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Banking on Transparency for the Poor: Experimental Evidence from India*

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Banking on Transparency for the Poor: Experimental Evidence from India

Abstract

Do information frictions limit the benefits of financial inclusion drives for the rural poor? We evaluate an experimental intervention among poor Indian women receiving digital cash transfers. Treated women got automated voice calls detailing transactions posted to their accounts. Treatment increased knowledge of account balances and trust in local banking agents. Indicative of improved consumption-smoothing, administrative data show that treated women accessed transfers faster, with impacts dissipating once notifications were discontinued. Consistent with account information benefiting those with high transaction costs more, the intervention increased account use among women who lived more than an hour from the kiosk.

1 Introduction

Transparency is widely considered a cornerstone of well-functioning financial service markets, disciplining firms' behavior (Fischer, 1999; Nier and Baumann, 2006) and fostering trust in institutions (Horvath and Katuscakova, 2016; Jansen et al., 2015). At the micro-level, a critical component of transparency – and, one that is foundational to consumer protection principles – is ensuring that consumers can easily access and understand information about account activity (Campbell et al., 2011; Garz et al., 2021). Such transparency promises to directly improve financial decision-making by individuals and, by building consumer trust in financial service providers and products, further spur service adoption and use.

Yet, transparency around account activity and trust in the financial sector remain major issues in lower-income countries, especially among the recently banked. Due in part to government-supported financial inclusion drives, more than 1.6 billion individuals, half of them women, have gained access to formal financial services over the last decade.[1] While an important impetus for this progress was to provide poor individuals a safe and reliable way of receiving government-to-person payments (G2P), reports suggest that beneficiaries often struggle to access these payments in a timely manner. Beneficiaries' challenges include unpredictability regarding when transfers arrive in their account, potential corruption among local banking agents, unreliable banking infrastructure, and limited comprehension of program rules (Stuart, 2018). There is a risk the resulting high transaction costs and information gaps then exacerbate low levels of trust in banks. Moreover, despite gains in account access, women remain at a disadvantage, as they are 6 percentage points less likely than adult men to have their own bank account in low and middle-income countries (Demirgüç-Kunt et al., 2022).

These concerns motivate our study, which experimentally evaluates the impact of a simple transparency mechanism – voice calls confirming recent transactions and bank account balances – on knowledge, trust, and account use among recently banked low-income Indian women. In 2014 India began an ambitious financial inclusion agenda that has since opened over 400 million

[1] Authors' calculations for 2011 to 2021 using population data on adults aged 15+, combined with percent of population aged 15+ with an account. We use population data from 2020, since 2021 estimates were not available at the time of writing. Data sourced from the World Bank Databank's World Development Indicators and Global Financial Inclusion Indicators, available at `https://databank.worldbank.org/home`. Accessed June 29, 2022.

"no frills" bank accounts (more than half owned by women) (RBI, 2020).[2] In FY 2021-22, this infrastructure supported the government in sending US$ 47 billion in digital cash transfers to 735 million beneficiaries.[3] Beneficiaries in our study, most of whom were banked during this inclusion push, had a history of receiving government benefits payments into their bank accounts. At the median, G2P transfers accounted for roughly two thirds of all deposits and over 98 percent of deposit value in our sample's accounts. Yet at baseline women were not always aware of G2P deposits and roughly half the sample did not know their account balance. These gaps reflect the high cost of acquiring information for a majority-illiterate sample: while most women got account information from their local banking agent, the average travel time to the nearest kiosk was roughly an hour; adding in waiting time, time to transact, and the trip home meant that the average beneficiary spent nearly three hours to make a deposit or withdrawal at the "local" banking point. As a result women in our study, who compared to men have fewer financial resources, are less mobile, and less educated – had minimal visibility into their account activity.

We partnered with a large public sector bank to design the voice notification service, which helped women monitor balances and identify when G2P payments arrived in their account. The service utilized voice calls to ensure timely delivery of information that was easily understood by those with limited literacy. The voice notification service sent automated messages recapping bank transactions 1-2 days after they posted. During weeks with no banking activity, women received calls summarizing their balance and noting that no transaction had occurred. The service was very popular: 79 percent of treatment group women signed up and 70 percent of the automated voice calls were picked up, which is noteworthy given that individuals regularly screen calls. Our intervention was active for just under one year.

Overall, voice notifications increased women's access to account information and trust in bank kiosks. Women in the treatment group were 16 percentage points more likely to rely on phone calls for balance information, 7 percentage points less likely to rely on the kiosk operator, and 6 percentage points more likely to know their account balance. Alongside, treated women reported significantly higher levels of trust in both the accuracy of information given at the kiosk, and in keeping savings at the kiosk.

[2]Account statistics current as of February 2022, retrieved from the Pradhan Mantri Jan Dhan Yojana website's progress report: `https://pmjdy.gov.in/account`. Accessed March 9, 2022.

[3]Official statistics posted on `https://dbtbharat.gov.in/`. Accessed June 2, 2022.

By reducing the time between arrival of G2P transfers and the next withdrawal, voice notifications also improved access to government benefits. Bank administrative data show that treatment group women were 3-5 percentage points more likely to withdraw government transfers made during our study period. After accounting for censoring, hazard models indicate an 8-16 percent increase in the likelihood a government transfer was withdrawn in any given period. We find no such pattern for traditional (non-transfer) deposits; this suggests that voice notifications reduced the transactions costs associated with G2P and increased beneficiaries' ready access to government transfers.

The service did not, however, change the number and value of non-transfer deposits, nor were changes in withdrawal behavior substantial enough to significantly change the average daily balance.[4] This is consistent with the fact that the majority of women stated that they opened accounts to access transfers and that our low income female population had limited engagement with labor markets. This holds both during our main study period (up to one year after the service started) and over the longer term – we exploit a second tranche of administrative data from our banking partner 10-15 months after voice calls were ended to show that our intervention had no impact on time to transfer withdrawal, average account balances, or other measures of account use leading up to and immediately following the onset of the Covid-19 pandemic.

To provide some suggestive evidence on which constraints to account use were impacted by treatment, we examine heterogeneity in treatment effects with respect to trust in the kiosk and travel time to the kiosk. Overall, we find no significant differences with respect to baseline trust. In contrast, we find significant evidence of increased non-transfer deposits among women who reported that it takes more than an hour to travel to the kiosk. Far-off women are less likely to use their accounts for non-transfer purposes absent our intervention, and the voice notification service effectively closed the account use gap between this group and their nearer-by peers.

The main contribution of our paper is to provide causal evidence showing how transparency and verifiability of banking transactions – which is typically presumed in economic models of saving and borrowing, yet often out of reach in lower-income settings – impacts beneficiaries' trust in banking outlets and use of bank accounts. Our paper builds on research by Bachas et al. (2021),

[4]This is not simply a matter of power – we are able to rule out positive effects on the average balance greater than 7 percent of the control group mean.

who find that giving Mexican G2P recipients debit cards, which both increase transparency (by making it easier to check account balances) and reduce transaction costs (by making withdrawals easier and enabling cashless purchases), boosts savings. Our design allows us to isolate the effect of transparency while holding transaction costs of accessing the bank constant. In contrast to our work, Bachas et al. (2021) and Galiani et al. (2020), who study the effects of a trust workshop for Peruvian beneficiaries of a conditional cash transfer program, find that growth in trust is associated with growth in savings. It is natural, then, to ask why we observe different effects. One possibility is that our sample is poorer, with less human capital, and greater constraints to their mobility and economic activity. For these women, optimizing consumption requires drawing down government transfers sooner, not later.[5]

The welfare consequences of this change could be significant: while we lack high-frequency data on consumption required to directly assess welfare effects in our setting, Bazzi et al. (2015) show that Indonesian cash transfer recipients reduce expenditure by 7.5 percentage points when their transfer payments are delayed, indicating difficulty in consumption smoothing. More generally, welfare losses due to delayed payments could be large in settings where households are risk averse and/or smoothing consumption following adverse shocks is difficult or costly (Chetty and Looney, 2006, 2007). We believe this is the case in our setting. Low income, rural Indians are vulnerable to shocks – for example Srinivas et al. (2021) estimate that 40 percent of households in the nationally-representative Indian Human Development Survey reported at least one illness in the past year, with associated medical expenditures and wage losses for the poorest households amounting to roughly one fifth of total household spending. While informal insurance networks can help households cope with some of these risks, research shows that consumption smoothing is typically incomplete (Gertler and Gruber, 2002; Ligon, 1998; Townsend, 1994). In line with this, social protection programs like India's workfare program have been shown to help households cope with adverse shocks (Dasgupta, 2017), with especially large benefits for the poorest (Deininger and Liu, 2019).

Our work also contributes to a broader literature on financial inclusion and G2P payment systems in lower-income settings. While benefits to financially including low-income consumers

[5]Paralleling our findings, Attanasio et al. (2019) find that a tablet-based financial education program, which increased trust in the bank, had no impact on account use of cash transfer beneficiaries in Colombia.

are well documented (see Demirgüç-Kunt and Singer (2017) for a review), many beneficiaries of inclusion interventions fail to use their new accounts (Karlan et al., 2014; Dupas et al., 2016). Identifying constraints that limit deeper use of financial services and understanding how to address them is therefore a policy priority. While trust is a vital precondition for facilitating economic transactions (La Porta et al., 1997), in our setting the primary constraint facing beneficiaries appears to be knowing when transfers are available to withdraw and timing that withdrawal to minimize costs associated with going to the bank. Without meaningful non-transfer income, such beneficiaries may have little reason to make their own deposits or engage in other banking transactions; and when transaction costs are high, withdrawing G2P deposits in one lump sum may be optimal (Baumol, 1952; Tobin, 1956). Seen through this lens, limited activity in G2P-linked accounts may simply reflect a well-functioning social protection system that is targeting particularly needy individuals.

Finally, our paper contributes to a growing literature evaluating mobile-driven interventions designed to support financial inclusion and development. Much of this work has focused on the use of SMS technology to deliver behavioral-focused interventions such as reminders and encouragement (see, e.g. Blumenstock et al., 2018; Cadena and Schoar, 2011; Dizon et al., 2020; Kast et al., 2018; Karlan et al., 2016). Like Cole et al. (2021), who study a business training intervention, and Cole and Fernando (2020), who focus on agricultural extension, our work shows that audio messages are a promising way to deliver information to low-income populations where limited literacy may otherwise limit the benefits of text-based approaches.

The rest of the paper proceeds as follows: Section 2 describes the experimental context and study design, Section 3 details the experimental design and presents a brief conceptual framework that highlights opposing effects transparency might have on account use. Finally Section 4 presents the results and Section 5 concludes.

2 Context and Study Sample

Our study takes place in the Indian state of Madhya Pradesh (MP), one of the country's largest and poorest states.[6] Gender norms in MP are particularly conservative, reflected by a sex ratio

[6]According to the Reserve Bank of India, 32 percent of the state's 23 million individuals fell below the poverty line in 2012 (RBI, 2020).

of 931 women per 1,000 men, ranking MP 20th among India's 28 states (Government of India, Ministry of Home Affairs, 2011). Women face significant mobility restrictions – according to the 2015-2016 National Family Health Survey, only 33 percent of women in MP reported they were allowed to go alone to the market, the health center, and places outside their village/community.

Despite these issues, MP was an early leader of India's ambitious financial inclusion efforts. Anticipating the shift in the disbursal of government social protection payments from cash to electronic payment into bank accounts, in 2011 MP began a program to expand rural kiosk banks. Under the program, banks were mandated to open rural banking kiosks, such that all citizens had access to individual, biometrically verified accounts within 5 kilometers of their village. This push intensified under the nationwide financial inclusion scheme called *Pradhan Mantri Jan Dhan Yojana*, which launched in 2014 and opened hundreds of millions of low-cost bank accounts in an effort to enable India's push towards electronic benefits payments, typically referred to as "direct benefits transfers" or DBTs.[7]

The kiosk banking network is especially important for mobility-constrained women, who are preferentially targeted for household-level entitlements under a number of government schemes. Yet women still face important challenges when accessing DBTs through the banking system. First, kiosk service can be poor, with unpredictable opening hours, long lines, and unreliable servers. Forty percent of kiosk customers in the nationally-representative 2018 Financial Inclusion Insights (FII) Survey reported finding the agent absent on at least one occasion. Second, not all agents are trustworthy, with 33 percent of customers in the FII reporting being overcharged at least once. Women in our study report similar issues with accessing accounts through kiosks. Appendix Figure A.1 shows that at baseline, over half of women cite kiosk closures, long lines, infrastructure issues (downed servers, broken point of service (POS) machines, inactive fingerprint readers etc), and lack of cash to render for withdrawals as challenges to banking with their local kiosk. Forty-four percent also feel that the kiosk is too far away. Customer service is also an issue, but to a lesser extent: 21 percent of women report the operator is not helpful and 11 percent report the operator is not trustworthy.

Our study sample is drawn from peri-urban and rural areas of Gwalior and Morena districts

[7]Here the term "low-cost" primarily focuses on the customer-focused implications of bank accounts. These accounts have minimal balance and activity requirements and fees to maintain.

of Madhya Pradesh. We partnered with a large public-sector bank to identify a sample of women who (a) had accounts at banking kiosks and (b) received DBTs into their accounts. To do so, the bank provided us a list of female account holders in these two districts who, prior to baseline, were receiving DBTs for cooking gas under the *Pradhan Mantri Ujjwala Yojana* scheme[8]. We restricted the sample to married women aged 20-50 per administrative records and identified 37 of the most active kiosks in terms of number of female accounts. We attempted to survey all 2,259 age-eligible women with accounts at these kiosks, completing interviews with 989.[9] We then verified baseline phone numbers from the respondents by conducting a post-survey call-back. All 870 women with verified phone numbers were eligible for randomization into treatment or control.

As shown by Appendix Table A.1, women in our sample are on average 37 years old and live in households with 5.4 members. Literacy is low – nearly 60 percent had no schooling and fewer than 20% could read a paragraph provided by our surveyors. Households in the sample are poor – on average control group households had monthly wage earnings of just Rs. 2500 (US$ 36 at an exchange rate of Rs. 69 per US$). Only 28 percent of women reported working for pay in the past month, with average earnings (including women with zero earnings) totaling just Rs. 300. Appendix Table A.2 shows that the average daily balance in partner bank accounts is around Rs 1,500-1,600 ($22-23). While relatively small both in absolute terms and relative to household income, it is five times larger than women's average monthly earnings. This reflects the large role that DBTs play in driving account use. Roughly two thirds of women report that their reason for having the account was to receive government transfers (Table A.1) and average DBT deposits are similar to non-DBT deposits – which we assume reflect personal savings – in the pre-intervention period (Table A.2). These means mask substantial heterogeneity, however. Appendix Figure A.2 uses administrative data from the bank to quantify the role benefits transfers play in driving account activity. In terms of the *value* of transactions (Panel A), the median woman has nearly all of her deposits come from DBTs, while roughly a quarter of women receive no DBTs after their accounts were flagged for us by the bank.[10] In terms of the *number* of deposits (Panel B),

[8]To be eligible for this program, women had to live in rural areas and hold an officially-provided "below poverty line" card.

[9]The most common reasons why women were not surveyed were that the respondent migrated/was not home/was unavailable for interview (53 percent of cases), the bank account details from our bank partner could not be verified or conflicted with the respondent's records (21 percent of cases) and the respondent's home could not be found (10 percent of cases).

[10]While all women in our sample had received DBTs for cooking gas in the past, future DBTs under this program

government benefits drive around two thirds of deposits into accounts for the median woman.

Appendix Figure A.3 graphs women's self-reported benefits receipt in the year prior to the baseline. Overall, 62 percent of women reported receiving at least one DBT. This is *prima facie* evidence that transparency regarding government transfers is a significant issue, as our analysis of administrative data shows that 79 percent of women received at least one transfer in the 4.5 months leading up to our baseline survey. Consistent with our sampling strategy, the most commonly reported benefit, named by 27 percent of our sample, related to cooking gas/LPG. Other common benefits include pensions (19 percent of women) and education benefits (12 percent of women).

A key challenge for women receiving DBTs is that their arrival is unpredictable, making account balances difficult to monitor. For example, over half of women at baseline reported that DBTs are "always" or "sometimes" late, and when asked about the purpose of their last visit to the kiosk, over a quarter of women reported they had gone to ask whether a DBT had arrived in their account or to ask the kiosk operator details of their account. These non-transactional trips to the bank can be costly in terms of both money and time, as the average women reported that it takes nearly an hour to walk to her usual banking point, with a round trip including time spent at the bank requiring nearly three hours. As a result, more than 50 percent of women could not report their account balance at baseline (Table A.1).

These observations motivate the design of our intervention, which aimed to increase the transparency of account activity for account holders with otherwise limited capacity to monitor. Given that many of the women in our sample are illiterate, we worked with our banking partner to design a voice notification system, described in more detail below, which was designed to help women quickly identify new DBT payments while breaking the de facto informational monopoly kiosk operators often have over women's accounts.

3 Experimental Design and Conceptual Framework

3.1 Experiment and Data

Intervention The voice notification service was designed to provide high frequency verification of new transactions alongside regular monitoring of account balances. After a withdrawal or deposit occurred in a participant's account, the service sent out an automated call 1-2 days later

are only triggered after gas cylinders are refilled.

informing the accountholder that on X date, Y amount of money was credited or debited, and the balance as of X date was Z.[11] If multiple transactions occurred on a single day, the message was formatted to include the total amount deposited, the total amount withdrawn, and the final balance. If no transactions were posted during a given week, participants received a call noting this, alongside their current balance.

Calls were made at a specific time of the day for each participant based on the participant's time preference for receiving calls (as repoorted in the baseline survey). If a notification call was not picked up, the system made up to two subsequent calls with the same message over the next two days. Overall, between April 2018 and March 2019 the voice notification service sent each woman an average of 36 notification calls, with an average pickup rate of 70 percent.[12] The service was offered to women in the treatment group at no charge.

Randomization Of the 870 women in the study, 437 were selected to receive voice notifications and the remainder were placed in the control group, which received no calls of any kind. We conducted the randomization in office, via computer after the baseline survey was completed. The randomization was stratified on four characteristics: district, whether the woman owned her own phone, above/below median frequency of visits to the kiosk in the previous 6 months, and above/below median level of trust in saving money at the kiosk.

We enrolled treatment group women in the service by calling them on phone numbers collected at baseline, describing the service, and asking them if they wished to receive voice notifications. Only those who provided informed consent received the treatment.

Data and Balance Check We utilize three sources of data – first, we received administrative data from our partner bank covering transactions into accounts owned by all of our sample beneficiaries between November 2017 to March 2019 and January to June 2020. For every transaction in an account we see the timestamp, amount, and whether it was a credit or debit on the account and post-transaction account balance. For data spanning November 2017-September 2018 the bank separately identified whether a transaction was a government DBT. For data after this period, we infer DBT deposits based on deposit amounts and each woman's past DBT history.

[11]See Appendix B for the wording of the automated statements.

[12]We classify a notification as "picked up" if the system recorded a call pickup on either the first, second, or third attempt.

Overall, these inference rules perform well: applying the rules to validation data in which we have actual DBT status, we successfully identify 96 percent of DBT deposits. This comes at the cost of mis-categorizing 8 percent of non-DBT deposits as DBTs.[13]

Second, we have administrative data from the voice notification service, which was active between April 28, 2018, and March 22, 2019. For each enrollee, we know the date and time when each call attempt was made, the outcome of the attempt, and the fraction of the message listened to (inferred from the duration of the call).

Finally, we collected baseline and endline data. The baseline was conducted between February and March 2018 and collected demographic information, information on phone ownership and usage, bank account usage, and knowledge about and trust in the banking system. The endline survey was conducted from May to June 2019 and focused on account usage, knowledge about account activity, and trust in the banking system. We were able to interview 791 of the 870 women enrolled in the study at baseline, with no differential attrition between the treatment and control group (see Appendix Table A.1). Appendix Figure A.4 provides a timeline of study activities, including survey waves, administrative data collection, and intervention rollout. Appendix B provides additional detail on data sources and key variable construction.

We verify randomization balance using the baseline survey (Appendix Table A.1) and bank administrative data (Appendix Table A.2). Overall, the randomization was successful with no imbalances significant at the 10 percent level or greater.

3.2 Conceptual Framework

Before turning to the results, consider how voice notifications may have affected women's engagement with the banking system. Prior to these notifications, the primary way a woman acquired information on her account balance and on whether anticipated DBTs had arrived in her account was by making a trip to the bank kiosk. At the kiosk she would largely rely on the kiosk operator to provide either a verbal report on her account or give her a printout of her balance. In this setting, the voice notification service served two purposes. First, it lowered the transaction costs involved in tracking own account activity and when DBTs reach the account. Second, it provided an independent source of account activity verification, which could affect trust in the kiosk. We

[13]Appendix B provides more detail on this procedure.

now discuss how these two channels of influence may impact women's financial behavior.

First, consider the *notification effect*: Reflecting the importance of DBTs for consumption smoothing for poor households, DBTs drive a substantial amount of account activity for our sample women (Appendix Figure A.2). However, roughly half the women in our sample report that the receipt of DBTs is typically delayed (Appendix Table 1). Given that bank visits involve significant time costs and that the likelihood of DBTs arriving in one's account increases in time, we posit that these women typically waited longer than ideal to access DBT payments. The treatment improved women's ability to identify when payments arrived in their account, helping them to better time their bank visits and withdrawals. This would work to shorten time to benefits withdrawal and improve consumption smoothing. This, in turn, will also reduce the average daily balance.

Second, consider the *trust effect*: the service made it easier for women to track their balances and keep account of changes. It also allowed them to compare kiosk operator verbal reports of balances to third party reports. We conceptualize trust as the woman's belief regarding the likelihood that money deposited in period t will be in her account in period $t + 1$. Especially when travelling to the kiosk is costly, greater trust will reduce the woman's incentive to withdraw government benefits deposits immediately. However, the impact of increased trust on a woman's financial behavior will depend on how immediate her need for funds is – income constrained women's withdrawal behavior may continue to be driven by consumption smoothing needs even when trust increases. Put differently, an increase in trust increases the expected return on kiosk savings, which will increase account balances, only as long as returns on savings dominates the direct consumption benefits from cash on hand.[14]

To summarize, if women are income-constrained then the main value of DBTs is realized as cash-in-hand for poor women. In that case, we expect the notification effect to dominate, reducing time to DBT withdrawal. The intervention also potentially increases trust in the banking system – however, this would work to increase savings only if the returns to savings dominate the value of cash in hand.

[14] Here we focus on trust in financial institutions. However, the intervention – by giving women better visibility into the timing of benefits payment – may have also affected trust in social protection programs, specifically trust in the government to make timely deposits. Women lacking trust would delay trips to the kiosk. To the extent that the service increased trust in timely payment, then women may be less likely to postpone withdrawals.

4 Results

4.1 Voice Notification Takeup and Engagement

The voice notification service was popular. Of the 437 women randomly selected for treatment, 344 (79 percent) signed up. Of those who did not consent to the intervention (96 respondents), in 60 percent of cases the phone number collected at baseline was incorrect, did not connect, or was not picked up during the verification process. In the remaining 40 percent of cases, the client or a family member indicated they did not want to participate in the intervention.

Administrative data indicates that engagement with the service remained high for the duration of the intervention. Figure A.5 plots the share of notifications that were received over time. Over the year, on average 70 percent of messages were received, with pickup rates declining from rates above 90 percent to a rate between 60-70 percent over the first six months. After roughly six months, pickup rates stabilized in the 60-70 percent range.

Appendix Table A.3 limits the sample to the treatment group and reports correlates of voice notification sign up (columns 1 and 2) and, for those who sign up, the pickup rate (columns 3 and 4). Columns 1 and 3 report results of bivariate regressions, while columns 2 and 4 report the result of a "kitchen sink" regression in which all baseline covariates are included simultaneously.[15] Although the service was designed to be accessible to illiterate women, those with more education and more literacy are significantly more likely to sign up. Women who live farther from the kiosk are also more likely to sign up, suggesting that the service is more valuable to those who incur greater transaction costs to checking their balance in person. Conditional on signing up, the pickup rate is higher among women who do not own their own phone and those who live in bigger families – this may reflect the fact that many women reported other household members answering the voice notification calls on their behalf. Finally, women who experienced regular DBT delays had a 6 percentage point higher pickup rate, suggesting the service was useful for monitoring the arrival of erratic benefits payments.

[15] We do not include strata fixed effects in these regressions, since some of the strata are variables of interest in the table.

4.2 Impacts on Knowledge and Trust

Next we turn to survey data to understand whether the voice notification service affected account knowledge and, in turn, trust in the kiosk. We use two core empirical specifications throughout our analysis. First, we focus on the post-treatment period and run intent-to-treat regressions of the following form:

$$y_i = \beta_0 + \beta_1 treat_i + \beta_2 y_{0i} + \lambda_s + \epsilon_i \tag{1}$$

where y_i is the outcome of interest for individual i, $treat_i$ is a dummy variable indicating assignment to the treatment group, y_{0i} is the baseline version of the outcome, λ_s are strata fixed effects, and ϵ_i is an error term. Standard errors are heteroskedasticity robust. As a robustness check we run differences-in-differences specifications as follows:

$$y_{it} = \gamma_0 + \gamma_1 treat_i \times post_{it} + \gamma_2 treat_i + \gamma_3 post_t + \delta_s + \xi_{it} \tag{2}$$

where $post_t$ identifies the post-intervention period, δ_s are strata fixed effects and ξ_{it} is the error term, which is clustered at the individual level.

The first two columns of Table 1 study impacts on women's sources of information about their accounts. Voice notifications reduced women's reliance on information from the kiosk by 7-8 percentage points, against a control mean of 72 percent. Alongside, women were 16 percentage points more likely to say they get balance information from a phone call. Appendix Figure A.6 shows that treatment had a similar 20 percentage point effect on the likelihood a woman reported learning about a DBT deposit from a phone call. The figure also reports some suggestive evidence that this saved women trips to the bank – women are 3 percentage points less likely to say their last trip to the kiosk was to check on the arrival of a DBT deposit (significant at the 10 percent level), but we see no significant effect on the total number of visits reported for this purpose. Overall, we infer that the voice notification service successfully gave women a way to monitor the account balances without relying on costly trips to the bank.

Columns 3 and 4 ask whether this translates into improved knowledge. Both regression specifications indicate women were 6 percentage points more likely to report knowing their exact bank balance.[16] Results are less clear when we use a more expansive definition of being well informed – while results from specification 1 indicate a marginally significant 4 percentage point increase in knowing one's balance either exactly or approximately, results attenuate to zero when we use a differences-in-differences approach. Taken as a whole, these results point to improved knowledge, which is notable given that the voice notification service stopped two months before the endline survey started (see Appendix Figure A.4 for a timeline of key study activities), and we see broad improvements in the information environment among all respondents in the intervention period.

The last two columns of Table 1 ask whether improved knowledge impacted women's trust in the kiosk. We used two Likert scales to measure trust: in one question we asked women to assess the accuracy of information provided at the kiosk. In a second question we asked women how much trust they placed in saving money at the kiosk. In both cases, response options ranged from 1 (very low) to 5 (very high). To account for the ordinal nature of these variables we present results from ordered logit specifications, converting coefficients into odds ratios. In column 5, results indicate that treatment increased the likelihood of a woman being in a higher trust bin (e.g. average or below vs. high or very high) by 43-50 percent. Treatment likewise increased the likelihood of higher trust in saving at the kiosk by 33-46 percent.

One disadvantage of an ordered logit is that its structure imposes a uniform effect across the distribution of outcome values. Figure 1 graphs the distribution of the two trust variables in the treatment and control group to study this nonparametrically. In both cases, we see that treatment effects are concentrated in the upper part of the trust distribution. Few women in both treatment and control reported having "low" or "very low" trust, with minimal differences across groups. Instead, women in the treatment group were significantly more likely to report having "very high" trust, with mass shifting out of "average" and "high" levels of trust.

4.3 Impacts on Account Use

Given that the intervention increased trust, it is natural to ask whether this in turn impacted women's engagement with their accounts. Recall from our framework in Section 3.2 that if trust constrains saving, increasing trust should encourage saving and increase the time between

[16] We rely on self reports because our agreement to access administrative data from the bank ended two months before the endline survey; thus we cannot reliably compare self reports to actual bank balances as a way of measuring knowledge.

deposits and withdrawals, provided the return to savings is higher than that of cash on hand to meet immediate consumption needs. It is not obvious that trust will be a binding constraint for our population, however. First, a relatively small share of the population reports low or very low trust in the kiosk (though this could in part reflect social desirability bias). Second, our population may already save at their preferred levels (i.e., they are not savings constrained): as shown by Figure A.2, DBTs drive most of the transaction value in our sample's accounts. Given unpredictable arrival times and non-trivial costs of accessing the kiosk, our sample may instead be *withdrawal constrained*, waiting longer than they would like to visit the kiosk to increase the probability that a DBT will be waiting when they arrive.

To test these hypotheses, we use administrative data from the bank to study how the intervention impacted account use. We focus on the period when our intervention was active, ending March 2019. First we study impacts on the time between a deposit and a subsequent withdrawal. The clearest prediction is for DBT deposits, which are arguably exogenous provided the intervention did not impact women's propensity to receive government transfers into our partner bank's accounts. The last two rows of Appendix Table A.2 verify that there are no significant treatment-control differences in the number and value of DBT deposits in the post-intervention period. Appendix Figure A.7 graphs the distribution of DBT deposit dates by treatment group, showing that arrival times are very similar, though in the post-intervention period, relatively more DBTs arrive in the control group towards the end of the intervention period. Our analysis also considers non-DBT deposits, though we interpret these results with caution, since these deposits may be endogenous to treatment.

The first two columns of Table 2 use linear probability models following specifications 1 and 2, modified so the unit of observation is the deposit day. The outcome is a dummy variable equal to one if a withdrawal occurred after the deposit and before the end of the observation period (March 18, 2019, in the case of post-intervention deposits and March 19, 2018, in the case of pre-intervention deposits).[17] Since control group deposits arrived somewhat later than treatment group deposits, we directly control for the number of days between the deposit date and the end

[17] We drop deposits that occurred during the enrollment period, as the effect of anticipating treatment could differ from the effect of treatment itself. In rare cases where both a DBT and non-DBT deposit occurred on the same day, we classify the deposit day as a non-DBT deposit day. In panel A we control for the share of deposits that were withdrawn, as well as a dummy variable identifying respondents with no deposits in the pre-baseline period.

of the observation period. DBT deposits were 3-5 percentage points more likely to be withdrawn by the end of the observation period in the treatment group. We see no impact on withdrawal of non-DBT deposits.

Columns 3 and 4 use Cox proportional hazard models to evaluate treatment effects on time to withdrawal while accounting for censoring.[18] We convert coefficients into odds ratios to facilitate interpretation. Column 3 shows that treatment increases the odds of DBT withdrawal by 8-16 percent; however only the 16 percent estimate, drawn from our differences-in-differences style specification, is significant. There are no significant effects on non-DBT deposits. Appendix Table A.4 verifies that treatment effects dissipate in 2020, when the voice notification service was no longer active. This supports the hypothesis that changes in withdrawal behavior are driven by the notification effect.

Appendix Table A.5 uses linear probability models to explore where in the distribution of withdrawal time our treatment has impacts. To do this, we create a series of dummy variables indicating whether a given deposit is withdrawn within 0-5, 6-15, 16-30, 31-45, 46-60, 61-90, and 91-120 day intervals. The sample size changes across columns because we limit attention to deposits where the time between deposit date and end of the observation period is at least as long as the withdrawal window. Interestingly, the intervention had no impact on the likelihood of deposits being withdrawn very quickly. Rather, voice notifications increased the likelihood of withdrawal occurring within 31-45 days. We view this pattern as most consistent with the following two possibilities: (1) DBTs are not well timed with acute liquidity needs; (2) the cost of traveling to the kiosk varies over time, but is often significant. The latter point is in line with the fact that trips to the bank take a significant amount of time for the average woman (Appendix Table A.1). These observations are in line with women's reported DBT withdrawal practices: in both the treatment and the control group, 19 percent of women report withdrawing a DBT as soon as it is deposited, while 79 percent report withdrawing when they needed funds (only 2 percent report saving their transfers).

Table 3 asks whether the intervention significantly impacted broader measures of account use, including the average daily balance, the number and value of deposits, and the number and value

[18] Pre-intervention deposits are censored if a withdrawal did not occur before the start of intervention enrollment; post-intervention deposits are censored if a withdrawal did not occur before the last day of bank administrate data.

of withdrawals. Here we focus on outcomes while the voice notification service was active (May 2018 to March 2019). We use administrative data at the account×month level, topcoding values measured in rupees at the 99th percentile and augmenting equations 1 and 2 to include month fixed effects. The specifications used in Panel A control for the pre-baseline average monthly value of the outcome of interest. The voice notification service had no significant impact on any of these outcomes; in general, confidence intervals in Panel A are such that we can rule out effects larger than 10 percent of the dependent variable mean (for example the confidence interval for the average daily balance rules out positive and negative effects equal to 7 and 9 percent of the dependent variable mean respectively); thus our null results do not simply reflect precision issues.

To understand whether these aggregate effects mask trends in treatment effects over time, Figure 2 graphs the average balance for the treatment and control group by month. Here we include additional, longer-run data that covers January to June 2020 to understand whether short-term gains in trust lead to longer-term shifts in savings behavior. Overall, there are no substantive time trends in treatment effects. Appendix Table A.6 confirms a lack of impacts in 2020 across all administrative measures of account use.

4.4 Heterogeneity by Trust and Transaction Costs

Overall, the voice notification system had limited impacts on account use despite significantly increasing knowledge of account balances and trust in banking kiosks. Our conceptual framework highlights why this might be: just as account information may encourage some women to use their accounts more (e.g., for savings), others may act on the information to withdraw government benefits sooner. Seen through this lens, our results suggest that most women in our sample were not "trust constrained" – meaning concerns about account security were not pervasive or large enough to meaningfully affect account use in the overall sample. One way to test this hypothesis is to examine heterogeneous treatment effects with respect to baseline trust, assuming that women with lower levels of trust at the outset of the experiment were more likely to be trust constrained. Another way to study this is to focus on transaction costs – trust constraints may be particularly binding when transaction costs associated with visiting the kiosk are large.[19]

[19] We stratified our randomization on trust because we hypothesized that this might be an important dimension of heterogeneity. Our AEA RCT registry entry notes our planned focus on measures of account activity and our intent to examine heterogeneity with respect to our trust stratum. The entry is available at `https://www.socialscienceregistry.org/trials/2917`. We did not pre-specify studying heterogeneity with respect to

Table 4 studies heterogeneity in impacts on overall account use with respect to baseline trust (Panel A) and travel time to the kiosk (Panel B). We modify equation 1 to include a dummy variable for heterogeneity (either above average baseline trust in saving at the kiosk, or more than 60 minutes travel time to the kiosk) and an interaction between heterogeneity and treatment. We drop other strata controls so the coefficient on high trust is easily interpreted; this decision has no substantive effect on our results. Overall, treatment had no differential effects on account use by baseline trust; nor do we observe significant heterogeneity in effects on knowledge and endline trust (Appendix Table A.7) or withdrawal behavior (Appendix Table A.8).

We do, however, observe significant differences in impacts on account use with respect to travel time to the kiosk. Treatment effects on the number and value of non-DBT deposits, as well as the value of withdrawals, are positive and statistically significant for the 23 percent of women who report it takes more than 60 minutes to travel to the kiosk. We also reject equality of these treatment effects across groups. The coefficient on η_3 shows that far-off women were significantly less likely to use their accounts for non-DBT purposes – the effect of the information service was large enough to effectively close the use gap between far-off women and their more proximate peers. Appendix Tables A.7 and A.8 point to no significant differences in effects on knowledge, trust, and benefits withdrawal. Thus, for far off women, the voice notification service increased the value of using the kiosk for personal purposes even absent differential effects on knowledge and trust. This suggests trust constraints may be particularly binding when the cost of visiting the kiosk in person is high.

5 Conclusion

We ask whether an intervention that increases the transparency of banking by providing low-literacy women regular voice updates on their account balances, transactions, and government benefits payments can increase knowledge, trust, and engagement with the banking system. The voice notification service was popular and, over the course of a year, successfully increased self-reported knowledge of account balances as well as trust in local bank kiosks. These gains in knowledge and trust do not, however, translate into increased account balances. Rather, women

transaction costs, and therefore interpret those findings as more exploratory. Also note that travel time was measured at endline, rather than baseline, but there are no significant differences across treatment and control in terms of reported travel time and household relocation in response to treatment is very unlikely.

offered voice notifications withdraw government benefits deposits faster than women in the control group. We see no such effect on non-benefit deposits, consistent with the hypothesis that voice notifications help women identify benefits deposits sooner, which allows them to better optimize withdrawals. We also find that transaction costs of traveling to the kiosk moderate the intervention's impact in important ways. While we find similar impacts on knowledge, trust, and benefits withdrawals for women who live more versus less than an hour's walk from the kiosk, far-off women who receive voice notifications make significantly more personal deposits than their control group peers.

Our results are informative for current financial inclusion policy dialogues, especially the argument that delivering government benefits payments via bank or mobile money transfer offers a promising way to mainstream economically marginalized citizens into the formal banking system (Baur-Yazbeck et al., 2019). India has been a global leader in this respect, which has undoubtedly catalyzed basic levels of engagement and conferred other important benefits. Our results highlight that sometimes, the policy goal should be to *accelerate* withdrawals: insofar as government benefits are intended to support immediate consumption needs, timely withdrawal is indicative of effective program targeting and delivery. Our findings also indicate that increasing transparency around government benefits payments and banking transactions may help lay a foundation of knowledge and trust that could facilitate deeper account engagement over the longer term, especially among those who otherwise have difficulty monitoring their accounts directly.

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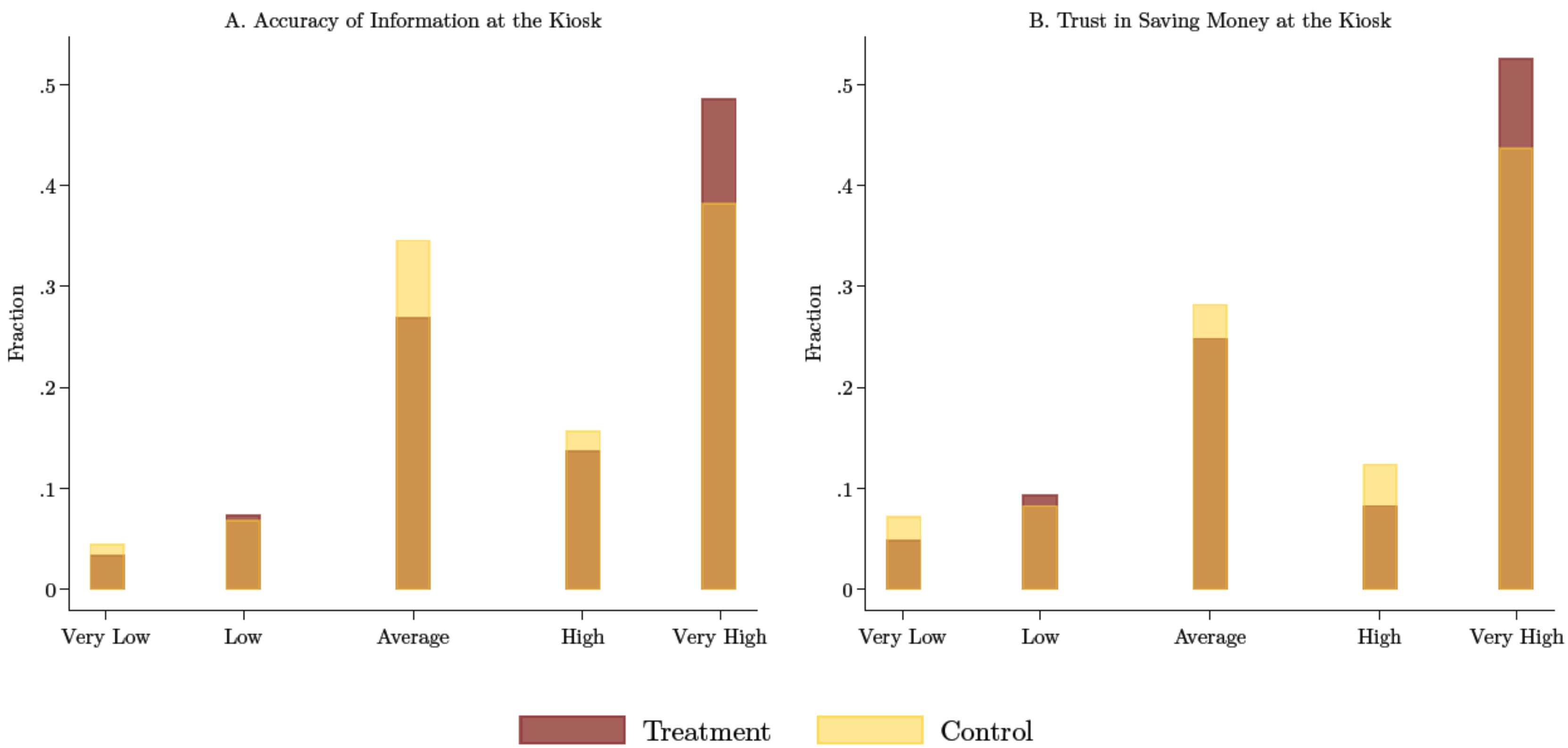
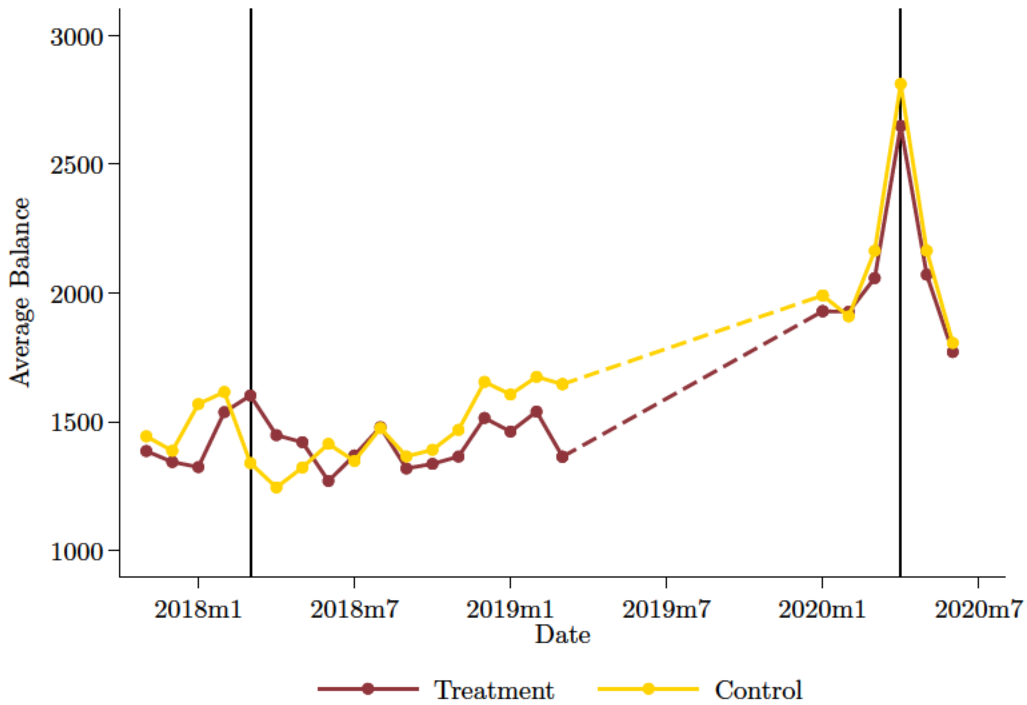
6 Tables and Figures

Figure 1: Impact of Intervention on Trust in the Banking Kiosk



Notes: sample limited to endline data. The sample size for Panel A is N=755. The sample size for Panel B is N=772.

Figure 2: Account Balances Over Time



Note: The first black vertical line denotes start of treatment. The second black vertical line denotes India's nationwide Covid-19 lockdown.

Table 1: Impacts on Account Knowledge and Kiosk Trust

	(1) Balance Information from Bank/Kiosk	(2) Balance Information from Phone Call	(3) Reports Knowing Balance: Exact or Approximate	(4) Reports Knowing Balance: Exact	(5) Accuracy of Information at the Kiosk+	(6) Trust in Saving at the Kiosk+
Panel A: Post Period Only						
β_1: Treatment	-0.068**	0.161***	0.041*	0.061**	1.431**	1.332**
	(0.032)	(0.019)	(0.023)	(0.030)	(0.201)	(0.183)
Control Mean	0.722	0.008	0.848	0.722	3.766	3.770
N	791	791	791	791	743	765
Panel B: Differences-in-Differences						
γ_1: Treatment $\times$ Post	-0.082*	0.162***	-0.006	0.055	1.504**	1.459*
	(0.043)	(0.019)	(0.039)	(0.042)	(0.288)	(0.320)
γ_2: Treatment	0.015	-0.000	0.055	0.007	0.986	0.982
	(0.034)	(0.002)	(0.034)	(0.034)	(0.122)	(0.089)
γ_3: Post	0.276***	0.008*	0.357***	0.281***	1.543***	1.763***
	(0.030)	(0.004)	(0.028)	(0.030)	(0.210)	(0.275)
Control Mean (Pre Period)	0.446	0.000	0.491	0.441	3.490	3.481
N	1582	1582	1582	1582	1533	1555

Notes: Heteroskedasticity robust standard errors in parentheses, clustered at the individual level when relevant. All regressions control for randomization strata fixed effects. Specifications in panel A control for the baseline value of the outcome (dummied out for ordered logit specifications).

+ Outcome is a 1-5 likert scale ranging from 1 (very low) to 5 (very high). We used ordered logit regressions to estimate the treatment effect on the likelihood of assigning a higher value of trust. Ordered logit results are reported in odds ratios.

* p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table 2: Impact on Time Between Deposit and Next Withdrawal

	Linear Probability Model		Hazard Model	
	DBT Deposits (1)	Non-DBT Deposits (2)	DBT Deposits (3)	Non-DBT Deposits (4)
Panel A: Post Period Only				
β_1: Treatment	0.034**	-0.007	1.078	1.008
	(0.014)	(0.019)	(0.054)	(0.054)
Control Mean (Post Period)	0.847	0.696	31.242	29.587
N	5909	5017	5902	5013
Panel B: Differences-in-Differences				
γ_1: Treatment × Post	0.054*	0.005	1.160**	0.975
	(0.029)	(0.031)	(0.081)	(0.075)
γ_2: Treatment	-0.025	-0.005	0.928	1.031
	(0.028)	(0.028)	(0.069)	(0.075)
γ_3: Post	-0.018	0.012	1.031	1.155***
	(0.021)	(0.022)	(0.048)	(0.063)
Control Mean (Pre Period)	0.734	0.629	20.871	23.635
N	8559	6573	8552	6569

Notes: Unit of observation is the deposit day. If a DBT and non-DBT deposit occur on the same day, we classify the observation as a non-DBT deposit day. Post-period data covers May 2018 to March 2019. We drop anticipation/partial treatment months of March and April 2018. For linear probability models, the outcome is a dummy equal to one if a withdrawal occurred after the deposit and during the observation period (either the pre-treatment period or the post-treatment period). In panel A linear probability models control for the pre-period mean of the outcome across all deposits. Respondents that had no non-missing pre-baseline values of the outomce are separately dummied out. Cox proportional hazard regressions model the hazard of a withdrawal occurring, with censoring at the end of the observation period. All specifications drop deposits that occurred during the intervention enrollment period. Robust standard errors, clustered at the individual level in parentheses. OLS linear probability models include strata dummies, Cox regressions explicity account for strata. Linear probability models additionally control for the number of days until the end of the observation period (the start of the baseline for the pre-period, and the end of the administrative data for the post period). Cox coefficients are converted into hazard ratios; values greater than 1 indicate a shorter time to withdrawal. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table 3: Impacts on Bank Account Use

	(1) Average Daily Balance	(2) Number Non-DBT Deposits	(3) Number Deposits	(4) Non-DBT Deposit Amount	(5) Deposit Amount	(6) Number Withdrawals	(7) Withdrawal Amount
Panel A: Post Period Only							
β_1: Treatment	-11.952	-0.013	-0.043	-5.957	-24.032	-0.044	7.907
	(60.621)	(0.022)	(0.029)	(45.895)	(54.486)	(0.029)	(58.869)
Control Mean	1487.572	0.588	1.304	901.338	1389.982	0.659	1353.903
N	9570	9570	9570	9570	9570	9570	9570
Panel B: Differences-in-Differences							
γ_1: Treatment $\times$ Post	21.702	-0.144	-0.173	-116.667	-82.569	-0.087	20.567
	(133.357)	(0.114)	(0.138)	(118.844)	(144.573)	(0.081)	(148.290)
γ_2: Treatment	-108.048	0.143	0.127	54.165	16.550	0.055	-48.463
	(144.128)	(0.125)	(0.154)	(115.332)	(138.339)	(0.079)	(143.278)
γ_3: Post	-82.871	0.041	-0.641***	198.148*	-303.201**	-0.209***	-428.088***
	(127.040)	(0.091)	(0.101)	(110.878)	(129.210)	(0.061)	(143.883)
Control Mean (Pre Period)	1503.548	0.508	1.416	555.695	1276.482	0.597	1290.802
N	13050	13050	13050	13050	13050	13050	13050

Notes: Heteroskedasticity robust standard errors in parentheses, clustered at the individual level when relevant. All regressions control for randomization strata fixed effects. Post-period data covers May 2018 to March 2019. We drop anticipation/partial treatment months of March and April 2018. Regressions are at the account-month level and also include month fixed effects. Outcomes measured in rupees are winsorized at the 99th percentile by month. Regressions in Panel A control for pre-baseline average monthly value of the outcome. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table 4: Heterogeneity in Impacts on Account Use

	(1) Average Daily Balance	(2) Number Non-DBT Deposits	(3) Number Deposits	(4) Non-DBT Deposit Amount	(5) Deposit Amount	(6) Number Withdrawals	(7) Withdrawal Amount
Panel A: Heterogeneity with Respect to Baseline Trust							
η_1: Treatment × High Baseline Trust	-58.971	-0.047	-0.005	17.417	6.170	-0.025	-63.635
	(121.681)	(0.041)	(0.058)	(95.550)	(114.559)	(0.042)	(124.205)
η_2: Treatment	16.556	0.009	-0.041	-15.187	-28.077	-0.032	37.011
	(82.547)	(0.027)	(0.039)	(52.476)	(64.278)	(0.032)	(66.602)
η_3: High Baseline Trust	85.785	0.063*	0.035	12.490	56.105	0.072**	100.247
	(85.081)	(0.033)	(0.041)	(62.638)	(73.078)	(0.036)	(77.247)
P-value: $\eta_1 + \eta_2 = 0$	0.640	0.260	0.277	0.978	0.815	0.156	0.797
Control Mean (High Baseline Trust=0)	1406.706	0.527	1.276	647.187	1120.290	0.565	1060.312
N	9570	9570	9570	9570	9570	9570	9570
Panel B: Heterogeneity with Respect to Distance from the Kiosk							
η_1: Treatment × Kiosk > 60 Minutes Away	225.694	0.141***	0.084	312.704***	313.507**	0.064	288.221**
	(143.742)	(0.045)	(0.063)	(109.679)	(126.839)	(0.054)	(130.137)
η_2: Treatment	-82.470	-0.051*	-0.068*	-108.975*	-139.404**	-0.075**	-109.776
	(77.100)	(0.030)	(0.038)	(57.902)	(69.172)	(0.036)	(75.909)
η_3: Kiosk > 60 Minutes Away	-164.221	-0.077**	-0.042	-261.965***	-206.110**	-0.068**	-205.472**
	(100.844)	(0.032)	(0.046)	(80.492)	(92.959)	(0.031)	(92.212)
P-value: $\eta_1 + \eta_2 = 0$	0.240	0.005***	0.737	0.029**	0.100	0.814	0.091*
Control Mean (Kiosk > 60 Minutes Away=0)	1572.771	0.625	1.346	1057.849	1540.628	0.722	1503.060
N	8624	8624	8624	8624	8624	8624	8624

Notes: Heteroskedasticity robust standard errors in parentheses. Regressions are at the account-month level and also include month fixed effects. Data covers May 2018 to March 2019. We drop anticipation/partial treatment months of March and April 2018. Outcomes measured in rupees are winsorized at the 99th percentile by month. Regressions in Panel A control for pre-baseline average monthly value of the outcome. High baseline trust identifies women who had high or very high trust in saving money at the kiosk at baseline. Kiosk > 60 minutes away identifies women who (at endline) report the nearest banking kiosk is more than a 60 minute walk away. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

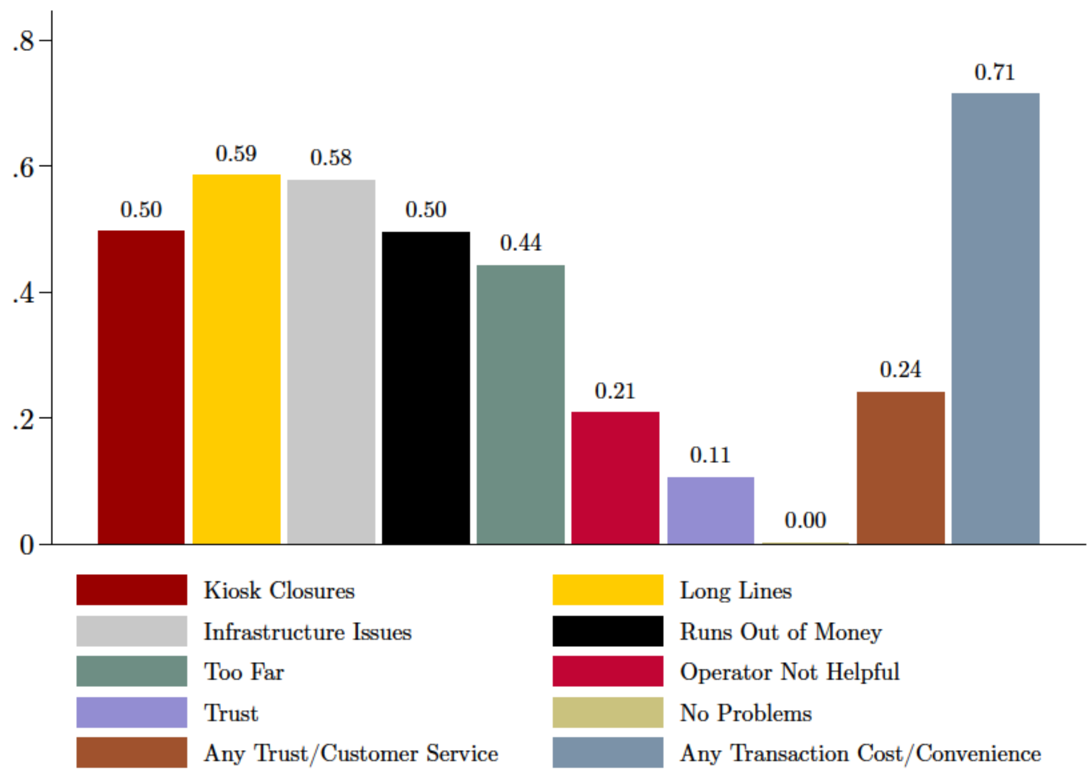
Table 5: Heterogeneity in Impacts on Account Use – Long Run Impacts

	(1) Average Daily Balance	(2) Number Non-DBT Deposits	(3) Number Deposits	(4) Non-DBT Deposit Amount	(5) Deposit Amount	(6) Number Withdrawals	(7) Withdrawal Amount
Panel A: Heterogeneity with Respect to Baseline Trust							
η_1: Treatment × High Baseline Trust	-14.286	0.013	-0.030	-201.032	-160.102	0.005	-55.801
	(264.463)	(0.122)	(0.132)	(202.866)	(228.393)	(0.088)	(248.213)
η_2: Treatment	63.647	-0.057	0.005	47.545	91.754	0.048	28.709
	(177.365)	(0.072)	(0.085)	(107.229)	(128.179)	(0.065)	(133.924)
η_3: High Baseline Trust	-154.262	-0.065	-0.009	58.427	10.339	-0.098	-19.150
	(195.910)	(0.083)	(0.091)	(125.320)	(139.292)	(0.064)	(151.989)
P-value: $\eta_1 + \eta_2 = 0$	0.801	0.634	0.807	0.361	0.710	0.471	0.893
Control Mean (High Baseline Trust=0)	2150.559	1.590	2.028	1209.196	1617.974	0.984	1635.090
N	5130	5220	5220	5220	5220	5220	5220
Panel B: Heterogeneity with Respect to Distance from the Kiosk							
η_1: Treatment × Kiosk > 60 Minutes Away	-502.780*	-0.033	0.024	-472.808**	-577.606**	-0.099	-555.033**
	(296.612)	(0.132)	(0.149)	(203.538)	(226.745)	(0.117)	(235.311)
η_2: Treatment	194.986	-0.051	-0.028	93.768	171.529	0.087	167.934
	(169.837)	(0.075)	(0.085)	(125.666)	(141.822)	(0.068)	(150.730)
η_3: Kiosk > 60 Minutes Away	420.144*	0.090	0.007	373.557***	340.985**	0.142**	387.284**
	(228.573)	(0.090)	(0.104)	(130.506)	(151.041)	(0.060)	(156.800)
P-value: $\eta_1 + \eta_2 = 0$	0.203	0.412	0.971	0.018**	0.021**	0.907	0.031**
Control Mean (Kiosk > 60 Minutes Away=0)	2117.649	1.634	2.096	1435.592	1838.053	1.051	1834.470
N	4626	4704	4704	4704	4704	4704	4704

Notes: Heteroskedasticity robust standard errors in parentheses. Regressions are at the account-month level and also include month fixed effects. Data covers January to June 2020. Outcomes measured in rupees are winsorized at the 99th percentile by month. Regressions in Panel A control for pre-baseline average monthly value of the outcome. High baseline trust identifies women who had high or very high trust in saving money at the kiosk at baseline. Kiosk > 60 minutes away identifies women who (at endline) report the nearest banking kiosk is more than a 60 minute walk away. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

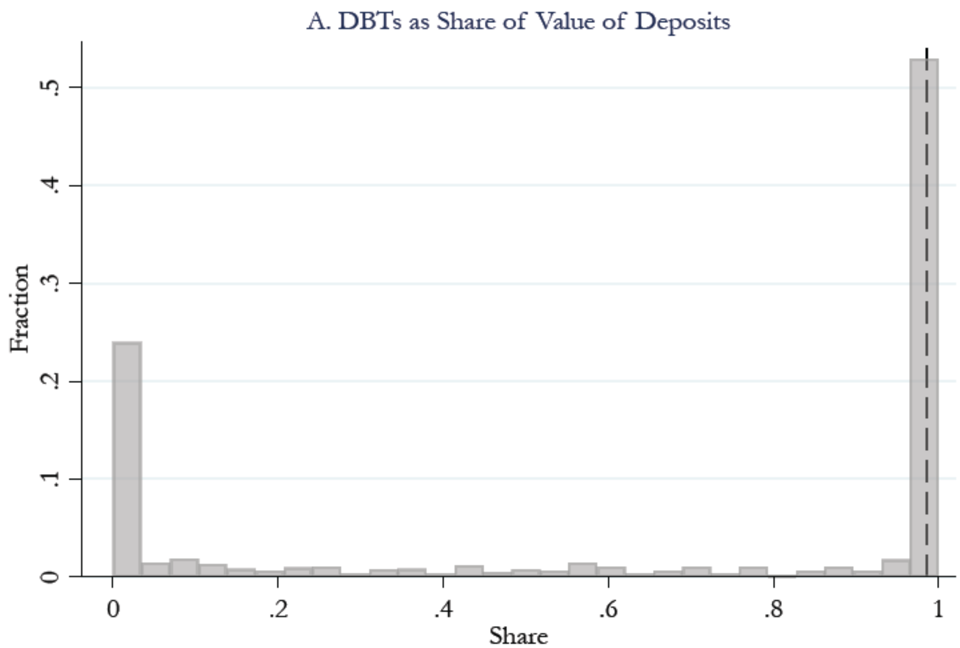
A Appendix Tables and Figures

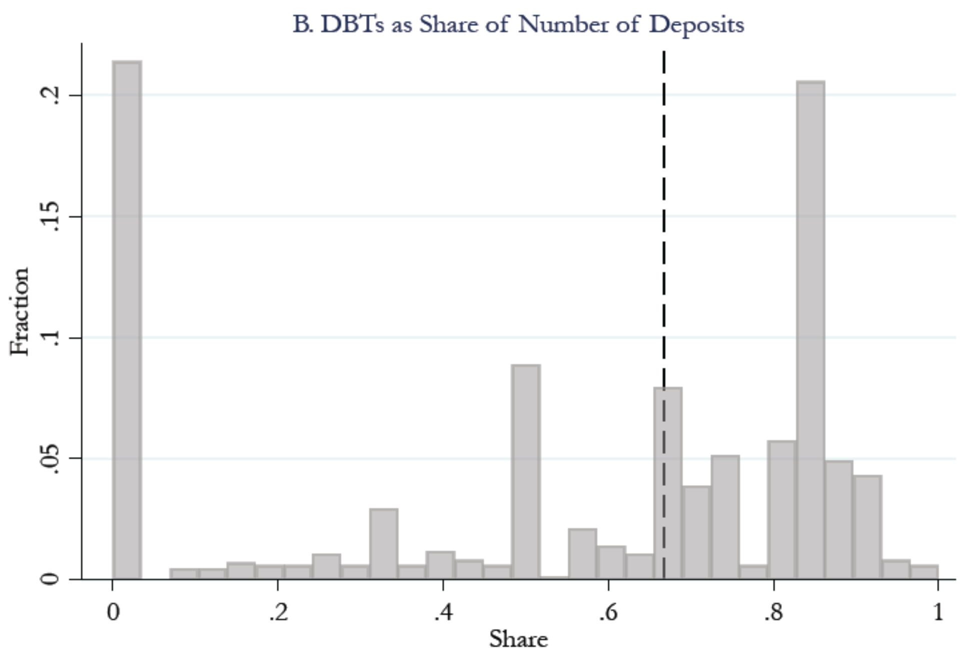
Figure A.1: Challenges in Using Kiosk Listed at Baseline



Notes: N=791.

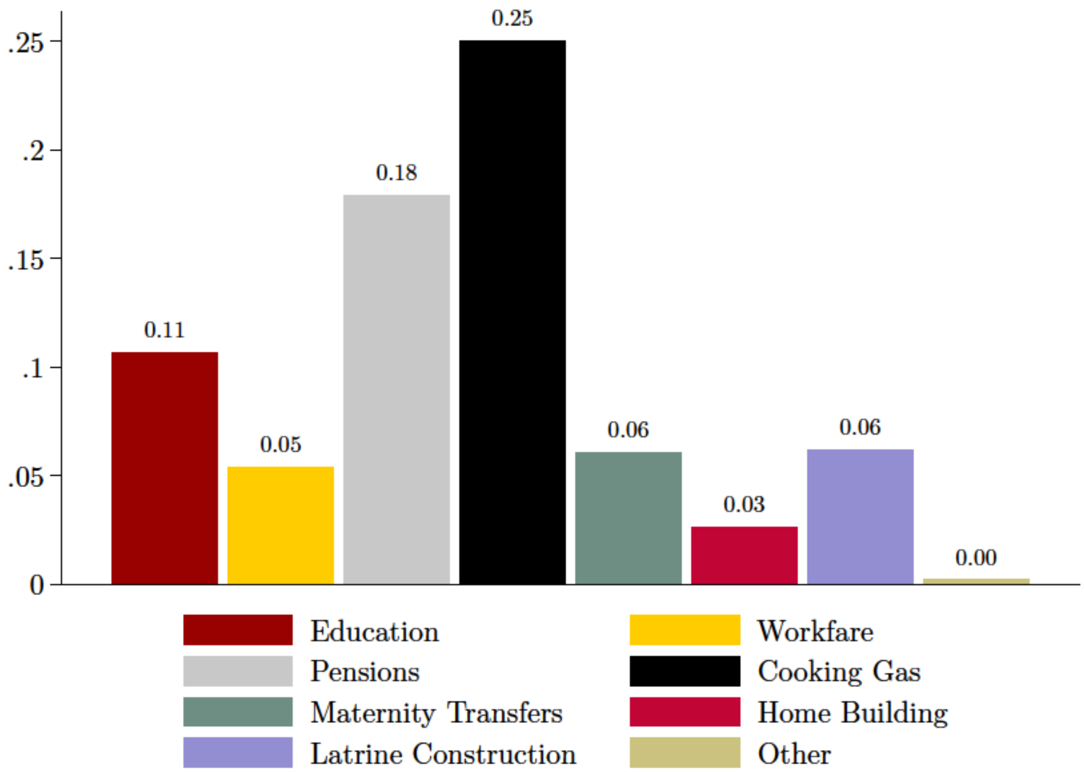
Figure A.2: Baseline Role of DBTs in Driving Account Deposits in Administrative Data





Notes: This figure uses administrative data from our banking partner. The sample is limited to deposits made into accounts in pre-baseline period, which spans November 1, 2017 to March 19, 2018. Vertical dashed lines indicate medians. N=855.

Figure A.3: Self-Reported Government Benefits Receipt in Past Year



Notes: N=791. Graph reports share of women who reported receiving benefits from specified programs into their own bank account in the past year.

Figure A.4: Timeline

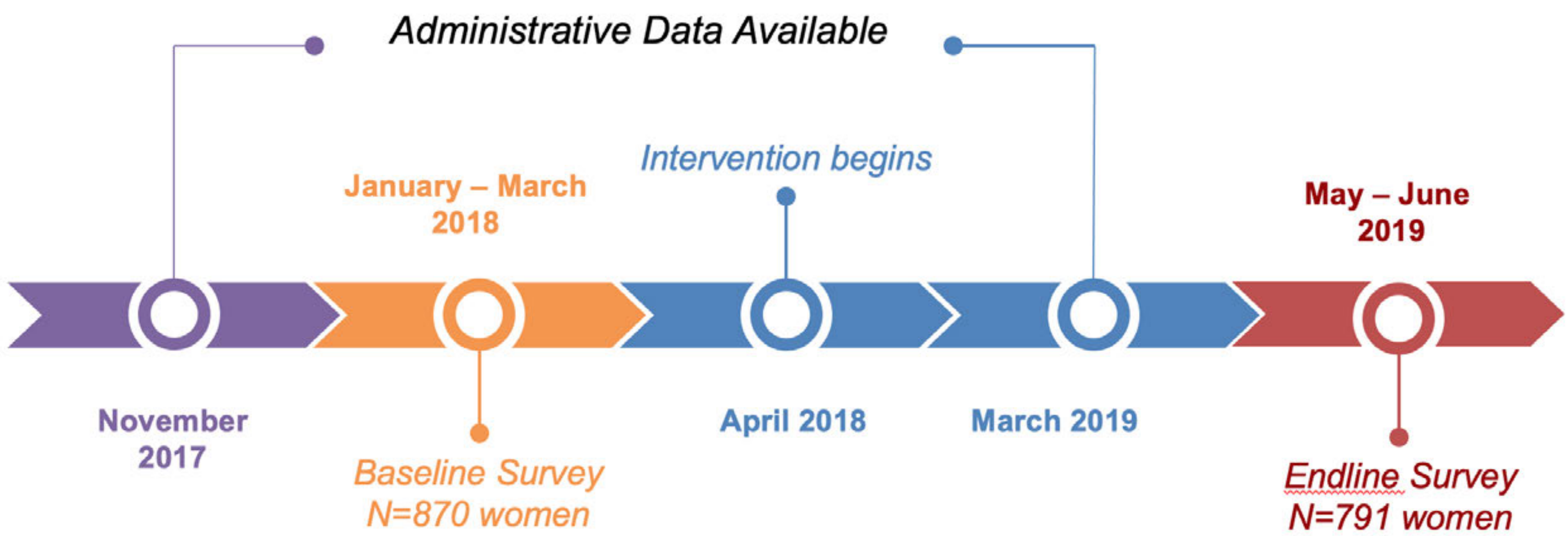
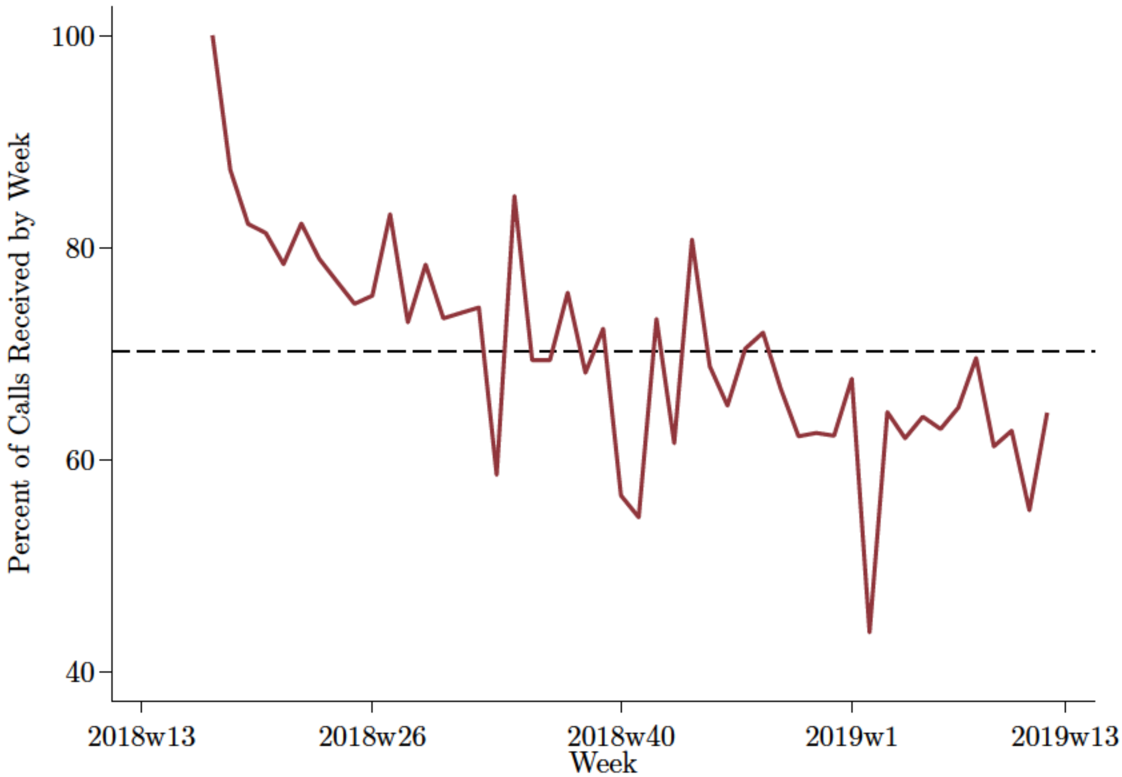
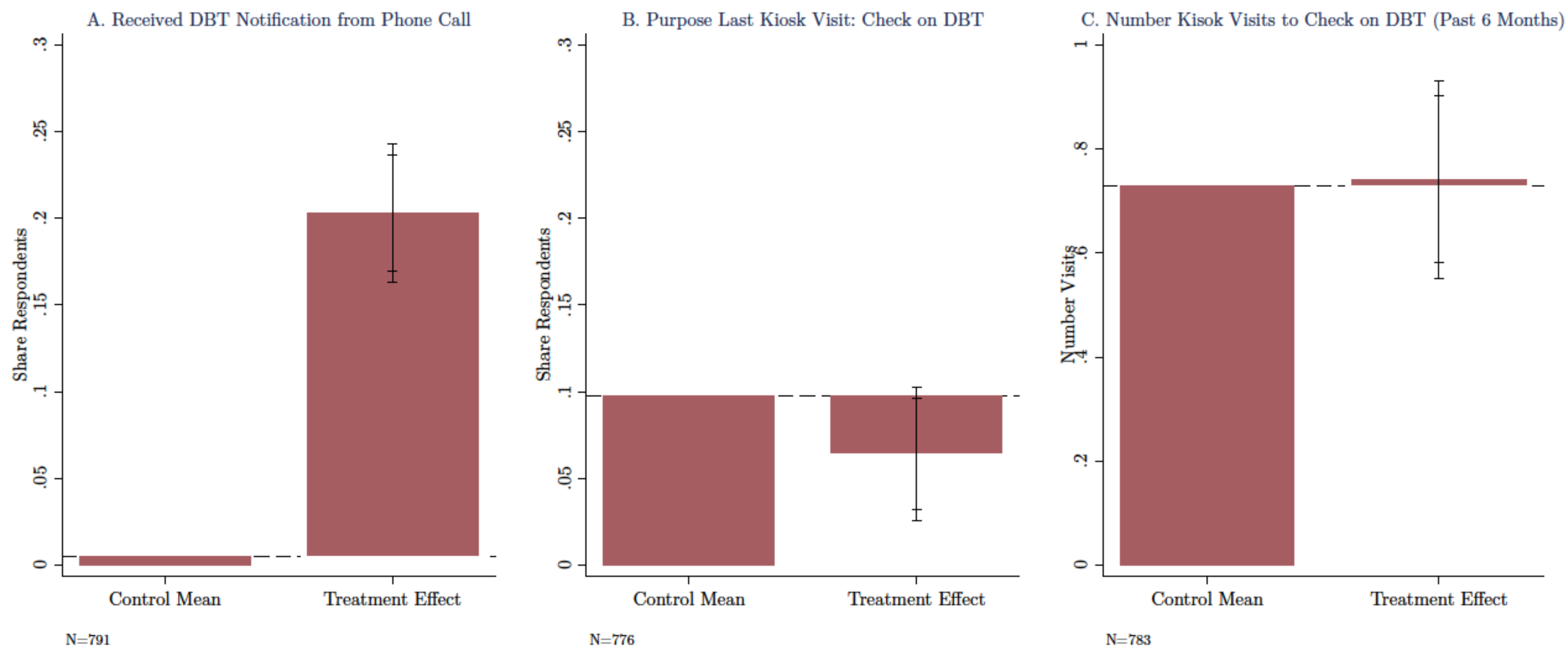


Figure A.5: Percent of Calls Received by Week



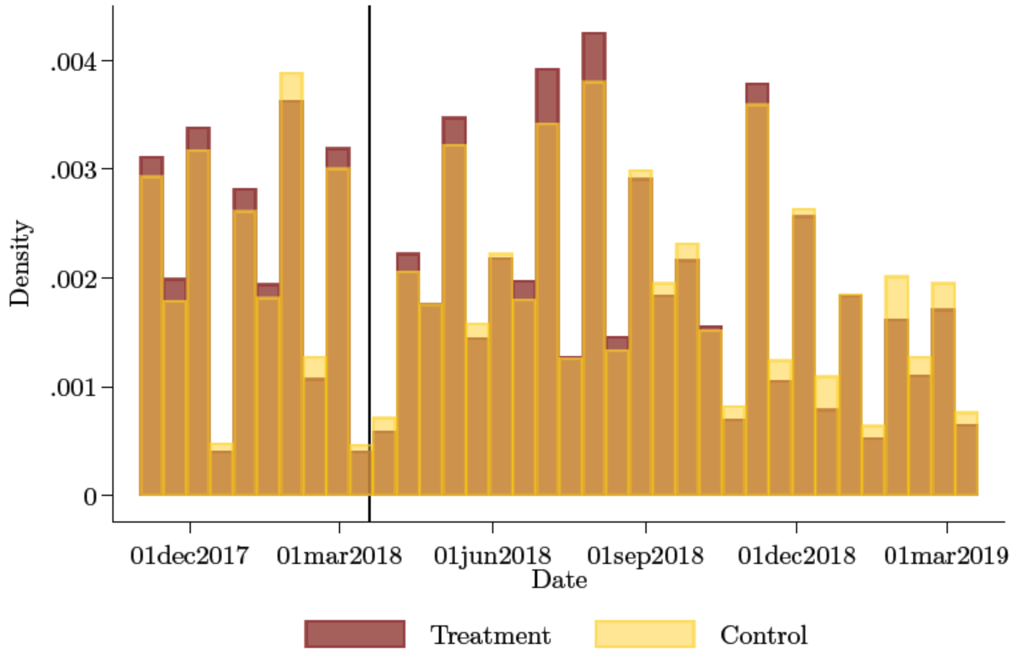
Notes: Sample includes 344 beneficiaries enrolled in the weekly calling service. Dashed horizontal line indicates the average pickup rate of 70 percent.

Figure A.6: Treatment Effects on DBT Notification and Withdrawals



Notes: Whiskers depict 90 and 95 percent confidence intervals on treatment effects, based on heteroskedasticity robust standard errors. All regressions control for baseline values of the dependent variable and strata fixed effects, except for Panel A, which omits the baseline control due to unavailability of data.

Figure A.7: Distribution of DBT Deposit Arrivals by Treatment Arm



P-values from a Kolmogorov-Smirnov test of equality of distributions are 0.57 in the pre-treatment period and 0.04 in the post-treatment period.

Note: Black vertical line denotes start of treatment.

Table A.1: Balance: Baseline Survey

Variable	N	(1) Control Mean/SD	N	(2) Treatment Mean/SD	T-test P-value (1)-(2)
A. Attrition					
In Endline	433	1.000 (0.000)	437	1.000 (0.000)	N/A
B. Baseline Characteristics					
Age	433	36.785 (10.600)	437	37.245 (11.100)	0.532
Education: No Schooling	433	0.580 (0.494)	437	0.581 (0.494)	0.963
Literacy: Can read paragraph	433	0.155 (0.362)	437	0.178 (0.383)	0.348
Has Own Phone	433	0.351 (0.478)	437	0.352 (0.478)	0.966
Phone Ability: Read SMS Hindi	433	0.157 (0.364)	437	0.176 (0.381)	0.449
Phone Ability: Can Receive Calls	433	0.903 (0.296)	437	0.876 (0.329)	0.211
Worked for Pay Last Month	433	0.275 (0.447)	437	0.309 (0.463)	0.269
Own Earnings Last Month	433	300.346 (921.637)	436	358.904 (970.408)	0.362
No. of HH Members	433	5.406 (2.065)	437	5.410 (2.176)	0.983
Household Earnings Last Month	433	2491.069 (4217.975)	437	2909.529 (4449.416)	0.155
Knows Balance (Exact or Approximate)	433	0.490 (0.500)	437	0.540 (0.499)	0.137
Balance Info Source: Kiosk	433	0.439 (0.497)	437	0.455 (0.499)	0.623
High or Very High Trust in Kiosk	433	0.469 (0.500)	437	0.471 (0.500)	0.939
Main Reason Opened Account: Receive DBTs	429	0.674 (0.469)	436	0.624 (0.485)	0.125
Main Reason Opened Account: Save	433	0.171 (0.377)	437	0.167 (0.373)	0.880
Experience DBT Delays: Always or Sometimes	406	0.515 (0.500)	415	0.569 (0.496)	0.122
Minutes Walking to Usual Banking Point^{+}	391	57.486 (61.265)	384	56.747 (59.695)	0.865
Total Time (Minutes) Required for Bank Trip^{+}	395	177.618 (152.891)	391	174.639 (129.655)	0.768
CSP further 60 minutes or more at EL	394	0.218 (0.414)	390	0.246 (0.431)	0.356

Notes: P-values reflect heteroskedasticity robust standard errors. Regressions include no controls other than a treatment dummy. +Indicates variable was measured at the endline survey. *, **, and *** indicate statistical signifiance at the 10, 5, and 1 percent levels respectively.

Table A.2: Balance: Administrative Data

Variable	N	(1) Control Mean/SD	N	(2) Treatment Mean/SD	T-test P-value (1)-(2)
Average Daily Balance	433	1663.991 (3608.468)	437	1512.709 (2694.883)	0.484
Number DBT Deposits	433	3.843 (3.455)	437	3.776 (4.164)	0.796
DBT Deposit Amount	433	3167.215 (5354.505)	437	2941.545 (4321.737)	0.494
Number Non-DBT Deposits	433	2.127 (3.136)	437	2.908 (13.027)	0.223
Non-DBT Deposit Amount	433	2915.485 (9020.274)	437	4654.864 (28149.238)	0.219
Number Withdrawals	433	2.751 (3.113)	437	3.011 (7.460)	0.500
Withdrawal Amount	433	6399.108 (11093.841)	437	7353.111 (29060.190)	0.522
Number DBT Deposits (Post-Intervention)	433	7.896 (6.156)	437	7.396 (6.381)	0.240
DBT Deposit Amount (Post-Intervention)	433	5311.409 (7840.406)	437	5103.330 (7780.174)	0.694

Notes: P-values reflect heteroskedasticity robust standard errors. Regressions include no controls other than a treatment dummy. Unless otherwise noted, bank administrative data cover the pre-baseline survey period, from November 1, 2017 to March 19, 2018. The post-intervention period spans April 28, 2018 to March 18, 2019. *, **, and *** indicate statistical signifiance at the 10, 5, and 1 percent levels respectively.

Table A.3: Baseline Predictors of Treatment Takeup and Engagement

	Signed Up		Pickup Rate	
	(1)	(2)	(3)	(4)
Age	-0.000	0.002	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.002)
Education: No Schooling	-0.084**	-0.047	-0.004	-0.005
	(0.039)	(0.052)	(0.029)	(0.038)
Literacy: Can read paragraph	0.103**	0.013	0.018	0.024
	(0.044)	(0.083)	(0.035)	(0.062)
Has Own Phone	0.028	0.024	-0.062**	-0.058*
	(0.040)	(0.043)	(0.031)	(0.033)
Phone Ability: Read SMS Hindi	0.116***	0.083	0.009	0.008
	(0.043)	(0.081)	(0.035)	(0.062)
Phone Ability: Can Receive Calls	0.074	0.060	0.002	0.001
	(0.064)	(0.071)	(0.047)	(0.053)
Worked for Pay Last Month	0.008	-0.052	-0.009	0.006
	(0.042)	(0.054)	(0.032)	(0.041)
Own Earnings Last Month	0.000	0.000*	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
No. of HH Members	0.009	0.009	0.013**	0.011
	(0.010)	(0.011)	(0.006)	(0.007)
Household Earnings Last Month	0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Knows Balance (Exact or Approximate)	-0.007	0.030	-0.017	-0.011
	(0.039)	(0.073)	(0.029)	(0.061)
Balance Info Source: Kiosk	-0.024	-0.055	-0.015	0.003
	(0.040)	(0.072)	(0.029)	(0.063)
High or Very High Trust in Kiosk	-0.038	-0.022	-0.020	-0.016
	(0.039)	(0.040)	(0.029)	(0.030)
Main Reason Opened Account: Receive DBTs	-0.045	-0.046	0.008	0.007
	(0.040)	(0.047)	(0.029)	(0.038)
Main Reason Opened Account: Save	-0.024	-0.066	-0.001	0.007
	(0.054)	(0.063)	(0.033)	(0.044)
Experience DBT Delays: Always or Sometimes	-0.053	-0.060	0.066**	0.064**
	(0.040)	(0.042)	(0.030)	(0.032)
Minutes Walking to Usual Banking Point+	0.001**	0.001**	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Total Time (Minutes) Required for Bank Trip+	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Dependent Variable Mean	0.787	0.787	0.703	0.703
N	437	437	344	344

Notes: Heteroskedasticity robust standard errors in parentheses. Columns 1 and 3 present results of bivariate regressions, where the outcome is the variable specified by the column and the covariate is the variable specified by the row. Columns 2 and 4 present results of a single regression where all covariates are included at once. When covariates are missing, we include a dummy variable to identify missing observations and recode missing values to zero. The sample in columns 1 and 2 includes all women in the treatment group. The sample size in columns 3 and 4 includes all women enrolled in the voice notification service. +Indicates variable was measured at the endline survey.

Table A.4: Impact on Time Between Deposit and Next Withdrawal – Long Run Impacts

	Linear Probability Model		Hazard Model	
	DBT Deposits (1)	Non-DBT Deposits (2)	DBT Deposits (3)	Non-DBT Deposits (4)
Panel A: Post Period Only				
β_1: Treatment	0.004	0.002	1.029	0.985
	(0.021)	(0.014)	(0.070)	(0.040)
Control Mean (Post Period)	0.813	0.725	21.761	16.669
N	1678	7339	1639	7260
Panel B: Differences-in-Differences				
γ_1: Treatment × Post	0.041	0.009	1.127	0.978
	(0.034)	(0.033)	(0.106)	(0.079)
γ_2: Treatment	-0.032	-0.006	0.927	1.008
	(0.027)	(0.030)	(0.068)	(0.076)
γ_3: Post	0.057**	0.131***	1.381***	1.994***
	(0.024)	(0.024)	(0.093)	(0.121)
Control Mean (Pre Period)	0.734	0.629	20.871	23.635
N	4328	8895	4289	8816

Notes: Unit of observation is the deposit day. If a DBT and non-DBT deposit occur on the same day, we classify the observation as a non-DBT deposit day. Post-period data covers January to June 2020. For linear probability models, the outcome is a dummy equal to one if a withdrawal occurred after the deposit and during the observation period (either the pre-treatment period or the post-treatment period). In panel A linear probability models control for the pre-period mean of the outcome across all deposits. Respondents that had no non-missing pre-baseline values of the outomce are separately dummied out. Cox proportional hazard regressions model the hazard of a withdrawal occurring, with censoring at the end of the observation period. All specifications drop deposits that occurred during the intervention enrollment period. Robust standard errors, clustered at the individual level in parentheses. OLS linear probability models include strata dummies, Cox regressions explicity account for strata. Linear probability models additionally control for the number of days until the end of the observation period (the start of the baseline for the pre-period, and the end of the administrative data for the post period). Cox coefficients are converted into hazard ratios; values greater than 1 indicate a shorter time to withdrawal. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table A.5: Impact on Time Between DBT Deposit and Next Withdrawal by Time Horizon

	(1) Withdrawn in 0-5 Days	(2) Withdrawn in 6-15 Days	(3) Withdrawn in 16-30 Days	(4) Withdrawn in 31-45 Days	(5) Withdrawn in 46-60 Days	(6) Withdrawn in 61-90 Days	(7) Withdrawn in 91-120 Days
Panel A: Post Period Only							
β_1: Treatment	0.008	0.001	0.001	0.025***	0.007	-0.004	-0.005
	(0.013)	(0.013)	(0.011)	(0.009)	(0.008)	(0.009)	(0.006)
Control Mean	0.179	0.234	0.173	0.097	0.058	0.074	0.041
N	5875	5816	5569	5405	5166	4821	4290
Panel B: Differences-in-Differences							
γ_1: Treatment $\times$ Post	0.011	0.018	-0.002	0.043***	-0.012	0.010	-0.018
	(0.019)	(0.019)	(0.019)	(0.015)	(0.013)	(0.019)	(0.022)
γ_2: Treatment	-0.001	-0.018	0.001	-0.020	0.021*	-0.013	0.013
	(0.020)	(0.018)	(0.017)	(0.013)	(0.011)	(0.018)	(0.022)
γ_3: Post	0.002	0.007	0.008	-0.009	0.015*	-0.022	0.005
	(0.014)	(0.014)	(0.014)	(0.011)	(0.008)	(0.013)	(0.016)
Control Mean (Pre Period)	0.177	0.228	0.166	0.106	0.042	0.094	0.034
N	8525	8466	7919	7444	6845	5921	4710

Notes: Robust standard errors clustred at the individual level in parentheses. Unit of observation is the DBT deposit day. If a DBT and non-DBT deposit occur on the same day, we classify the observation as a non-DBT deposit day. Outcomes are dummy variables equal to one if a withdrawal occurred after a deposit during the specified time frame. The outcome is coded to missing if the time between the deposit date and the end of the observation period is less than the specified time frame. In panel A we control for the pre-period mean of the outcome across all deposits. Respondents that had no non-missing pre-baseline values of the outomce are separately dummied out. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table A.6: Impacts on Bank Account Use – Long Run Effects

	(1) Average Daily Balance	(2) Number Non-DBT Deposits	(3) Number Deposits	(4) Non-DBT Deposit Amount	(5) Deposit Amount	(6) Number Withdrawals	(7) Withdrawal Amount
Panel A: Post Period Only							
β_1: Treatment	56.667	-0.050	-0.009	-46.800	17.174	0.050	3.219
	(129.598)	(0.054)	(0.065)	(94.464)	(106.724)	(0.053)	(114.017)
Control Mean	2140.886	1.606	2.037	1416.095	1800.528	1.023	1811.812
N	5130	5220	5220	5220	5220	5220	5220
Panel B: Differences-in-Differences							
γ_1: Treatment × Post	28.961	-0.176	-0.139	-142.021	-30.677	0.011	23.881
	(240.970)	(0.142)	(0.169)	(158.000)	(183.373)	(0.103)	(190.870)
γ_2: Treatment	-107.250	0.144	0.127	54.585	16.827	0.055	-47.968
	(146.408)	(0.125)	(0.154)	(115.740)	(138.463)	(0.079)	(143.352)
γ_3: Post	200.134	1.405***	0.639***	768.091***	481.808**	0.010	267.197
	(181.777)	(0.112)	(0.124)	(146.579)	(188.217)	(0.069)	(187.478)
Control Mean (Pre Period)	1503.548	0.508	1.416	555.695	1276.482	0.597	1290.802
N	8610	8700	8700	8700	8700	8700	8700

Notes: Heteroskedasticity robust standard errors in parentheses, clustered at the individual level when relevant. All regressions control for randomization strata fixed effects. Post-period data covers January to June 2020. Regressions are at the account-month level and also include month fixed effects Outcomes measured in rupees are winsorized at the 99th percentile by month. Regressions in Panel A control for pre-baseline average monthly value of the outcome. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table A.7: Heterogeneity in Impacts on Account Knowledge and Kiosk Trust

	(1) Balance Information from Bank/Kiosk	(2) Balance Information from Phone Call	(3) Reports Knowing Balance: Exact or Approximate	(4) Reports Knowing Balance: Exact	(5) Accuracy of Information at the Kiosk[+]	(6) Trust in Saving at the Kiosk[+]
Panel A: Heterogeneity with Respect to Baseline Trust						
η_1: Treatment $\times$ High Baseline Trust	0.039	-0.014	0.075	0.064	1.116	1.067
	(0.065)	(0.039)	(0.047)	(0.060)	(0.313)	(0.293)
η_2: Treatment	-0.088**	0.168***	0.006	0.030	1.351	1.270
	(0.045)	(0.028)	(0.031)	(0.041)	(0.256)	(0.226)
η_3: High Baseline Trust	-0.009	-0.014*	-0.057	-0.046	1.307	1.369
	(0.044)	(0.008)	(0.036)	(0.044)	(0.280)	(0.395)
P-value: $\eta_1 + \eta_2 = 0$	0.292	0.000***	0.021**	0.034**	0.044**	0.147
Control Mean (High Baseline Trust=0)	0.729	0.014	0.876	0.748	3.665	3.673
N	791	791	791	791	743	765
Panel B: Heterogeneity with Respect to Distance from the Kiosk						
η_1: Treatment $\times$ Kiosk > 60 Minutes Away	-0.036	-0.011	-0.041	-0.027	0.757	0.991
	(0.079)	(0.044)	(0.057)	(0.074)	(0.243)	(0.310)
η_2: Treatment	-0.051	0.167***	0.059**	0.074**	1.505**	1.330*
	(0.036)	(0.023)	(0.026)	(0.034)	(0.241)	(0.210)
η_3: Kiosk > 60 Minutes Away	-0.032	-0.010*	0.006	-0.037	0.997	0.660*
	(0.055)	(0.006)	(0.044)	(0.055)	(0.221)	(0.142)
P-value: $\eta_1 + \eta_2 = 0$	0.216	0.000***	0.724	0.468	0.636	0.307
Control Mean (Kiosk > 60 Minutes Away=0)	0.734	0.010	0.851	0.734	3.762	3.835
N	784	784	784	784	740	761

Notes: Heteroskedasticity robust standard errors in parentheses.

[+] Outcome is a 1-5 likert scale ranging from 1 (very low) to 5 (very high). We used ordered logit regressions to estimate the treatment effect on the likelihood of assigning a higher value of trust. Ordered logit results are reported in odds ratios.

High baseline trust identifies women who had high or very high trust in saving money at the kiosk at baseline. Kiosk > 60 minutes away identifies women who (at endline) report the nearest banking kiosk is more than a 60 minute walk away. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.01.

Table A.8: Heterogeneity in Impacts on Time Between Deposit and Next Withdrawal

	Linear Probability Model		Hazard Model	
	DBT Deposits (1)	Non-DBT Deposits (2)	DBT Deposits (3)	Non-DBT Deposits (4)
Panel A: Heterogeneity with Respect to Baseline Trust				
η_1: Treatment × High Baseline Trust	0.010	0.025	1.037	1.063
	(0.028)	(0.040)	(0.109)	(0.127)
η_2: Treatment	0.031*	-0.019	1.073	0.987
	(0.017)	(0.029)	(0.066)	(0.079)
η_3: High Baseline Trust	-0.042**	-0.023	0.943	0.980
	(0.021)	(0.027)	(0.075)	(0.089)
P-value: $\eta_1 + \eta_2 = 0$	0.069*	0.838	0.208	0.585
Control Mean (High Baseline Trust=0)	0.866	0.714	32.378	32.602
N	5909	5017	5902	5013
Panel B: Heterogeneity with Respect to Distance from the Kiosk				
η_1: Treatment × Kiosk > 60 Minutes Away	0.045	-0.013	1.079	0.991
	(0.035)	(0.046)	(0.137)	(0.133)
η_2: Treatment	0.022	0.003	1.056	1.031
	(0.016)	(0.024)	(0.065)	(0.077)
η_3: Kiosk > 60 Minutes Away	-0.009	0.016	0.937	1.022
	(0.030)	(0.034)	(0.097)	(0.105)
P-value: $\eta_1 + \eta_2 = 0$	0.032**	0.806	0.244	0.845
Control Mean (High Baseline Trust=0)	0.856	0.695	30.799	28.704
N	5367	4582	5361	4578

Notes: Unit of observation is the deposit day. If a DBT and non-DBT deposit occur on the same day, we classify the observation as a non-DBT deposit day. Post-period data covers May 2018 to March 2019. We drop anticipation/partial treatment months of March and April 2018. For linear probability models, the outcome is a dummy equal to one if a withdrawal occurred after the deposit and during the observation period (either the pre-treatment period or the post-treatment period). Cox proportional hazard regressions model the hazard of a withdrawal occurring, with censoring at the end of the observation period. All specifications drop deposits that occurred during the intervention enrollment period. Robust standard errors, clustered at the individual level in parentheses. Cox coefficients are converted into hazard ratios; values greater than 1 indicate a shorter time to withdrawal. Linear probability models control for the average value of the outcome in the pre-interention period. This is coded to zero for respondents with no deposits in the pre-intervention period and separately dummied out. Linear probability models additionally control for the number of days until the end of the observation period (the start of the baseline for the pre-period, and the end of the administrative data for the post period). High baseline trust identifies women who had high or very high trust in saving money at the kiosk at baseline. Kiosk > 60 minutes away identifies women who (at endline) report the nearest banking kiosk is more than a 60 minute walk away. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

B Data Appendix

This appendix provides detail on the datasets used in our analysis and how we constructed outcome variables. We also describe how we inferred DBT deposits based on amount and patterns in the bank administrative data.

B.1 Survey Data

We collected original baseline and endline survey data for the purposes of this study. The baseline was administered to 870 women during February and March 2018. The endline took place a little over a year later, during May and June, covering 791 women. Both surveys collected detailed data on women's attitudes towards and use of banks and banking kiosks. We also administered a phone use module to better understand how women use mobile phones.

We construct parallel versions of baseline and endline variables to facilitate differences-in-differences analysis. Key variables based on survey data include the following:

- **Source of balance information** At both baseline and endline, we asked individuals how much money was in their bank account and what the source of this information was. At baseline this question was only asked to women who reported they knew their balance; at endline, enumerators also asked women who did not know their balance to report where they usual get this information. We use these questions to construct two dummy variables: *Balance information from bank/kiosk* is equal to one for all women who report getting balance information from the bank branch or banking kiosk, or from getting their passbook filled. *Balance information from phone call* is equal to one for all women who report getting information from a phone call, or specifically name the voice notification service. At baseline, these variables are coded to zero for women who did not know their balances. At endline, we use direct reports on information source.

- **Knows bank balance** In both survey rounds we asked women whether they knew their account balance – either exactly, approximately, or not at all. We use this variable to create dummy variables for balance knowledge.

- **Measures of trust** We asked individuals to report the amount of trust they had in saving money at the bank kiosk and the accuracy of information at the bank kiosk. Responses were coded from 1 (very low) to 5 (very high) with 3 indicating average.

- **Problems at the banking kiosk** In both survey rounds we asked women if they had encountered any problems operating their account at the banking kiosk. Enumerators read out a list of options and marked items the respondent reported she had encountered.

B.2 Administrative Data

We worked with our banking partner to identify female-owned accounts that had received government transfers in the past. Once we enrolled women into the study, we obtained their consent for the bank to share administrative transaction records associated with their account. The bank shared transaction records spanning November 2017 to March 2019. The records include the amount and date of the transaction, as well as the balance at the end of the transaction. The bank shared a separate set of files that included amounts and dates of all DBT transactions from November 2017 to September 2018. This file also contained partial information on the type of DBT (LPG subsidy, workfare payment, or other). We use these data to create our main administrative outcomes of interest.

We use rules of thumb to identify DBT deposits after September 2018. The three rules are as follows:

- We classify all deposits that are multiples of MP's workfare wage (Rs 190, 176, 174, 172, 167, 159, 157, and 146) as workfare deposits.
- We classify all deposits greater than Rs 50 that include fractions of a rupee (called paisa) as DBTs based on the observation that LPG deposits often include paisa and very few non-DBT deposits include paisa.
- A share of women in our sample receive either old age or widow pensions, which provide monthly stipends of Rs 300 or Rs 1,000. We classify women as "pensioners" if they receive DBTs in either of these amounts prior to September 2018. We then classify all deposits of Rs 300 or Rs 1,000 sent to pensioners as DBT deposits in the post-September 2018 period.

When we apply these three rules to the pre-September 2018 data, we correctly identify 96 percent of DBT deposits, while mis-classifying 8 percent of non-DBT deposits as DBTs. To maintain continuity in classification we apply these rules to both the pre- and post-September 2018 transactions data.

B.3 Voice Notification Service

Finally, we have administrative data from the voice notification service provider, which includes the type of message, as well as the date and outcome of each attempt.

We operationalized the voice notification service by designing and pre-recording update templates, which were then populated with transaction and/or balance amounts. We reproduce examples of those templates below:

- *Namaste [NAME]. On [DATE] a total of [AMOUNT1] rupees [AMOUNT2] paise got deposited into your [PARTNER BANK] account. After these transactions, you have [BALANCE1] rupees [BALANCE2] paise in your account. Thank you.*

- *Namaste [NAME]. On [DATE] total of [AMOUNT1] rupees [AMOUNT2] paise was withdrawn from your [PARTNER BANK] account over [NUMBER TRANSACTIONS] transactions. After these transactions, you have [BALANCE1] rupees [BALANCE2] paise in your account. Thank you.*

- *Namaste [NAME]. There were no transactions on your [PARTNER BANK] account between [DATE1] and [DATE2]. Your current balance is [BALANCE1] rupees [BALANCE2] paise. Thank you.*