Show or Tell? Improving Inventory Support for Agent-Based Businesses at the Base of the Pyramid

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Problem definition: Firms providing products and services to low income Base of the Pyramid (BOP) customers are increasingly utilizing independent contractor *agents* rather than employees in their distribution models. We empirically investigate the best way to help agents perform better. **Academic/Practical Relevance:** BOP customers represent one-third of the world's economy, but make 5 USD or less daily. Providing goods and services to these customers is difficult for traditional firms because most retail activity occurs at small-scale independent outlets. Improving agent performance can help firms reach customers in this environment. We enhance the literature on agent-based models in BOP settings, decision making, technology in developing economies, and field experiments. **Methodology:** In partnership with a Tanzanian mobile money operator, we perform a randomized controlled trial with 4,771 agents to examine how differing types of guidance, and whether in-person training is offered, impact agents' inventory management. Mobile money is a platform whereby firms in developing economies provide financial services to customers via cell phones. Mobile money agents service customer withdrawals and deposits as branchless banking outlets. Every day, they decide how much money to stock to service customers' transactions, from which they earn commissions. **Results:** We find that those agents given only explicit recommendations (as opposed to summary statistics or both) who were invited to in-person training (as opposed to simply received an automated notification) improve their performance. Agents in other treatments showed no statistically significant change. The effect is concentrated in agents who never replenished their money at a bank, and whose money inventory levels were low in the pre-treatment period. **Managerial Implications:** We show empirically how firms can better manage agents, thereby improving the value proposition of serving BOP customers. We show the utility of segmentation based on agent heterogeneity. This can improve firm performance, agent profits, and customer service.

1. Introduction

There are four billion people at what is known as the Base of the Pyramid (BOP) whose spending represents one-third of the world's economy, but who make 5 USD or less per day (Rangan et al. 2011). It is difficult for traditional firms to offer products and services to BOP customers (Rangan et al. 2011), in part because the retail landscape is comprised of small-scale and individual entrepreneurs. For example, more than 90% of retail sales in India occur at 12 million small, independent stores such as kirana shops, pushcarts, and street vendors (Kohli and Bhagwati 2011). For such outlets, there is no central office with which a firm can make a single distribution deal; instead, firms must rely on other distribution means for BOP consumers.

One model that many firms operating at the BOP rely on is the utilization of 'agents'—entities (often individuals) that help to distribute products and services on behalf of the firm. The agent is not a direct employee, but rather is incentivized to perform well through an agreement. The agent-based model has been successful in many contexts, such as solar lamp distribution (Guajardo 2019), demand generation and last mile delivery (Iyer and Palsule-Desai 2019), and product education and handling of returns (Calmon et al. 2018). However, there are still challenges. Managing agents who are contractors is not the same as managing employees due to the autonomy agents typically enjoy. Specifically, firms may wonder what kind of guidance is best to give agents to help them improve performance for themselves and the firm? We examine empirically the best way to help agents make better operational decisions in a BOP setting, specifically, mobile money in Tanzania.

In many developing economies, adults lack access to banks and other formal financial institutions, making them susceptible to theft (of their cash or assets), susceptible to high interest rates on loans (because they must rely on neighbors to borrow money), and vulnerable to economic shocks such as crop failure and medical events (because they lack savings and cannot receive transfers from distant personal connections that may be able to help smooth such shocks). On the other hand, mobile phone penetration rates are generally high. For example, recently, 21% of Tanzanian adults (age 15+) had a financial institution account (The World Bank 2017), but 73% of adults (age 18+) owned a mobile phone (Pew Research Center 2015). In the mid 2000s, mobile network operators began offering 'mobile money' services where customers can deposit physical currency (cash) at any of the operator's agents in return for immediately receiving an equivalent amount of digital currency (referred to as 'float') credited to their mobile money cell phone account. There are 2.9 million active agents (GSMA 2018) around the world, who can be found in markets, on street corners, in stores, at bus stops, and so on. Once a customer has money 'in the system' he can pay bills electronically, instantly send money over long distances, make loan payments, purchase insurance, and more. The digital currency is managed by the mobile money operators (MMOs). There were 247 million active accounts with transactions totalling 31.5 billion USD in December 2017 (GSMA 2018). These figures represent transaction volume growth of 29% and active account growth of 21% over December 2016 (GSMA 2018).

Providing financial access where it has generally been lacking can have significant societal benefits: Suri and Jack (2016) estimate that just having access to the mobile money platform in Kenya lifted 194,000 households—2% of all Kenyan households—out of poverty. However, mobile money deployments around the developing world have struggled with inventory management. In Kenya, customers reported being unable to complete one out of every five attempted transactions, primarily because the agent had insufficient cash or float (World Bank 2017). This has led USAID to describe improved inventory management as one of the top potential accelerators for mobile money penetration (USAID, Citi Foundation 2012).

As agents are not employees of the MMO, they put up their own capital to handle transactions, and in return make commissions on customer deposits and withdrawals. These agents face a complicated inventory decision every day: how much money should they stock? Currently, agents are making suboptimal inventory decisions, stocking out at least once during 49% of the agent-days in our historical data. This is greater than what is optimal for agents, the firm, or customers. Our research examines the best way to design and implement a support system in order to reduce agent stockouts through a randomized controlled trial (RCT). Specifically, we ask questions along two dimensions, corresponding to two treatments: 'what guidance should be provided,' and 'is in-person training necessary for agents to benefit from that guidance.'

For the 'guidance' treatment, we send personalized daily text messages to a randomly selected set of agents in Dar es Salaam, Tanzania to test which of three text message-based interventions help agents reduce stockouts. The first is a recommendation that 'tells' an agent how much to stock, which is specific to each agent for each day and is intended to maximize each agent's profits. The second is information that 'shows' an agent summary statistics of historical transaction volume. The third includes both the recommendation and the information. For the 'training' treatment, we examine whether in-person training plus an electronic notification or electronic notification only is better to introduce agents to and build trust in the guidance provided in the text messages.

Our results indicate that *training agents in person* and providing them with only *explicit recommendations* reduces the daily probability that agents stock out by between 2.1 and 3.9 percentage points (4.2% and 7.8% relative improvement based on a baseline stockout rate of 49.4%). Further, the same treatment increases the probability that an agent replenishes their inventory on a given day by between 2.5 and 4.6 percentage points (6.6% and 12.4% relative improvement based on a baseline replenishment rate of 37.5%). Agent stockout behavior was not significantly changed by other treatments. We do not find statistical significance in other aspects of agent operations, such as profit, adherence to recommendations, or increase in replenishment levels.

We also segmented agents by their replenishment behavior. Performance improvements for agents trained in person and provided only explicit recommendations is concentrated on the roughly 57% of agents who had never replenished with a bank before the experiment (replenishing exclusively through firm-provided 'float runners' who visit agents regularly). The effect is also concentrated on agents who kept sub-optimal replenishment levels during the pre-treatment period. Thus, there is evidence in this context that most of the performance benefit can be achieved by focusing on training a subset of agents in-person, and providing them with explicit recommendations.

2. The Mobile Money Market

In this section we provide an introduction to the mechanics of mobile money transactions, to the inventory and rebalance decisions made by agents, and to the details of our specific context.

2.1. Mobile Money Transactions

A common mobile money use-case illustrates the mechanics of how exactly mobile money works. Assume a provider works and lives in an urban area but financially supports a dependent living in a distant rural area. The provider is paid in cash. Before the advent of mobile money, the provider would physically carry cash to the dependent, requiring a long bus trip. With mobile money, however, the provider can send it electronically to the dependent.

Table 1 shows the steps of cash and float moving through the system for this example. The urban agent starts the day (step 1) with 200 units of cash and no float. The urban agent believes she has too much cash and not enough float. Before her work day starts, she travels to a bank where she replenishes her inventory by 'rebalancing' her allocation of cash and float. She hands 100 units of cash to the bank, who then credits her account with 100 units of float (step 2). Later, the provider arrives at the agent and wants to transfer 150 units of cash to the dependent. The provider gives the agent 100 units of cash (step 3), which is called a Cash-In (CI). In return, the agent gives the provider 100 units of float via mobile phone technology. Notice that for the provider and urban agent each, the total amount of money is constant, but the allocation across cash and float changes. Also, the provider would have liked to convert the full 150 units of cash to float, but the agent had only 100 units of float. Thus, the provider 'maxed out' the agent, who experienced a stockout. The provider then sends 100 units of float to the dependent (step 4). This is known as a peer-to-peer transfer. The dependent gives 100 units of float to a rural agent in return for 100 units of cash (step 5), which is called a Cash-Out (CO).

Table 1 **Example Flows of Currency for a Typical Use Case.**

	Balance									
	Provider		Urban Agent		Dependent		Rural Agent		Bank	
Step	Cash	Float	Cash	Float	Cash	Float	Cash	Float	Cash	Float
1. Start of day	150	0	200	0	0	0	100	100	10000	10000
2. Agent rebalances	150	0	100	100	100	0	100	100	10100	9900
3. Provider performs CI	50	100	200	0	0	0	100	100	10100	9900
4. Provider sends float	50	0	200	0	0	100	100	100	10100	9900
5. Dependent performs CO	50	0	200	0	100	0	0	200	10100	9900

Dashed ovals represent changes in float and cash amounts from the previous row. For this simple example, we assume there are no tariffs or fees for either withdrawals or peer-to-peer transfers.

Note that for each row, the total amount of float in the system is 10100; the only thing that changes is how this float is distributed among the participants. Float exists only in the computer servers of the MMO, but is backed up at a 1-to-1 ratio by traditional deposits that the MMO has set aside at a regulated financial institution. Thus, float has the same value as cash, and—unless the MMO makes additional deposits—there is always the same amount of float in the system.

Customers pay fees to the MMO to withdraw money and to perform peer-to-peer transfers and other transactions, while depositing money is free. Although customers pay a fee only for CO transactions, the agent receives a commission from the MMO for both CI and CO transactions, with the effective commission rate lower for larger transactions. CO commissions (0.91% of CO volume on average) are generally larger than the CI commissions (0.64% of CI volume on average) because it is perceived that there is more risk or cost to carrying cash than float.

2.2. Mobile Money Agents' Inventory Decisions

The decisions of how much cash and float to start the day with are the agent's alone. If the agent has too much money, she will lose profit by inefficiently using capital. Other barriers to carrying more inventory include lack of access to more capital and risk of theft. If the agent has too little cash or float, she will stock out and lose out on valuable commissions.

Determining the 'right' amount of cash and float is difficult for several reasons. First, there is a complicated relationship between cash and float: one replaces the other in any given transaction and stockouts can occur in both directions. Second, agents do not have access to their transaction history or summary statistics. Third, even if agents had access to their transaction history, forecasting and optimizing cash and float levels are non-trivial problems. Although the one-sided version of the optimal cash inventory problem was formulated over 130 years ago *also* in the banking industry (leading to the first newsvendor formulation (Edgeworth 1888)), the two-sided version that includes cash *and* float has no simple optimal solution. In the mobile money context, Balasubramanian et al. (2018) show that calculating agents' optimal inventory decisions requires solving a non-stationary dynamic program with temporally correlated demands and no obvious structural properties to mitigate the curse of dimensionality. Even before accounting for the difficulty of estimating the required parameters with sufficient precision, such a dynamic program is intractable even for large firms with access to cloud computing facilities. In another context, several authors working in closed-loop supply chains examine optimal inventory policies where demands can be positive (traditional sales) or negative (due to returns) (Beltrán and Krass 2002, Calmon and Graves 2017). Because the net demand can be positive or negative, the inventory level can increase or decrease just as it can with mobile money agents' inventory. Calmon and Graves (2017) develop a Monte-Carlo-based algorithm to determine an optimal inventory control policy for a Fortune 500 wireless service provider; such a policy is likely out of reach for individual entrepreneurs, who may therefore benefit from a support system.

2.3. Mobile Money Agents' Rebalance Decisions

Throughout the day, agents can proactively rebalance their inventory by changing the allocation of cash and float. Agents can rebalance at banks; however, banks in Tanzania are often sparse. Furthermore, queues at banks of over an hour in duration are not uncommon. Thus, to reduce transaction costs of rebalancing, many MMOs pay 'float runners' to travel to the agents regularly—typically in the early part of the day—and rebalance agents at their kiosk. During a rebalance, the float runner converts an agent's cash to float or vice versa. When a float runner stops by an agent, it is up to the agent to decide whether to rebalance at all, and if so, how to allocate cash and float. Rebalancing can reduce stockouts, which occur when an agent does not have enough cash or float to complete a transaction. However, rebalancing takes effort, depending on the distance to a bank and whether or not a float runner shows up. Thus, agents must balance the fixed cost of rebalancing with the variable cost of not having the 'right' split of inventory.

2.4. Our Specific Context

We partnered with an MMO that provides both wireless telecom and mobile money services in Tanzania. In 2016, we made three trips to Dar es Salaam, logging a total of twelve weeks there across all co-authors. We met with employees of the MMO to understand the business, both in the main office and in the field. We traveled with agent supervisors around the city, interviewing 10 agents in the Temeke region and 5 agents in the Mbezi region, a float runner, two branch managers (responsible for agents in one of 24 regions in Tanzania), and an aggregator (who manages float runners). While in Tanzania, we worked from the offices of the MMO to easily verify assumptions and ask questions.

For agents, the commissions earned far outweigh the costs of capital, and thus the current stockout rate of 49% is significantly higher than what is optimal. Agents are wholly responsible for the capital cost of the money they hold. Therefore, in this particular context, incentives are aligned and it is optimal from the agents', the MMOs', and customers' perspectives for agents to reduce stockouts.

These decisions have a material and immediate effect on agents' livelihoods. In our survey of 369 agents who showed up to training, more than two thirds reported mobile money as their main source of income. Other surveys show that the median income for mobile money agents in Dar es Salaam is more than 60% higher than the country-wide minimum wage (McCaffrey et al. 2014, Legal Human Rights Centre 2017).

However, there is no obvious way for the firm to get its agents to hold larger amounts of inventory. Firms working in Africa have tried approaches such as providing short term loans and providing other incentives for agents to carry larger inventory amounts. These have met with mixed success due to loan defaults, fraud, and lack of trust. Added to this is the fact that agents in general are working in a low resource setting; agents will not do anything that seems wasteful. While stocking out 49% of the time may seem high, it also means that 51% of the time the agent had too much inventory. From an agent's point of view, there may be resistance if the firm is telling agents to carry higher levels of inventory—without justification—which could lead to lower stockout rates, or, equivalently, more days where the agent's inventory is not 100% utilized. Additionally, agents who perceive that they have a very loyal customer base might be more willing to endure high stockout rates if they believe customers will just come back later. Thus, our MMO partner was amenable to trying out a decision support system (DSS) that optimized agents' own profits. Given the difficulties of calculating optimal levels, and the unique aspects of the context, agents could significantly benefit from such a system.

3. Related Literature

This research contributes to four streams of literature: agent-based models at the BOP, decision making, technology in developing economies, and field experiments.

3.1. Agent-Based Business Models at the Base of the Pyramid

Recently, researchers have analyzed several operations problems within agent-based models at the BOP. Pedraza-Martinez and Van Wassenhove (2013) note that for the International Committee of the Red Cross, incentives are mis-aligned between the head office and national delegations (agents) when determining when to replace 4x4 vehicles in the field. The authors find that agents hold vehicles for too long, and propose the optimal vehicle replacement policy. In their paper analyzing Essmart, Calmon et al. (2018) model a distributor trying to incentivize a local retailer to carry life-improving products in a low-income market. A unique and key aspect of Essmart is the after-sales service it provides on its durable products. The authors analyze whether—in order to increase adoption—it is better to provide customers with education about the products or a return policy to mitigate regret. Surprisingly, the authors find that, under certain circumstances, providing education can hurt the distributor's objective. Guajardo (2019) partners with a solar technology firm that gives its lamps to customers, who then pay agents regularly until the lamps are paid off. He finds that when customers bundle their payments, their usage of the lamps decreases, while increased usage leads to more on-time payments. By analyzing a fashion accessory manufacturer that partners with Kenyan artisans, Araman et al. (2018) develop a model that optimizes the best way to assign work to these agents. Finally, Iyer and Palsule-Desai (2019) develop screening contracts that a manufacturer can use to work with stockists in India. By leveraging a principal-agent model, the manufacturer can optimize the level of assistance and retail margin it offers stockists. These papers either highlight incentive concerns, analyze customer behavior in an agent-based-model context, or propose theoretical models grounded in reality. Several of them implicitly assume that if incentives are aligned, agent performance will accordingly respond to incentives. Our paper enhances this previous literature by 1) showing empirically how to improve agent performance, 2) doing so in an environment where incentives are aligned but performance still suffers, and 3) basing this on a field experiment in a BOP setting.

3.2. Decision Making

Behavioral operations management research explores the effects of suboptimal decision-making heuristics and resulting biases on business outcomes. Research has shown that inventory decisions made by humans err away from optimal levels (e.g., see Bendoly et al. 2006, for a review). Information technology (IT) generally, and DSSs in particular, have the potential to reduce the extent to which decision makers succumb to these biases. Indeed, in many cases, access to IT has been shown to improve human decisions. Sharda et al. (1988) demonstrate that access to a DSS led to more effective decisions in the context of students playing a business simulation game. Hu et al. (2017) show that human judgement, combined with decision support, can lead to better forecasts than human judgement alone. However, benefits are realized only if the technology is used (Devaraj and Kohli 2003), which may require overcoming people's distrust in imperfect algorithmic recommendations by giving them the ability to slightly modify the recommendation (Dietvorst et al. 2016). Sáez de Tejada Cuenca (2019) shows that managers are more likely to adhere to a DSS in apparel pricing when reference values are provided: merely making the 'revenue metric' more salient without also showing the reference values of 'revenue earned the previous year' had no effect. Flicker (2019) shows that when managers have private information, they may still be susceptible to ordering biases typical with newsvendor lab experiments.

The guidance provided to a user and the way in which it is displayed drive a user's willingness to adhere to the advice. Even on important decisions such as which portfolio to invest in for retirement savings, the default option can be a key driver of the option chosen (Cronqvist and Thaler 2004). We contribute to this area by examining how we can help managers make difficult inventory decisions via a DSS in a setting that has significant implications for their livelihoods.

3.3. Technology in Developing Economies

There is substantial research examining how mobile phones change market interactions in developing economies—either by their existence or through services provided by third parties via mobile phone technology—especially in rural agricultural markets (e.g., Parker et al. 2016). Mobile money is one such service. The popularity and rapid adoption rates of mobile money have led to a growing research stream investigating its impact on developing economies. Jack and Suri (2014) find that access to mobile money helps users respond to economic shocks such as drought or illness. Mobile money also has an effect on long-run consumption and poverty reduction (Suri and Jack 2016). Economides and Jeziorski (2017) estimate the price Tanzanian users are willing to pay for the safety and convenience of mobile money as opposed to transporting cash or storing cash at home.

While economists have largely focused on the impact of mobile money on risk, savings, and general economic welfare, operations management has focused on improving the quality of mobile money services. Balasubramanian and Drake (2015) show that greater competition among mobile money agents is associated with greater inventory holdings—a sign that agents are competing on service quality in this largely undifferentiated space. Building on their previous work, Balasubramanian et al. (2018) develop a model of an agent's demand and a newsvendor-like heuristic to determine her optimal stocking levels for cash and float. Their results suggest that agents who adhere to the recommended stocking levels can substantially increase profits.

However, these results are based on perfect adherence by the agents, which the behavioral inventory literature demonstrates is seldom the case. A natural progression, therefore, is to determine what gains can be made under which circumstances in the field.

3.4. Field Experiments

Many laboratory experiments have investigated how subjects make inventory-related decisions in controlled environments (e.g., Schweitzer and Cachon 2000). More recently, field experiments have measured consumer behavior (e.g., Craig et al. 2016, Zhang et al. 2019), racially-biased agent behavior (e.g., Mejia and Parker 2019), and the impact of interventions on non-operations managerial behavior (e.g., Berge et al. 2014). Buell et al. (2016), in a related paper in the service operations setting, investigate the effect of operational transparency on the throughput rate of service employees. In contrast to these experiments, our research is the first—as far as we know, in any setting—to use a field experiment to test what interventions impact the inventory-related behavior of real managers making real inventory decisions that affect their actual performance.

4. Research Questions and Experimental Design

In this section we first describe the managerial decisions related to providing guidance and training to agents. We then describe how we generate recommendations and information for this field experiment. We conclude with details of the experiment itself and the experimental design.

4.1. Managerial Decisions

Generally speaking, firms can manipulate the format and depth of training they provide to agents, and the type of guidance provided to agents, to enable agents to perform at their best. Firms can either train agents in person or simply notify them—e.g., electronically by text message—that they will now have access to greater information or a recommendation via a DSS. In-person training allows for clarification questions to be asked, more in-depth knowledge to be conveyed, and may develop more trust. However, it comes at high financial and time costs. Our training cost more than $10USD per invited agent, or about a tenth of a median agent's monthly profit from mobile money (McCaffrey et al. 2014).

In contrast, simply notifying agents about the DSS and its benefits has the potential to reach many agents at a low cost and whenever it is most convenient for each agent. Based on the success of Massive Open Online Courses (MOOCs) in other contexts (Zhang et al. 2017), remote notification and training may be adequate. In our context, it is not obvious that in-person training would provide much improvement over simple notification. Agents regularly rebalance with float runners and banks already. By informing agents via text messages what float and cash levels to rebalance to, the agents might perform the same rebalance transactions they are used to, except they may adhere to the new recommended amounts. If notification leads to operational improvements, it could be rolled out quickly across the country. However, it is unclear whether trust in the recommendations can be sufficiently established with notification only.

Along the guidance dimension, managers can either make an explicit recommendation or provide information meant to guide an agent towards a good decision. The benefit of an explicit recommendation is that agents do not need to understand how the recommendation is created but only how to follow the recommendation. Given that the decision agents are making is difficult, it is possible that an explicit recommendation will be the best type of guidance to provide.

The argument for providing information is that agents can combine the information and their own private signals of demand based on local knowledge to potentially make decisions that are better than the recommendation. For example, an agent may know that today there is a festival in the area so people will need more cash. Or the agent may have access to weather or pay-day information that our recommendations do not incorporate.

Of course, agents can also receive both the information and the recommendation. There are two possible outcomes of doing this. First, agents could use the recommendation as a starting point and incorporate their own beliefs about how the day's demand will be relative to the information provided and end up making better inventory decisions. This would be a synergistic benefit of providing both types of guidance. Alternatively, agents may get confused or overwhelmed by the combination of signals and perform worse than they would have had they received a recommendation or information alone. For instance, Chervany and Dickson (1974) show that too much information in a DSS led experiment participants to make worse decisions.

Finally, the types of guidance and training could interact. For example, because the recommendation is fairly easy to follow, agents may be able to benefit from the recommendation even after only being notified, whereas training may be necessary to help agents in the group which receives only summary statistics. Which combination(s) of training and guidance provide agents with the tools necessary to improve their performance is an empirical question.

4.2. Generating Recommendations and Information

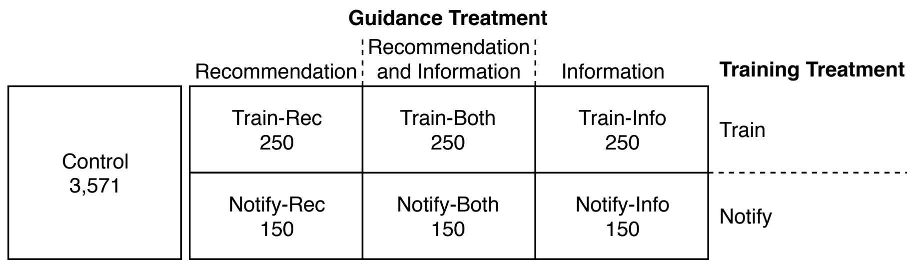
We must decide specifically what *recommendation* and what *information* content to include in the guidance text messages. For the *recommendation*, we utilize a modified heuristic proposed by Balasubramanian et al. (2018) to generate improved cash and float inventory level suggestions specific to an agent on a particular day. These recommendations are based on each agent's historical demand and are updated daily. An example *recommendation* text message is "Tomorrow (Sunday), we suggest that you have 185,000 float and 125,000 cash for [MMO partner]." On real data, while assuming agents adhere perfectly to the recommendations, Balasubramanian et al. (2018) show that the heuristic increases expected agent profit by 15% relative to agents' actual profits. Furthermore, the heuristic is shown to capture over 99.9% of the optimal profit in simulation studies. The heuristic has a newsvendor-like formulation that balances the overage cost of investing too much capital with the underage cost of losing out on commissions. Recommended inventory levels of float (cash) are based on the critical fractile of the distribution of the maximum (minimum) value of *net* demand within a day for float (cash). Net demand treats float transactions as positive and cash transactions as negative. Due to high levels of stockouts, demand unobserved due to low inventory levels must be estimated to approximate the true distribution of net demand. Further details of the heuristic itself as well as our specific implementation are in Appendix A. In current practice, it is good for both agents and the firm to increase service levels.

For *information*, we strove to provide summary statistics that would 1) be perceived as trustworthy, 2) help agents make better decisions, and 3) would not inadvertently induce the agent to make bad decisions. For instance, providing our estimate of stockout rates could be perceived as untrustworthy (because they are merely estimates and might conflict with the agents' direct observations) and could induce agents to carry far too much in an effort to get their stockout rates to zero. We took the 'do no harm' clause of running human experiments seriously, as for many agents, mobile money is their main source of income. Because it is unclear what the optimal information to give is that satisfies all these criteria, we chose to provide the agent with four values each day: the 50^{th} and 90^{th} percentiles of total historical CI demand for that specific day of week and the 50^{th} and 90^{th} percentiles of total CO demand. Implementation details are in Appendix A. While stocking these amounts is not optimal, this information could be useful to both sophisticated and unsophisticated agents while not harming anyone. For sophisticated agents, it conveys relative day-of-week magnitudes (as agents can monitor the relative volume day-to-day), as well as a sense of variability (by showing the spread between the 50^{th} and 90^{th} percentiles). The ratio of the cash to the float volume gives agents a sense of the expected balance between CI and CO transactions. Thus, if a sophisticated agent knows how to make good decisions but does not remember day-specific patterns, the information could help her by providing day-of-week-specific values of mean, CI-to-CO ratio, and variance. On the other hand, an unsophisticated agent might not know how to use this information and might conceivably take the mean of the information provided. These specific percentiles and data were selected because CI and CO recommendations most of the time (79% and 70% respectively) fell between the 50^{th} and 90^{th} percentiles of transaction volume based on pre-experiment data. Thus, if the agent merely rebalanced to the midpoint of the 50^{th} and 90^{th} percentiles, the number would be close to the recommendation and there would be little risk of harming the agents. At the recommendation of managers at our MMO partner, the actual text messages we sent to agents used the language 'typical' and 'high' to convey the concepts of 'median' and '90^{th} percentile.' An example *information* text message is "On a typical Sunday, you have 90,000 cash in and 55,000 cash out. On a high Sunday, you have 275,000 cash in and 130,000 cash out." All messages were sent in Swahili, the first language of the agents in our study.

4.3. Experimental Design

We ran a field experiment in partnership with our MMO partner which has nearly 40,000 agents throughout Tanzania. We focused on the 16,000 agents in Dar es Salaam from which we identified a subset of 4,771 agents to include in the treatable population based on characteristics of the agents' transaction histories during the pre-treatment period. In general, the filters applied before

Figure 1 Experimental Design.



Note. Breakdown of agents in each treatment. The treatment period lasted 27 days.

randomly assigning them to the treatment and control groups described below ensure that agents had a minimum threshold of activity and experience, and also that agents' recommendations were consistent throughout the day so that they would not be harmed if they rebalanced at 1PM to the recommended levels intended for 8AM. See Appendix B for details on the exact filters applied.

The experiment (summarized in Figure 1) proceeds as follows. First, agents are randomly assigned to be in one of six treatments or the control group. The six treatments come from three types of guidance provided via daily text message(s) delivered to the agent and two types of training provided. Guidance can be (1) information (Info), (2) recommendation (Rec), or (3) both (Both). The training provided to the agents could be either (1) in-person plus a voice recording notification (Train) or (2) a voice recording notification only (Notify). Second, treated agents were invited to training and trained (if they chose to attend), or sent a voice recording notification depending on their treatment. Third, during the treatment period (August 23, 2016 to September 18, 2016), treated agents receive daily text messages at the end of the previous working day according to their guidance treatment. Throughout the paper we refer to the control group as $Control$. Each of the six treatment groups is denoted by two terms separated by a hyphen: $\{Train, Notify\}$-$\{Rec, Info, Both\}$. The first term denotes the type of training. The second term denotes the type of guidance they received.

Our MMO partner allowed us to treat 1200 agents. Of the treated population, 750 were randomly assigned to the in-person training treatment with the remaining 450 receiving a voice recording notification. There are more agents in the training treatment because we were maximizing the number of agents trained given our research budget. With 150 agents in the smallest treatment group, we would be able to observe a reduction in our main dependent variable (the probability of a stockout, $P(Stockout)$, defined in § 5.1.2) of 3.25 percentage points with power of 0.8. For the in-person training treatment, agents were invited by our MMO partner to one of ten locations in Dar es Salaam. During training, our MMO partner's trainers—all native Swahili speakers using presentation materials developed by the researchers (and translated into Swahili)—spoke to agents about the motivation of the DSS, ideas behind the recommendations, and how uncertainty plays a role in profits. The trainers were trained by the researchers and two of the researchers visited several of the training locations on the day of training to make sure training proceeded as expected. Agents in all treatments were invited—via a text message—to call a pre-recorded message which explains that the program can help them, but does not go into as much depth as the in-person training. Text messages, the training presentation materials, and the questionnaire were all written by the researchers, translated from English into Swahili by a professional translation company in Dar es Salaam, and verified/approved by our MMO partner both before and after translation. All materials are available at `https://tinyurl.com/mobilemoneyresourcesfolder`.

Originally, we had a second treatment period (September 19, 2016 to October 17, 2016) during which *all* treated agents received *Both* information and recommendation text messages. For none of the dependent variables do we observe statistically significant coefficients for any of the treatments when analyzing our secondary treatment period. We briefly discuss our motivation and speculate on the lack of significance in Appendix C.

5. Data and Measurement

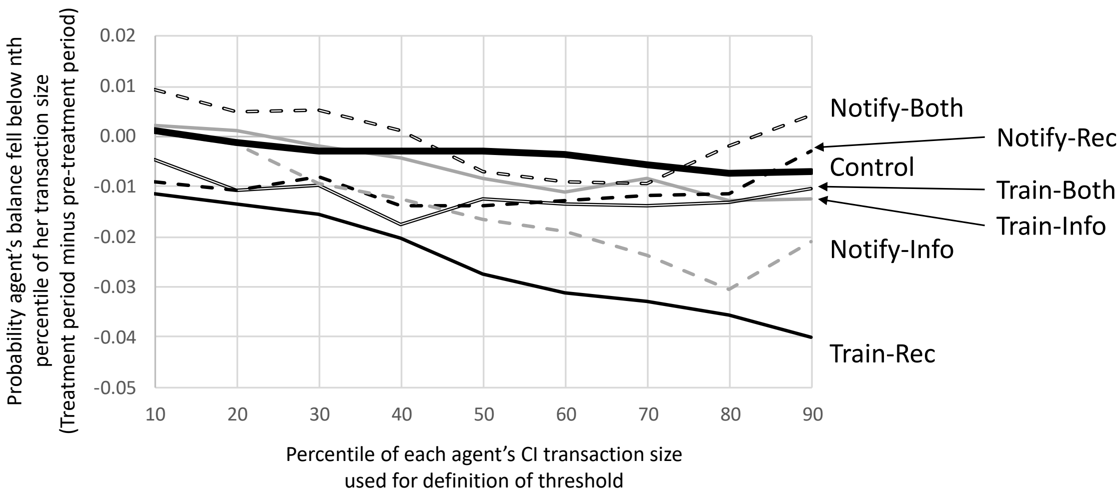
Our primary dataset is transaction data from our MMO partner in Tanzania going back to January 1, 2016, the date on which our pre-treatment period begins. The data include the anonymized sender and receiver IDs, date, time, and magnitude of every transaction as well as the pre- and post-transaction float balances of both the sender and the receiver. Our analysis focuses on float stockouts because, without visiting agents, we cannot accurately measure cash levels.

We supplement this with information about the 373 agents that attended training as identified by questionnaires and sign-in sheets. Four agents signed in but did not hand in a questionnaire and 369 completed a questionnaire. Five research assistants coded the paper questionnaires and sign-in sheets into a computer database. This process is described in Appendix D.

5.1. Measuring stockouts

A customer will first verbally check with an agent to see whether the agent has enough cash or float before conducting a CI or CO transaction. If the agent does not have enough cash or float, the customer may either walk away ('balk') or make a partial transaction up to the agent's balance ('max out'). Because demand is censored, we do not directly observe stockouts. Thus, we must resort to *estimating* stockout events. First, to motivate our dependent variable and present model-free evidence, we visualize data using a naïve measure that records whether an agent's float balance fell below a set of thresholds. Second, we incorporate the idea behind this measure into a variable ($P(Stockout)$) which looks at the probability that a customer arrived during the day who wanted to perform a transaction bigger than the agent's current balance.

Figure 2 **Change in $mean(CILowBal^n)$ From Pre-Treatment to Treatment Periods Across Different Thresholds.**



Note. Treatment period minus pre-treatment period empirical probability that an agent's balance dropped below a threshold on a given day. For each agent, the threshold is the n^{th} percentile of her CI transaction sizes.

5.1.1. A Naïve Approach A first naïve approach is to simply ask whether the agent's balance fell below a threshold each day. For instance, if an agent's balance fell below the median CI transaction size, then during this time block at least half of the customers could not have their demand satisfied by this agent. For each agent, we measure the n^{th} percentile of her CI transaction sizes, where n varies from 10 to 90. The 10^{th} (90^{th}) percentile transaction size on average is 18% (670%) of the median transaction size. For each agent-day, we set $CILowBal^n$ to one if the agent's balance dropped below this threshold and zero otherwise. A simple measure as to whether agents' balances dropped low enough to prevent them from handling CI transactions at any point in the day is the mean value of $CILowBal^n$ across days and across agents, grouping by pre- and post-treatment periods and treatment.

Figure 2 shows the change in $mean(CILowBal^n)$ from the pre-treatment period to the treatment period. The *Control* group and most other treatments experience lower stockout rates during the treatment period than pre-treatment. The *Train-Rec* group has the largest drop in $mean(CILowBal^n)$ for every percentile threshold. For lower percentiles, the *Notify-Rec* group also had lower stockouts, and for higher percentile thresholds *Notify-Info* also had lower stockouts. This is model-free evidence that the *Train-Rec* group had a reduction in stockouts (albeit using a naïve approach to define stockouts).

5.1.2. Measuring the probability of a stockout We build on the naïve measure and adapt it both to account for arrival rate, distribution of transactions sizes, and the length of time an agent's balance was at each level, and also to collapse it from a set of thresholds into a single variable. We measure the probability for each day that an agent encounters at least one customer who wants to make a CI transaction larger than the agent's float balance ($P(Stockout)$). This measurement is agnostic to what the customer decides to do afterwards, but rather focuses solely on the fact that such an event happened. As an overview, to estimate $P(Stockout)$, we measure the uncensored customer arrival rate per day of week and uncensored demand distribution for each agent individually. Combining this with each agent's float balance for each minute of each day gives us $P(Stockout)$.

We utilize the following assumptions. **A1:** Customers arrive according to a Poisson process, depending on day of week and agent. **A2:** Transaction sizes are independent from each other including across consecutive time intervals, depending only on the agent. **A3:** The customer arrival process is independent from the transaction size, conditional on agent. **A4:** Examining customer transactions when agent balances are high (above the 95$^{\text{th}}$ percentile of each agent's transaction sizes) is a good estimate of uncensored customer behavior. Implicitly, we 1) ignore time-of-day effects and 2) assume stockouts are independent across agents depending only on the agent, her balance, and the day of week.

The assumptions above do not always hold in the field. Arrival rates may differ by time of day; balking customers may visit several agents until they make a successful transaction; the arrival rate and distribution of transactions when agent balances are high may overestimate these parameters. However, the model we propose balances simplicity with reality. Nevertheless, caution should be taken when interpreting the absolute values of stockouts relative to our assumptions.

We define variables as such:

$\mathbf{H} \ni h$ – The set of all within-day time periods across agents and days during which an agent's balance remained constant. For instance, h_i may be 4 hours long for one agent for one day between two consecutive transactions while h_j is 4 minutes long for another agent on another day.

$T \ni t$ – The set of days from the start of pre-treatment to the end of treatment. We slightly abuse notation, writing $h \in t$ to denote time blocks occurring on day t.

$W \ni w$ – The set of day-of-week indicators: Sunday, Monday, ..., Saturday

$\mathbf{H}_{iwt}^{Uncen} \subset \mathbf{H}$ – The subset of all time periods up to $max(Pre\text{-}TreatmentEndDay, t-1)$ for agent i during which the agent's float balance was above her 95$^{\text{th}}$ percentile of CI transactions for day of week w. It is upon this subset that agent parameters are constructed for the analysis of events occurring on day t.

V_h – Duration in hours of the h^{th} time block.

N_h – The number of observed CI transactions during the h^{th} time block.

$\mathbf{D} \ni d$ – The set of all observed transactions. The superscript $Uncen$ and subscripts i, t, and w have the same meaning as above for $\mathbf{H}$.

$\tilde{D}_{it}$ – Random variable for uncensored CI transaction size for agent i calculated at t.

B_{ih} – Agent i's observed float balance level during time block h.

$P_{i,\{h,t\}}$ – The probability that agent i stocks out during either time block h or day t.

We estimate the arrival rate λ_{iwt} for agent i on day of week w calculated at t as $\lambda_{iwt} = \sum_{h\in\mathbf{H}_{iwt}^{Uncen}} N_h / \sum_{h\in\mathbf{H}_{iwt}^{Uncen}} V_h$. We include a day-of-week effect in the arrival rate to account for differences in agents' work habits as well as customer arrival rates. For transaction sizes, we ignore day-of-week effects because 1) for some agents the data is too sparse to accurately estimate transaction size distributions for each day-of-week and 2) we already account for day-of-week heterogeneity through arrival rates. Transactions sizes are approximately log-normally distributed. We let $\tilde{D}_{it} \sim LNorm(\mu_{it}, \sigma_{it})$ where μ_{it} and σ_{it} are the mean and standard deviation of the underlying normal distribution, that is of $ln(\tilde{D}_{it})$, calculated at time t. For each agent, we calculate the uncensored sample mean μ_{it} and sample standard deviation σ_{it} of the log of transactions in the set $\mathbf{D}_{it}^{Uncen}$.

For a given time block, the probability that an agent stocks out is the probability that at least one customer arrives whose uncensored transaction size is bigger than the agent's balance. This is one minus the probability that no customer arrives whose intended transaction size is above the balance: $P_{ih} = 1 - \sum_{n=0}^{\infty}\left(\frac{(\lambda_{iwt}V_h)^n e^{-\lambda_{iwt}V_h}}{n!} Pr\left(\tilde{D}_{it} \le B_{ih}\right)^n\right)$, where w and t correspond to the appropriate day of the week denoted by h. Within the summation term, the probability that there are n arrivals in time block h follows a the Poisson distribution.

The quantity of interest $P(Stockout)_{it}$ — the probability that agent i experiences a stockout on day t — is calculated as one minus the probability that no stockouts occur within any time block in day t, or $P(Stockout)_{it} = 1 - \Pi_{h\in t}(1 - P_{ih})$, where $h \in t$ denotes time blocks h that are within day t. Note that this relies on assumptions A2 and A3.

5.2. Measuring Rebalance Events and Levels

Agents in our sample rebalance on average only about 38% of days. On the days for which we do not observe rebalances, we assume that the agent had the opportunity to rebalance but decided not to. This is reasonable as our MMO partner has dedicated float runners who rebalance agents regularly. Agents may also rebalance more than once a day which occurs about 12% of days (or 31.6% of days on which a rebalance occurred). Finally, agents may rebalance at times other than before the start of business. We addressed this by excluding agents from the study (before randomization) whose recommendations substantially varied throughout the day. While the heuristic proposed in Balasubramanian et al. (2018) can be adapted to handle this heterogeneity among agents, due to implementation constraints, for this field experiment we implement a version of the heuristic where we assume each agent rebalances once per day before customers arrive.

Identifying whether or not a rebalance event occurred is surprisingly nontrivial. For each agent-to-non-customer transfer, we can identify whether the transaction was at a bank, with a float runner, or with another agent. However, we cannot discern whether the agent-to-agent transfers are non-professional (perhaps between friends) or professional (colleagues helping each other out when their inventory levels are low). For this reason, we identify only transfers that occur with a bank or with a float runner as rebalances. We set the variable *Rebalance* to 1 if an agent rebalanced with a bank or float runner at least once on a day and 0 otherwise.

As a control variable, we include the ratio of an agent's balance after her first rebalance event in a day to the float recommendation for the day ($RatioFloatRebalToRec$, or $RatioFloatRebalToRec\_Med$ is the median across the pre-treatment period). If an agent did not rebalance on a day, we measure her balance at the time she typically rebalances. We describe in further detail the complexity involved with measuring rebalance events and levels—as well as float runner behavior—in Appendix E.

6. Results

In this section, we discuss two types of treatment effects. We first estimate the Intent to Treat (ITT) effect, which is an estimate of the net effect of rolling out the DSS throughout Tanzania where agents may decide not to attend training. Then we account for the fact that agents ultimately chose to come to training in order to create a Local Average Treatment Effect (LATE) estimate.

We estimate which combinations of training and guidance led to improvements on two different dependent variables: $P(Stockout)$ and *Rebalance*. The first dependent variable is $P(Stockout)$, which represents the probability that agent i experienced a stockout on a day. The second dependent variable is *Rebalance*, which measures whether or not an agent rebalanced at least once on a given day. We provide more details on how these variables are calculated in §5. Because training occurred at a single point in time at the start of the treatment period, and to keep the ITT analysis aligned with the LATE analysis below, we aggregate these two dependent variables as well as the independent variables by taking the mean (or median where noted) value across days for each agent with the pre-treatment and treatment periods.

To verify that the assignment to different treatments is reasonably random, we show that the treatment and control groups have similar pre-treatment characteristics. Table 2 shows pre-treatment averages for thirteen metrics (further described in Appendix F) on which agents may differ. While there are some statistically significant differences in two-sided t-tests between the treatment group and the control group (which may be expected with $6 \times 13 = 78$ different tests), there does not appear to be any consistent differences for any of the six treatments. Nevertheless, in the analysis below we control for this agent-level heterogeneity by including pre-treatment measures of these variables as covariates in all regressions.

Table 2 Pre-treatment Randomization Checks.

		Treatment					
	Control	*Train-Info*	*Train-Rec*	*Train-Both*	*Notify-Info*	*Notify-Rec*	*Notify-Both*
P(Stockout)	0.492	0.500	0.495	0.493	0.545***	0.496	0.531**
	(0.227)	(0.220)	(0.226)	(0.234)	(0.226)	(0.221)	(0.220)
Rebalance	0.371	0.353	0.386	0.385	0.399	0.396	0.390
	(0.204)	(0.213)	(0.202)	(0.207)	(0.202)	(0.194)	(0.203)
NumTransCICO_Mu	1.843	1.821	1.929**	1.905	1.935	1.895	1.819
	(0.599)	(0.636)	(0.579)	(0.597)	(0.643)	(0.651)	(0.524)
NumTransCICO_CV	0.492	0.495	0.485	0.481	0.490	0.485	0.503
	(0.115)	(0.117)	(0.116)	(0.114)	(0.112)	(0.116)	(0.112)
TransSizeCI_Mu	10.068	10.073	10.129	10.086	9.967**	10.082	10.121
	(0.654)	(0.727)	(0.673)	(0.667)	(0.583)	(0.793)	(0.696)
FracVolCI	0.618	0.619	0.611	0.629	0.618	0.621	0.627
	(0.095)	(0.096)	(0.096)	(0.098)	(0.086)	(0.101)	(0.096)
RatioFloatRebalToRec_Med	0.805	0.758	0.788	0.862	0.726**	0.800	0.740**
	(0.469)	(0.380)	(0.411)	(0.670)	(0.405)	(0.420)	(0.364)
FracDaysWorked	0.911	0.906	0.922**	0.923**	0.915	0.916	0.915
	(0.089)	(0.091)	(0.076)	(0.084)	(0.081)	(0.081)	(0.090)
HoursWorked	11.809	11.724	11.780	11.899	12.162***	11.785	11.858
	(1.044)	(1.049)	(0.988)	(1.106)	(1.125)	(1.010)	(1.000)
PostRebalanceBalance	11.580	11.538	11.643	11.708	11.418	11.607	11.545
	(1.094)	(1.149)	(1.088)	(1.037)	(1.029)	(1.318)	(1.003)
*Rebalance*MoreThanOnce	0.011	0.009	0.015	0.010	0.012	0.012	0.011
	(0.031)	(0.030)	(0.039)	(0.032)	(0.023)	(0.031)	(0.025)
Never*Rebalance*FloatRunner	0.037	0.052	0.024	0.028	0.033	0.020	0.053
	(0.188)	(0.222)	(0.153)	(0.165)	(0.180)	(0.140)	(0.225)
Never*Rebalance*Bank	0.577	0.560	0.560	0.576	0.613	0.553	0.620
	(0.494)	(0.497)	(0.497)	(0.495)	(0.489)	(0.499)	(0.487)
# Agents	3571	250	250	250	150	150	150

Mean and standard deviations (in parentheses) of key dependent (top portion) and control variables (bottom portion). *** p<0.01, ** p<0.05 denote p-values from two-sided t-tests for differences in means between the treatment group and the non-treated group assuming unequal variances.

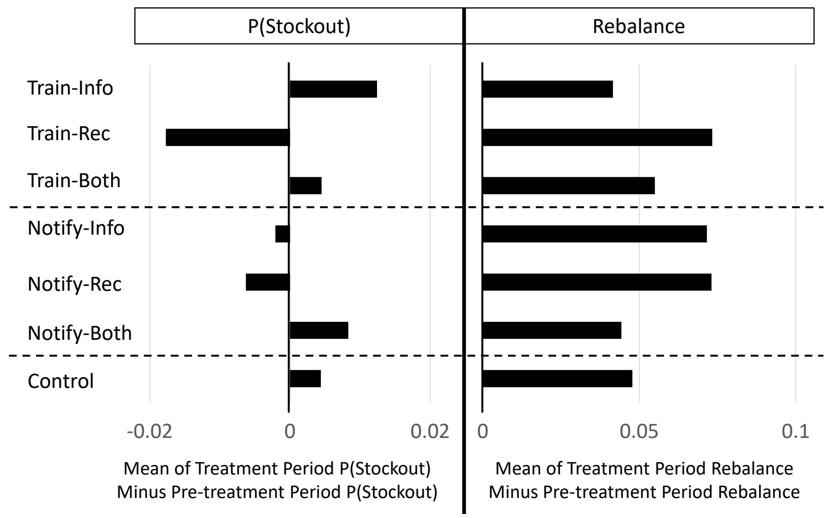
Before estimating the models, we first provide model-free evidence that the treatments had an effect on key agent metrics. Figure 3 shows the difference between the treatment and pre-treatment *P*(*Stockout*) (left) and *Rebalance* (right) rates for each treatment individually and for the *Control* group. It seems that improvements are likely focused in the *Train-Rec* group.

6.1. Intent to Treat Effect

With one observation per agent, we obtain ITT effects via the following econometric model:

$$Y_i = \alpha_d + \beta_1 \times Train\text{-}Info_i + \beta_2 \times Train\text{-}Rec_i + \beta_3 \times Train\text{-}Both_i \qquad (1)$$
$$+ \beta_4 \times Notify\text{-}Info_i + \beta_5 \times Notify\text{-}Rec_i + \beta_6 \times Notify\text{-}Both_i + \Gamma \times X_i + \epsilon_i$$

Figure 3 Model-free Evidence of Treatment Effects.



Note. Averages of differences in key variables between pre-treatment and treatment periods.

where Y_i is our dependent variable of interest, α_d are dummy variables for geographic districts within Dar es Salaam, the treatment variables ($Train\text{-}Rec$, $Train\text{-}Info$, etc.) are defined to take a value of one if agent i is in that treatment group (as described in §4.3) and zero otherwise, X_i is a matrix of agent-level control variables measured during the pre-treatment period (as reported in Table 2), and ϵ_i is the idiosyncratic robust error term. The district dummy variables control for any time invariant differences across Dar es Salaam's districts such as the training location used in that district as well as the average effect of time varying (but relatively stable in our short time frame) differences such as incomes, population density, etc. The ITT is estimated using OLS to be consistent with the LATE specification below.

The ITT results from model (1) with $P(Stockout)$ as the dependent variable are presented in Column 1 of Table 3. We find that agents in the $Train\text{-}Rec$ group have a significantly lower probability of a float stockout than stockout rates observed for the control group. The coefficient on the $Train\text{-}Rec$ group means that these agents are 2.1 percentage points (4.2% percent relative improvement based on a baseline stockout rate of 49.4%) less likely to stock out on a given day. Put another way, if this treatment were implemented across all 4,771 treatable agents in our dataset of whom 4,356 work on a typical day, we would expect 90 ($4,356 \times 0.0207$) fewer agents to experience a stockout on a given day versus the average 2,085 that we currently observe.

For agents trained in-person, it appears that providing information alone or both a recommendation and information does not lead to a lower probability of a stockout day as those agents stock out at rates similar to the rates found in the control group. This is also the case for all treatments within the notification treatment group. This is a surprising finding as the results suggest that providing more guidance to the agents is not helpful.

Table 3 ITT Results.

	(1) $P(Stockout)$	(2) $Rebalance$
$Train$-$Info$	0.00722	-0.0121
	(0.00911)	(0.0101)
$Train$-Rec	-0.0207**	0.0249**
	(0.00921)	(0.0106)
$Train$-$Both$	0.00235	0.00280
	(0.00900)	(0.0103)
$Notify$-$Info$	-0.00211	0.0255*
	(0.0119)	(0.0150)
$Notify$-Rec	-0.00998	0.0272**
	(0.0112)	(0.0119)
$Notify$-$Both$	0.00369	0.00378
	(0.0111)	(0.0136)
Adjusted R^2	0.710	0.597
Full Controls	Yes	Yes
Observations	4625	4625

*** p<0.01, ** p<0.05, * p<0.1, robust standard errors reported in parentheses. These are OLS regressions to be consistent with the LATE estimates below. All regressions include full controls and district-level fixed effects. There are not 4,771 observations because some agents did not work during the treatment period.

The ITT results from model (1) with *Rebalance* as the dependent variable are presented in Column 2 of Table 3. These results broadly agree with the results for $P(Stockout)$—agents who are given an explicit recommendation and trained perform better (rebalance more often) than the control group. For agents whose stockout rates are higher than optimal, rebalancing more often is a way to decrease stockouts. The 2.5 percentage point coefficient is equivalent to a 6.6% relative increase in probability of rebalancing on a day (on a baseline of 37.5%). However, agents in the $Notify$-Rec and (marginally) $Notify$-$Info$ groups also improve performance. It could be that the agents who are notified try to improve their performance by rebalancing more frequently, but that this increased rebalance behavior does not translate into stockout reductions.

6.2. Local Average Treatment Effect

While the ITT model above measured the average effect on all agents, the LATE can be interpreted as the effect of the treatments on only those agents who choose to attend training. We estimate this effect using two-stage least squares where the random invitation to training and assignment to different guidance treatments act as instrumental variables (IVs) for attending training and receiving the different guidance text messages. For randomized controlled trials where subjects are invited to treatment ('encouragement designs'), this is the advised LATE estimation methodology (Angrist and Pischke 2008) and is common in economics (e.g., Banerjee et al. 2010) and operations management (e.g., Zhang et al. 2019). Data on who listened to the notification is not available, so we can estimate LATE for the training treatments only. Notify agents are retained as they help to provide better estimates of attending training in the first stage. However, the coefficients for these agents should still be thought of as ITT. As such, we will focus all analysis on the Training treatments only. Specifically, we estimate the following first stage regressions:

$$\begin{aligned} \mathit{CameToTrain\text{-}Info}_i = \alpha_d + \beta_1 \times \mathit{Train\text{-}Info}_i + \beta_2 \times \mathit{Train\text{-}Rec}_i + \beta_3 \times \mathit{Train\text{-}Both}_i \\ + \beta_4 \times \mathit{Notify\text{-}Info}_i + \beta_5 \times \mathit{Notify\text{-}Rec}_i + \beta_6 \times \mathit{Notify\text{-}Both}_i + \Gamma \times X_i + \epsilon_i \end{aligned} \tag{2}$$

$$\begin{aligned} \mathit{CameToTrain\text{-}Rec}_i = \alpha_d + \beta_1 \times \mathit{Train\text{-}Info}_i + \beta_2 \times \mathit{Train\text{-}Rec}_i + \beta_3 \times \mathit{Train\text{-}Both}_i \\ + \beta_4 \times \mathit{Notify\text{-}Info}_i + \beta_5 \times \mathit{Notify\text{-}Rec}_i + \beta_6 \times \mathit{Notify\text{-}Both}_i + \Gamma \times X_i + \epsilon_i \end{aligned} \tag{3}$$

$$\begin{aligned} \mathit{CameToTrain\text{-}Both}_i = \alpha_d + \beta_1 \times \mathit{Train\text{-}Info}_i + \beta_2 \times \mathit{Train\text{-}Rec}_i + \beta_3 \times \mathit{Train\text{-}Both}_i \\ + \beta_4 \times \mathit{Notify\text{-}Info}_i + \beta_5 \times \mathit{Notify\text{-}Rec}_i + \beta_6 \times \mathit{Notify\text{-}Both}_i + \Gamma \times X_i + \epsilon_i \end{aligned} \tag{4}$$

where the Training treatment variables (e.g., $\mathit{Train\text{-}Rec}$) are used as instruments for whether an agent in a particular group came to the training session (e.g., $\mathit{CameToTrain\text{-}Rec}_i$).

The predicted values from these first stage regressions are then used in a second stage regression:

$$\begin{aligned} Y_i = \alpha + \beta_1 \times \widehat{\mathit{CameToTrain\text{-}Info}_i} + \beta_2 \times \widehat{\mathit{CameToTrain\text{-}Rec}_i} + \beta_3 \times \widehat{\mathit{CameToTrain\text{-}Both}_i} \\ + \beta_4 \times \mathit{Notify\text{-}Info}_i + \beta_5 \times \mathit{Notify\text{-}Rec}_i + \beta_6 \times \mathit{Notify\text{-}Both}_i + \Gamma \times X_i + \epsilon_i \end{aligned} \tag{5}$$

where variables are as described above except that the $\widehat{\mathit{CameToTrain}}$ variables contain the predicted (fitted) probabilities of attending training for each agent based on the stage one regressions. The same control variables are included in both stages.

The first stage IV regression results based on models (2)-(4) are presented in Table 4. Importantly, in all three regressions the instrument of interest is positive and significant indicating that being invited to training and receiving the different types of guidance significantly increased the probability of attending training and receiving guidance. While we do observe significant district-level differences in training attendance, the largest district effect increases the probability of attending training by only 5%. None of the other control variables were consistently significant.

LATE results based on model (5) with $P(\mathit{Stockout})$ and *Rebalance* as the dependent variable are presented in Columns 1-2 of Table 5. The effects are generally consistent with the ITT estimates—the only significant coefficient is for the $\mathit{Train\text{-}Rec}$ group and the coefficients are about twice as big as those in Table 3. This is consistent with the effect being concentrated in the half of agents that attended training.

For robustness, we also calculated results using two alternate dependent variables related to stockouts. The variable $\mathbb{E}[\mathit{NumUnservedCustomers}]$ estimates not just *if* a stockout occurred in a day, but the log of the expected number of customers who could not be fully served by an agent. The variable $\mathbb{E}[\mathit{Loss}]$ is more sophisticated, and estimates the log of the expected transaction

Table 4 **IV First Stage Results.**

	(1) *CameToTrain-Info*	(2) *CameToTrain-Rec*	(3) *CameToTrain-Both*
Train-Info	0.479***	0.000577	-0.0000267
	(0.0318)	(0.00104)	(0.00106)
Train-Rec	0.000215	0.537***	0.000513
	(0.00112)	(0.0316)	(0.000984)
Train-Both	0.000352	0.000768	0.476***
	(0.00125)	(0.00106)	(0.0322)
Notify-Info	0.0154	-0.00188	0.0000831
	(0.00978)	(0.00144)	(0.00136)
Notify-Rec	-0.00149	0.0192	-0.000534
	(0.00146)	(0.0117)	(0.00128)
Notify-Both	-0.000425	-0.000198	0.0288**
	(0.00156)	(0.00139)	(0.0140)
Adjusted R^2	0.463	0.513	0.450
Full Controls	Yes	Yes	Yes
Observations	4625	4625	4625

*** p<0.01, ** p<0.05, * p<0.1, robust standard errors reported in parentheses. All regressions include full controls and district-level fixed effects. There are not 4,771 observations because some agents did not work during the treatment period.

Table 5 **LATE Results.**

	(1) *P(Stockout)*	(2) *Rebalance*
Train-Info	0.0151	-0.0253
	(0.0190)	(0.0214)
Train-Rec	-0.0386**	0.0463**
	(0.0173)	(0.0196)
Train-Both	0.00499	0.00583
	(0.0189)	(0.0216)
Notify-Info	-0.00241	0.0260*
	(0.0118)	(0.0150)
Notify-Rec	-0.00921	0.0263**
	(0.0112)	(0.0118)
Notify-Both	0.00354	0.00361
	(0.0110)	(0.0135)
Adjusted R^2	0.709	0.597
Full Controls	Yes	Yes
Observations	4625	4625

*** p<0.01, ** p<0.05, * p<0.1, robust standard errors reported in parentheses. All regressions include full controls and district-level fixed effects. There are not 4,771 observations because some agents did not work during the treatment period.

amounts that could not be completed due to low balances. This measure models customers both balking and maxing out. The qualitative conclusions mirror those found in Table 3 and Table 5. An explanation of $\mathbb{E}[NumUnservedCustomers]$ as well as the regression results can be found in Appendix G, whereas the description and results for $\mathbb{E}[Loss]$ is available from the authors upon request.

7. Treatment Heterogeneity Based on Rebalance Behavior

As other research has shown, heterogeneity in terms of who benefits the most is important to take into account, although it often tends to be context specific. Pedraza-Martinez and Van Wassenhove (2013) show that optimal fleet management policies for different field delegation agents can vary by physical context. Calmon et al. (2018) show that optimal operations policies vary depending on whether customers are in the BOP designation or not, while Guajardo (2019) finds benefits in segmenting customers by their payment and product usage patterns. Iyer and Palsule-Desai (2019) segment stockist agents by their capabilities.

Often, the data firms use to segment their agents is limited because agents act more autonomously and with less oversight as compared to employees. In mobile money, one important dimension on which agents may differ is how they rebalance, a vital task directly linked to their ability to serve customers. By examining the mechanism whereby agents rebalance as well as the rebalance amount, we may be able to tease out the impact of training and recommendations on different segments of the agent population. We first focus on whether agents incorporated banks into their rebalancing behavior, and then briefly discuss segmenting on agents' pre-treatment rebalance amounts.

Table 2 shows that more than 94% of agents used a float runner at least once in the pre-treatment period, but only 40-45% ever rebalanced at a bank. While access to banks is likely higher in Dar es Salaam and among agents—who have an incentive to have a bank account—the relative dearth of banks, difficulty in travelling in general, and long queues at tellers result in banks being generally inconvenient in Tanzania. MMOs utilize float runners to compensate for this inconvenience and to increase the liquidity and profitability of their agents. We split agents into two types based on their pre-treatment bank utilization: $SomeBank$ agents rebalanced with a bank at least once and $NeverBank$ agents never rebalanced at a bank. Table 6 shows summary statistics of each type.

Both groups stock too little (i.e., their $RatioFloatRebalToRec\_Med$'s are below one). However, *a priori*, it is not obvious which type may benefit more from training. On the one hand, the $NeverBank$ type has more room to improve: their balance levels are lower relative to their float recommendations (0.77). On the other hand, $SomeBank$ agents may have the means to increase their rate of rebalancing more because they have access to a bank in addition to a float runner.

Table 6 Summary Statistics Based on Pre-Treatment Bank Utilization.

Metric	*SomeBank*	*NeverBank*
RatioFloatRebalToRec_Med	0.85	0.77
Fraction of days float balance below recommendation	0.62	0.66
$P(Stockout)$	0.499	0.511
Mean days rebalance at all	0.410	0.347
Mean days rebalance at bank	0.085	0
Mean days rebalance with float runner	0.354	0.347
Fraction showed up to training	0.52	0.45

Table 7 LATE Results Split by Bank Utilization.

	$P(Stockout)$		*Rebalance*		*FRRebalance*	
	(1)	(2)	(3)	(4)	(5)	(6)
	SomeBank	*NeverBank*	*SomeBank*	*NeverBank*	*SomeBank*	*NeverBank*
Train-Info	0.000705	0.0281	-0.0159	-0.0269	-0.000767	-0.0242
	(0.0214)	(0.0313)	(0.0264)	(0.0337)	(0.0280)	(0.0336)
Train-Rec	-0.0303	-0.0449*	0.0275	0.0588**	0.0385	0.0597**
	(0.0256)	(0.0232)	(0.0271)	(0.0273)	(0.0290)	(0.0274)
Train-Both	0.00420	0.00557	0.00978	0.00826	-0.00550	0.00732
	(0.0253)	(0.0277)	(0.0283)	(0.0320)	(0.0316)	(0.0321)
Notify-Info	-0.00901	0.00263	0.0398*	0.0139	0.0413*	0.0131
	(0.0182)	(0.0155)	(0.0232)	(0.0195)	(0.0241)	(0.0197)
Notify-Rec	-0.00505	-0.0118	0.0321*	0.0209	0.0196	0.0216
	(0.0142)	(0.0167)	(0.0165)	(0.0165)	(0.0213)	(0.0165)
Notify-Both	0.0164	-0.00232	0.0227	-0.0104	0.0117	-0.0102
	(0.0165)	(0.0148)	(0.0200)	(0.0182)	(0.0214)	(0.0180)
Adjusted R^2	0.729	0.691	0.609	0.574	0.533	0.574
Observations	1969	2656	1969	2656	1969	2656

*** p<0.01, ** p<0.05, * p<0.1, robust standard errors reported in parentheses. All regressions include full controls and district-level fixed effects. There are not 4,771 observations because some agents did not work during the treatment period. *FRRebalance* refers to balancing with a float runner.

We estimate LATE as described in §6.2 separately for the *SomeBank* and *NeverBank* types, the results of which are in Table 7. The improvements for the *Train-Rec* agents are concentrated in the *NeverBank* group with both variables at least marginally significant (Columns 2 and 4). Conversely, *SomeBank* agents showed no performance improvements within the *Train* groups as a whole (while they do show marginal changes in some *Notify* groups for *Rebalance*, these should be interpreted as ITT estimates). Appendix G shows that these are robust to using $\mathbb{E}[NumUnservedCustomers]$ as an alternate dependent variable.

One explanation for the concentration of the result on *NeverBank* agents is that not using a bank in the pre-treatment period might be a proxy for a relative lack of motivation to actively manage inventory. This lack-of-motivation hypothesis is consistent with the *NeverBank* type's under-representation at training seen in Table 6. After training and receiving recommendations, the agents then had more motivation to rebalance when the float runner came by or may have been more proactive in requesting float runners come to their locations. Evidence supporting this conjecture is in Columns 5 and 6 of Table 7 which shows that there is a significant increase in float runner rebalance frequency for $NeverBank$ agents. The coefficient is about the same size as the increase in rebalance frequency overall, suggesting that the improvement in *Rebalance* is due to $NeverBank$ agents rebalancing more often with float runners. (We omit the analogous result for *bank* rebalance frequency because many agents never rebalanced with a bank. The preponderance of zeros in the dependent variable makes results from our linear probability model unreliable.) Based on these observations, managers in this context should focus on $NeverBank$ agents.

While the firm has access only to a limited amount of data in general, in this instance we administered a questionnaire to agents who showed up to training, and thus have access to richer self-reported agent data than is typical. We note three instances where the agents differ based on the questionnaire data. First, agents were asked a question about calculating a commission value based on a percentage of a transaction size. (The English text was "If your commission is 5% (0.05 portion) of the transaction amount and you transact 20,000TZS, how many TZS do you earn?") This question was perceived by us and our partner to be difficult and $SomeBank$ agents were more likely to answer this question correctly (69% vs. 53%; p-value = 0.007). This difference does not appear to be due to higher educational or math training as agents are similar on those dimensions. This difference may tie back to the hypothesis that $NeverBank$ could be a proxy for motivation; in this case $NeverBank$ agents might have had less motivation to learn the commission structure. Second, $SomeBank$ agents were also more likely to state that they believe the MMO is fast at resolving their issues (2.81 vs. 2.55 out of 4; p-value = 0.012). (The English text was "How quickly does MMO resolve any issues you have with MMO money?" with the options "slowly," "somewhat slowly," "somewhat fast," and "fast," which we converted to a numerical scale.) It is possible that this latter point speaks to a matter of trust in the MMO by the agents. The $NeverBank$ agents did not think the MMO resolved their problems well, meaning the agents might not have trusted the MMO. By offering training, the MMO was able to earn the trust of these agents through the in-person presentations. This led to improved performance among this segment. Third, $SomeBank$ agents were marginally less likely to run more than 1 mobile money kiosk (0.283 vs. 0.405, p-value = 0.062). Perhaps agents who run more than one kiosk face limited bandwidth to make decisions due to the complexity of their operations, and benefited disproportionately from the training and recommendations.

In addition to segmenting agents by their bank utilization, we also segment agents by rebalance amount, that is, by whether $RatioFloatRebalToRec\_Med$ is less than ($LowRebalAmt$) or greater than ($HighRebalAmt$) 1. For brevity, we report the full results in Appendix H. Agents in the $Train\text{-}Rec$ group in the $LowRebalAmt$ type (which comprised about 75% of the agent population) had a negative and marginally significant effect for $P(Stockout)$ and a positive and significant effect for $Rebalance$. These findings match those for bank utilization above where $LowRebalAmt$ is analogous to $NeverBank$. In terms of the questionnaire data, the $LowRebalAmt$ type agents were more likely to run more than one kiosk (0.395 vs. 0.188, p-value = 0.003) but did not differ along other dimensions including those mentioned above for the bank utilization segmentation.

Thus, in this context, there is evidence to suggest that the firm can target specific agent segments, thereby reaping most of the benefit of training at lower cost. The MMO should focus its efforts on agents who may be struggling to effectively utilize the main tool whereby they increase their ability to serve customers: either through rebalance mechanism or rebalance amount.

8. Conclusion

Organizations are increasingly offering products and services to customers at the BOP via agent-based models. While attractive due to their scalability and flexibility, managing agents remains a challenge. We conducted a field experiment in the context of mobile money to understand how firms can best leverage training and guidance to help improve service quality in agent-based models. Our results show that in-person training paired with only explicit recommendations led to improved agent performance. As each firm's process improvement (or stockout reduction) initiatives differ in value, our proportional improvements can serve as a guide as to the overall impact of rolling out such an inventory support system. We also find that this effect tends to be concentrated in agents who have never utilized a bank, and thus mainly rely solely on float runners to rebalance.

Our work has academic, managerial, and policy implications stemming from four main contributions. First, we show that even in a situation where a recommendation is easy to follow, in-person training is still needed in this context. Second, we show that more guidance is not always better, as there was no observed improvement among agents who received both recommendations and information. Third, we show that there is agent-level heterogeneity in the treatment for which we observe an effect. Managers can use the categorization of bank utilization to create prioritized/phased roll-outs in the future. This is especially useful in resource-constrained environments. In the mobile money context, managers can first invite agents that have never rebalanced with a bank. By targeting these agents, the MMO can reduce training costs and complexity while still maintaining the quality improvements. Additional agents can be trained and receive the recommendations as more resources become available. Fourth, it is difficult to 'move the needle' through training and recommendations. A separate analysis we performed in which we simulated lost sales suggests that agents closed between 17% and 32% of the gap between their current $P(Stockout)$ rate and the rate achieved if they perfectly adhered to recommendations. (The current stockout rate is 49%. Our separate analysis showed the perfect-adherence stockout rate would have been 37%. Agents in the *Train-Rec* group closed between 2.1 and 3.9 percentage points of this 12 percentage point gap.) Even with in-person training, high-quality recommendations, and access to float runners, agents still did not improve their performance to the degree possible.

Several options exist for trying to close this gap further. First, training did not explicitly highlight the fact that agents tend to carry too little float, and that (in newsvendor parlance) float and cash inventory are high margin products (because commissions are about ten times our estimate of cost of capital). Thus, agents should want to hold much more than their mean levels to maximize their own profits. Perhaps highlighting this, or improving the training in other ways, could help.

Second, in our experiment, float runners were not involved in the roll out. The onus was on the agent to stock to the recommended level by utilizing the bank or float runner. Another experiment during which float runners enforce—or emphasize—float level recommendations for each individual agent could have a greater impact, albeit at greater effort and cost.

Third, an agent will always possess some local information that a centralized forecasting engine will not have access to. In this experiment, providing the 50^{th} and 90^{th} percentiles of past demand did not increase agent performance. However, there may be other ways to provide guidance that they can better blend with their own private information.

There are also limitations to our study. While we attempted to include as many agents as we could in the study, we still had to make some choices. We have no expectation that results would change based on these filters, but it is possible that agents with other characteristics—or in different regions—would not benefit as much or would benefit more from the treatment. Lastly, variance in the response variables relative to the sample size makes it impossible to tell whether there was a null effect in non-*Train-Rec* groups or whether the effect was too small to measure. Nevertheless, we enhance the discussion of and provide future research directions for how best to manage agents in BOP business settings. This has the potential to have a positive impact not only on consumers and beneficiaries, but also on the agents, who are often part of the BOP population themselves.

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Online Appendix for "Show or Tell? Improving Inventory Support for Agent-Based Businesses at the Base of the Pyramid"

July 4, 2020

Appendix

A. Details of Recommendation and Information Implementation

A.1. Recommendation

Here, we rely on the heuristic proposed in Balasubramanian et al. (2018)—which we implemented for this field experiment—and discuss our actual implementation details.

Balasubramanian et al. (2018) construct a newsvendor-like model balancing the cost of a stockout (no commission is earned) with the cost of having too much cash or float (cost of capital), where the 'demands' to be forecasted are the maximum float and cash levels agents would need each day to handle all customers' requests.

Specifically, our implementation details are as follows. First, an agent's historical CI and CO transactions are recorded, removing the largest 1% of CI and CO transactions from this history because some very large outliers exist in the data. For those blocks of time during which an agent was stocked out of either float or cash (defined as the agent's on-hand balance dropping below the median individual transaction size), 'ghost' transactions are added into the data set by calculating the expected CI or CO transaction amount that would have occurred during that block of time. In this way, we approximately correct for censored demand, as is outlined in Balasubramanian et al. (2018). While we used this straightforward approach to uncensor data and generate recommendations for the field experiment, we utilized a more sophisticated approach to measure the main dependent variable (see §5.1). For these CI and CO transactions (observed plus ghost), for each day, the maximum float and cash levels necessary to handle all customers' requests are calculated. We then use these historical data to forecast future cash and float requirements.

The 'forecast' R package (Hyndman 2017) was used to generate point forecasts of the maximum float and cash requirements for each agent for each day, using the 'forecast' function within this package with the 'robust' option set to $TRUE$ and day-of-week seasonality. This option of 'robust=$TRUE$' accounts for outliers and missing values. In our tests of using this function versus other forecasting algorithms (such as ARIMA, exponential smoothing, simple mean or median, and with or without day-of-week seasonality and with or without interpolation of missing values), the approach we used resulted in the best forecasts (as measured by mean squared error). To estimate the distribution of the error around the point forecast, we fitted a $Student's\ t$ distribution to historical forecast errors for each agent, as the distributions tend to have fatter tails than a normal distribution.

We emphasize that all of these quantities, probability distributions, forecasts, and recommendations are estimated at the agent level using each agent's own historical data, with the exception of the cost of capital for each agent. Based on extensive interviews with agents, we assume that the cost of capital is 20% per year for all agents. The commission rates for each agent that go into Balasubramanian et al. (2018)'s model are a weighted average of a specific agent's commissions on her historical transactions.

Once the model in Balasubramanian et al. (2018) is used to generate raw recommendations, these values are processed further. First, if the sum of the cash and float recommendations exceeds more than twice our (very rough) estimate of an agent's budget, the recommendations are scaled down. Second, if the cash or float recommendations are below 25,000 TZS we increase them to be 25,000 TZS (approximately $11USD based on a 2281 exchange rate). Third, we rounded the cash and float recommendations to the nearest 5,000 TZS (approximately $2.19 USD). All of these decisions were made in consultation with or suggested by the mobile money operator to ensure the recommendations would be more likely to be followed and they did not disrupt daily operations too much.

A.2. Information

For generating information text messages, looking back 28 weeks (to maintain a relevant time frame), we calculate the sum total of CI transactions (not cumulative, but actual sum) and the sum total of CO transactions for each agent for each day. For each agent, for each day of the week, if there are at least 20 historical observations (meaning days on which they worked) for that day of the week, then we calculated the 50^{th} and 90^{th} percentiles on the past 28 days of data of sum totals of cash in and cash out transaction amounts. If we do not have a record of the agent working at least 20 days of the past 28 on that day of the week, we take the 50^{th} and 90^{th} percentiles of all days the agent worked (regardless of day of week). In these cases, the text message is changed slightly to say 'On a typical day' instead of 'On a typical Monday.'

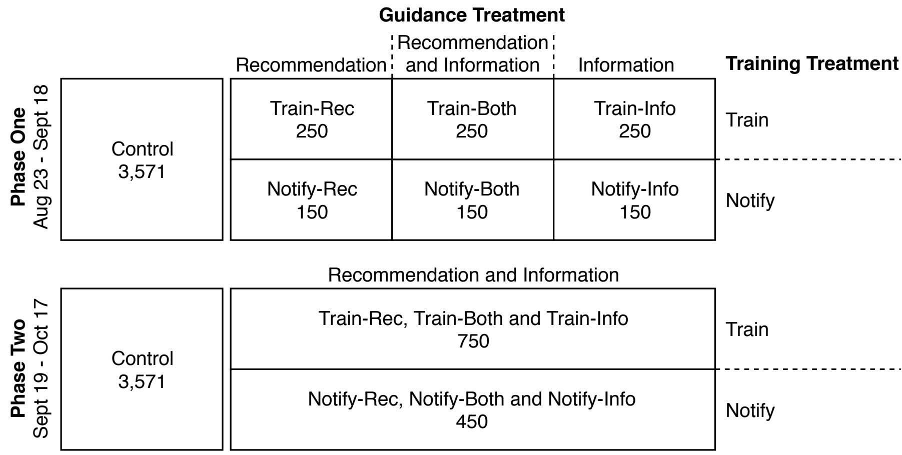
In addition to deposits and withdrawals, agents also perform merchant payments (by which customers can pay a utility bill) and recharge payments (by which customers can increase their phone credits). In conducting these transactions, agents still receive cash from the customer and transfer float (to a merchant or mobile operator, respectively), and so we include these two transactions in our definition of a CI. There are also transactions where a customer sends money directly to another entity, including peer-to-peer transfers. These transactions do not involve an agent so we exclude them from our analysis.

B. Filters to Identify Treatable Population

There are two high-level characteristics used to identify the treatable population. First, we wanted to ensure that agents had a minimum threshold of activity and experience. To do so, we removed agents where any of the following criteria were not met: (1) the number of CI plus CO transactions is at least 70% of all transactions; (2) the number of CI plus CO transactions plus agent-to-non-customer transfers is at least 90% of all transactions; (3) the agent has at least 134 days with at least one CI and/or one CO transaction. This is 4 out of 7 days on average since January 1, 2016, even if they started working later. This limits the analysis to relatively experienced agents; (4) the agent worked at least 12 of the 30 days leading up to the experiment; (5) the average number of CI plus CO transactions per day worked (defined as having CI and/or CO activity) is at least 3.

Second, we wanted agents whose recommendations were consistent throughout the day. Morning and afternoon recommendations may differ if the ratio of CI to CO is significantly different at different times of the day. If an agent followed a recommendation intended to be followed at 8AM at a later point in the day, we could potentially do financial harm to the agent. We address this by first creating hypothetical

Figure A1 Experimental Design.



Note. Breakdown of agents in each treatment. After 27 days in the first treatment period (Phase One), all agents receive both recommendation and information text messages beginning in the second treatment period (Phase Two).

recommendations for 1PM in addition to the 8AM recommendations we already generate. We calculate the median float recommendation at 8AM and the median hypothetical float recommendation at 1PM. Agents are removed from the treatable population if the difference between these two recommendations is more than 10% of the average of the recommendations, i.e., if $\frac{|Rec_{8AM} - Rec_{1PM}|}{(Rec_{8AM} + Rec_{1PM})/2} > 0.1$.

C. Second Treatment Period

Figure A1 shows our initial experimental design, which included two separate treatment periods.

Changing the guidance treatment in the second treatment period was intended to be used as a test for whether there is a sequencing effect—are agents better off receiving first the information messages and then both or first receiving recommendation messages and then both or should they immediately receive both messages? This was based on our idea that more guidance would be better than providing only one text message. Due to the lack of statistically significant coefficients, this idea turned out to be unsupported by the evidence.

What are the reasons that agents in the *Train-Rec* treatment reduce stockouts when they receive a recommendation but the effect is unobservable when they start receiving both recommendations and information? It could be that agents experience large initial benefits that wear off over the course of treatment. Another plausible explanation is that providing both the recommendation and the information delivers no benefits to the agents, perhaps because the agent tries to marry the data in an sub-optimal way or because the agent is overloaded with data. Because we altered guidance provided to agents in the second treatment period, we cannot disentangle providing too much guidance from decay of the effect over time, and leave this for future research.

D. Methodology to Encode Paper Questionnaires into Database

From the training sessions, we collected 458 paper questionnaires. We built a Microsoft Access database with a data entry form that looked exactly like the physical paper questionnaire with the Swahili text. Mouse-over helper text was provided in English for each question. A hired team of five people entered the data from each questionnaire into the database. Two people independently entered in each questionnaire for redundancy. If every single field matched for one questionnaire across two data entry workers, then the data from that questionnaire would be recorded and finalized. If even one field differed between the two data entry workers, a third worker would input the entire questionnaire again. For each field for this questionnaire, the correct answer with a consensus of at least two data entry workers would be recorded. If a particular question had three different answers, a fourth person and one of the co-authors manually investigated the paper form.

In this way, we found that of the 458 questionnaires, only 369 of them match agents who were invited to training. The other 89 questionnaires either could not be matched to any agent, were matched to agents in the control group, or were matched to agents outside the treated or control group.

E. Rebalance Behavior

To estimate the level to which an agent rebalances, we examine an agent's balance just after her first legitimate rebalance of the day. Some rebalance events are quickly cancelled out with an equal and opposite transaction due to error, due to agents' acting as an intermediary to other agents, or due to float runners gaming the system by inflating rebalance statistics. We call such rebalances 'pseudo-rebalances,' which are not considered legitimate.

We estimate that pseudo-rebalances account for over a third of all transfers. To elaborate on where pseudo-rebalances come from, we give two anecdotal examples found in the data. First, imagine that one agent with a bank account helps out another agent without a bank account. The first 'helper' agent withdraws one million TZS from the bank, and then immediately transfers 400,000 TZS to another agent. Here, the first agent has two transfers, only one of which should be classified as a true rebalance. For the second agent, the data make it look like a transfer happened with another agent, but in reality it was a rebalance. In a second example, imagine a float runner who is compensated more if he performs higher numbers and volumes of rebalances with agents. In order to artificially increase his monthly statistics and make it appear as if he is working hard, this float runner may ask one or more agents to perform several high-value transfers within the course of a few minutes that end with everyone having the same float as they began with. For instance, if the float runner transfers one million TZS to Agent A, who then immediately transfers one million TZS to Agent B, who then immediately transfers one million TZS back to the float runner, everyone ends with the same amount they started with (because transfers among agents and float runners do not incur transactions fees), except the float runner's monthly statistics are inflated.

Measuring rebalance levels are complicated for reasons just mentioned: there are many transfers that are not true rebalance events. Recall the Agent A above who—within the course of minutes—receives then sends out one million TZS of float, a large sum relative to her typical balance.

We wrote an algorithm that merges most of the pseudo-rebalances as well as rebalances involving 'helper' agents. Once pseudo-rebalances are removed or merged, the algorithm measures the float balance after the first valid rebalance of the day. If no valid rebalance occurred on a day, then the algorithm measures the float balance at the time of the day that the agent typically rebalances. This is the agent's 'float rebalance level,' and is measured each day whether the agent actually performed a rebalance or not. Admittedly, this does not capture all anomalous rebalances—such as rebalances in anticipation of a known, large customer transaction—so there is still noise in the data. Because of this and other noise, we utilize 'rebalance level' as a descriptor and control variable only but do not use it as a dependent variable.

F. Variables Used for Balance Checks and as Control Variables in Regressions

Table A1 Variables Used as Controls.

Variable	Description
$P(Stockout)$	Mean of probability that agent stocked out on a day.
Rebalance	Fraction of working days the agent rebalanced.
NumTransCICO_Mu	Mean of log of number of CI plus CO transactions per day worked.
NumTransCICO_CV	Coefficient of Variation of number of CI and CO transactions.
TransSizeCI_Mu	Mean of log of CI transaction size.
FracVolCI	Fraction of all transaction volume that is CI.
RatioFloatRebalToRec_Med	Median of the daily ratio of agent's float balance after a rebalance to the float recommendation.
FracDaysWorked	Fraction of agent days worked since first day (or 01-Jan-2016).
HoursWorked	Mean hours worked on a working day.
PostRebalanceBalance	Mean of balance immediately after rebalancing.
*Rebalance*MoreThanOnce	Fraction of days an agent rebalanced more than once.
Never*Rebalance*FloatRunner	Dummy variable for whether an agent ever rebalanced with a float runner.
Never*Rebalance*Bank	Dummy variable for whether an agent ever rebalanced with a bank.

Description of variables used in balanced checks and as controls in the regressions.

G. Robustness to Alternate Measure of Stockouts

In this section we describe an alternate way to measure stockouts which focuses on the number of stockout events. We first define and describe $\mathbb{E}[NumUnservedCustomers]$—the expected number of customers whose desired transaction level is greater than the agent's float balance at the time the customer showed up—and then show that our qualitative results are robust to this alternate stockout definition.

G.1. Description of $\mathbb{E}[NumUnservedCustomers]$

Recall λ_{iwt} is the estimate of the uncensored arrival rate for agent i on day of week w calculated at t. Then $\lambda_{iwh}^{Stockout} \equiv \lambda_{iwt} \cdot Pr\left(\tilde{D}_{it} > B_{ih}\right)$ is the arrival rate of customers whose desired transaction amount is greater than the agent's balance at time block h, where t corresponds to the appropriate time block h contained within that day t. Thus, the expected number of stockouts during time block h of duration V_h is $\lambda_{iwh}^{Stockout} \cdot V_h$. Summing over the time blocks h that comprise the day t for a specific agent i, we calculate $\mathbb{E}[NumUnservedCustomers]_{it} \equiv log\left(\sum_{h\in t} \lambda_{iwh}^{Stockout} \cdot V_h\right)$. We take the natural logarithm because the un-logged dependent variable is heavily right-skewed.

G.2. Results

Table A2 **Dependent Variable Robustness Results.**

	$\mathbb{E}[NumUnservedCustomers]$ (1) ITT	(2) LATE
Train-Info	0.0416	0.0871
	(0.0465)	(0.0971)
Train-Rec	-0.114**	-0.212**
	(0.0483)	(0.0912)
Train-Both	0.0353	0.0743
	(0.0482)	(0.101)
Notify-Info	-0.00642	-0.00816
	(0.0658)	(0.0655)
Notify-Rec	-0.0209	-0.0167
	(0.0565)	(0.0563)
Notify-Both	0.0446	0.0425
	(0.0575)	(0.0570)
Adjusted R^2	0.717	0.716
Observations	4625	4625

*** p<0.01, ** p<0.05, * p<0.1, robust standard errors reported in parentheses. All regressions include full controls and district-level fixed effects. There are not 4,771 observations because some agents did not work during the treatment period.

Table A2 shows the results for $\mathbb{E}[NumUnservedCustomers]$ for both the ITT (Column 1) and LATE (Column 2) estimates. The main results hold: the treatment affected the *Train-Rec* group and no other treatment group was affected.

Table A3 **Dependent Variable Robustness Results for LATE Split by Bank Utilization.**

	$\mathbb{E}[NumUnservedCustomers]$	
	(1)	(2)
	SomeBank	*NeverBank*
Train-Info	-0.000326	0.170
	(0.109)	(0.161)
Train-Rec	-0.134	-0.269**
	(0.130)	(0.125)
Train-Both	-0.000487	0.155
	(0.134)	(0.151)
Notify-Info	-0.0987	0.0502
	(0.103)	(0.0848)
Notify-Rec	0.0411	-0.0591
	(0.0675)	(0.0861)
Notify-Both	0.139	-0.00596
	(0.0975)	(0.0703)
Adjusted R^2	0.744	0.692
Observations	1969	2656

*** p<0.01, ** p<0.05, * p<0.1, robust standard errors reported in parentheses. All regressions include full controls and district-level fixed effects. There are not 4,771 observations because some agents did not work during the treatment period.

Table A3 shows the LATE results for $\mathbb{E}[NumUnservedCustomers]$ for *SomeBank* and *NeverBank* agents. The main results hold: the treatment affected the *Train-Rec* group for the *NeverBank* agents and no other treatment group was affected.

H. Heterogeneity of LATE Results Segmented by Rebalance Amount

Table A4 LATE Results Split by Rebalance Level.

	$P(Stockout)$		$Rebalance$	
	(1)	(2)	(3)	(4)
	HighRebalAmt	LowRebalAmt	HighRebalAmt	LowRebalAmt
$Train\text{-}Info$	-0.0117	0.0214	-0.0410	-0.0198
	(0.0356)	(0.0221)	(0.0485)	(0.0238)
$Train\text{-}Rec$	-0.0334	-0.0397*	0.0498	0.0448**
	(0.0243)	(0.0211)	(0.0385)	(0.0225)
$Train\text{-}Both$	0.0341	-0.0115	0.0171	0.0000968
	(0.0321)	(0.0232)	(0.0327)	(0.0276)
$Notify\text{-}Info$	-0.0194	0.000303	0.0407	0.0229
	(0.0213)	(0.0137)	(0.0259)	(0.0173)
$Notify\text{-}Rec$	0.0259	-0.0196	0.0402	0.0217
	(0.0182)	(0.0135)	(0.0258)	(0.0132)
$Notify\text{-}Both$	0.0126	0.00109	-0.00302	0.00563
	(0.0252)	(0.0121)	(0.0279)	(0.0152)
Adjusted R^2	0.301	0.606	0.591	0.598
Observations	1090	3535	1090	3535

*** p<0.01, ** p<0.05, * p<0.1, robust standard errors reported in parentheses. All regressions include full controls and district-level fixed effects. There are not 4,771 observations because some agents did not work during the treatment period.

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