PLEASE HOLD THE PHONE:
A FIELD EXPERIMENT ON MOBILE TECHNOLOGY,
UPTAKE OF DIGITAL FINANCIAL SERVICES AND WOMEN'S EMPOWERMENT

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ABSTRACT

As the number of cellphones owned worldwide approaches the global population total, a general perception pervades that mobile telecommunications technology is revolutionizing the developing world by giving the global poor access to better market information, mobile banking, and richer social connectivity. Despite this received wisdom, to our knowledge no field experiment has yet randomly assigned cellphone ownership to poor people to learn the causal effects of mobile technology on different aspects of wellbeing. In three studies of increasing size and complexity, we randomly assign free cellphones and airtime to poor women in Tanzania compared to wait-list controls. We seek to learn the effects on subjects' economic welfare, personal security, healthcare quality, social connectedness, and political empowerment.

1-INTRODUCTION

As a low-cost device that enables instant communication over vast distances by text or voice as well as money transfers, the mobile phone holds immense potential, especially for those who traditionally face high costs and steep barriers to long-distance communication, access to information, and secure banking. Despite the exponential spread of mobile phones and their increasing adoption in digital development initiatives, such as financial inclusion, improved health and real-time citizen feedback, there have been few randomized controlled trials (RCT) that rigorously assess the effects of mobile phone ownership. This paper presents the research design for one of the first large-scale RCTs in this field focusing on women in Tanzania. Similar to other low-income countries, in Tanzania women are significantly less likely to own mobile phones than men. Our research seeks to better understand the consequences of this gender gap and how it can be overcome.

The RCT presented in this paper represents the third phase of a multi-phase study we initiated in 2014 to assess the impact of mobile phone ownership on women's empowerment in Tanzania.

Phase 1 was a small pilot conducted in 2014 over two months with some 50 women around Dar es Salaam. Working with Kidogo Kidogo, a social venture created to help close the mobile gender gap by providing low-income women with cost-free mobile handsets, we employed a wait-list experimental design (in which half of the participants received a basic phone handset [Nokia 105], SIM card and starter credit at the beginning of the study and the other half received the package at the end) to assess the short-term effects of mobile phone ownership among female small business owners.

Phase 2 is also in collaboration with Kidogo Kidogo as well as the mobile network operator, Tigo. It is about to be implemented and will run for six months. It includes around 400 women in Rufiji and

Kilwa Districts in Coastal Tanzania. In addition to the control and phone groups included in phase 1, it also includes a cash placebo condition—in which some participants will randomly be assigned the equivalent of the phone package as an unconditional cash transfer.

Phase 3 builds on and represents a major extension of Phase 1 and Phase 2. It will run for one year and include 2,000 women across Tanzania in urban, peri-urban and rural settings. Building on phase 2, phase 3 will include the cost-free distribution of basic mobile phones and a low-cash placebo ($25) as well as the introduction of smartphones and higher-cash placebo ($90). Through a full factorial design, the experiment will also include the assignment of data plans, solar electrical chargers, and onboarding (group distribution and training).

In the rest of this paper we elaborate on the planned research in Phase 3, including the experimental design and procedures, the treatments and treatment assignments, and outcomes of interest and theoretical expectations. We begin with a discussion of the study's motivation and the findings from the Phase 1 pilot study.

2-MOTIVATION

One of the most important advances over the last quarter century has been the advent of low-cost mobile phones and the subsequent introduction of smartphones. These devices have revolutionized not just communication but also banking, citizen engagement, and access to information for people worldwide (Cairncross 2001; Aker and Mbiti 2010). As fast as this technological revolution is spreading, mobile and smartphone ownership is far from universal. Critical disparities exist. Two of the most significant are the gender gap and the urban-rural gap. According to existing research, women are 14 percent less likely to own a mobile phone than men in a low or middle-income country (GSMA 2015). The gap is even starker among rural women. This differential access is important in that it potentially reinforces or even worsens social inequality—a significant barrier to economic development. When women face political, social, and economic discrimination and marginalization, society as a whole is less productive, its institutions are less responsive, and it produces fewer public goods; these negative effects are also borne by future generations (World Bank 2012).

At the micro-level, women without low-cost access to information and communication are denied the agency and voice that come from mobile phone technology (Klugman et al. 2014). Moreover, as mobile money platforms come to dominate personal banking in developing countries, unequal access of mobile phone technology prevents women and the rural poor from receiving the financial security and inclusion that comes from mobile money[1] (Jack and Suri 2014; Kendall and Voorhies 2014; Gates 2015).

Increasing evidence points to the negative effects of the mobile phone-ownership gap on uptake and use of digital financial services. Financial Inclusions Insights finds in Tanzania, the country of

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[1] The vast majority of sub-Saharan Africans remain unbanked, lacking access to financial services such as savings, formal payments, and credit. Based on extensive surveys, the Global Findex, a partnership of the World Bank, the Gallup Poll, and the Bill & Melinda Gates Foundation, estimates that only 34 percent of sub-Saharan Africans have a financial services account of any kind (Demirguc-Kunt et al. 2014). This number represents a significant increase from 24 percent

interest in this study, that rural women have the lowest usage rate of mobile money—around 28% (compared to the national average of 48%)—largely because they have the lowest mobile phone ownership rate—30% (compared to the national average of 60%) (Null et al 2015). This is not just a problem of the rural poor, however. Though the scale of the problem is smaller in urban areas, even among those around Dar es Salaam, the lack of cellphone ownership constrains mobile money use as we found in Phase 1 of our RCT (see discussion in section 3).

Our study aims to directly address the question of the link between mobile phone ownership and use of mobile financial services. We are particularly interested in the pathway and conditions by which new phone owners become mobile money adoptees and mobile money users, enabling greater savings and fuller engagement with the formal economy. This study would directly complement the aforementioned financial inclusion tracking services and pioneering work by Jennifer Aker and colleagues (Aker et al. 2014) by providing rigorous evidence of the impact of mobile phone ownership on the uptake and use of digital financial services. To date there has been little experimental evidence brought to bear on this foundational step in the process of financial inclusion. But as is becoming clear, potential access to and ownership of mobile phone technology are not equivalent (Gates 2014). Until obstacles to mobile phone ownership for women, the poor, and rural residents are overcome, the development potential of mobile phone technology, especially digital financial services, will not be realized.

2.1-Banking the Unbanked: The Promise of Digital Financial Services

In practice the lack of financial services means that most exchanges take place using cash, which entails significant transaction costs. It also puts large purchases or receipts at high risk of theft. And, perhaps most importantly, cash economies demotivate savings. Extensive evidence from psychology and behavioral economics suggests that humans engage in a form of "mental accounting" in which they treat various budget categories as having different levels of liquidity, with cash being the most likely to be spent immediately and savings the least (see Kahneman and Tversky 1984; Thaler 1980, 2015; Heath and Soll 1996; Hastings and Shapiro 2013). While access to financial services does not guarantee savings, of course, it does appear to significantly increase the likelihood that people will delay gratification and put money away for later use (Dupas et al. 2014)—which has impressive downstream effects on household livelihood and on enabling credit markets.

The challenge then becomes how to get poor people access to financial services in systems where barriers to formal banking—through demanding reference requirements, prohibitive minimum balances, and extortionary withdrawal fees—are exceptionally high (see Beck et al. 2008). Mobile financial platforms present a possible solution. Just as mobile phones circumvented the bureaucratic red tape and technical barriers of land-line telephony and therefore gave billions of poor people around the world access to communications technology for the first time, so too might the low entry barriers, ubiquity, and user friendliness of mobile money cause a leapfrogging of stodgy banking practices and an enabling of financial services for the poor. However, here again the gender gap plays a key role, as women are significantly less likely than men to own mobile phones and therefore to possess the platform necessary for mobile payments (Scharwatt and Minischetti 2014).

While digital financial services are not a panacea, and users report problems with network systems going offline and agents proving unreliable (Altai Consulting 2015), prior studies suggest that mobile

money indeed improves the range of financial services available to the poor and reduces their vulnerabilities to negative economic shocks (Jack and Suri 2014). Households using mobile money are more likely to save money, send or receive remittances, make other formal financial transactions, have a bank account, and own insurance (Mirzoyants 2013). Moreover, studies of certain sectors, such as smallholder farming, find that access to mobile money is associated with improved agricultural production and profitability related to greater investment in fertilizer, pesticides and hired labor and lower transaction costs in selling produce (Kikulwe, Fischer, and Qaim 2014). The potential objections to such observational studies, however, take aim at the direction of causality. Do people with better financial means and savvy happen to use mobile money, which therefore leads to a mere statistical correlation, or does mobile money actually cause improved financial services outcomes?

To our knowledge only one randomized control trial, whose methodology enables an unambiguous answer to the question of causality, bears directly on this question. Aker, Boumnijel, McClelland and Tierney (2014) randomly assigned whether recipients of a cash transfer program in Niger received their payments in envelopes containing paper currency or through a mobile money platform. Interestingly, subjects who received the transfer through the mobile platform showed a greater diversity in their diets and their children on average ate more meals per day than subjects in the cash by envelope condition. Aker et al. (2014) speculate that this was likely due to the lower time demands of receiving the payments by cellphone than by traveling and waiting in line to collect the cash. The results of the study are provocative, but they leave open many questions about which factors condition the adoption of mobile financial services and subsequently lead to better outcomes.

Randomized evaluation, because in expectation the random assignment of the intervention balances and therefore neutralizes all other potentially confounding factors, enables the precise estimation of causal effects. However, to our knowledge no randomized control trial has yet been performed that assesses the factors that cause uptake of mobile financial services for the poor. Given the importance of access to formal accounts and payments in combatting poverty, this study will therefore tackle the question in the most rigorous and scientific manner available. Curiously, no large-scale experiment assessing the general effects of mobile ownership on women's welfare has likewise yet been undertaken.

3-SUMMARY OF RESULTS FROM PHASE 1 AND RESEARCH DESIGN FOR PHASE 2

With funding from the Institute for the Theory and Practice of International Relations (ITPIR) at the College of William and Mary in Virginia, USA, our research team from REPOA, a Tanzanian research institute focusing on poverty reduction, the College of William and Mary, and Brigham Young University in collaboration with Kidogo Kidogo carried out an impact evaluation based on the distribution of phones to a small sample of 62 women (of whom 54, or 86%, completed the study) who lived around Dar es Salaam.

Working through Kidogo Kidogo's established partnership with FINCA, a global, nonprofit microfinance institution serving low-income entrepreneurs in developing countries, we recruited 62 female FINCA clients from three FINCA branch areas around Dar es Salaam—Magomeni, Kibiti, and Tegeta. The primary criterion for recruitment was that the women at the time of the baseline

survey did not own mobile phones (though they may have owned one in the past). Participants were recruited by local FINCA officers who informed the women that researchers from REPOA, W&M and BYU were interested in conducting a survey on the clients' views on women's issues and in exchange for their participation each woman would receive a mobile phone, Tigo sim card, start-up credit of 5000 TSH, and training on how to use the mobile phone and value-added services, such as mobile money.

The phones were all donated by Kidogo Kidogo and distributed through a wait-list design, in which half of the participants received the mobile phone package after the baseline survey was conducted at the end of June or early July 2014 and the rest of the women received the phone package after the endline survey in early September 2014. The initial distribution of the mobile phone packages was undertaken via random assignment. To ensure balance between the *treatment group* (those who initially received the phones) and the *control group* (those who initially did not receive the phones), we ensured equal distribution (block randomization) across three dimensions: 1.) one's village banking group; 2.) level of reported income; and 3.) whether one reported having a phone in the household.

Despite the low-statistical power and short treatment period, the findings from Phase 1 were striking. The random assignment of Kidogo Kidogo's phone packages to mobile-less women (many of whom were small-business owners) significantly increased beneficiaries' mobile connectivity and led to improvements in their access to market information, size of their customer base, use of mobile money, overall business operations, and their subjective welfare. Importantly, the experiment produced no evidence that cost-free phone distribution caused jealousy in the household or increased women's vulnerability to spousal abuse. Null effects were seen on standard measures of political engagement and self-efficacy, though this may be attributable to the short treatment period and high baseline levels of efficacy.

One takeaway from the pilot study is that it highlights the difference between *mobile ownership*—in which one individually owns a phone—and *mobile access*—in which one must borrow or share a phone. Due to the exponential worldwide increase in mobile phone penetration rates, nearly anyone can access this technology. In fact, at baseline nearly all reported being regular (although not daily) mobile phone users, despite not owning a device. But this access was constrained: to use a phone the women had to turn to family or friends to borrow a device. The strong treatment effects in the experiment show that these constraints matter; needing to borrow a device denies women the right to use the phone as much as they want and significantly inhibits their effectiveness as traders and small-business owners.

The results from Phase 1 are provocative, but they leave open many questions. One important question is whether we *see similar treatment effects among women in rural areas of Tanzania.* As educated, relatively well-off market traders and small-business owners with extensive prior phone experience, the women in Phase 1 were well-positioned to immediately take advantage of full phone ownership. They reported not being constrained by costs of charging or top-up credit. These factors may have contributed to the strong immediate impact of the phone distribution. On average, women in rural areas will be less educated, less independent and less well-off. Overall this may constrain their phone use. At the same time, however, as these women face steeper barriers to long-distance communication, access to information, and secure banking, even low levels of phone use may lead to significant improvements in terms of economic independence, empowerment and self-efficacy.

A second key question is *how phones compare as a development intervention to the equivalent value of the phone as cash*. Introducing a placebo cash condition will be useful both methodologically and substantively. Methodologically, it will help to reduce concern that treatment effects are due to desirability bias from the experimental design (in which subjects report a positive change in their behavior not necessarily due to the benefits of the phone but because of a conscious or subconscious social desire to please the surveyors who distributed the valuable good). Substantively, it will speak to the broader policy question of the merits of technology transfers vs. unconditional cash transfers as poverty-alleviation tools. While there is growing evidence extolling the benefits of both mobile phone ownership and unconditional cash transfers,[2] these two instruments have rarely been compared to each other.[3] Is a woman better off receiving a phone that gives her the power of low-cost communication, information access and mobile banking or collecting the equivalent in cash (representing two weeks' worth of wages for the average Tanzanian) that she can decide how to use as she best sees fit?

These two questions are at the heart of the second phase of the RCT we are currently undertaking among 400 women in Rufiji and Kilwa Districts in Coastal Tanzania. The study is in collaboration with Kidogo Kidogo, Tigo, CARE International and the Journalists Environmental Association of Tanzania (JET) and will run for six months.

Similar to Phase 1, treatment effects will be assessed using pre- and post-experimental surveys conducted by a team of female Tanzanian enumerators from REPOA on empowerment broadly defined (including social connectedness, economic independence and welfare, civic and political engagement, personal security, and individual efficacy) as well as participants' livelihoods as small-holder farmers (including farming productivity, access to market and weather information, and market opportunities). In addition to self-reported outcomes, we also plan to include a number of behavioral outcomes (e.g., the level of one's participation in local politics after the 2015 elections, willingness to sign a petition encouraging greater legal protection of women's right to own land, etc).

4-PHASE 3: A LARGE-SCALE RCT ON UPTAKE AND USAGE OF DIGITAL FINANCIAL SERVICES

4.1-Research Objectives

As discussed above, historically most people in sub-Saharan Africa, especially women, have been unbanked—they tend not to use formal financial institutions for savings or payments. The increasing accessibility of mobile money platforms, therefore, is potentially transformative—it opens the door, even among the poorest around the world, to financial inclusion. The potential of digital financial services has attracted great interest from development agencies, NGOs, and academics focusing on

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[2] Haushofer, Johannes, and Jeremy Shapiro. "Household Response to Income Changes: Evidence from an Unconditional Cash Transfer Program in Kenya." (2013). Jack, William, and Tavneet Suri. "Risk Sharing and Transactions Costs: Evidence from Kenya's Mobile Money Revolution." *American Economic Review* 104, no. 1 (2014): 183-223. Kendall, Jake, and Rodger Voorhies. "The Mobile-Finance Revolution How Cell Phones Can Spur Development." *Foreign Affairs*, 2014. *The Economist*, "Cash to the Poor: Pennies from Heaven," October 26, 2013.

[3] Though see the study by Aker et. al, but which does not include a control group because of the humanitarian nature of the intervention and limits how much we learn about the absolute benefits of a tech transfer vs. a cash transfer. Aker, Jenny C, Rachid Boumnijel, Amanda McClelland, and Niall Tierney. "Zap It to Me: The Short-Term Impacts of a Mobile Cash Transfer Program." *Center for Global Development Working Paper*, no. 268 (2011).

poverty alleviation and empowerment. Extant research and programming, however, have tended to take it as a given that potential users of digital financial services already own a mobile device. There is less systematic knowledge of the actual effects of mobile phone ownership on access and use of digital financial services. This represents an important question given the critical disparities in mobile and smartphone ownership, especially among women, the poor, and rural residents. How much does unequal access to mobile devices affect use of digital financial services? What is the uptake rate of mobile money for new phone users? What factors lead new phone users to become active mobile money users? Does the migration to smartphones lead to even greater gains in use of digital financial services? What are the effects of access to financial services on women's welfare and empowerment?

To address these questions, Phase 3 will undertake what to our knowledge is the first randomized-controlled trial (RCT) of its kind that leverages random assignment of mobile phones and smartphones to learn about mobile money uptake and usage. Building on previous research on financial inclusion in Tanzania and other developing countries, we focus on mobile phone ownership because it represents the foundational step in the customer's journey to active usage of digital financial services. According to survey evidence from the Financial Inclusion Insights program (http://finclusion.org/), in Tanzania the two key factors that differentiate between registered and unregistered mobile money users are mobile phone ownership and extreme poverty.

Random assignment of mobile phone handsets allows for a more controlled and precise analysis of how mobile phone ownership affects the uptake of digital financial services. Recruiting 2,000 women from village savings and loans associations (VSLAs) across Tanzania into the study will also allow us to analyze important subgroup effects, such as the conditional effects of literacy, urbanicity, employment type (e.g., smallholder farmers, market traders), technological openness, poverty status and levels of education on the phone intervention. The effects on smallholder farming—a sector traditionally dominated by women in Tanzania—are of particular interest as existing research demonstrates how credit constraints represent one of the key causes of low productivity yields, leading some to see great promise for mobile financial services (see Grossman and Tarazi 2014 for a review).

4.2-Treatment Conditions

Similar to Phase 1 and Phase 2, one of the core treatments in Phase 3 will be the random assignment of a basic but durable mobile handset with SIM card, roughly $5 of starter credit, and a training session on how to use the phone (which includes instructions on how to add credit, check one's balance, make and receive calls, send SMS, register and use mobile money, and use the mobile money account for savings). Beyond the basic phone package and training, we also include a number of novel instruments in the experiment, including the introduction of smartphones, the mode of phone distribution to groups or individuals, data plans, and solar chargers. We also feel it is necessary and important to include a cash placebo condition. This multi-arm experimental design significantly expands the exploration of the necessary conditions that lead mobile phone owners to become active users of digital financial services. We discuss each condition in turn.

Smartphones

The introduction of smartphones allows for a rigorous comparison of whether the distribution of an internet-enabled device with an advanced mobile operating system and touchscreen user interface leads to even larger increases in the adoption and use of digital financial services relative to basic phone users, as is seen in some developed countries (Federal Reserve Board 2015). In Tanzania the smartphone penetration rate remains low—about 8% (Pew Research Center 2015)—but is increasing. Through the random assignment of smartphones to both non-phone owners and basic-phone owners, we will be able to estimate how this more advanced technology affects uptake of digital financial services. As well, we will be able to identify if the effective uptake of digital financial services among smartphone owners is contingent on one migrating from previous ownership of a basic phone or is equally likely among new phone owners who leapfrog the basic phone.

Mode of Phone Distribution: Group vs. Individual

The mobile phone by its very nature is a social technology—it enables effectively instant person-to-person communication by voice and text. Smartphone users gain access to social networking sites and can send pictures and videos to each other. Mobile money is primarily used by Tanzanians to send remittances to each other (Financial Inclusion Insights 2015). Moreover, the cheap, removable SIM card enables different users to share a single device. In our pilot study we conducted around Dar es Salaam in 2014 among non-phone owners, 72% reported owning a SIM card and all reported being able to borrow someone else's phone (though, as mentioned, this did not allow women to use the phone as much as they wanted).

The social nature of mobile technology raises the question of whether there are benefits of group distribution of mobile phones—what we refer to as onboarding—versus individual distribution. To test for this, we will randomly assign some women to receive phones and training in mobile technology on an individual basis and some to receive phones and training in a group. We also will randomly assign the number of phones and beneficiaries that each group will have—from two recipients to eight recipients. This design will allow us to see if group distribution matters at different saturation points (for example, if there is a critical threshold for the number of phones in a group that significantly increases individual use). It will also allow us to determine if there are spillover effects for other group members assigned to the control group.

We expect group distribution to matter in a number of ways. For one, it may help to advance knowledge sharing that results in greater use of the mobile phone and digital financial services. For new mobile phone owners or migrators to smartphones, the new technology can be highly complex and overwhelming to use, especially for those with low levels of literacy and numeracy. Though, as explained above, we will offer each recipient a training session on how to use the phone, one session may not be sufficient and could lead to under-use of the phone or even cause individuals to forego use altogether.

Group distribution may help to mitigate against such problems, however, as members of a group work together to figure out how to use different services on the phone and share knowledge with each other about how to operate the phone. Extensive management research elucidates the benefits of knowledge sharing through organizations (see Ipe 2003 for a review). We would expect group distribution of new technology to follow a similar dynamic. Data from GSMA's 2015 report, *Connected Women*, reveals that women are significantly more likely than men to turn to others,

whether a spouse or female relative, to learn how to use their handset (GSMA 2015, 124). Onboarding women with new devices as groups compared to individually may help to formalize this process and benefit even more participants. We would expect onboarding to be especially important when it comes to smartphone distribution. While women who receive basic phones will most likely know a number of others who own a phone and can teach them how to use it, this is unlikely to be the case when it comes to smartphones.

Another benefit participants may derive from group onboarding is the social effect of seeing other women receive a phone and becoming socialized as a phone user. While the gender gap in phone ownership is shrinking, in rural areas and certain parts of Tanzania (such as Lindi) the disparity remains quite significant. Moreover, beyond ownership, there is also a gender gap in mobile phone usage, especially when it comes to the adoption of more advanced services like mobile internet (GSMA 2015). Similar to other types of information and communication technology (ICT), these disparities are partially driven by structural differences in employment, education and income (Hilbert 2011). But they also are a function of cultural factors, as men serve as phone gatekeepers in the household and social norms discourage women from accessing mobile technology (GSMA 2015). Group distribution may help to overcome the cultural barriers to phone ownership and usage. Seeing other women receive and use phones may lead to a shift in social identification as well as provide women with a support network of other phone owners to which they can turn for counsel in how to navigate the social consequences of mobile ownership.

Data Plans

Another treatment arm we plan to incorporate is to provide some users with additional credit or data throughout the study. According to GSMA, the financial cost of credit is one of the top barriers women report to mobile access and use. For example, in Kenya 45 percent of female respondents to a GSMA survey reported that they agreed or strongly agreed that the cost of mobile phone credit was a barrier that prevented them from owning or using a mobile phone (GSMA 2015, 92).

At the outset of the study, basic phone and smartphone recipients will be provided with a start-up bundle worth $5 and $15, respectively, to help them get through the first month of use. To test the benefits of additional mobile credit and data access on uptake of digital financial services, we will randomly assign some participants in the study to receive additional monthly $5 top-ups to their credit/data plans.

Chargers

A final treatment arm randomly assigns participants to receive solar phone chargers. According to USAID, only 24 percent of Tanzanians have access to electricity. Not surprisingly, the charging of mobile phones represents a major obstacle to effective phone use. In Wave 1 of the Financial Inclusion Insights in Tanzania, many respondents, especially in the rural areas, complained of the lack of electricity as a barrier to use. While mobile phone charging stations are popping up throughout Tanzania, phone owners still face the non-trivial costs in fees and time to keep one's phone charged.

To determine how big of a constraint mobile phone charging is on use of digital financial services, we will randomly assign some participants in the study to receive a solar phone charger (at a value of $20) so they can keep their phone charged daily.

Placebo Cash Condition

Beyond comparison of the phone treatment groups to each other and to control, we also feel it is prudent to include a placebo cash condition in the experiment to allow for a comparison of the phone package (phone + start-up credit) to the equivalent in cash (roughly $25 for basic phone and $90 for smartphone). We believe it is interesting and important to include this condition not necessarily to advance knowledge on the effects of unconditional cash transfers as a vehicle for poverty alleviation, which would require a much larger and sustained cash infusion and is being rigorously studied by others (see for example Baird, McIntosh, and Ozler 2011; Haushofer and Shapiro 2013). Instead, we see the cash as a "placebo" condition—in the sense that it simulates the actual treatment by giving the participants a valuable good but it does not contain the information, communication, and banking properties that come with the phone. Thus, it will help us to isolate the precise effects of these properties of interest (ICT + financial services) while helping to rule out any bias that may arise from social desirability effects in which subjects given a valuable good feel compelled to tell the enumerators what they believe the researchers want to hear. Accordingly, the placebo condition need not be cash; we could offer a radio that is the same value as the phone but does not it include the communication and banking properties. We prefer to use cash as the placebo condition, however, because it is valuable in its own right vis-à-vis the control group to learn how a sudden non-trivial windfall affects non-phone owners' use of digital financial services. Moreover, we could also learn whether participants who receive the cash transfer convert it into a phone, and if not, why not.

5-PHASE 3 EXPERIMENTAL DESIGN AND PROCEDURES

5.1-Recruitment and Random Assignment to Treatment

To undertake the RCT, we will work with an established implementing partner with a national presence in Tanzania and strong experience working with a range of women clients (e.g., market women, smallholder farmers, educators, small-business owners, the unemployed) to recruit around 1,350 mobile-less women and 650 women with basic phones. Based on our pilot experimental study we carried out around Dar es Salaam in summer of 2014 among mobile-less women, formal power analysis (see Appendix 1) suggests that such a sample size will be sufficiently large to detect even small effects of the impact of mobile phone ownership on the uptake of digital financial services. In the pilot we saw quite significant uptake of mobile money in the treatment group (those who received the basic phones) compared to the control group. The difference in uptake of treatment to control was in the order of 30-40% – thus quite large, albeit with a small sample size (around 50 women with half in control and half treatment). (This is one of the key hypotheses we are testing in the medium-size study we are putting in the field now among smallholder farmers in more rural parts of Tanzania; thus we hope to be able to update our power calculations with this new data before implementing the larger study). Even if we temper our expectations for the larger study (considering that all of the women in the pilot were small-business owners based around Dar es Salaam and eager to use digital financial services to improve their businesses whereas in the larger

study we will have more rural women), based on the standard deviations we saw from the pilot, we should be sufficiently powered to detect differences as low as 5 to 8 percentage points between the treatment and control in the uptake of mobile money.

As mentioned, to facilitate the recruitment of study participants and distribution of the intervention, we plan to work with a reputable implementing partner. For the medium-size study we are currently undertaking, we are collaborating with CARE International and anticipate if the collaboration goes well we can likewise work together on this proposed project.

CARE works with women organized in village savings and loans associations (VSLAs). We plan to leverage these existing groups to implement the RCT. Working with these groups (which average roughly 20 to 25 women) facilitates the safe and secure distribution of the phones and cash, while reducing administration costs. It also makes it easier to study group-level effects of the phone intervention—which is an important dimension of the study. The limitation of working with women in such groups is that the results will be conditional on those who select into the VSLAs (the most marginalized and poorest are unlikely to participate in such groups) and existing group structures and social capital among members will produce individual floor effects (all women in the study will have the benefits of group membership and any pre-existing spillover effects from sharing phones).

The first activity we will undertake will be to identify those VSLAs we will work with. We estimate we will need to work with roughly 210 different groups to recruit some 1,350 mobile-less women. This calculation is based on the fact that the existing mobile phone penetration rate in Tanzania among women is around 60% (based on latest Afrobarometer data and FITS data). Thus if each group has 20-25 women, there will be some 8 to 10 without phones in each group eligible for participation in the part of the study involving mobile-less women. On average we will need 6-7 mobile-less women from each group.

We will use these same groups to recruit 650 basic phone owners for the second part of the study that examines the effect of migration from basic phone to smartphone. Since 60 percent of women on average will own a basic phone, we will have a larger pool of women to work with (12-15 women per group).

Figure 1. Mean Distribution of Phone Owners in VSLAs
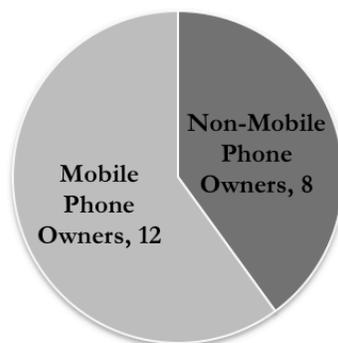
We will select the 210 groups we need overall through random selection of all eligible VSLAs across Tanzania. We plan to block randomize these groups based on location, size, and proportion of women who own phones, using data collected on these groups from our implementing partner.

To maximize statistical power, we will then assign individuals within these groups to the different treatment conditions (basic phone, smartphone, low cash placebo, and high cash placeo). To do so, we will proceed as follows:

Step 1: Identification of Potential Participants from Randomly Selected VSLAs

After we select the pool of VSLAs we will work with, we will then ask our implementing partner for a list of all women with and without phones in each group and then randomly select participants based on the number of total of non-phone owners (1350 participants) and phone owners (650 participants) we need.

Step 2: Randomly Vary the Number of Participants Across VSLAs (from 0 to 12 Participants)

To test the impact of potential spillover effects at different saturation points (i.e., as more women in the group have higher levels of phone ownership are there stronger spillover effects in terms of digital financial services among the control subjects?) as well as cumulative benefits to treatment subjects as more VSLA members receive phones, we will vary the number of participants we randomly draw from different VSLAs. For example, some VSLAs will have participants who receive none of the treatments (serving as a pure control), others with 2 participants (one in control and one in one of the treatment conditions), others with 4 participants (split between control and treatment), others with 6 participants (split between control and treatment), and so on up until 12 participants.

Overall this will give us a nice distribution across our VSLA groups of 1 to 10 women receiving a mix of basic phones, smartphones or cash with other participants in these same groups (1 to 6 women per group) receiving no treatment during the study period. Each saturation point of 0 (pure control) to 10 will have roughly 18-20 VSLA groups.

Step 3: Undertake Pre-test Survey to Establish Baseline Levels Across Key Study Dimensions and Data on Blocking Factors

Once we have selected the pool of participants, we will conduct the baseline survey. During the recruitment process, we will assemble and train our team of 30 Tanzanian female field research assistants at REPOA in the survey methodology we will use to establish baseline levels of mobile phone access, uptake and use of digital financial services, socio-economic characteristics, and overall welfare. Before we administer the survey among all 2,000 women who volunteer for the study, we will extensively pilot it.

During recruitment of individuals, we will explain that each person who volunteers to participate in the study will be eligible for a basic phone (for non-phone owners), a smartphone, cash, charger, or data plan as part of a development initiative undertaken by the implementing partner and REPOA. We will also explain and make sure everyone understands that not all of these items are available now but will be available within the next year. Since only some of the items are available now, we will conduct a lottery to decide who receives the goods. Those assigned to the control group will not

receive the items (a basic phone or the equivalent in cash) until after the endline—one year from the outset of the study.

Step 4: Assignment of Individuals to Treatment Conditions

After the baseline, we will then assign individuals to the main treatment conditions (basic phone, smartphone, cash) or control, using the data collected from the survey to block randomize on VSLA, age, level of education, socio-economic status, and current phone access.

We will then independently assign individuals to receive the data plan and chargers. Finally, we will assign each group to receive individual-level training or group-training (onboarding) on how to use the mobile phone and mobile financial services

The overall research design is reported in Table 1 and Table 2. As the design is full factorial, each treatment condition (phone/cash, group training, charger, and data) is assigned independently and it is possible for a given person to receive all four, though this is unlikely.

Table 1. Non-Phone Owner Experimental Design: Subjects Across Different Treatment Conditions				
	Total Subjects Per Condition	Group Training (of total subjects)	Data Plan (of total subjects)	Solar Charger (of total subjects)
A. Basic Phone	385	193/385	100/385	100/385
B. Smartphone	385	193/385	100/385	100/385
C. $25 Cash Transfer	175	88/175	50/175	50/175
D. Control Group	405	NA	50/405	50/405

Table 2. Phone Owner Experimental Design: Subjects Across Different Treatment Conditions

	Total Subjects Per Condition	Group Distribution	Data Plan	Solar Charger
A. Smartphone	280	140/280	100/280	100/280
B. $90 Cash Transfer	105	73/105	50/105	50/105
C. Control Group (Current basic phone owners)	265	NA	50/265	50/265

Step 5. Distribution of Intervention

After assignment of individuals to treatment groups, our team of field assistants working with our implementing partner will distribute the basic phones, smartphones, cash, solar chargers, and data plan to the relevant individuals. We will explain to the participants that the distribution of the technology items is an initiative of our implementing partner to improve the livelihoods of those in its VSLAs. We will further explain that the implementing partner does not have enough phones to give to everyone right now. The mobile phones and cash that are available now will be distributed to program recipients by lottery or luck of the draw. Others who do not receive a phone now will be eligible to receive one later, once more become available.

Each participant assigned the phone will be trained in its use, including working with them to register for mobile money and use it for savings. For those assigned to receive the group-level distribution, this training will be conducted with all group members present in a more interactive format and working together to learn how to operate the mobile phone—including calling, sending messages, and sending mobile money to each other.

6-OUTCOMES OF INTEREST AND MEASUREMENT

Two overriding questions motivate this research project. The first is what is the pathway from mobile phone ownership to uptake and use of digital financial services. The second is what are the downstream effects of mobile phone ownership and uptake of digital financial services on women's welfare and empowerment.

To assess the effect of the intervention on these two dimensions, we will conduct follow-up surveys at the treatment midpoint (6-months) and at endline (12-months).

To assess the uptake and use of digital financial services and participation in the formal economy, we plan to use the following indicators and measures:

- Self-reported use of mobile money; more frequent use of mobile payments; number of remittances sent and received each month; level of savings in mobile money account; use of cellphone SMS to receive economic information; recipient of loans through mobile phone.
- For phone recipients, the amount of credit they load on to their SIM cards throughout the study as shared by the mobile network operators.
- To assess whether at the endline one has an active mobile money account, we will offer the participant 4000 TSh (around $2) via mobile money or 2000 TSh (around $1) in cash as compensation for their participation in the endline survey. We would expect if the respondent does have a valid mobile money account, and knows how to use it, she will accept the mobile money offer; if she does not, she will prefer the cash offer. This would detect account usage at a relatively low cost.

To assess the effect of mobile phone ownership and use of digital financial services on welfare and women's empowerment, we plan to use the following indicators and measures:
- Self-reported information on:
  - business conditions and operations (e.g., use of phone to more frequently connect with customers; use of phone to receive price and other market information; use of phone to expand customer base and sales channels; use of phone to gain access to weather information and crop insurance)
  - social connectedness (e.g., use of phone to connect with family and friends)
  - welfare and vulnerability to economic shocks (e.g., use of phone to receive remittances during times of crisis; self-reported income level; employment)
  - political and civic engagement (e.g., engagement with local or national government, frequency of participation in community meetings, demonstrations)
  - empowerment (e.g., greater feeling of financial independence and individual efficacy)
  - security (e.g., level of intimate partner violence)
- Willingness of participants to sign a petition in favor of stronger constitutional safeguards on women's rights, such as property rights and greater representation in the national assembly.
  - To implement this measure we plan to work with local women's organizations that have been involved in mobilizing support in favor of the constitutional referendum in Tanzania on these very issues (postponed until after the 2015 elections).
  - We will then work with the local women's organizations to include VSLAs in our study in their areas of canvassing and identify if women in the treatment group are more likely to sign a petition in favor of holding the postponed constitutional referendum.

6.1-Discussion of Expected Results

The main results of the project will focus on learning what causes new female mobile phone owners to adopt mobile financial services and to use them in their personal and business savings and transactions. The study will also contribute a better understanding of the effects the gender gap in mobile phone ownership has on the use of digital financial services.

More specifically, we make novel contributions to understand how basic cellphones compare to smartphones in improving access to financial services; whether the distribution of phones in groups leads to greater technological learning, socialization and adoption; how subsidized data plans affect

mobile payments uptake; and finally how ownership of solar electrical chargers might further increase usage. Moreover, through a blocked randomization design, we can explore these effects across a number of important sub-groups: urban vs. rural residents; smallholder farmers vs. other sectors; literate vs. illiterate; technology adoptees vs. technology resistors.

Finally, through the employment of numerous survey outcome measures, we will be able to test the effects of mobile ownership on women's financial and social welfare more broadly. All things being equal, how much does owning a mobile device boost the income and welfare of those living in poverty? How much does it improve one's access to market or weather information that are important for farming? How much does it reduce credit or liquidity constraints in the agricultural sector? How much does it increase one's access to health care, civic and political engagement, and individual efficacy? How does it affect gender relations and power dynamics at home and in the workplace?

ESTIMATION OF TREATMENT EFFECTS

The core analysis will be performed using difference-in-means tests comparing subjects in the various treatment conditions to those in control. The main method of estimation will be randomization inference, which offers the advantages of an intuitive quantity of interest in its *p*-values as well as limited assumptions regarding underlying distributions. Indeed, RI undertakes no assumptions about the distribution of the test statistic, but rather generates *p*-values from the data itself by multiply reassigning the data to all of the possible treatment vectors and constructing the range of hypothetical treatment effects that might occur from random chance. It thus accounts for all potentially favorable and unfavorable assignments to treatment that might co-vary with either high or low values of outcome measures. It therefore calculates a precise *p*-value from the imputed set of possible assignments scrambled with the actual experimental data.

Regression analyses based on covariates drawn from the survey will be employed as robustness checks. Ordinary least squares regression with covariates will be used for analysis of treatment effects on the multiple outcome measures. Standard controls including demographic characteristics, especially those employed for block randomization, will be included in regression models.

The large number of outcomes measured in the survey is prone to the problem of multiple comparisons. We will adopt multiple strategies to account for the probable over-estimation of treatment effects that might be attributable to random chance. First, we will employ the primary analysis on index measures capturing each of the main families of outcomes: economic well-being, social connectedness, healthcare quality, personal security, and political engagement. The index measures will be constructed from the items categorized by each family in the survey itself, but factor analysis will also be employed to verify that the items load significantly together as predicted. The employment of the indices will reduce the total number of comparisons made.

Second, we will make adjustments for multiple comparisons by controlling the false discovery rate at conventional levels (i.e. an alpha of .05) using the Benjamini-Hochberg procedure. Alternative adjustments, including methods suggested by Bonferroni and Sidak, will be employed to check robustness.

Randomization will occur at the individual level with VSLA groups employed as the primary sampling unit in blocking. This should improve power compared to a standard cluster design. The saturation design with random assignment of the number of subjects per group should enable the precise estimation of spillover effects. However, it is possible that the intra-cluster coefficient, even under block randomization, will be high, in which case we will use standard cluster estimation procedures in randomization inference and regression models.

The main treatment effects based on random assignment to experimental conditions will be the main quantities of interest in estimation. However, the design includes key subgroups in which treatment effects will be specifically examined. Namely, the effects of the treatments on subjects in urban, peri-urban, and rural locales will be examined separately, and power calculations have been made with the subgroup analysis in mind.

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APPENDIX 1-POWER CALCULATIONS

We have the benefit of drawing from our pilot study of mobile-less women to make power calculations that will inform this larger study. In the pilot we saw quite significant uptake of mobile money in the treatment group (those who received the basic phones) compared to the control group. The difference in uptake of treatment to control was in the order of 30-40%--thus quite large, albeit with a small sample size (around 50 women with half in control and half treatment).

Based on the standard deviations we saw from the pilot, for most of the interventions we are well powered to detect relatively small differences in the 5-6% range in the main analysis without sub-grouping by urban, peri-urban, and rural. Even with the subgroups, power should be sufficient to detect differences of 10-12%.

All of these estimates were made assuming the .05 significance level, .80 power, a baseline mean of .20, and a standard deviation of .30 (the latter two of which correspond to the results from our pilot study). We note that the power calculations for the group- vs. individual-level training should account for the fact that randomization must necessarily occur at the group rather than individual level. For the most complicated analysis with the weakest power focusing on sub-groups by urban, peri-urban, and rural, the detectable difference for the group treatment is .18. This is large but rather inevitable with a cluster design and subgroup analysis such as that we are employing.

APPENDIX 2-IMPLEMENTATION AND TIMELINE

November-December 2015: Sign MOU with implementing partner; work with wholesalers to procure phones; sign MOU with major mobile network operators in Tanzania to improve data collection; write and pilot baseline survey.

January-February 2016: Work with implementing partner to identify pool of eligible VSLAs; randomly select VSLAs; identify of list of women in VLSAs; undertake baseline survey; randomly assign subjects to study groups; distribute intervention.

August-September 2016: Conduct mid-line survey.

March-April 2017: Conduct endline survey and distribute treatment to control group participants.