18	1,717	0.31
Jun-06	2,877	0.16	1,746	0.27
Jul-06	2,716	0.18	1,678	0.29
Aug-06	2,590	0.20	1,613	0.32
Sep-06	2,502	0.17	1,554	0.28
Oct-06	2,386	0.17	1,512	0.27
Nov-06	2,351	0.17	1,499	0.27
Dec-06	2,241	0.18	1,441	0.27
Jan-07	2,181	0.18	1,405	0.28
Feb-07	2,327	0.16	1,469	0.25
Mar-07	3,145	0.14	1,830	0.25
Apr-07	3,693	0.15	2,132	0.25
May-07	4,224	0.16	2,425	0.28
Jun-07	4,296	0.16	2,497	0.28
Jul-07	4,161	0.16	2,429	0.28
Aug-07	4,156	0.15	2,434	0.26
Sep-07	4,008	0.15	2,362	0.25
Oct-07	2,817	0.16	1,624	0.28
Nov-07	2,788	0.15	1,602	0.26
Dec-07	2,674	0.14	1,552	0.25
Jan-08	2,566	0.14	1,500	0.24
Feb-08	2,483	0.14	1,450	0.24
Mar-08	2,522	0.12	1,464	0.21
Apr-08	2,574	0.15	1,497	0.26
May-08	2,921	0.14	1,647	0.25
Jun-08	2,992	0.16	1,678	0.28
Jul-08	2,889	0.16	1,610	0.29
Aug-08	2,871	0.15	1,589	0.27
Sep-08	2,795	0.16	1,545	0.29
Oct-08	2,795	0.16	1,544	0.29
Nov-08	2,720	0.16	1,506	0.28
Dec-08	2,558	0.06	1,422	0.11
Total	102,334	0.16	60,096	0.27

Notes: Active panelist is defined as having at least one spending (check, debit card, ATM, ACH etc.) transaction during sample period. Overdraft prevalence is lower in December 2008 because the statement portion of the data is truncated: if someone's statement date is December 15th, we only observe activity for December 1st-15th. For each of the other months we can complement mid-month statement data with data from the next statement.

Table A2. Demographic characteristics

Category	Our sample		Population share
	Share months with at least one overdraft	Share of sample	
Education: measured for 3,675 panelists			
Associate's degree or less	0.34	0.60	0.64
Four-year degree or higher	0.27	0.40	0.36
Financial literacy: measured for 1,852 panelists			
Low/Medium	0.33	0.74	0.65
High	0.26	0.26	0.30
HH Income: measured for 3,634 panelists			
<=$45,000	0.32	0.44	0.50
>$45,000	0.30	0.56	0.50

Notes: "Our sample" is composed of the panelists analyzed in Table 5, with the characteristics here measured in the registration survey. Our financial literacy sample is relatively small because Lightspeed dropped the literacy question from the registration survey. U.S. data: education and income from the Survey of Consumer Finances; literacy from responses to the question: "How would you assess your overall financial knowledge? [on scale of 1 (very low) to 7 (very high)]" in Lusardi and Tufano (2009) Table 4. We consider 6 and 7 on their scale to be "high."

Table A3. Overdraft-related survey questions

Survey date	Questions
Panel A. Overdraft-mentioning surveys (overdraft questions no more than 5% of survey content)	
Mar-05	How important, if at all, are each of the following [8 features, including overdraft protection] when you are choosing a new bank for your main checking account and other bank services?
	How likely would you be to switch your checking account to a different bank, if it offered you [each of 8 features asked about, one is overdraft protection]?
Aug-05	How likely would you be to switch your checking account to a different bank, if it offered you [each of 8 features asked about, one is overdraft protection]?
	Please tell us how much you agree or disagree with the following [9] statements concerning fees assessed on your primary checking account: "I have overdraft protection to avoid overdraft fees"
	… off the top of your head, which of the following [8] fees do you believe were charged to your primary checking account in July 2005?
	Do you have overdraft protection for your primary checking account?
Dec-05	
	How satisfied are you with [the following 6] primary services at your bank? One of 6 is: "Clear and easy-to-find information about bank fees and charges (ATM fees, overdraft fees, minimum balance charges, etc.)"
Aug-06	Do you have overdraft protection?
	Which of the following types of loans do you have? Please select all that apply. "Overdraft protection on a checking account" is one of 12 choices.
Nov-06	What, if anything, frustrates you about your primary bank? Select all that apply. "Overdraft fees" is one of 14 choices.
Panel B. Overdraft-focused survey (overdrafts mentioned in 12 out of the 15 questions) designed by Center for Responsible Lending: October 2006	

Q2. Do you have overdraft protection for your checking account?
Q3. Think back to when you most recently opened a checking account. What products, if any, were described that would cover transactions if you did not have enough money in your account? (check all that apply)
Q4. In the past six months, about how many times have you overdrawn your checking account?
Q5. Why did you overdraw your account? (select all that apply)
Q6. What types of purchases or payments have caused your overdrafts? (open-ended)
Q7. Do you think that having overdraft protection makes you more or less likely to overdraw your account?
Q8. Have you ever had a checking account that was closed by you or your bank because of overdraft fees or a negative balance?
Q10. Would you like to have a warning displayed on ATM screens that lets you know when you will be withdrawing more cash than you have in your account?

Q11. Overdraft fees generally range between $25-30 If you received a warning from an ATM that your withdrawal would cause an overdraft fee, would you continue with the transaction or decide to cancel to avoid being overdrawn?
Q12. You are at a checkout paying with your debit/ATM card. Your purchases cost more than you have in your checking account. Would you rather:
    Have the bank automatically cover your overdraft without letting you know you are overdrawn, and charge you a fee
    Have the bank automatically decline your debit card transaction, to avoid the overdraft
    No preference
    Don't know
Q13. Say you made a purchase and did not have enough in your checking account to cover it. Given the following choices, how would you want the bank to handle your overdraft?
    Give me an overdraft line of credit with a $5 transfer fee
    Put the overdraft on my credit card and charge me a $5 fee plus 25% annual interest
    Pay the overdraft fee, charge me $25, and take the money I owe out of my next deposit
    Refuse to debit my account for more money than I have in it, return the check unpaid, and charge me a $25 NSF fee
    No preference
    Don't know
Q15. Has your bank ever deducted a portion of these [Social Security, unemployment, TANF, etc.] benefits to pay your overdraft fees?

Table A4. Survey-taking and demographics.

	LHS: Overdraft surveys taken	
	(1)	(2)
Education/literacy/income dummies	Yes (0.00)	Yes (0.69)
Any surveys dummies	No	Yes (0.00)
Tenure in sample dummies	Yes (0.00)	Yes (0.02)
Sample entry/exit date dummies	Yes (0.00)	Yes (0.00)
N	3630	3630
Adjusted r-squared	0.23	0.79

Notes: OLS regressions at panelist level. Dependent variable is the count of overdraft surveys taken. Education/literacy/income variables are indicators for 6/3/17 categories respectively. Tenure in sample measures total months in sample. Entry/exit dates are dummies for first month-year and last month-year in the sample. Yes/no indicates whether dummies were included; number in parentheses is p-value on exclusion test for the vector of dummies.

Table A5. Alternative specifications of stock effect.

stock indicators for:					last two years	last year	ever
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
OD surveys taken, last two years	-0.017**			-0.027**			
	(0.008)			(0.012)			
OD surveys taken, last year		-0.006		-0.004			
		(0.006)		(0.007)			
OD surveys taken, ever			-0.006	0.022			
			(0.010)	(0.016)			
One OD survey taken					-0.017	-0.004	0.000
					(0.015)	(0.010)	(0.022)
Two OD surveys taken					-0.029	-0.012	0.006
					(0.020)	(0.014)	(0.028)
Three OD surveys taken					-0.050*	-0.020	-0.000
					(0.027)	(0.019)	(0.036)
Four OD surveys taken					-0.067*	-0.045	-0.029
					(0.035)	(0.044)	(0.044)
Five OD surveys taken					-0.090**		-0.049
					(0.044)		(0.053)
Six OD surveys taken					-0.081		-0.011
					(0.058)		(0.066)
N	59564	59564	59564	59564	59564	59564	59564

* $p<0.10$ ** $p<0.05$ *** $p<0.01$

Notes: First specification is identical to that in Table 4, Column 5. Other specifications are identical but for functional form on stock effect. Columns 5/6/7 measure stock in last 2 years/1 year/ever. Coefficients on "took any" and lagged overdraft variables not shown.

Table A6. Alternative definitions of dependent variable.

Dependent variable:	Number of OD fees				Dollar amount of OD fees			
Sample	All	"edge"	low-ed.	low finlit	All	"edge"	low-ed.	low finlit
mean of LHS:	0.79	1.11	0.92	0.88	23.63	33.32	27.71	26.53
	(2)				(3)			
Immedate (Temporary) Effect:	-0.109	-0.338**	-0.206*	-0.171	-2.536	-8.674**	-5.171	-5.192
Took overdraft survey this month	(0.078)	(0.136)	(0.118)	(0.119)	(2.456)	(4.229)	(3.822)	(3.843)
Stock (Cumulative) Effect:	-0.031	-0.054	-0.032	-0.036	-0.647	-0.841	-0.593	-0.625
OD surveys taken last two years	(0.035)	(0.069)	(0.048)	(0.051)	(1.055)	(2.001)	(1.494)	(1.646)
Took any survey this month	0.143***	0.158*	0.250***	0.276***	4.384***	5.787*	6.834***	8.154***
	(0.053)	(0.086)	(0.078)	(0.081)	(1.657)	(2.984)	(2.567)	(2.487)
Number of OD fees last month	0.239***	0.214***	0.249***	0.218***				
	(0.016)	(0.018)	(0.019)	(0.018)				
Amt of OD fees last month					0.242***	0.214***	0.262***	0.229***
					(0.020)	(0.020)	(0.025)	(0.022)
N	59564	21369	33094	38886	59564	21369	33094	38886

* p<0.10   ** p<0.05   *** p<0.01

Notes: But for the specification of the lagged dependent variable, RHS specification is identical to the one used in Table 4, Column 5. "Edge" sample is from Table 4, column 6. "low-ed." and "low finlit" are as in Table 5 cols. (1) and (3).