CENTRE FOR THE STUDY OF AFRICAN ECONOMIES

The Economic Impact of Mobile Phone Ownership:

Results from a Randomized Controlled Trial in Tanzania*

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Abstract

We study the causal impact of reducing the mobile gender gap. Leveraging one of the first large-scale experimental studies on women's mobile phone ownership, we find that in Tanzania over thirteen months smartphones increased households' annual consumption per capita by 20% compared to control. Consumption gains operated through women's control and use of the smartphones. However, treatment effects were attenuated by handset turnover. By endline only 34% in the smartphone condition still possessed their handsets. This highlights the economic benefits of closing the mobile gender gap but also the tenuous nature of productive asset ownership for women in low-income households.

JEL Codes: J16, L96, O12, O33

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Introduction

One of the most important technological advances over the last quarter-century has been the global diffusion of mobile phones. Mobile devices have transformed not just communication and access to information (Jensen, 2007), but also, with the advent of mobile money and digital banking, access to financial services (Suri, 2017). While the economic benefits from the mobile phone revolution are far-reaching, the greatest potential impact holds for the poorest[1]—those who traditionally face steep barriers to long-distance communication, acquiring market information, and gaining access to financial institutions (Aker and Mbiti, 2010). In many low-income countries, however, a stubborn mobile gender gap persists—in which women are not only less likely to own a phone, especially a smartphone, but less likely to use mobile money and mobile internet (Santosham and Lindsey, 2015; Mushi et al., 2017; Women, 2019; Demirgüç-Kunt et al., 2020). This digital inequality risks compounding existing structural disparities (Bank, 2011), and, by one estimate, will represent a global welfare loss of \$700 billion in GDP growth over the next five years (Women, 2019).

In this article we analyze the impact of reducing the mobile gender gap in low-income households. We administered the Mobile Phone and Livelihoods of Women Program in Tanzania in 2016 and 2017 among women who did not own a phone. The program offered all participants SIM cards and mobile money accounts, and then, based on random assignment, provided some a basic phone (Samsung B110), smartphone (Huawei Y3C) or a cash grant (40,000 TZS, $\sim$ PPP US\$55) in year 1 of the program, while others were waitlisted for a phone in year 2 (serving as a control

[1] There is a large and growing body of scholarship analyzing the impact of mobile technology on the livelihoods of the poor. See for example Labonne and Chase (2009); Aker (2010); Blumenstock et al. (2015); Asongu (2015); Aker et al. (2016b); Suri and Jack (2016); Aker and Ksoll (2016); Abor et al. (2018); Wantchekon and Riaz (2019).

group).[2] This represents one of the first large-scale experimental studies on women's mobile phone ownership—and in fact one of the first on mobile phones more generally.[3]

To evaluate the program's impact, we surveyed participants prior to recruitment (baseline) and then six months (midline) and thirteen months after the intervention (endline).[4] Overall, the program was effective at bolstering women's mobile phone ownership, including in the cash group—in which more than half of participants used their cash transfers to buy handsets.[5] But we also observe a high degree of mobile phone turnover—in which handsets were subsequently sold, exchanged, lost or otherwise not retained. Turnover occurred across all conditions (see Figure 1), but was especially high for the smartphone intervention. By endline only 34% in this group had the program handset on their person. (See Figure A4.3).

Our design enables us to compare the economic impact of providing women non-phone owners with smartphones, basic phones, or cash (used to buy basic phones). Our main pre-registered outcomes were participants' use of digital financial services; effects on livelihoods and levels of empowerment; and household economic well-being as measured by consumption. On mobile money, consistent with many observational studies, we demonstrate the importance of women's mobile phone ownership, not only for use, but also for control over any transfers they receive. Employing a pre-registered on-the-spot digital payments test to capture revealed preferences for mobile money versus cash, we find mobile money uptake was highest in the basic phone condition.

[2]We also provided several cross-cutting treatments: solar chargers, monthly airtime vouchers and phone training. See details in Section 2.

[3]Important exceptions include the pioneering experimental work by Aker and colleagues (Aker, 2010; Aker et al., 2016b; Aker and Ksoll, 2016; Aker et al., 2016a) and, more recently, an RCT on cell phone towers in Philippines by Blumenstock et al. (2020).

[4]See CONSORT diagram in Figure A1.2.

[5]We intended our cash group to serve as a placebo condition and allow for a cash benchmark. Yet embedded as part of the Mobile Phone Program, we have strong evidence that cash beneficiaries felt encouraged to buy mobile phones and did so at a rate much higher than they otherwise would have if they had received the cash grant in a different context.

Uptake in the cash group was attenuated by lower mobile phone ownership, while in the smartphone condition it was sensitive to participant literacy levels. However, while we observe some positive effects on weekly income, effects on monthly income were null. Likewise, beyond social connectedness, effects on women's empowerment were generally null—at least after thirteen months.

At the household level, however, we see much more pronounced average treatment effects as measured by consumption—with these effects strongest in the smartphone condition. Compared to control, smartphones boosted average annual per capita consumption by 20%, an increase of \$118.89 PPP (2017) (95% CI: \$26.61-\$211.16). Spending increased across a range of consumption baskets beyond mobile phone airtime and fees, including education, transportation, contributions to social functions and community activities, entertainment, and health. These consumption gains did not correspond to significantly deeper levels of mobile loan indebtedness. Smartphone effect sizes were 3 times higher than the basic phone (p=0.12) and 3.6 times higher than the cash grant (p=0.09).

We see no evidence that the smartphone consumption gains stemmed from selling the premium handset. To the contrary, consumption gains were concentrated among those whom our research team verified still possessed the smartphone at endline. Moreover, we find that participant mobile phone use was an important mediator of the smartphone's consumption effects. One possible channel, in line with Suri and Jack (2016) and Batista et al. (2018), was through occupational change. Smartphone recipients were more likely to specialize in market trading.

Just as important were the positive externalities of the smartphone for the household, which lifted a given family's mobile phone capacity from 0.85 mobile phones at baseline to 1.64 phones at endline (versus only 1.44 and 1.04 phones in the basic phone and control conditions, respectively). We estimate the increase in household mobile

phone capacity mediated some 36% of the effect of smartphones on consumption. Taken together with the individual impact, this suggests that closing the mobile phone gap, such that each adult income-earner possesses their own phone, helps households twice over: by boosting new phone owners mobile connectivity as well as by reducing the constraints and opportunity costs that existing phone owners face from joint dependence on a single handset.

Our paper contributes to a number of different research streams in economics. First, despite the voluminous literature on mobile technology, there have been very few experimental studies on mobile phone ownership. Our study helps fill this gap with a focus on increasing women's mobile connectivity. The results point to the significant economic impact of reducing the mobile gender gap. Doubling the number of handsets in the household from one to two brings large increases in household consumption, especially when that second handset is a smartphone. From a benefit-cost perspective, we estimate the smartphone had a multiplier effect—the total consumption increase to the household was 3.5 times the value of the smartphone handset and double the value after subtracting program costs (see Table A19.1.)

Second, our study is one of the first to track the high turnover in mobile phone ownership. (See Figure 1.) This suggests that the digital gender gap arises not just from the challenges women face to acquire handsets in the first place but also from the inability to retain control over them. It also points to how poverty constrains the impact of mobile technology: low-income households turned to selling handsets that over time may have brought significant welfare gains. This finding has important implications for mobile-for-development programming as turnover can cripple the efficacy of interventions that rely on continuous participant mobile connectivity, such as cash transfers.

Third, this article aligns with existing research on the importance of better un-

derstanding the effects of household bargaining on the impact of mobile technology (Aker et al., 2016a; Riley, 2019; Barboni et al., 2018). Descriptively, we observe that the strongest consumption gains accrued to those women whom we verified still had the smartphone on their person at endline (demonstrating a high degree of control over the technology) *and* also reported that besides them their husbands also used the handset. The latter finding points to the potential benefits of increasing intra-household cooperation and knowledge sharing on mobile technology, whereas the former aligns with existing research on the link between women's property rights and control over productive assets on household welfare (Peterman, 2011; Duflo, 2012).

Finally, our paper contributes to one of the most active streams of mobile technology research—on the uptake and use of mobile money (for a useful overview see Suri (2017)). Access to mobile money in low-income countries is found to increase remittances (Jack et al., 2013; Batista and Vicente, 2020; Lee et al., 2021); boost household consumption (Suri and Jack, 2016; Munyegera and Matsumoto, 2016; Lee et al., 2021); enable risk sharing and smoothing consumption in the face of shocks (Jack and Suri, 2014; Batista et al., 2018; Riley, 2018; Abiona and Koppensteiner, 2020; Ahmed and Cowan, 2021); and induce more efficient allocation of labour (Suri and Jack, 2016; Batista et al., 2018; De Gasperin et al., 2019; Lee et al., 2021). Welfare gains from mobile money are found to be especially strong for female-headed households (Suri and Jack, 2016) and for women microfinance recipients who control their own mobile money accounts (Riley, 2019). Most existing studies take mobile phone ownership as given (for an exception, see Aker et al. (2016a)). Our study overcomes this limitation and enables a more comprehensive picture of the impact of mobile phone ownership on economic well-being, including via the uptake of mobile money. Paradoxically, despite the smartphone producing the largest consumption gains, it had weaker effects on mobile money uptake, both among individual participants and their households. This is an

important finding for two reasons. It is a cautionary tale that technological advances may lead to new barriers to use with adverse effects. It is also an important reminder that mobile technology's economic impact does not only work through mobile money but enables a broad set of technical capabilities, including enhanced communication and access to the internet, that are important to improving the livelihoods of the poor.

Mobile Phone Program and Experimental Design

Despite how far and fast the mobile phone revolution has spread, significant disparities in mobile phone ownership persist. Consider Tanzania, one of the the earliest adopters of mobile money in 2008. By 2016, 48% of the population owned a mobile phone and mobile money account. Yet, women's rates of ownership of this potent combination lagged behind men by some 37%.[6] To better understand how to reduce this mobile gender gap and increase financial inclusion, our research team administered the Mobile Phone and Livelihoods of Women Program in Tanzania in 2016 and 2017, building on a series of pilot studies completed in the years before. The program was designed to improve women's access to and use of mobile technology.

Working primarily with non-phone owners recruited among members of BRAC (a microfinance organization) and the Tanzanian Social Action Fund (TASAF) across five different regions of Tanzania (Arusha, Iringa, Mwanza, Tanga, and Ruvuma), the two-year program offered all participants SIM cards and a mobile money account at the start of the project[7] and then, based on random assignment, provided some a basic phone, smartphone, or cash grant in year 1 of the program, while others were waitlisted

[6] Whereas 59.4% of men owned a phone and possessed a mobile money account in 2016 in Tanzania, only 37.7% of women did. Based on authors' analysis of Financial Inclusion Insights (FII) Tracker Survey, Tanzania Wave 4 conducted in August-September 2016. Financial Inclusion Insights Program, InterMedia. Available at www.finclusion.org.

[7] SIM cards were offered from one of the three major mobile network operators (MNO) in Tanzania with strong coverage in their area. MNO agents were on site to register SIMs.

Main Conditions	Sample Size	Proportion Assigned to Cross-Cutting Conditions		
		Training	Solar Charger	Voucher
Control				
SIM + waitlisted for basic phone in year 2	411	0.34	0.14	0.13
Cash				
SIM + PPP US$55	177	0.75	0.29	0.32
Basic Phone				
SIM + Samsung B110 (PPP US$64 value)	384	0.85	0.26	0.25
Smartphone				
SIM + Huawei Y3C (PPP US$186 value)	380	0.85	0.25	0.25

Table 1: **Experimental design.** This table illustrates the distribution of participants across conditions. Column 2 indicates the total number of participants assigned to the main conditions. Columns 3-5 indicate the proportion within the main conditions of those assigned to the cross-cutting conditions. As the design was full-factorial, it was possible for participants to receive multiple cross-cutting treatments.

for a phone in year 2 (serving as a control group).[8] The program also included three cross-cutting conditions: mobile phone training delivered either one-on-one or in a group of roughly 15; solar chargers; or a $15 monthly-credit voucher for 12 months.[9]

To assess the impact of the phone program on individual and household outcomes, we conducted baseline surveys prior to participant invitation to the program as well as midline (t + 6 months) and endline surveys (t + 13 months). Table A2.1 reports descriptive statistics of key covariates from the baseline survey and Table A2.2 reports covariate balance across the study conditions: cash, basic phone, smartphone, solar charges, vouchers and control.

Following our pre-analysis plan, we use treatment assignment to the cash, basic

[8]We also administered a parallel but smaller project among phone owners, who tended to come from wealthier households and were much more active mobile phone and mobile money users, to better understand the migration from basic handsets to smartphones. See A26.

[9]Each cross-cutting condition was theorized to further accelerate mobile phone use by relaxing resource or knowledge constraints (e.g., access to electricity; phone credit; or digital literacy). Yet, generally, each failed to boost mobile phone use, either on their own or in interaction with the phone conditions. See A25.

phone, or smartphone conditions to estimate intention-to-treat (ITT) effects, primarily employing randomization inference (RI) (Gerber and Green, 2012). Using RI, we derive p-values that assess the probability that the treatment effects observed could be drawn from 10,000 alternative random assignments. In the RI analyses we include covariates for the solar charger and voucher cross-cutting treatment conditions, our blocking strata (BRAC or TASAF membership; income; urban or rural), baseline measures on each index when available, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size. We use robust standard errors at the individual-level, the unit of randomization. In the main analysis we follow our pre-registered specification and report the "short" model (treatment conditions without interactions). Following Muralidharan et al. (2019) we also rerun the analysis of the main outcomes employing a fully-saturated "long" model with the main treatment effects and all interactions. The interaction terms are nearly always insignificant, nor do they change the effect sizes on the phone conditions, suggesting it is the receipt of the handsets rather than the solar chargers or credit vouchers that are driving the results. (See A25.)

Findings

Mobile phone uptake and turnover

The program provided SIM cards to all participants, so any effects operate through phone ownership rather than SIM registration. As reported in the top panel of Figure 1 and A4, we observe significant increases in phone and SIM ownership across all conditions. In the control group, rates of mobile ownership returned to pre-baseline

levels, reflecting irregular phone retention in low-income households[10]—in which handsets are acquired and subsequently sold, exchanged, lost or otherwise not retained, and then, for some, acquired again after saving up or receiving a windfall. As is clear in the bottom panel of Figure 1, such turnover in mobile phone ownership is prevalent across all treatment conditions. This is an important finding in and of itself, as there are few panel studies on mobile phone ownership.

As expected, by endline self-reported phone ownership in the treatment conditions far outpaced control (72% in the pooled phone conditions to 27%) but also revealing the attenuating effects of handset turnover. Pinning down the exact mechanisms that account for handset loss among those in the phone groups is challenging because of survey demand effects.[11] We could, however, verify whether participants had the program handset on their person during the endline survey: while 50.7% of those in the basic group still had the basic handset, only 34.2% in the smartphone group did. Below we refer to subjects displaying the project phone at endline as compliers.

The higher levels of churn in the smartphone group stemmed in part from some 40% of participants reportedly trading down their smartphone for a basic handset—often with someone else in their family. (See A4.) Despite the prevalence of such phone trading, individual and household phone ownership were highest in the smartphone condition. At baseline participants reported 0.85 handsets in their households, in the smartphone group this increased to 1.64 phones at endline compared to only 1.44, 1.39 and 1.04 handsets in the basic, cash and control groups, respectively (differences significant at $p<0.001$). This difference in household mobile phone count cannot

[10]Prior to baseline, we screened for phone ownership with a survey question inconspicuously mixed with other asset questions to avoid alerting potential participants that our study focused on mobile technology. So no participants owned a handset at baseline, but roughly 25% reported owning one in past six months.

[11]"Stolen" was the modal reason given for non-retention, but this was unverifiable. Smartphone subjects were more likely to report giving the handset to another family member as well as selling the phone.

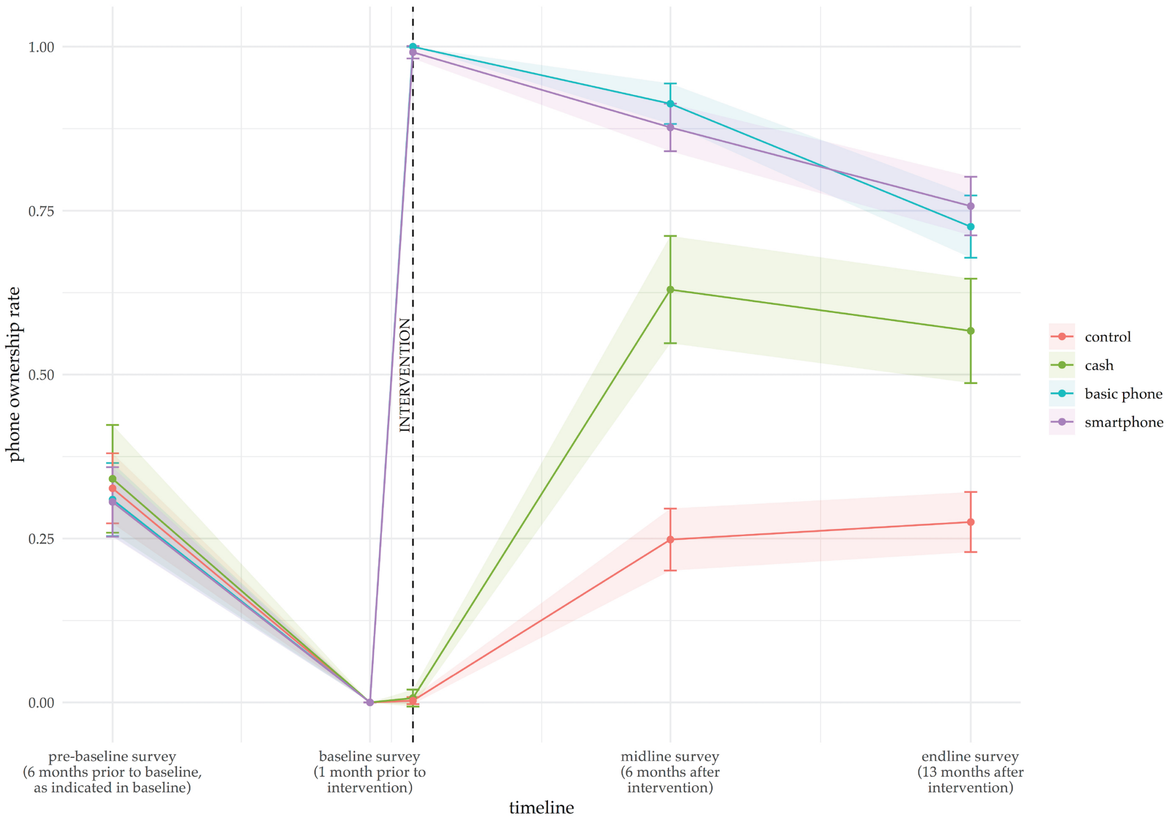
simply be attributed to those in the smartphone group selling their handsets. It holds among compliers as well. One possibility is the smartphone, relative to the basic phone, was more likely to be seen as a complement to the household's existing phone than a substitute.

Finally, we also find high uptake of mobile phones in the cash group. At endline 55% of those in the cash group reported owning a phone.[12] This points to latent demand for mobile technology. But we also have reason to believe the program intervention had an encouragement effect on participants' use of their cash transfers—inducing greater rates of mobile phone purchasing (perhaps by a factor of 6) than if subjects had received a similar cash transfer in a different context.

This estimate derives from comparison to a randomized evaluation of the Productive Social Safety Net (PSSN) program (Rosas et al., 2019), a conditional cash transfer program deployed by TASAF at the same time as our intervention. (Fully 55% of our program participants were TASAF beneficiaries, so PSSN is a good comparison.) PSSN, on average, provided households cash transfers totalling PPP US$588, or $49 bimonthly, disbursed to the women heads of households. Yet, it only increased household mobile phone ownership rates (whether a household owned at least one phone) by 6% (55.8% in control group vs. 59.4% in PSSN group) (Rosas et al., 2019). In contrast, our cash grant caused a 43% increase in household mobile phone ownership—from 52.1% in control to 74.5% in the cash group. We interpret this difference as largely the encouragement effect produced by our program, in which participants were provided with cash grants but also received and were registered on-the-spot for SIM cards, provided mobile phone training, and observed participants in the phone groups receiving handsets.

[12] Based on an endline survey item asking participants if they personally owned a mobile phone. We also asked cash transfer recipients how they used their cash grant: 54% reported buying a mobile phone, followed by 27% who reported buying food.

Thus, as a cash + mobile phone encouragement, our intervention enables us to compare the efficacy of in-kind or cash transfers on mobile phone adoption. From this perspective, cash proved less effective as phone ownership rates lagged those in the pooled phone conditions by some 29 percent. Nor did buying phones with cash make participants any more likely to retain their phones. As illustrated in the bottom panel of Figure 1, a high rate of mobile phone loss is seen across *all* conditions. This suggests turnover was not a by-product of the distribution of cost-free phones. (Also see Figure A5.1.)



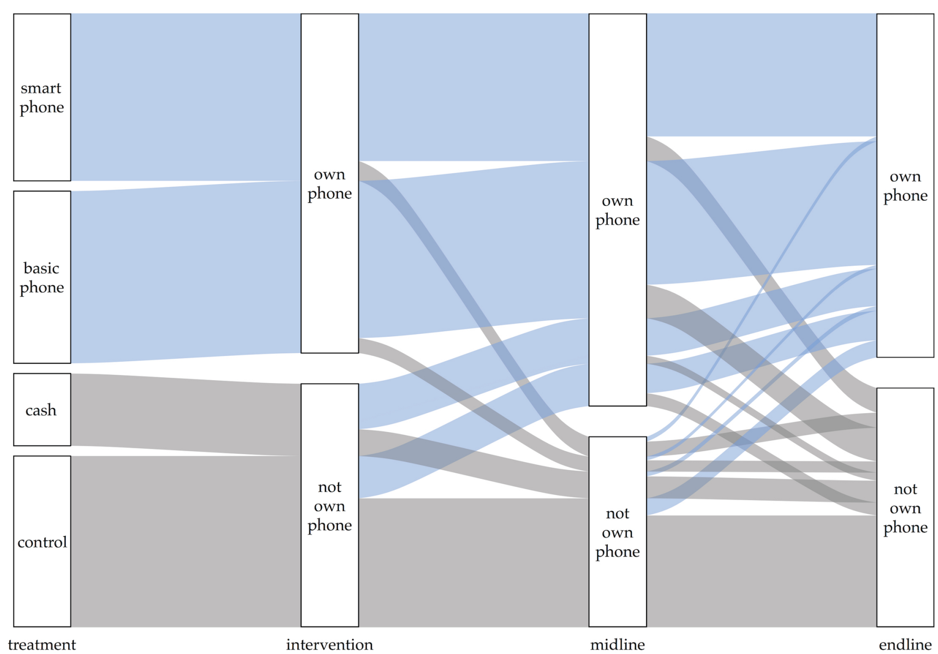


Figure 1: ***Upper panel*: Average phone ownership rates by treatment condition during study period.** The figure depicts the proportion of respondents who reported owning any mobile phone six months before baseline, at midline and at endline. (Shading represents 95% confidence intervals.) As non-phone ownership was a criterion for the study, at baseline none owned a phone. ***Lower panel*: Sankey diagram illustrating change in phone ownership across treatment conditions over time.** "Own phone" indicates ownership of any phone, not necessarily the project phone.

Effects on uptake of mobile money, occupational choice and income

Our primary pre-registered outcomes were the uptake and use of digital financial services (DFS) and the effects on economic well-being as measured by household consumption. We first report the ITT effect of assignment to experimental conditions on indices of overall phone use and DFS uptake. We also administered a pre-registered behavioral measure of mobile money use. After the endline surveys we offered participants a micro-grant, varying the amount if received as cash (PPP US$5.50) or as a mobile money transfer (PPP US$11.00)—both of which were paid out on-the-spot. We then recorded whether participants chose mobile money or cash and, if they chose mobile money, whether they had the offer sent to their own mobile wallet.

Figure 2 reports the results. (For full regression tables and extensions, see Section A10, A11, and A12.) As expected, phone ownership substantially increased phone and mobile money use. In the behavioral test, those assigned to the basic phone and smartphone conditions chose mobile money and had it sent to their own accounts at a respective rate of 40.4% and 33.7% compared to a control mean of 22.8%. As is clear, we do not observe any leapfrogging benefits from smartphones on uptake of mobile money. Indeed, those who received basic handsets were more likely to develop higher levels of mobile money proficiency. This also holds at the household level. See A12.2. In an exploratory analysis to account for this difference, we observe that the effect of handset type on mobile money use is mediated by literacy. Among low-literacy women, the basic phone led to much higher levels of mobile money proficiency than the smartphone; whereas among fully literate participants both handset types caused a similarly large and robust effect. See A12.3.

Beyond enabling use of digital financial services, mobile technology has also been

found to improve access to market information (Jensen, 2007). However, we see null effects on this outcome. (See section A13.) One potential reason to account for this, highlighted in other studies, is that broader market failures limit the informational benefits that come from mobile technology (Aker et al., 2016b; Aker and Ksoll, 2016). While we find no effect of phone ownership on access to market information, we do find, as expected, those in the smartphone group were much more likely to access the internet. Absolute levels of internet use remained very low, however. (See Figure A13.1.)

Other studies highlight the occupational changes mobile technology enables from farming to retail and via rural-to-urban migration (Suri and Jack, 2016; Batista et al., 2018; Lee et al., 2021). In line with these studies, we find mobile phone ownership, especially the smartphone, reduced time spent farming and led to specialization in market trading (i.e., selling produce, cash crops, crafts, etc.) as the primary source of income. (See also Figure A13.1.) This latter change seems to have been a function of enhanced communication capabilities. Among participants who reported using a mobile phone for income-generating purposes, 63% stated the primary purpose was communicating with customers versus only 3.4% who said they used it to make and receive payments. (See figure A13.2.) The increased use of the phone for economic activities, however, did not correspond to a robust increase in self-reported income (while we see some movement on weekly income especially among the basic phone group, we see null effects on monthly income). (See A14.)

Effects on household economic well-being

To measure the impact of increasing women's mobile phone ownership on the household's economic well-being, we employed a consumption module that provides an

extensive picture of expenses. First, we focus on spending on the mobile phone itself (e.g., for airtime credit or data) as a validity check of continued investment in the technology a year later. Consistent with the effects on the mobile phone use index, we see strong treatment effects on mobile spending, with the highest effects in the smartphone group (0.35 SD increase compared to control). (See Figure A17.1.) This is driven by those in the smartphone condition who possessed their smartphones at endline. In Figure 2, we turn to overall household spending, first excluding mobile spending to test for broader welfare gains. We then report total household consumption, including mobile expenditures.

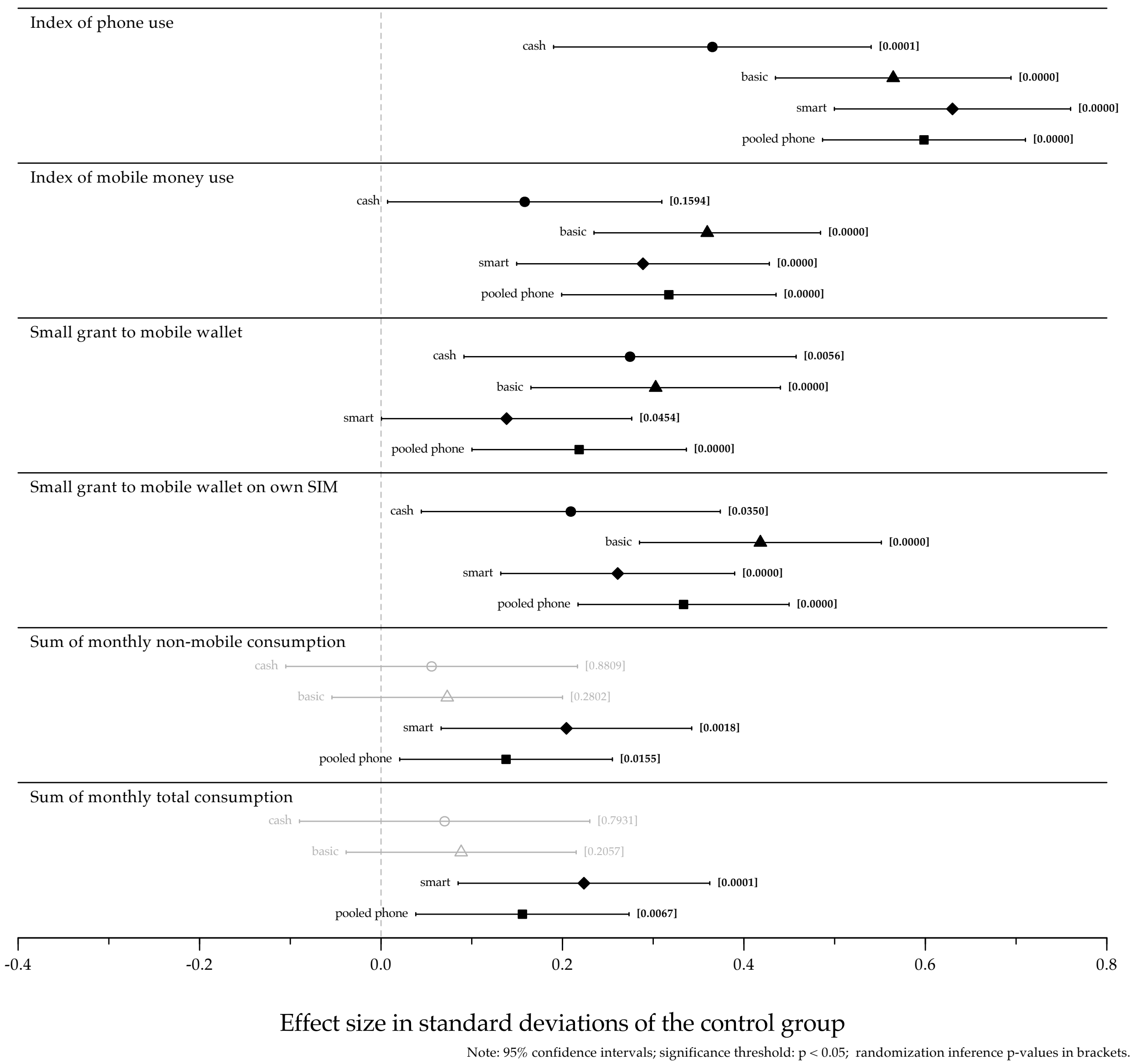


Figure 2: **The impact of treatment assignment on mobile phone use, mobile money use and household consumption.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size. For full regression results and formal tests of significance between treatment conditions, see Section A10.

Overall, we find the provision of phones caused a significant increase in household consumption. Participants in the pooled phone conditions report a 13% increase in the sum of non-mobile consumption baskets (0.14 SD increase) and a 14% increase in total consumption over control (0.16 SD increase). As seen in Figure 2, assignment to the smartphone condition produced the largest and most robust effects (0.20 SD and 0.22 SD increase, respectively, in non-mobile and total consumption over control). The smartphone effect was 3 times higher than the basic handset (p=0.12) and 3.6 times higher than the cash grant (p=0.09). Reported p-values for the pooled and smartphone conditions compared to control are robust to multiple-comparisons adjustment using the Benjamini-Hochberg procedure (Benjamini and Hochberg, 1995), and the smartphone p-values also clear the recommended redefined threshold for statistical significance of 0.005 (Benjamin et al., 2018).

By all indications these consumption gains operated through participants' keeping and investing in the smartphone rather than selling it. Figure 3 analyzes only those in the smartphone condition, comparing consumption levels at baseline and endline among compliers and non-compliers. As is clear, we observe increases only among compliers—indicating that the largest consumption gains correspond to those who *did not* sell their handsets. Consistent with this, the complier average causal effect (CACE) of smartphone ownership on consumption is 3.5 times that of the ITT. See section Figure A16.1. Additionally, as discussed below and in Section A19, the smartphones did not simply lead to a one-to-one consumption gain but had a multiplier effect—the total consumption increase to the household was 3.5 times the value of the smartphone handset.[13] Finally, these gains were distributed across a

[13]The average resale value of the smartphone was only 75,000 TZS, or 56% of original cost. If the consumption gains primarily operated from smartphone liquidity, this would require the cash proceeds to lead to a 6-fold increase in household consumption, which seems implausible compared to existing cash transfer studies that point to a 1.1:1 effect of cash on consumption (Haushofer and Shapiro, 2016).

diverse set of consumption baskets: education, fuel, transportation, contributions to social functions and community activities, entertainment, and health. One concern is that this spending was financed through mobile loans, resulting in higher levels of indebtedness. Smartphone participants reported taking out more mobile loans; however, this did not significantly increase indebtedness. (See Figure A17.2.)

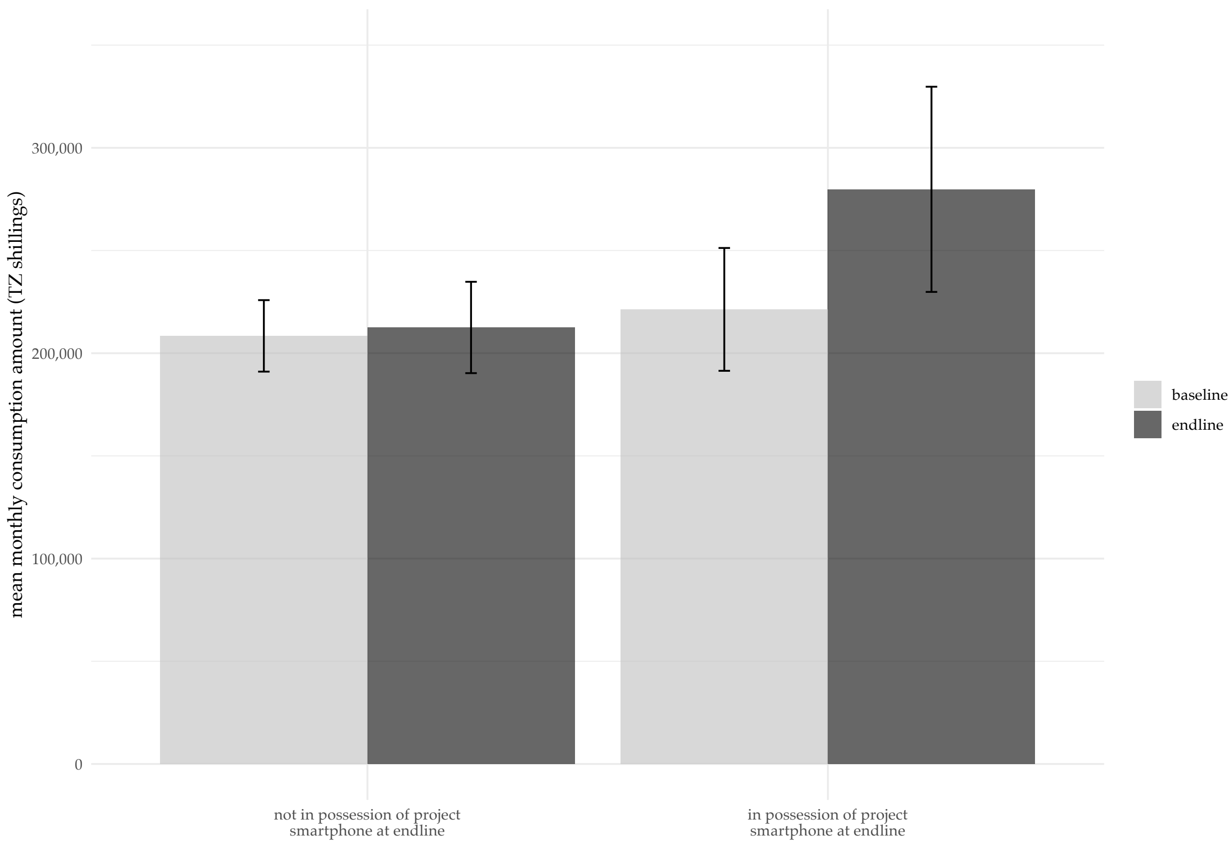


Figure 3: **Household consumption among smartphone compliers and non-compliers.** Analysis is among smartphone recipients only. Bars indicate mean total monthly consumption at baseline and endline across smartphone compliers (those verified to have the project smartphone on their person at endline) and smartphone non-compliers (those who could *not* show the project smartphone at endline). 95% confidence intervals.

We preregistered several subgroup analyses to check the mediating effects of various dimensions on mobile phone impact, including literacy and among farmers in rural

localities. The former is reported in Section A18. We find a significant mediating effect of literacy. Among those who were fully literate at baseline, smartphones led to a 0.40 SD increase in household consumption compared to control. This is an additional piece of evidence in support of the smartphone-use channel. Women with higher levels of literacy were much more likely to be in possession of the smartphone at endline and reported significantly higher levels of phone use. We also observe significant literacy subgroup effects on household consumption in the basic phone condition.

In the second subgroup analysis, we compare effects based on participants' organizational membership, either microfinance (BRAC, 45% of sample) or poverty reduction program (TASAF, 55% of sample)—one of our blocking criteria and which reflects urban/rural-smallholder farmer differences. The results suggest that among the poorest households (those in the poverty reduction program subgroup) both the basic handset and smartphone significantly increased household consumption. Whereas among the microfinance sub-sample, only the smartphone significantly boosted living standards (Figure A18.2). As reported in Table A18.1, these subgroup effects track the degree to which the treatments increased the number of handsets in the household. For whatever reason, those in the microfinance group were less likely to retain the basic handset, attenuating their household's mobile phone capacity and corresponding with limited consumption gains.

From a benefit-cost perspective, mobile phones appear to be an efficient means to increase living standards of low-income households. As our cash group does not enable a pure cash benchmark due to the mobile encouragement effects, we compare our estimates to the PSSN cash transfer program in Tanzania as more than half our sample were TASAF beneficiaries and the evaluation of the intervention occurred at the same time as our study. As noted among the TASAF participants (the poverty reduction program group), both basic and smartphones caused a significant increase

in consumption, so we pool the phone groups. Using the "long" model to isolate the effects of the phones independent of interactions with the cross-cutting conditions, we estimate that in the TASAF subgroup mobile phones caused a 16% increase in monthly consumption. Overall the PSSN caused a slightly higher increase in monthly household consumption (19.5%) but the average cost of the PSSN cash transfers over two years was three times that of the smartphone and nine times that of the basic handset ($588 to $186 and $64, respectively), pointing to the cost effectiveness of mobile phones as an anti-poverty intervention.[14] One important caveat, of course, is that our mobile phone study was only administered for 13 months. Additional research is needed to assess the durability of these effects over the long-run.

Exploratory Analysis of Mechanisms

Here we assess possible mechanisms that account for the positive effect of smartphones on household consumption. This goes beyond our pre-registration so should be seen as exploratory. As a multifaceted technology, the smartphone in effect is a bundled treatment that enables improved communication capabilities, access to information, and use of mobile money. Furthermore, as we have shown, smartphones are likely to be shared and thus may have significant externalities for other household members. We address each of these dimensions as much as our data allow.

First we report the results of a mediation analysis to better understand the impact of the smartphone on household consumption. Despite the high turnover in smartphones observed, recall that the strongest consumption effects accrued to those women still in possession of the smartphone at endline—pointing to participants' use of the phone as

[14] As we do not know the overall program costs for PSSN, here we focus the comparison on the handset and cash transfer values. In Table A19.1, we estimate the benefit-cost ratio including program costs and compare to other anti-poverty programs.

an important channel. We focus on three potential individual mediators that reflect different capabilities enabled by smartphones—communication; mobile money; and internet access—which we measure using our mobile phone use index, mobile money index, and internet access variable, respectively. We also include a mediator to capture household effects as measured by mobile phone capacity. The results are reported in Section A20. We find the index of phone use mediated some 24% of the effect. This largely captures making and receiving calls, and corresponds with reports of using the phone to aid in market trading and communicating with customers. In contrast, a smaller and statistically insignificant share of the consumption effect is mediated by mobile money use. A bit surprisingly, given how low internet use was in absolute terms, we find a more significant role for this channel (mediating some 18% of the effect). Even accounting for increased phone use among individual recipients, we observe a substantial share of the effect (36%) stems from the increase to the household's mobile phone capacity (as measured by a count of the phones in the household). The smartphone led to a doubling of mobile phone capacity such that at endline nearly 50% of households possessed two or more phones—enabling two adult income earners to each use their own handset—compared to only 25% in the control condition. Overall, this suggests closing the mobile gender gap increases household welfare both by boosting women's mobile connectivity *and* reducing the constraints and opportunity costs that existing phone owners face from joint dependence on a single handset.

Unfortunately, we do not have data on other family members' phone use and economic livelihoods, which makes it difficult to precisely estimate smartphone externalities. Descriptively, we do note, however, that the strongest consumption gains within the smartphone condition were among those participants who reported that besides themselves their spouses also used their smartphones (see Figure A21.1 and

A21.2)—which may have been another channel through which the handset increased household welfare. Without additional information, there are limited inferences we can draw from this pattern. We only point to it as a call for future research on the intersection of household dynamics and smartphone technology.[15]

Whereas existing household dynamics may have structured the impact of mobile technology, the effects of women's mobile phone ownership on participants' levels of empowerment is more mixed—at least after thirteen months (A22). On some dimensions, such as participation in the formal economy and social connectedness, phone ownership exhibited significant positive effects. Yet on others, such as political engagement, individual efficacy, and household influence, we find null results. (See Figure A22.1 and Figure A22.8.) Finally, we find no effects on intimate partner violence. However, among basic phone recipients, we do observe marginal upticks in reports of humiliation and threats from someone other than spouses or partners. This points to the importance of shifting community norms around women's rights to own mobile phones.

Another important scope condition is that the marginal effects of such a phone intervention appear greatest among households without full phone saturation among adult income-earners (less than one handset per adult). In a smaller parallel experiment among phone owners from wealthier households with full phone saturation (Experiment 2), receipt of a smartphone or the equivalent value as cash had positive (but substantively weaker) effects on phone use and null effects on mobile money use

[15] Adding to the importance of this line of inquiry, we also learn that how other household members used the phone may have been important. When participants reported their spouses, and especially sons, appropriated the handsets, allowing little participant use, the household experienced no rise in consumption and, if anything, was left worse off. While this ex-post analysis should be taken with caution as the patterns are merely descriptive and require the comparison of small sub-groups, these exploratory findings point to the potential underlying effects of household bargaining and gender norms on the impact of smartphones, resonating with other research on this topic (Barboni et al., 2018).

and household consumption. (See A26.) It appears, as participants in this second experiment at baseline tended to be much more active mobile phone and mobile money users, the receipt of a smartphone did not have the same transformative effects on household consumption—at least as observed among this smaller sample during the study period. We did, however, observe robust effects on internet access and use in this second experiment (see Figure A26.2).

Conclusion

Overall, our results affirm the benefits of closing the gender gap in mobile phone ownership, which continues to persist in many emerging economies (Women, 2020). Our analysis indicates that boosting women's mobile phone ownership and control, especially of smartphones, brings significant economic dividends. However, the observed individual and household economic gains emerged only in the face of substantial turnover in mobile phone ownership. This suggests that the mobile gender gap stems not only from barriers women face to obtaining handsets but also the challenges of retention when confronted by significant financial and social constraints. Also, as is increasingly recognized, our study demonstrates just how important literacy is to mobile technology adoption—both in terms of mobile money uptake and smartphone utilization. Unless more is done to ensure equitable use, the mobile phone revolution may merely deepen structural disparities.

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Appendix: The Economic Impact of Mobile Phone Ownership

Table of Contents

A1 Background and Methods

Here we report the details of our experimental design, including recruitment of participants, blocking and assignment to treatment.

Research sites and recruitment of participants

To undertake the RCT, we collaborated with two well-established implementing partners, BRAC and the Tanzania Social Action Fund (TASAF), with a national presence in Tanzania and extensive experience working with a range of women clients (e.g., market women, smallholder farmers, educators, small-business owners, the unemployed) to recruit our target of 1,350 non-phone owners (Experiment 1) and 650 basic phone owners (Experiment 2). As participants were existing beneficiaries of TASAF and BRAC, this represents an important scope condition of our study—we are able to precisely estimate the impact of mobile phone ownership among women cash transfer recipients and microfinance clients. (Importantly both groups represent a sizable subset of Tanzania's population. TASAF covers roughly 10% of Tanzanians and BRAC reaches 200,000 beneficiaries in Tanzania of which 98% are women). In future studies it will be important to test the impact of mobile phone ownership among those not participating in such programs.

We purposively selected five different areas of Tanzania in which to work to ensure coverage of the country's major geographic regions (e.g., lakes, coast, Arusha, and Southern Highlands). Within these regions, we used data from where BRAC operates to ensure we had coverage of urban, peri-urban, and rural areas. See map of study sites.

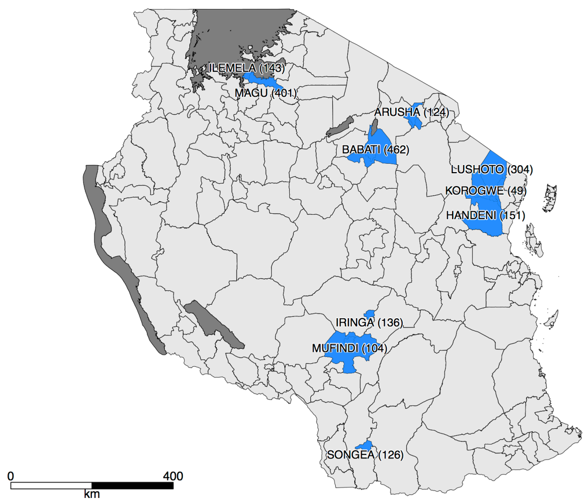

Figure A1.1: Illustrates experimental sites across Tanzania with total participants in both experiments for each district reported in parentheses.

To recruit our sample, we worked closely with local-level BRAC and TASAF officers to locate potential study participants. Initially, we only partnered with BRAC, but the mobile penetration rate among BRAC clients was very high and we were unable to recruit a sufficient number of non-phone owners for experiment 1. Thus, we established a partnership with TASAF, which targets the poorest Tanzanians in its programming, and it was more easily able to help recruit non-phone owners. (In Experiment 1, 54 percent of participants came from TASAF; whereas in Experiment 2, 79 percent came from BRAC.) We stratified our randomization by development partner (BRAC or TASAF) to ensure group membership was balanced across conditions. Figure A1.2 depicts a flow diagram of participant screening, recruitment, receipt of intervention and evaluation.

For our recruitment process with BRAC, the microfinance organization provided us with lists of members across the branches from which we were sampling, as well as some information about past lending history. We used these data to select a set of

BRAC lending groups that had the lowest average past loan amounts (in an effort to identify those least likely to own a phone). Where possible, we randomly ordered these groups before selection. For recruitment of TASAF members, we worked with TASAF staff to either gather groups of TASAF women together or to visit the women in their homes.

Prior to the baseline survey, we administered a short screening questionnaire to each prospective participant about their household characteristics (type of roof, house/dwelling, land, radio ownership) and whether they personally owned a mobile phone. (We embedded the mobile phone question among these other questions so as to avoid alerting participants at the recruitment stage that this program centered on mobile technology.) We used this information to build a sample of 2,232 women, from which we then selected 2,000 participants to take part in the study. Among this larger sample, we excluded participants who already owned smartphones (Experiment 2 was only among basic phone owners to test migration from a basic phone to a smartphone, see A26). Given the generally older age-set of our recruited population relative to the general female population of Tanzania, we excluded the oldest participants. In experiment 1, this included those who reported being older than 82; and in experiment 2, those older than 64.

Experimental intervention and data collection

Once we selected the pool of participants, we then conducted the full baseline survey administered by 30 female field research assistants trained by REPOA in survey methodology. The pre-study survey sought to establish baseline levels of mobile phone access, uptake and use of digital financial services, socio-economic characteristics, and overall welfare.

After the baseline survey, we introduced the respondents to the Mobile Phone

and Livelihoods of Women Program. We explained that BRAC and TASAF were partnering with REPOA and the major mobile network operators of Tanzania to undertake the program, which was designed to improve women's access to and use of mobile technology. Those who chose to participate in the program would be eligible for a basic phone (Samsung B110) (for non-phone owners), a smartphone (Huawei Y3C), unconditional cash grant (either 40,000 TZS, ∼ PPP US$64, or 130,000, ∼ PPP US$186), a solar charger (Sun King Pro 2), or monthly mobile credit vouchers. It was also explained that the program would run for two years with some receiving mobile technology in year 1 and others in year 2, and that a lottery would determine which items participants would receive and whether in year 1 or year 2. Those assigned to year 2 served as the control group and did not receive their mobile phones (a basic phone in experiment 1 or a smartphone in experiment 2) until after the endline—13 months from the outset of the study.

After the baseline, using the randomizr package in R we randomly assigned participants to the main treatment conditions (cash, basic phone, smartphone) or control, stratifying on income level, rural/urban location, and BRAC/TASAF membership. In a full factorial design, participants were simultaneously block randomized into different cross-cutting treatment conditions: a.) different types of training—individual, group, or no training; b.) solar chargers; or c.) mobile credit vouchers. Training was concentrated in the phone conditions to enable the pre-registered comparison of group versus individual training on phone recipients. The overall distribution of treatment conditions in Experiment 1 is reported in Table 1 in the main paper.

After assignment to treatment, our team of REPOA field assistants working with BRAC and TASAF invited all participants (including those in the control) to the kickoff of the Mobile Phone and Livelihoods of Women Program. We informed participants to make sure they brought an identification card for SIM registration. For those without

an ID card, we asked them to receive a letter from the local authorities to ensure they could register their SIM card. At the program, all participants, including those in control, were offered a SIM card that was registered on site by an agent from one of the three major mobile network operators in Tanzania. We used the quality of the mobile network in the area to determine which SIM card to provide. Thus, a single mobile network operator registered participants at each research site. Providing registered SIMs to all participants was intended to enable us to use call detail records to compare actual mobile use across treatment conditions. However, SIM churn and regulatory changes prevented us from obtaining complete call record details for all participants. Nonetheless, this design choice had the effect of ensuring we did not stack the deck in favor of those in the phone groups by giving them a double advantage—a handset and a registered SIM card (the costs of acquiring and registering a SIM card are non-trivial, especially for the poor). In fact, the intervention increased SIM ownership in the control condition nearly two-fold, as shown in A4 (from 32.7% at baseline to 63.9% at endline).

In eliminating the barriers to acquisition and registration of SIM cards for all participants, including control, the experiment represents a conservative test of the impact of mobile phone ownership as it compares phone-and-SIM owners to a SIM-only owner rather than to one with no mobile connectivity at all (no handset and no SIM). Yet, in reality most non-phone owners do not possess a SIM card. According to the 2017 Financial Inclusion Insights survey in Tanzania, 80% of non-phone owners in Tanzania did not have a SIM card.

After receiving a SIM card, participants were then given a ticket to receive their assigned program items—a basic handset, smartphone, cash grant, solar charger, vouchers, or training. In the full factorial design, some women received more than one item as randomly assigned. Upon receiving the items, participants were provided

with a certificate stating the name of the program, the participant's name, and the technology received. Based on our piloting, providing such certificates increased women's sense of ownership and helped validate to family members and others from where they received the mobile technology.

To assess the impact of the phone program on participants' uptake and usage of mobile money and their economic well-being, midline and endline surveys were conducted at 6 months and 13 months. Attrition (survey non-response) was higher at midline than at endline with 84.5% and 94.2% of participants interviewed, respectively. Midline attrition was attributed to farming seasonality. At midline, differential attrition was observed with those in the control and cash groups significantly less likely to be surveyed. (See A3.) Phone ownership potentially made it easier to reach those in the phone groups during this period. At endline attrition was much lower, but remained differentially higher among the cash group. Therefore, in our analysis we focus on endline results comparing the phone treatments to control.

Our primary pre-registered outcome was participants' uptake and use of digital financial services (DFS)—that is, the use of a mobile-based wallet for money transfers, deposits, withdrawals, savings, and loans. We used both survey and behavioral measures of DFS use. Survey measures included questions about whether the participant had a mobile money account, how often she used mobile money to save money, times sent and received mobile money transfers over the past month, etc. (See Table A8.1 for complete list of indicators and descriptive statistics.) We used these individual indicators to create a composite index of mobile money use, employing inverse covariance weighting following Anderson (2008) and code from Samii (2016). Each standardized index has a mean of 0 and a standard deviation of 1. (This method drops missing values. Also as shown in A8, missing observations are generally not correlated with treatment. Nonetheless, we report the results from all analyses imputing missing

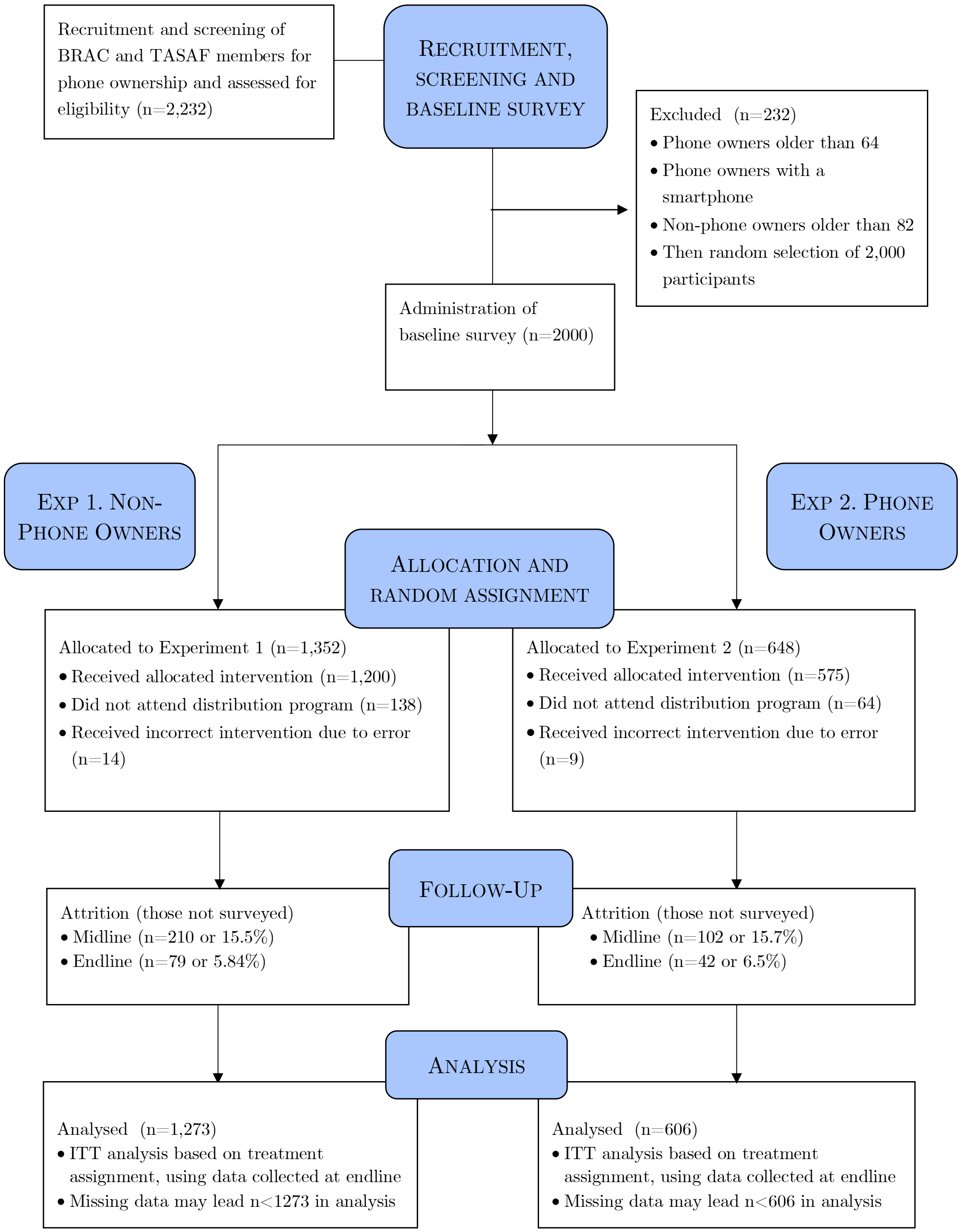

Figure A1.2: A CONSORT diagram of participant recruitment, screening, assignment to treatment, and evaluation as part of the Mobile Phone and Livelihoods of Women Program and experiment.

observations from baseline outcome measures. The results are highly consistent when imputing missing observations as those reported in A9.)

To supplement the survey indexes, we also administered a pre-registered behavioral measure of mobile money use. After the endline surveys we offered participants a small cash grant, varying the amount if received as cash (PPP US$5.50) or via mobile money (PPP US$11.00)—both of which were paid out on-the-spot. We then recorded whether participants chose mobile money or cash and, if they chose mobile money, whether they had the offer sent to their own mobile wallet or someone else's account. We view this as a behavioral test of the use of DFS.

To test potential effects on household welfare, we employed a consumption module that provides an extensive picture of expenses. This measure was adapted from Suri and Jack (2016). It includes survey questions on recent spending across 15 different baskets, covering common items such as food, fuel, transportation, water, and electricity, as well as community functions and investment in education and healthcare. As these items are discrete and cover a wide range of expenditures, their sum should be relatively insensitive to social desirability bias. In our analyses we compare the phone conditions to control.

We report consumption results without mobile spending (e.g., buying airtime, data, or mobile money transactions) to test if the intervention led to broader welfare gains rather than just increasing demand for phone use. We also report total household consumption, including mobile expenditures, to ensure we capture as complete a picture of a household's living standards as possible.

To ensure the results are not sensitive to this aggregation decision, in the appendix (see A15) we re-estimate the consumption results: dropping any inconsistencies between amount and range answers; without winsorising; taking the row mean of the consumption components (thus, setting missing values as equivalent to the mean of the

reported consumption components); coding non-reported consumption components as 0 before summing; logging consumption; and calculating consumption per capita. (We also imputed non-reported consumption components from overall household consumption at baseline before summing the baskets, as reported in A9.) All aggregation measures produce very similar results.

The final set of measures cover different dimensions of empowerment: subjective welfare, formal economic engagement, allocation of time, social connectedness, individual efficacy, household bargaining, intimate partner violence, norms of gender equality, and political engagement. We use inverse covariance weighting to construct indexes for these various survey-based measures. For components of indexes and descriptive statistics, see A8.

Analysis methods

Following our pre-analysis plan, we use treatment assignment to estimate intention-to-treat (ITT) effects, primarily employing randomization inference (RI) (Gerber and Green, 2012). Using RI, we derive p-values that assess the probability that the treatment effects observed could be drawn from 10,000 alternative random assignments. In the RI analyses we include the solar charger and voucher cross-cutting treatment conditions, our blocking strata (program membership, income, urban or rural), baseline measures on each index or a core variable in the index when available, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size. We use robust standard errors at individual-level, the unit of randomization. In the main analysis we follow our pre-registered specification and report the "short" model (treatment conditions without interactions). We also rerun the analysis of the main outcomes employing a fully-saturated "long" model with the main treatment effects and all interactions (Muralidharan et al., 2019).

The interaction terms are nearly always insignificant nor do they change the effect sizes on the phone conditions, suggesting it is the receipt of the handsets rather than the solar chargers or credit vouchers that are driving the results. (See A25.)

A2 Summary Statistics of Main Variables and Balance Across Treatment Conditions

Given our targeting of non-phone owners in the main experiment, the sample of women tended to be older and have lower socioeconomic levels than the female population of Tanzania. Table A2.1 reports summary statistics comparing participants from Experiment 1 (non-phone owners) and Experiment 2 (phone owners) along with the female population of Tanzania on selected variables (as measured in the nationally representative 2016 Financial Inclusion Insights (FII) Tracker Survey undertaken around the same time as our baseline survey.) The summary statistics also reveal qualitative differences in the characteristics of participants in experiment 2 compared to experiment 1.

	Experiment 1				Experiment 2		Female Pop. TZ 2016	
	BRAC		TASF					
Variable Name	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Age	38.74	11.54	49.76	16.29	39.62	9.60	33.76	13.59
Education	7.32	3.56	4.96	3.65	8.61	3.07	NA	NA
Completed Primary School	0.68	0.47	0.34	0.47	0.85	0.79	0.74	0.44
Married	0.79	0.41	0.55	0.50	0.63	0.48	0.54	0.50
Full Literacy	0.58	0.49	0.28	0.45	0.73	0.45	0.56	0.50
Weekly Income Range	7.29	3.87	5.44	2.66	7.77	4.18	NA	NA
Head of Household	0.24	0.43	0.44	.50	0.39	0.49	0.26	0.44
Farmer	0.16	0.37	0.55	0.50	0.13	0.34	0.34	0.47
Own SIM	0.53	0.50	0.15	0.36	0.99	0.06	0.60	0.49
Own Mobile Money Account	0.40	0.49	0.09	0.29	0.87	0.34	0.45	.50
Phones in Household	1.09	0.90	0.66	0.84	2.16	1.10	1.46	1.09

Table A2.1: Baseline summary statistics in experiment 1 and experiment 2 compared to the female population in Tanzania (2016)

Table A2.2 reports the results of OLS regressions of baseline covariates on assignment to the main treatment conditions to check for statistical balance in experiment 1. Across all baseline covariates, mean levels in the treatment conditions are not statistically different from control (the reference category), indicating successful random assignment.

	(1) Urban	(2) Income (L/H)	(3) TASAF	(4) Age	(5) Literate	(6) Education
Cash	-0.009	0.026	0.011	-1.082	0.040	-0.119
	(0.035)	(0.046)	(0.045)	(1.285)	(0.045)	(0.351)
Basic	-0.004	0.012	0.003	-0.627	0.045	-0.082
	(0.028)	(0.036)	(0.036)	(1.109)	(0.036)	(0.270)
Smart	-0.010	0.007	-0.002	0.130	-0.004	-0.293
	(0.028)	(0.036)	(0.036)	(1.127)	(0.035)	(0.272)
Solar	0.010	-0.008	0.010	-1.057	-0.002	-0.070
	(0.028)	(0.035)	(0.035)	(1.067)	(0.035)	(0.262)
Voucher	0.004	-0.015	-0.017	1.338	-0.046	0.077
	(0.028)	(0.035)	(0.035)	(1.059)	(0.035)	(0.256)
Joint test p-value	0.99	0.99	0.99	0.72	0.48	0.93
Control mean	1.187	0.504	1.550	45.063	0.403	6.139
Observations	1352	1352	1352	1352	1329	1351

	(7) Married	(8) HH Size	(9) HH Consumption	10) HH Phone Count	(11) MM Account	(12) Phone in Past
Cash	0.016	0.015	27127.263	0.064	0.048	0.016
	(0.042)	(0.220)	(14523.830)	(0.082)	(0.041)	(0.045)
Basic	-0.007	0.019	6485.598	0.005	-0.040	-0.005
	(0.034)	(0.181)	(10610.523)	(0.063)	(0.030)	(0.036)
Smart	-0.024	0.079	3424.640	0.038	-0.025	-0.023
	(0.034)	(0.188)	(10717.261)	(0.065)	(0.030)	(0.036)
Solar	-0.004	-0.049	-6441.607	0.001	-0.005	0.023
	(0.033)	(0.167)	(10107.178)	(0.059)	(0.029)	(0.035)
Voucher	-0.064	0.010	7539.605	-0.033	0.000	0.015
	(0.034)	(0.170)	(11153.201)	(0.063)	(0.030)	(0.035)
Joint test p-value	0.37	0.99	0.53	0.95	0.34	0.89
Control mean	0.674	5.533	207766	0.835	0.241	0.550
Observations	1352	1352	1352	1352	1352	1349

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2.2: OLS regressions checking statistical balance in experiment 1 of key covariates across main treatment conditions compared to control (the reference category).

	(1) Cash	(2) Basic	(3) Smart	4) Solar	(5) Voucher
Joint F-test of treatment with key baseline covariates	0.71	0.56	0.29	0.40	0.96
p-value	0.74	0.88	0.99	0.97	0.48

Table A2.3: Joint statistical significance of each treatment in experiment 1 with 12 baseline covariates reported in Table A2.2.

A3 Program Non-compliance and Attrition

Critical to the efficacy of the intervention was participants' attendance at the Mobile Phone and Livelihoods of Women Program distribution. In the invitation to the program, all participants were informed that: the program was funded by the Bill & Melinda Gates Foundation; it was in collaboration with BRAC, TASAF, and the three major mobile network operators in Tanzania; that as part of the program they would be entered into a lottery or random draw to be eligible to receive mobile technology items at the training meeting, including a new mobile phone, solar charger, and/or airtime and data; and that they must be present at the meeting to be eligible for the random draw. The results of the lottery were disclosed at the program distribution; no one could have known her assignment ahead of time.

Of those invited, 10.3% in experiment 1 and 9.9% in experiment 2 did not attend program distribution. Here we document the correlates of program non-compliance. As illustrated in Table A3.1, the strongest correlate of program non-compliance was age: both the youngest and oldest participants were more likely to not show up for program distribution.

	(Exp. 1) Program Non-compliance	(Exp. 2) Program Non-compliance
Cash	0.055	-0.010
	(0.031)	(0.035)
Basic	0.018	
	(0.022)	
Smart	-0.034	-0.002
	(0.020)	(0.026)
Solar	-0.019	0.015
	(0.019)	(0.027)
Voucher	-0.029	0.026
	(0.019)	(0.026)
Age	-0.010**	-0.020
	(0.004)	(0.010)
Age^2	0.000*	0.000
	(0.000)	(0.000)
Married	-0.026	0.030
	(0.020)	(0.024)
Education	-0.003	0.006
	(0.003)	(0.004)
Own Phone in Past	-0.007	
	(0.017)	
HH Size	-0.004	-0.006
	(0.003)	(0.005)
Urban	0.020	-0.046
	(0.026)	(0.050)
TASAF	-0.008	-0.057
	(0.023)	(0.051)
Income (L/H)	0.005	0.017
	(0.020)	(0.024)
Control mean	0.102	0.097
Observations	1352	648

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3.1: Analysis of Program Non-compliance. OLS regression of non-attendance of program distribution on treatment conditions and covariates.

Midline and endline surveys were conducted at 6 months and 13 months. Attrition was higher at midline than at endline with, respectively, 84.5% and 94.2% of participants in Experiment 1 interviewed. Figure A3.1 reports attrition across treatment conditions. Midline attrition was attributed to farming seasonality. Phone ownership potentially made it easier to reach those in the phone groups during this period. At endline attrition is much lower and only deferentially higher in the cash group.

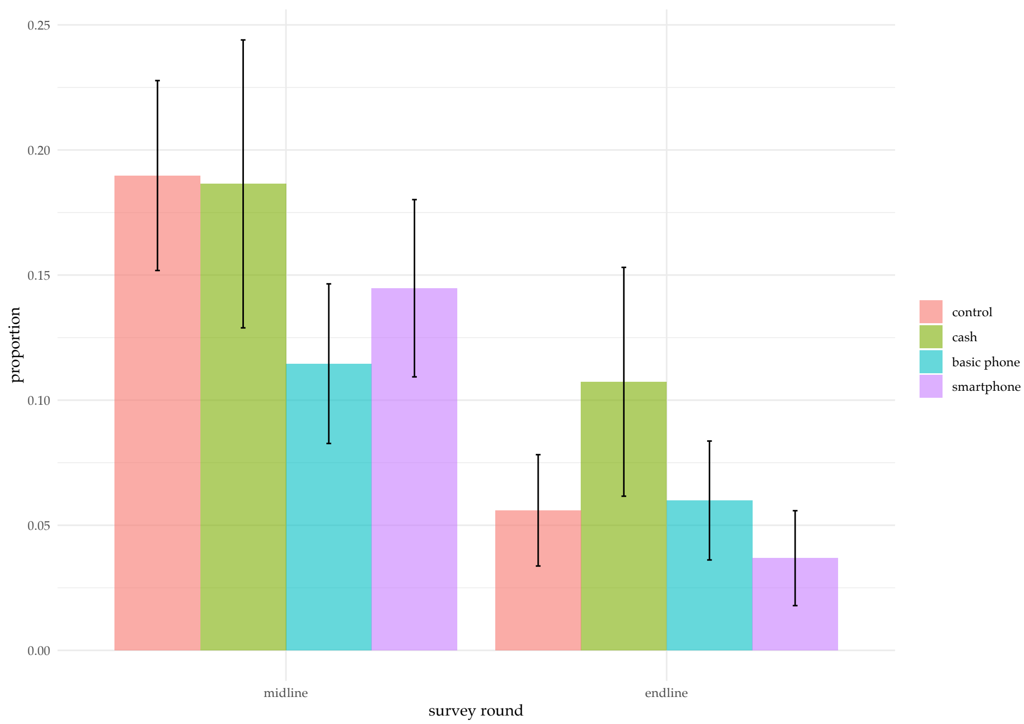

Figure A3.1: **Participant attrition at midline and endline surveys across treatment conditions.** The y-axis indicates the proportion of participants who could not be surveyed for a given survey round.

A4 Handset and SIM Turnover Across Treatment Conditions

The experimental intervention caused significant changes in handset and SIM ownership over time. There was non-trivial decay in possession of both SIM cards and handsets over time.

Figure A4.1 reports participant SIM ownership during the period of the study. As all participants were offered SIM cards, we see significant increases in SIM ownership across all conditions, including the control. Those in the treatment conditions tended to retain a SIM card at a higher rate, however.

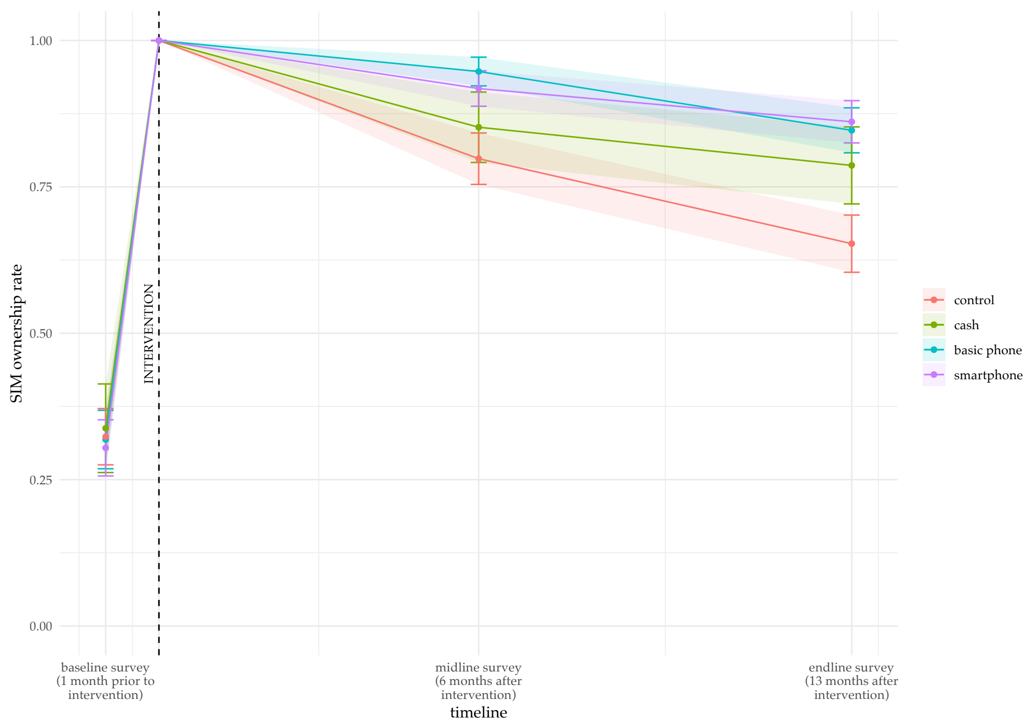

Figure A4.1: **Ownership of at least one SIM card before and throughout the study period.** (Shading represents 95% confidence intervals.) This figure excludes those who did not attended program distribution.

Figure A4.2 delineates the type of phones participants in the phone conditions

report owning at baseline and endline. It indicates the high-rate of handset turnover in both the basic and smartphone conditions but also reveals that many in the smartphone condition traded "down" their smartphones for basic handsets.

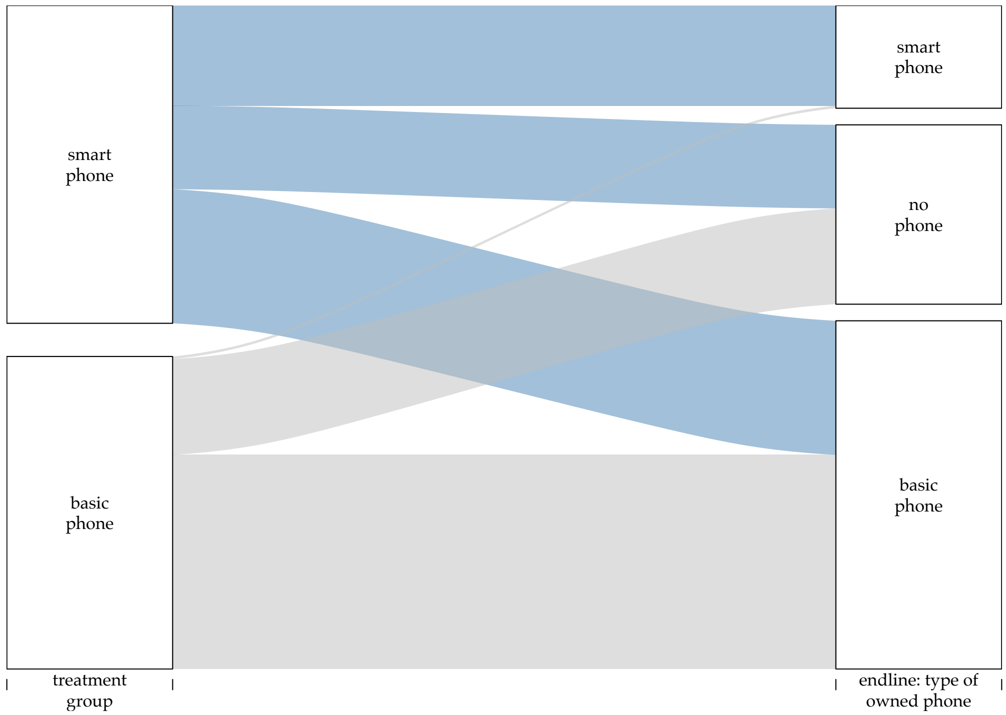

Figure A4.2: **Sankey diagram documenting change in handset type among those in the basic phone and smartphone conditions from intervention to endline.** Handset type and phone ownership status are self-reports from endline survey.

Figure A4.3 and Figure A4.4 provide additional information on handset turnover. Figure A4.3 reports the results from a series of questions we ask phone recipients to probe whether they still own the project phone. First we ask whether they own *any* phone? Second do they still have the project phone? If yes, do they have it with them? If yes, can they show our field assistant the phone? Does the handset match the types of basic phones and smartphones distributed? As is clear, participants were much more likely to have the basic phone on their person at the endline of the study.

Figure A4.4 then probes what happened to the project phone, among those who

said they no longer have the phone given to them as part of the Mobile Phone and Livelihoods of Women Program. Those who received smartphones were more likely to report giving to a family member or friend and to have sold the phone—though generally reports of selling the handset were low.

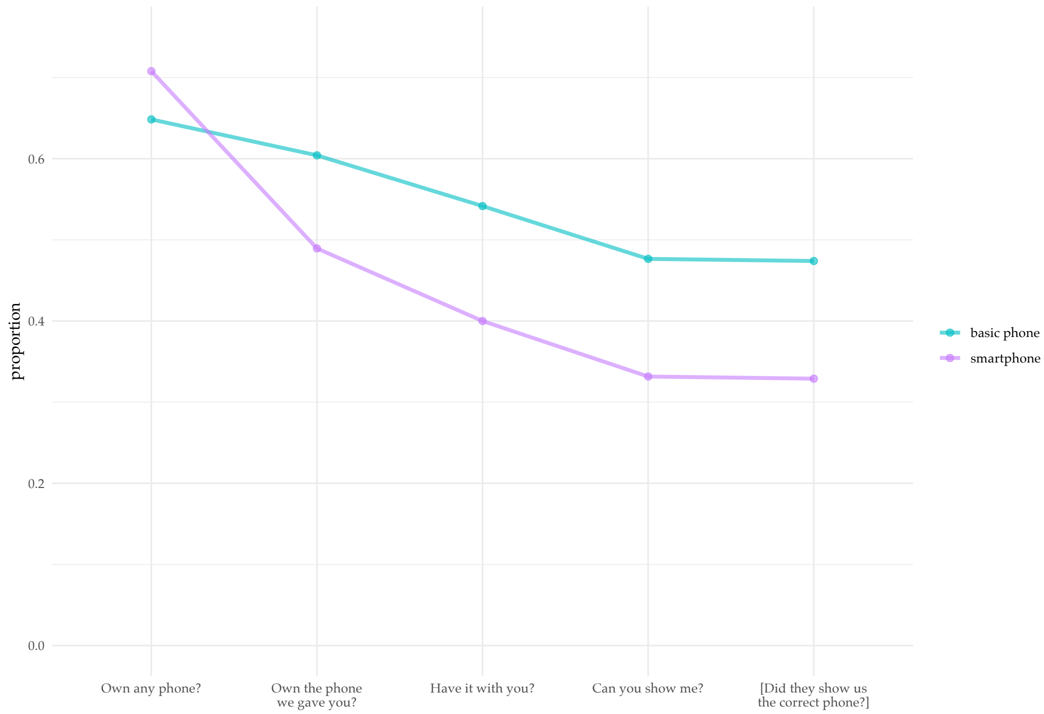

Figure A4.3: **Phone retention analysis.**

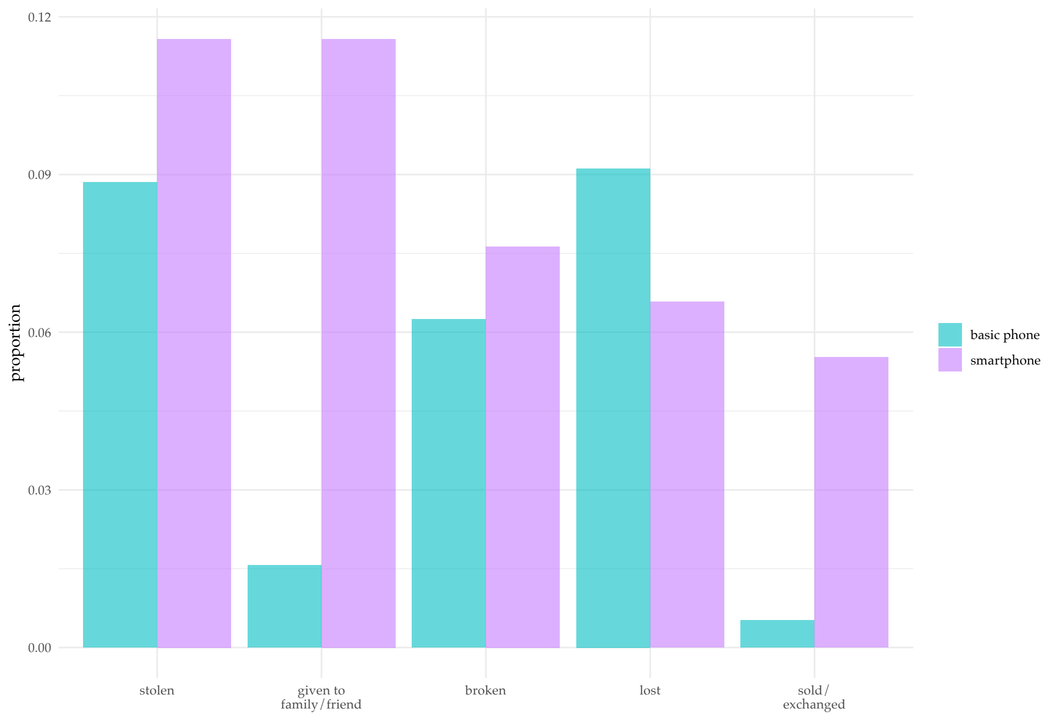

Figure A4.4: **Mode of non-retention of project phone.**

A5 Handset Non-retention from Midline to Endline

Consistent with the Sankey diagram in Figure 1, Figure A5.1 indicates the proportion of those who reported owning a phone at midline but no longer possessing one at endline. Handset turnover is consistent across treatment conditions and does not appear to be a 'free-phone' effect.

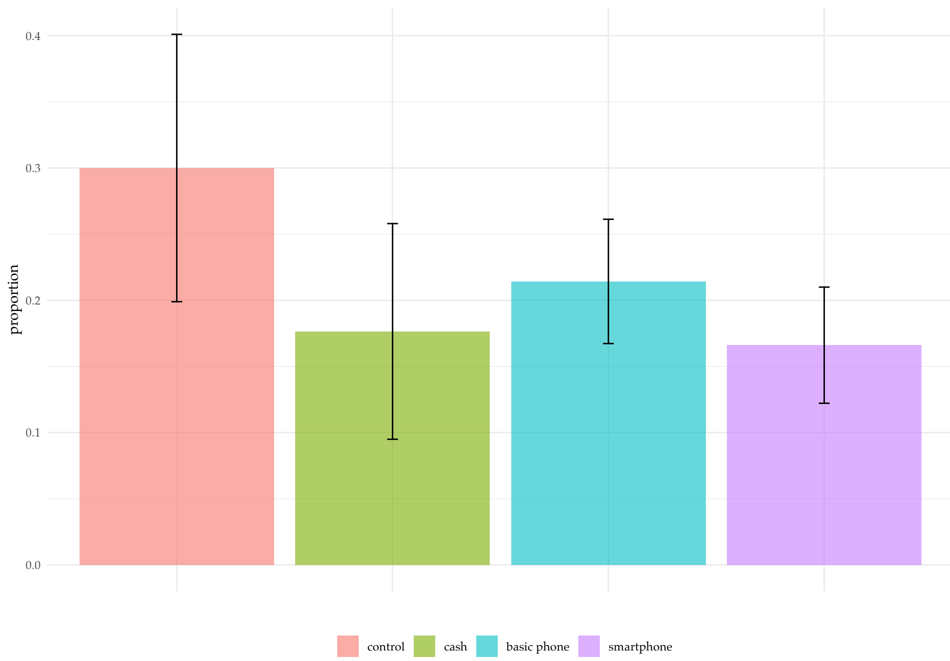

Figure A5.1: **Phone loss between midline and endline across treatment conditions.** Each bar indicates the proportion of participants who reported owning any phone at midline but then did not report owning a phone at endline.

A6 Impact of Intervention on Household Mobile Phone Count

As shown in Figure A6.1, the net effect of the experiment was, on average, a two-adult household increasing from 0.85 mobile phones at baseline to 1.44 or 1.64 phones at endline in the basic phone and smartphone conditions, respectively. Thus, in addition to possessing higher-capacity phones, households in the smartphone condition experienced the largest increase in mobile phone count. As illustrated in Figure A6.1, the divergence in household phone count between the basic phone and smartphone condition arose between midline and endline.

The stacked bar chart in Figure A6.2 shows the distribution of phones per household by treatment condition. Whereas in the control condition three-quarters of households

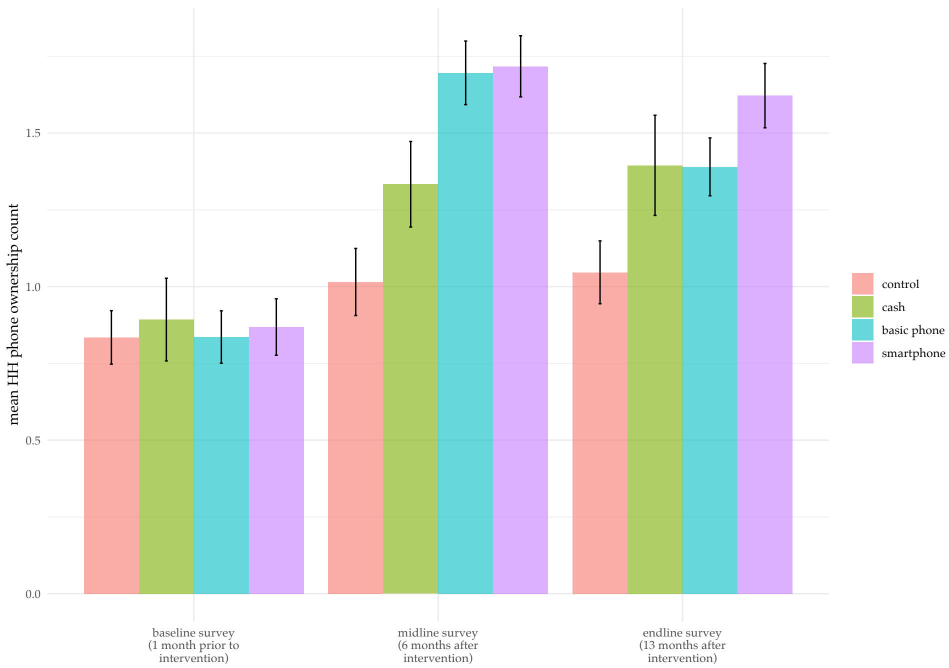

Figure A6.1: **Household mobile phone count between midline and endline across treatment conditions.** Each bar indicates the average number of mobile phones reported in the household at baseline, midline, and endline.

possess one phone or less (it is evenly divided between one-phone and no-phone households), in the smartphone condition half of households now possess two phones or more.

In both phone conditions, the largest increases were among the compliers (those who kept their project phones throughout the duration of the study and had them in their possession during the endline survey) rather than the non-compliers (those who did not retain their project phones and thus may have sold or exchanged them). (See Figure A6.3.) This is an important validity check, especially for the smartphone group given its higher asset value. It suggests that the boost in household handset count was driven by the receipt and retention of the smartphone, rather than cashing it in.

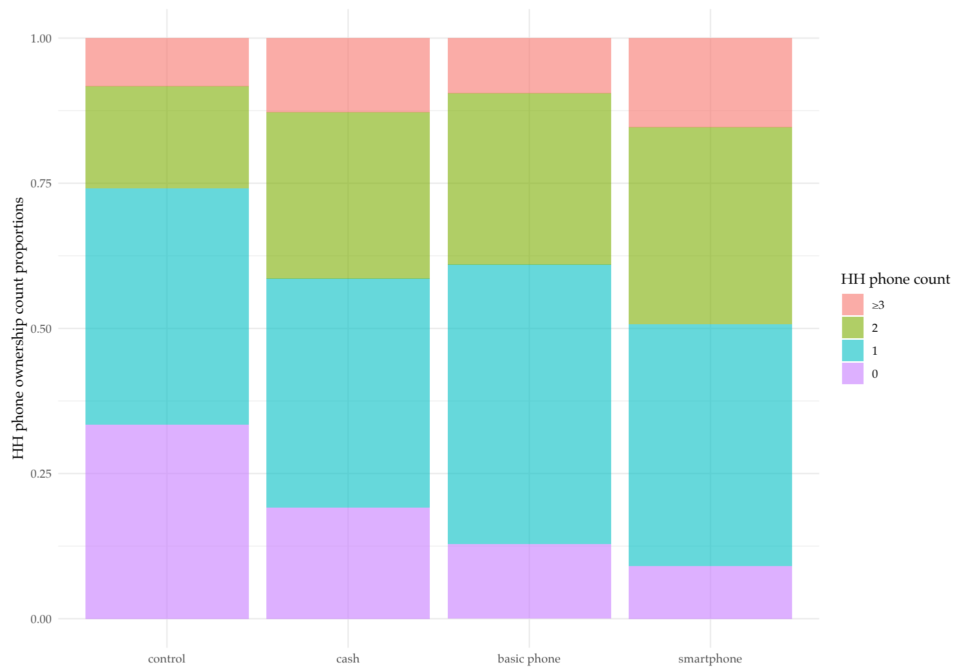

Figure A6.2: **Distribution of phone count per household across treatment conditions at endline.**

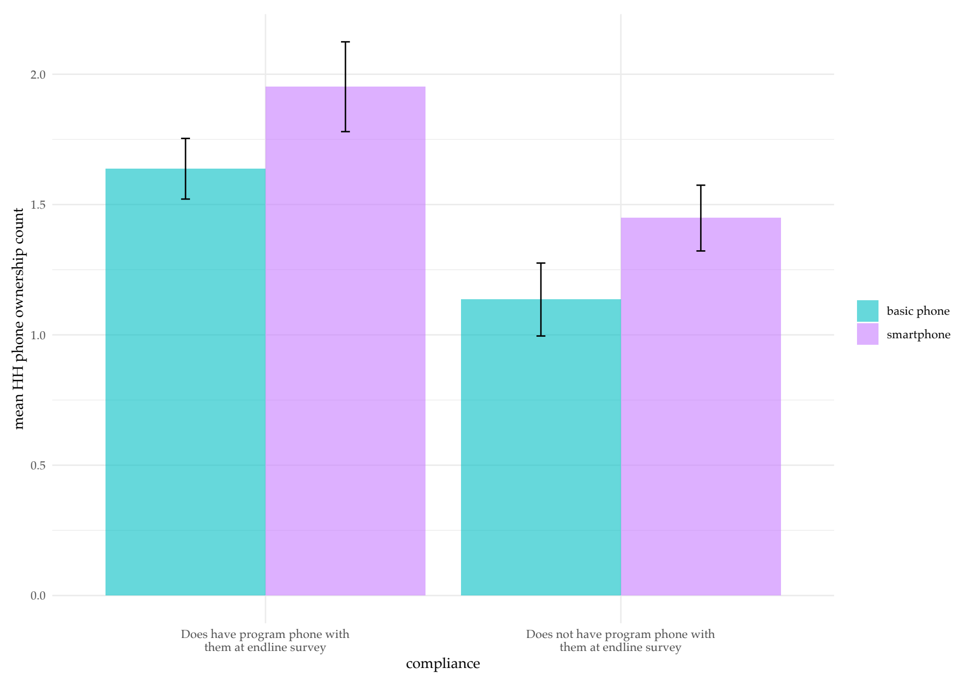

Figure A6.3: **Household handset count across compliers (those who kept their project phones throughout the duration of the study and had them in their possession during the endline survey) and non-compliers in basic and smartphone conditions.**

Finally, Figure A6.4 illustrates the effects on women's mobile phone ownership in the household. The smartphone intervention boosted women's mobile phone ownership almost up to 1 handset per household.

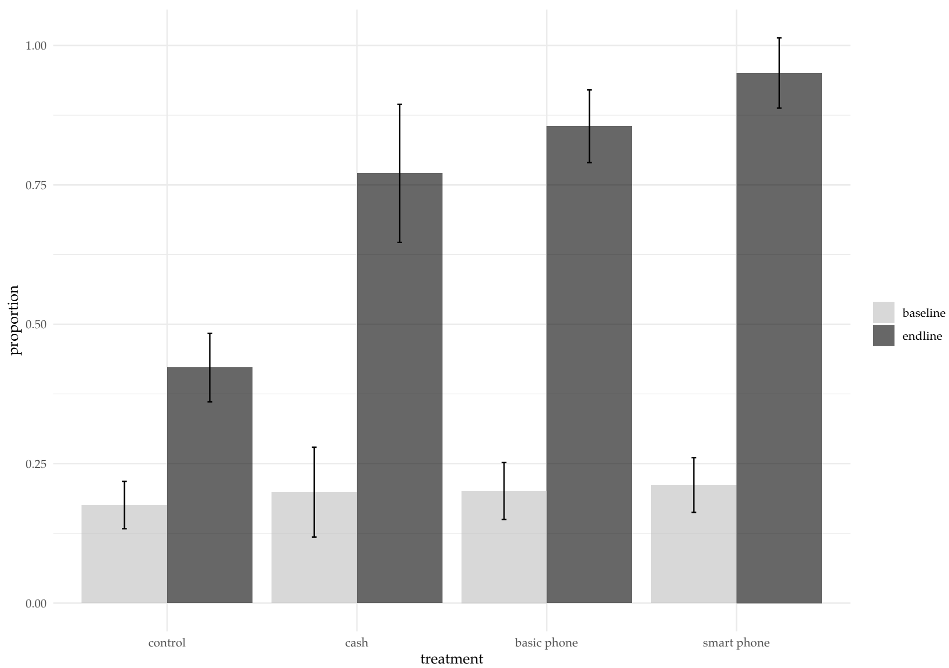

Figure A6.4: **Mean women's phone ownership in the household, by treatment (95% confidence intervals).**

A7 Baseline Preferences for Mobile Phone Use

At baseline the vast majority of participants report wanting a mobile phone to stay in touch with friends and family. Far fewer see it as a financial or business tool.

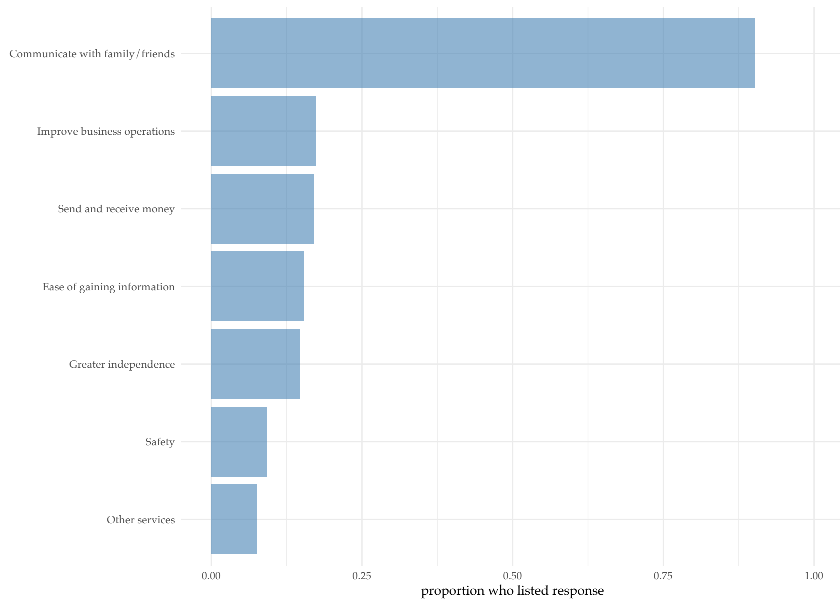

Figure A7.1: **Participants' baseline preferences for mobile phone use.**

A8 Description of Indexes and Analysis of Missing Data

In this section we detail the components of each index and provide descriptive statistics. We also report any differential missingness across treatment conditions. Tables A8.1 to A8.3 provide a description of each variable and summary statistics. Following from Anderson (2008), indexes were created using inverse covariance weighting.[16] Each table is organized into different indexes (mobile phone use, mobile money use, etc.) with each row representing a different component of a given index. For household bargaining and the intimate partner violence index, we create two indexes. One includes the set of variables that is applicable to married participants; the other is for all participants, married and non-married.

Missingness is analysed in Tables A8.4 to A8.6. Generally, missing observations on

[16]We used the mean effects index Stata package created by Cyrus Samii to produce the indexes.

the indexes and standalone outcome variables are not correlated with the treatment conditions. One exception is on the indexes of phone use and mobile money use. For these outcomes, those in the phone conditions, especially the smartphone group, appear more likely to report their mobile use. One possible explanation for this is that phone ownership increased women's awareness of their phone use. We expect that this missingness biases against finding treatment effects on economic livelihoods as, generally, poorer, rural women who are members of poverty reduction program are less likely to report phone and mobile money use (see Table A8.7) and thus are more likely to be dropped from the control condition at a higher proportion than in the treatment conditions.

Variable Description	n	Range	Mean	SD
Mobile phone use				
Count of sim cards owned	1265	0-4	0.968	0.683
Frequency of phone use	1258	1-5	3.940	1.252
Able to use phone as much as wanted	1248	1-5	3.344	1.472
Frequency of phone use for income-generation	1258	0-4	1.079	1.529
Index of phone use	1231	-1.83-3.21	0	1
Mobile money use				
Personally use mobile money to save	1268	0-1	0.156	0.363
Mobile money preferred financial instrument	1267	0-1	0.193	0.394
Strength of preference for mobile money	1226	1-5	3.728	1.287
Frequency of mobile money use	1246	0-5	1.700	1.375
Own mobile money account	1267	0-1	0.554	0.497
Count of mobile money services used	1267	0-10	1.186	1.693
Mobile loans taken out over past year	1238	0-11	0.286	1.212
Times sent mobile money in past month	1263	0-10	0.299	0.849
Times received mobile money in past month	1263	0-13	0.677	1.201
Chose mobile money in small grant	1267	0-1	0.558	0.497
Index of mobile money use	1187	-1.47-5.99	0	1
Small grant outcomes				
Chose mobile money in small grant	1267	0-1	0.558	0.497
Chose mobile money in small grant and received on own SIM	1267	0-1	0.320	0.467
Income				
Weekly income range	1255	1-12	4.621	3.802
Monthly income range	1256	1-12	4.052	3.239
Consumption				
Monthly food spending (winsorised (w))	1238	2800-320000	93903.39	69067.39
Monthly spending on household items (w)	1258	800-80000	13324.83	13836.48
Monthly spending on household fuel (w)	1262	0-100000	14504.91	18988.19
Monthly spending on electricity (w)	1257	0-40000	1754.02	6014.543
Monthly spending on airtime and other mobile expenses (w)	1247	0-40000	5991.66	7683.02
Monthly spending on water (w)	1262	0-70000	7172.599	12674.75
Monthly spending on alcohol and tobacco (w)	1243	0-80000	2559.292	10231.47
Monthly spending on entertainment (w)	1261	0-60000	2393.021	9058.551
Monthly spending on transportation (w)	1263	0-120000	13218.13	20101.10
Monthly spending on household maintenance (w)	1265	0-700000	23168.7	95130.78
Monthly spending on clothing (w)	1253	0-41666.67	5686.42	8392.12
Monthly spending on community activities (w)	1253	0-33333.33	3590.64	5773.29
Monthly spending on medical and health care (w)	1252	0-33333.33	3590.64	5773.29
Monthly spending on schooling (w)	1251	0-266666.7	15804.58	42493.02
Monthly spending on taxes (w)	1264	0-66666.66	4182.09	11125.89
Sum of total monthly spending (w)	1151	3600-1378700	215163.20	183534.20

Table A8.1: **Variable descriptions, composition of indexes, and summary statistics.**

Variable Description	n	Range	Mean	SD
Subjective welfare				
Assessment of current living conditions	1268	1-5	3.49	0.967
Living conditions relative to one year ago	1267	1-5	3.73	1.21
Index of subjective welfare	1267	-2.72-1.47	0	1
Formal economic engagement				
Personally use bank to save	1268	0-1	0.055	0.228
Personally use mobile money to save	1268	0-1	0.156	0.363
Use mobile money to pay bills	1267	0-1	0.032	0.175
Use mobile money for loan payments	1267	0-1	0.028	0.164
Use mobile money to receive payments	1267	0-1	0.039	0.192
Count of mobile loans in past year	1238	0-11	0.286	1.213
Use mobile money for loans	1263	0-1	0.054	0.226
Index of formal economic engagement	1234	-.50-6.18	0	1
Access to information				
Ease of obtaining financial info for job or bus.	1074	1-4	2.580	0.951
How often access the internet	1185	1-5	1.215	0.780
Allocation of time				
Hours spent on primary occupation	1262	1-22	11.349	5.807
Hours spent on chores	1267	1-22	6.922	3.686
Hours spent commuting	1263	1-22	3.298	2.115
Hours spent farming	1266	1-22	9.778	4.970
Hours spent on leisure	1262	1-22	2.618	2.813
Social connectedness				
Lack of companionship (inv)	1265	0-3	1.309	1.073
People to talk to	1265	0-3	2.324	0.798
Stay in touch with those who live far away	1075	0-3	2.336	0.837
Count of strong social connections (up to 5)	1242	0-5	4.733	0.948
Frequency of contact with strong social connections	1242	0-5	4.026	0.887
Non-relatives in social network	1170	0-5	1.642	1.502
Certainty of network helping	1242	1-5	4.406	0.773
Certainty of network providing financial assistance	1242	1-5	3.880	1.018
Index of social connectedness	1067	-3.90- 2.53	0	1
Individual efficacy				
Able to achieve goals	1249	1-5	3.728	1.483
Enough time to work toward goals	1239	1-5	3.729	1.456
Able to overcome challenges	1256	1-5	3.866	1.426
Change things that are wrong	1258	1-5	3.980	1.330
Can provide for myself and family	1261	1-5	3.640	1.573
Person of self-worth	1259	1-5	4.357	1.210
Index of individual efficacy	1217	0-1	-3.01	1.04

Table A8.2: **Variable descriptions, composition of indexes, and summary statistics continued.**

Variable Description	n	Range	Mean	SD
Household bargaining				
Control of joint income—you or husband	796	1-5	2.780	1.449
Control of personal income—you or husband	788	1-5	3.796	1.340
More influence in household—you or husband	788	1-5	3.796	1.340
Influence over food expenses	1263	1-3	1.671	0.781
Influence over education expenses	1188	1-3	2.471	0.625
Influence over health expenses	1263	1-3	2.507	0.619
Influence over land use	1242	1-3	2.377	0.672
Influence over household finances	1261	1-3	1.520	0.622
Free to go out of house	1267	1-5	4.169	1.315
Index of household bargaining (married)	723	-4.86-5.09	0	1
Index of household bargaining (all)	1162	-3.62-1.05	0	1
Intimate partner violence				
Husband humiliated you	801	0-3	0.196	0.637
Husband threatened you or someone close to you	803	0-3	0.174	0.633
Husband physically harmed you	807	0-3	0.131	0.539
Someone other than husband humiliated you	1168	0-3	0.043	0.277
Someone other than husband threatened you	1151	0-3	0.076	0.369
Someone other than husband physically harmed you	1165	0-3	0.068	0.356
Incidence of different types of IPV events	1268	0-6	0.986	1.284
IPV justified circumstances	1268	0-5	1.291	1.746
Index of IPV (married)	793	-0.91-6.70	0	1
Index of IPV (all)	1150	-0.86-8.76	0	1
Gender equality norms				
Disagree man should have final word	1339	1-5	3.709	1.653
Disagree education more important for boys	1339	1-5	4.100	1.531
Women should be able to work outside home	1340	1-5	4.302	1.308
Women have right to express opinion	1336	1-5	4.251	1.333
Women have right to leave abusive spouses	1339	1-5	4.476	1.151
Women have right to own land and property	1339	1-5	4.435	1.210
Important women serve in government	1344	1-5	4.722	0.763
Political engagement				
Interest in government and politics	1259	1-4	2.001	1.038
How often discuss politics	1259	0-4	1.395	1.181
Organized a community meeting (prior 6 months)	1255	0-4	0.850	0.957
Participated in demonstration (prior 6 months)	1263	0-4	0.419	0.786
Attended an election rally (prior 6 months)	1265	0-4	2.465	1.082
Contacted government official (prior 6 months)	1255	0-4	1.019	0.997
Signed a petition (prior 6 months)	1255	0-4	0.593	0.813
Attended workshop (prior 6 months)	1255	0-4	0.593	0.813
People like me have say in government	1207	1-5	3.424	1.526
Index of political engagement	1171	-2.63- 4.19	0	1

Table A8.3: **Variable descriptions, composition of indexes, and summary statistics continued.**

	(1) Phone use	(2) MM use	(3) Weekly income	(4) Monthly income	(5) Consumption
Cash	-0.023	-0.043*	-0.011	0.012	-0.036
	(0.018)	(0.021)	(0.010)	(0.012)	(0.024)
Basic	-0.033*	-0.015	-0.007	0.006	0.011
	(0.013)	(0.020)	(0.009)	(0.008)	(0.022)
Smart	-0.038**	-0.044*	-0.015*	-0.005	0.004
	(0.012)	(0.018)	(0.007)	(0.005)	(0.022)
Solar	0.003	-0.014	0.005	0.016	0.022
	(0.012)	(0.016)	(0.009)	(0.010)	(0.023)
Voucher	0.003	-0.012	-0.008	-0.019**	-0.021
	(0.011)	(0.016)	(0.006)	(0.006)	(0.021)
Observations	1268	1268	1268	1268	1268

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A8.4: **Missingness analysis.** Each model regresses missing observations for a given index or outcome variable on treatment conditions and blocking variables (not shown).

	(6) Subjective welfare	(7) Formal economy	(8) Social connectedness	(9) Efficacy
Cash	0.000	-0.012	0.036	-0.045**
	(0.000)	(0.014)	(0.035)	(0.015)
Basic	0.000	-0.000	0.018	-0.011
	(0.000)	(0.013)	(0.026)	(0.016)
Smart	0.003	-0.012	0.052*	-0.023
	(0.003)	(0.012)	(0.027)	(0.015)
Solar	-0.001	-0.018	-0.011	-0.007
	(0.001)	(0.010)	(0.026)	(0.013)
Voucher	-0.001	-0.004	-0.014	0.015
	(0.001)	(0.011)	(0.026)	(0.015)
Observations	1268	1268	1268	1268

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A8.5: **Missingness analysis.** Each model regresses missing observations for a given index or outcome variable on treatment conditions and blocking variables (not shown).

	(10) HH bargain all	(11) HH bargain married	(12) IPV all	(13) IPV married	(14) Political engage
Cash	-0.020	-0.028	0.013	-0.000	-0.007
	(0.026)	(0.046)	(0.027)	(0.045)	(0.025)
Basic	-0.015	-0.017	0.006	0.019	-0.005
	(0.021)	(0.036)	(0.021)	(0.035)	(0.020)
Smart	-0.022	-0.012	0.013	0.038	-0.008
	(0.020)	(0.036)	(0.021)	(0.035)	(0.020)
Solar	-0.012	0.030	0.022	0.011	-0.042*
	(0.019)	(0.035)	(0.021)	(0.034)	(0.017)
Voucher	-0.006	0.057	-0.020	0.066	-0.002
	(0.020)	(0.035)	(0.019)	(0.034)	(0.019)
Observations	1268	1268	1268	1268	1268

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A8.6: **Missingness analysis.** Each model regresses missing observations for a given index or outcome variable on treatment conditions and blocking variables (not shown).

	(1) Phone use	(2) Phone use	(3) Phone use	(4) MM use	(5) MM use	(6) MM use
Urban	-0.030***			-0.067***		
	(0.007)			(0.010)		
TASAF		0.049***			0.073***	
		(0.008)			(0.013)	
Income (L/H)			-0.031**			-0.035*
			(0.009)			(0.014)
	1268	1268	1268	1268	1268	1268

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A8.7: **Determinants of missingness.** Missing observations on phone use index and mobile money use index regressed on three blocking variables: rural/urban; program membership (BRAC/TASAF); low/high income.

A9 Re-Analysis of Main Outcomes in Figure 2 Imputing Missing Observations

As explained in A8, missing observations on the outcomes of interest are relatively low and generally not correlated with treatment. Nonetheless, here we rerun the analyses of phone use, mobile money use and household consumption reported in Figure 2 imputing missing observations using baseline measures of the outcomes. To do so, we replace missing observations using ten sets of simulated values. We then estimate OLS regressions using all ten sets of imputed values, combining the results.

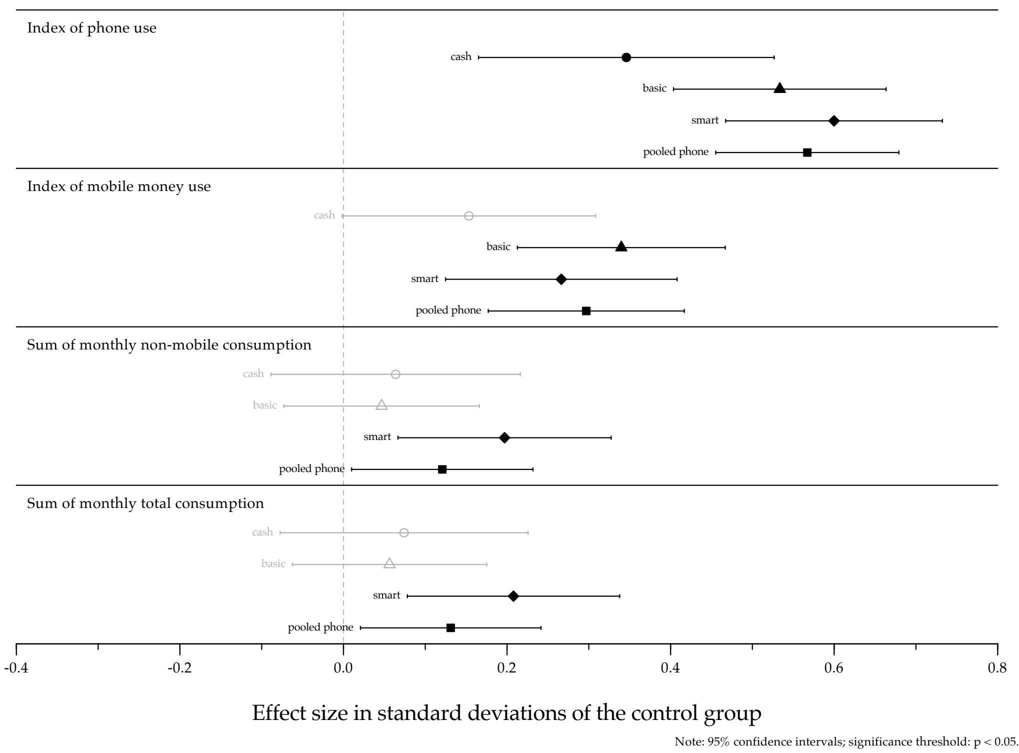

Figure A9.1: **Phone use, mobile money use and household consumption with missing observations imputed from baseline dependent variables.** OLS regressions include the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, blocking variables, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

A10 Full Regression Results of Main Outcomes

A10.1 Full Regression Results of Main Outcome: Index of Phone Use

	(1)	(2)	(3)	(4)	(5)
Pooled Phone	0.598***				
	(0.057)				
Basic		0.565***			0.570***
		(0.066)			(0.066)
Smart			0.630***		0.635***
			(0.066)		(0.066)
Cash				0.365***	0.351***
				(0.094)	(0.089)
Baseline phone use	0.013	0.018	0.011	0.025	0.018
	(0.023)	(0.027)	(0.028)	(0.033)	(0.022)
Solar	0.094	0.141	0.098	0.083	0.062
	(0.072)	(0.092)	(0.094)	(0.112)	(0.066)
Voucher	-0.003	-0.037	0.016	-0.017	-0.004
	(0.071)	(0.093)	(0.091)	(0.112)	(0.065)
Age	0.027*	0.021	0.030*	0.026	0.029**
	(0.010)	(0.013)	(0.012)	(0.015)	(0.010)
Age^2	-0.000*	-0.000	-0.000*	-0.000	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Married	-0.067	-0.025	-0.091	-0.045	-0.064
	(0.065)	(0.081)	(0.080)	(0.096)	(0.061)
Education	0.034***	0.035***	0.036***	0.046***	0.037***
	(0.008)	(0.010)	(0.010)	(0.011)	(0.007)
Own Phone In Past	0.248***	0.238**	0.319***	0.337***	0.247***
	(0.061)	(0.074)	(0.073)	(0.086)	(0.057)
HH Size	0.008	0.001	0.008	-0.007	0.005
	(0.010)	(0.013)	(0.013)	(0.015)	(0.010)
Urban	0.184*	0.201	0.203	0.305*	0.206*
	(0.088)	(0.110)	(0.109)	(0.136)	(0.084)
TASAF	-0.356***	-0.408***	-0.341***	-0.377***	-0.342***
	(0.077)	(0.095)	(0.093)	(0.110)	(0.072)
Income (L/H)	0.085	0.025	0.105	0.066	0.104
	(0.064)	(0.080)	(0.078)	(0.095)	(0.060)
Control mean	-0.380	-0.380	-0.380	-0.380	-0.380
Observations	1077	717	727	519	1229
Wald tests of equality p-values					
Basic=Smart					0.3198
Basic=Cash					0.0136
Smart=Cash					0.0014

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A10.1: **Treatment effects on index of phone use.** Results derived from OLS models that estimate effect of a given treatment compared to control. Observations vary depending on size of treatment groups. Endline control mean reported at bottom of table. Robust standard errors clustered at individual level. P-values from the Wald tests of equality of coefficients derived from model 5 are reported at the bottom of the table.

A10.2 Full Regression Results of Main Outcome: Index of Mobile Money Use

	(1)	(2)	(3)	(4)	(5)
Pooled Phone	0.317***				
	(0.057)				
Basic		0.360***			0.360***
		(0.065)			(0.064)
Smart			0.289***		0.279***
			(0.072)		(0.071)
Cash				0.158*	0.144
				(0.079)	(0.076)
MM Index (baseline)	0.189***	0.224***	0.169***	0.194***	0.183***
	(0.032)	(0.033)	(0.040)	(0.037)	(0.029)
Solar	-0.102	-0.156	-0.061	-0.124	-0.100
	(0.076)	(0.080)	(0.105)	(0.092)	(0.067)
Voucher	0.041	0.135	-0.088	-0.022	0.034
	(0.076)	(0.093)	(0.103)	(0.105)	(0.067)
Age	0.011	0.014	0.004	0.013	0.012
	(0.010)	(0.013)	(0.012)	(0.014)	(0.009)
Age^2	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Married	-0.093	-0.082	-0.055	0.035	-0.062
	(0.067)	(0.071)	(0.084)	(0.077)	(0.061)
Education	0.026**	0.030***	0.026**	0.035***	0.027***
	(0.008)	(0.009)	(0.010)	(0.009)	(0.007)
Own Phone In Past	0.189**	0.128*	0.284***	0.259***	0.197***
	(0.059)	(0.064)	(0.074)	(0.069)	(0.054)
HH Size	0.022*	0.011	0.039**	0.036*	0.025*
	(0.011)	(0.013)	(0.013)	(0.015)	(0.010)
Urban	0.223*	0.057	0.354**	0.313*	0.276**
	(0.101)	(0.105)	(0.127)	(0.121)	(0.095)
TASAF	-0.285***	-0.343***	-0.227*	-0.240*	-0.267***
	(0.079)	(0.089)	(0.097)	(0.094)	(0.071)
Income (L/H)	0.024	0.030	0.001	0.004	0.028
	(0.062)	(0.066)	(0.079)	(0.075)	(0.057)
Control mean	0.0383	0.0383	0.0383	0.0383	0.0383
Observations	1033	684	701	502	1183
Wald tests of equality p-values					
Basic=Smart					0.2727
Basic=Cash					0.0051
Smart=Cash					0.1013

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A10.2: **Treatment effects on index of mobile money use.** Results derived from OLS models that estimate effect of a given treatment compared to control. Observations vary depending on size of treatment groups. Endline control mean reported at bottom of table. Robust standard errors clustered at individual level. P-values from the Wald tests of equality of coefficients derived from model 5 are reported at the bottom of the table.

A10.3 Full Regression Results of Main Outcome: Small Grant to Mobile Wallet

	(1)	(2)	(3)	(4)	(5)
Pooled Phone	0.109***				
	(0.030)				
Basic		0.150***			0.149***
		(0.035)			(0.035)
Smart			0.069*		0.065
			(0.035)		(0.034)
Cash				0.136**	0.136**
				(0.047)	(0.046)
MM Index (baseline)	0.039*	0.040*	0.030	0.038	0.048***
	(0.016)	(0.019)	(0.019)	(0.022)	(0.014)
Solar	-0.063	-0.036	-0.049	0.061	-0.041
	(0.036)	(0.045)	(0.047)	(0.057)	(0.033)
Voucher	-0.004	-0.027	-0.026	-0.061	0.002
	(0.036)	(0.046)	(0.047)	(0.058)	(0.033)
Age	0.003	0.002	0.002	0.006	0.004
	(0.006)	(0.007)	(0.007)	(0.008)	(0.005)
Age^2	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Married	-0.022	-0.026	0.001	0.072	0.001
	(0.033)	(0.041)	(0.041)	(0.051)	(0.031)
Education	0.010*	0.011*	0.010	0.008	0.008
	(0.004)	(0.005)	(0.005)	(0.006)	(0.004)
Own Phone In Past	0.075*	0.041	0.119**	0.122*	0.086**
	(0.034)	(0.041)	(0.042)	(0.048)	(0.032)
HH Size	0.010	-0.002	0.015*	-0.004	0.009
	(0.006)	(0.007)	(0.007)	(0.009)	(0.005)
Urban	0.130**	0.141**	0.136**	0.124*	0.108**
	(0.041)	(0.049)	(0.051)	(0.061)	(0.039)
TASAF	-0.095*	-0.099	-0.128*	-0.135*	-0.095*
	(0.042)	(0.052)	(0.050)	(0.060)	(0.039)
Income (L/H)	0.008	0.037	-0.021	0.007	0.008
	(0.034)	(0.041)	(0.041)	(0.050)	(0.032)
Control mean	0.49	0.49	0.49	0.49	0.49
	1106	742	749	542	1263
Wald tests of equality p-values					
Basic=Smart					0.0150
Basic=Cash					0.7838
Smart=Cash					0.1150

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A10.3: **Treatment effects on choosing mobile money in small grant test.** Results derived from OLS models that estimate effect of a given treatment compared to control. Observations vary depending on size of treatment groups. Endline control mean reported at bottom of table as proportion of those who chose mobile money. Robust standard errors clustered at individual level. P-values from the Wald tests of equality of coefficients derived from model 5 are reported at the bottom of the table.

A10.4 Full Regression Results of Main Outcome: Small Grant to Mobile Wallet on Own SIM

	(1)	(2)	(3)	(4)	(5)
Pooled Phone	0.156***				
	(0.026)				
Basic		0.195***			0.186***
		(0.032)			(0.032)
Smart			0.122***		0.123***
			(0.030)		(0.030)
Cash				0.098*	0.092*
				(0.042)	(0.041)
MM Index (baseline)	0.061***	0.081***	0.038*	0.062**	0.067***
	(0.016)	(0.019)	(0.018)	(0.021)	(0.015)
Solar	-0.042	-0.059	-0.018	-0.004	-0.032
	(0.034)	(0.042)	(0.043)	(0.049)	(0.032)
Voucher	0.008	-0.015	0.007	-0.011	0.015
	(0.035)	(0.043)	(0.044)	(0.049)	(0.032)
Age	0.006	0.009	0.004	0.011	0.007
	(0.005)	(0.006)	(0.006)	(0.007)	(0.005)
Age^2	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Married	-0.002	0.006	0.002	-0.018	-0.017
	(0.030)	(0.038)	(0.034)	(0.042)	(0.028)
Education	0.014***	0.015**	0.015**	0.018***	0.014***
	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)
Own Phone In Past	0.127***	0.097**	0.180***	0.141***	0.117***
	(0.031)	(0.037)	(0.035)	(0.039)	(0.028)
HH Size	0.000	-0.008	0.008	-0.001	-0.000
	(0.006)	(0.007)	(0.006)	(0.007)	(0.005)
Urban	0.172***	0.198***	0.169**	0.162*	0.147***
	(0.045)	(0.054)	(0.054)	(0.063)	(0.042)
TASAF	0.036	0.048	-0.017	-0.058	0.017
	(0.038)	(0.047)	(0.045)	(0.052)	(0.036)
Income (L/H)	0.020	0.027	-0.004	-0.001	0.024
	(0.031)	(0.037)	(0.035)	(0.041)	(0.029)
Control Mean	0.23	0.23	0.23	0.23	0.23
Observations	1106	742	749	542	1263
Wald tests of equality p-values					
Basic=Smart					0.0658
Basic=Cash					0.0295
Smart=Cash					0.4626

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A10.4: **Treatment effects on choosing mobile money and having it sent to own SIM in in small grant test.** Results derived from OLS models that estimate effect of a given treatment compared to control. Observations vary depending on size of treatment groups. Endline control mean reported at bottom of table as proportion of those who chose mobile money and had it sent to own SIM. Robust standard errors clustered at individual level. P-values from the Wald tests of equality of coefficients derived from model 5 are reported at the bottom of the table.

A10.5 Full Regression Results of Main Outcome: Standardized Monthly Non-mobile Consumption

	(1)	(2)	(3)	(4)	(5)
Pooled Phone	0.138*				
	(0.057)				
Basic		0.073			0.083
		(0.064)			(0.064)
Smart			0.204**		0.193**
			(0.072)		(0.071)
Cash				0.056	0.052
				(0.082)	(0.080)
Consumption (baseline)	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Solar	0.085	0.191	0.053	0.139	0.065
	(0.088)	(0.113)	(0.113)	(0.124)	(0.077)
Voucher	-0.092	-0.126	-0.153	-0.236*	-0.083
	(0.079)	(0.096)	(0.098)	(0.114)	(0.073)
Age	0.039***	0.033**	0.046***	0.028*	0.034***
	(0.010)	(0.012)	(0.012)	(0.014)	(0.010)
Age^2	-0.000***	-0.000**	-0.000***	-0.000	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Married	0.115	0.097	0.137	0.045	0.094
	(0.066)	(0.077)	(0.085)	(0.088)	(0.060)
Education	-0.001	-0.004	-0.002	-0.003	0.002
	(0.008)	(0.009)	(0.010)	(0.008)	(0.007)
Own Phone In Past	0.130*	0.109	0.128	0.136	0.145**
	(0.061)	(0.072)	(0.073)	(0.079)	(0.056)
HH Size	0.019	0.001	0.025	0.008	0.020
	(0.013)	(0.013)	(0.016)	(0.016)	(0.012)
Urban	0.367**	0.299*	0.433**	0.374**	0.369***
	(0.114)	(0.121)	(0.145)	(0.144)	(0.107)
TASAF	-0.408***	-0.371***	-0.494***	-0.383***	-0.365***
	(0.083)	(0.103)	(0.096)	(0.108)	(0.077)
Income (L/H)	0.055	0.040	-0.058	-0.067	0.084
	(0.065)	(0.083)	(0.072)	(0.080)	(0.059)
Control mean	-.082	-.082	-.082	-.082	-.082
Observations	1012	679	689	504	1160
Wald tests of equality p-values					
Basic=Smart					0.1331
Basic=Cash					0.7028
Smart=Cash					0.1007

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A10.5: **Treatment effects on standardized monthly non-mobile consumption.** Results derived from OLS models that estimate effect of a given treatment compared to control. Observations vary depending on size of treatment groups. Endline control mean reported at bottom of table. Robust standard errors clustered at individual level. P-values from the Wald tests of equality of coefficients derived from model 5 are reported at the bottom of the table.

A10.6 Full Regression Results of Main Outcome: Standardized Monthly Total Consumption

	(1)	(2)	(3)	(4)	(5)
Pooled Phone	0.156**				
	(0.057)				
Basic		0.088			0.100
		(0.064)			(0.064)
Smart			0.224**		0.214**
			(0.073)		(0.071)
Cash				0.070	0.067
				(0.082)	(0.079)
Consumption (baseline)	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Solar	0.087	0.193	0.057	0.138	0.066
	(0.088)	(0.112)	(0.113)	(0.124)	(0.077)
Voucher	-0.087	-0.115	-0.149	-0.227*	-0.080
	(0.079)	(0.096)	(0.099)	(0.113)	(0.073)
Age	0.039***	0.033**	0.047***	0.030*	0.035***
	(0.010)	(0.012)	(0.012)	(0.014)	(0.010)
Age^2	-0.000***	-0.000**	-0.000***	-0.000	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Married	0.116	0.096	0.140	0.051	0.097
	(0.067)	(0.077)	(0.086)	(0.088)	(0.060)
Education	-0.000	-0.004	-0.001	-0.003	0.003
	(0.008)	(0.009)	(0.010)	(0.008)	(0.007)
Own Phone In Past	0.118	0.087	0.116	0.116	0.136*
	(0.061)	(0.071)	(0.073)	(0.078)	(0.056)
HH Size	0.019	0.001	0.026	0.008	0.020
	(0.013)	(0.013)	(0.016)	(0.016)	(0.012)
Urban	0.365**	0.300*	0.428**	0.376**	0.370***
	(0.114)	(0.120)	(0.145)	(0.143)	(0.106)
TASAF	-0.418***	-0.387***	-0.509***	-0.404***	-0.372***
	(0.083)	(0.102)	(0.096)	(0.107)	(0.076)
Income (L/H)	0.063	0.051	-0.048	-0.054	0.090
	(0.065)	(0.082)	(0.072)	(0.079)	(0.059)
Control mean	-.093	-.093	-.093	-.093	-.093
Observations	1003	673	681	499	1151
Wald tests of equality p-values					
Basic=Smart					0.1212
Basic=Cash					0.6832
Smart=Cash					0.0877

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A10.6: **Treatment effects on standardized monthly total consumption.** Results derived from OLS models that estimate effect of a given treatment compared to control. Observations vary depending on size of treatment groups. Endline control mean reported at bottom of table. Robust standard errors clustered at individual level. P-values from the Wald tests of equality of coefficients derived from model 5 are reported at the bottom of the table.

A11 Components of Phone Use Index

Figure A11.1 reports regression results from individual components of the phone use index on treatment conditions.

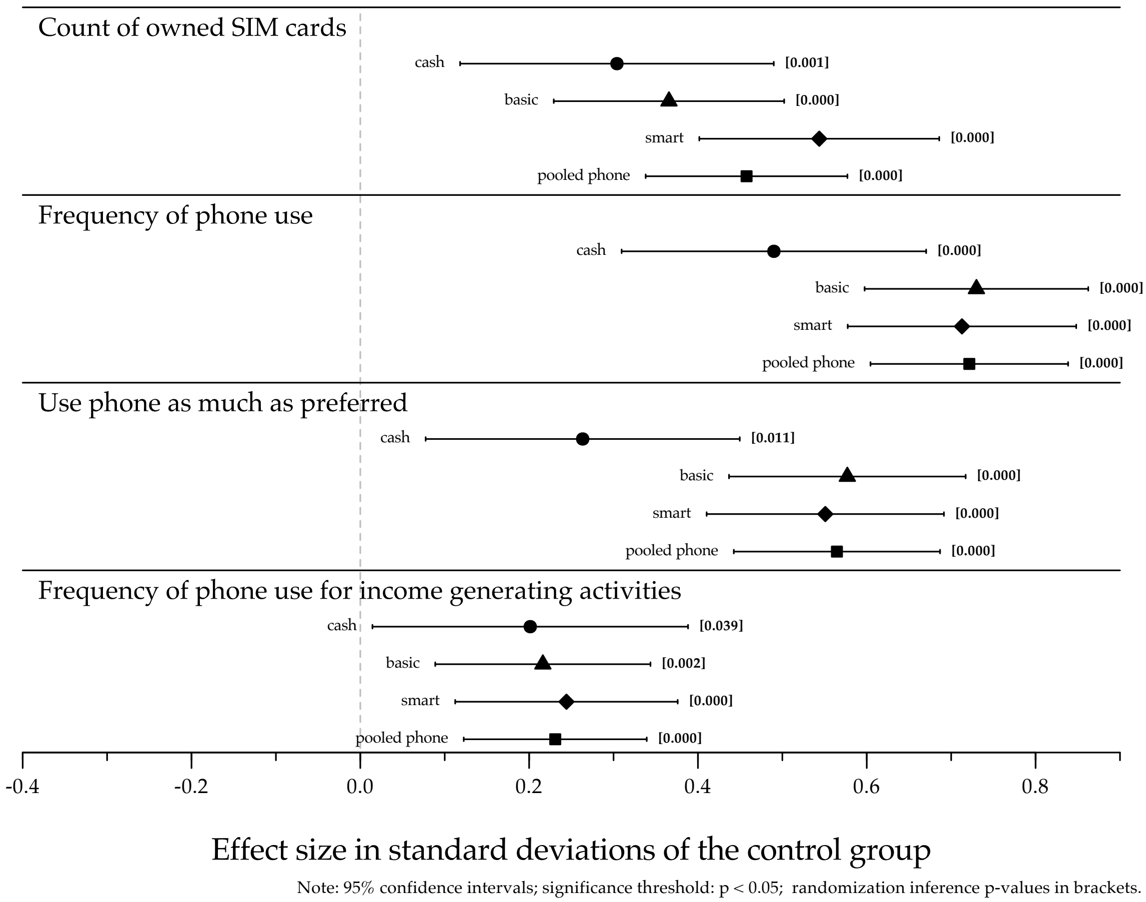

Figure A11.1: **Treatment effects on individual components of phone use.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

A12 Mobile Money: Index Components and Household Use

Figure A12.1 reports regression results from individual components of the mobile money use index on treatment conditions.

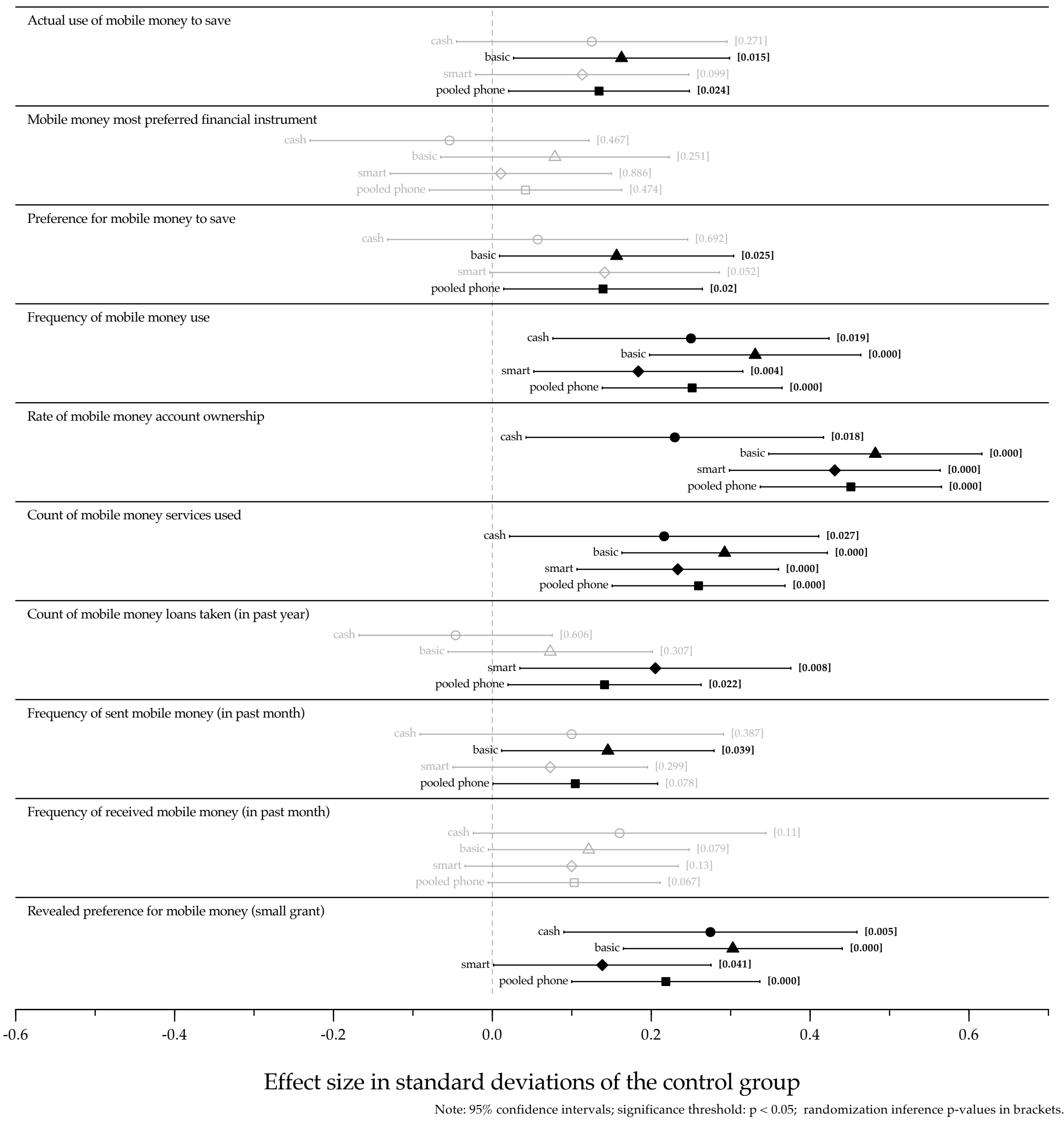

Figure A12.1: **Treatment effects on individual components of mobile money use.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

Figure A12.2 reports regression results from mobile money use at the household level. It is derived from a survey question that asks: "How does your household send and receive money from friends and family from within Tanzania?" Participants are able to list multiple modes, including: hand delivery by self, hand delivery by friends, delivery through driver, mobile phone, bank, postal banking services or don't send remittances. We create a binary variable equal to 1 if participants report using mobile money and 0 otherwise. Mobile money use is the modal method, with 65% of all respondents at endline reporting that their households use mobile money. Whereas the basic and cash treatments led to higher levels of household use of mobile money for remittances compared to control, we see no effect from the smartphone. Part of this difference appears to be due to smartphone participants reporting their households were less likely to send and receive remittances altogether. This is in line with reports on income sources. Whereas smartphone participants were more likely to specialize in market trading (see A13.1), they tended to rely less on cash transfers from another person as a source of income.

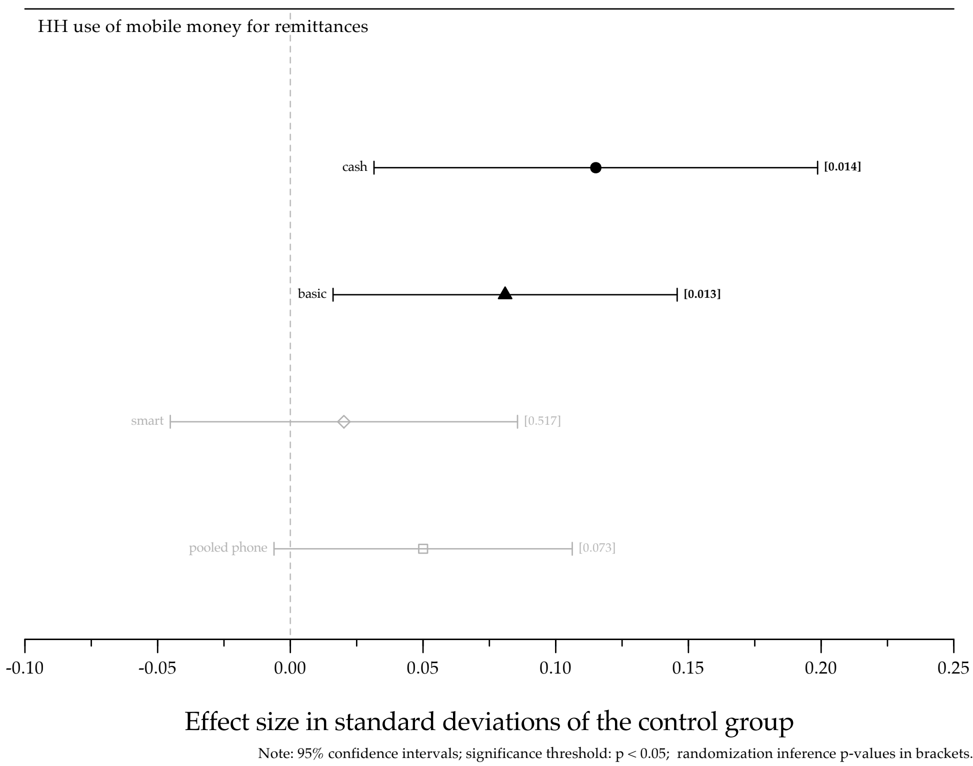

Figure A12.2: **Treatment effects on household use of mobile money for sending and receiving remittances.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

Figure A12.3 reports sub-group treatment effects of revealed preferences for mobile money use by baseline levels of literacy. Baseline literacy was determined by respondents' abilities to read the consent form for survey participation. We distinguish between those who read the statement fluently versus those who read only part or none of the statement (and thus had to have it read to them). Among women with lower levels of literacy, the basic phone led to much higher levels of mobile money proficiency

(as measured by choosing mobile money in the on-the-spot digital payments test and having it sent to one's own SIM) than the smartphone; whereas among fully literate participants both handset types caused a similarly large and robust effect. See A12.3.

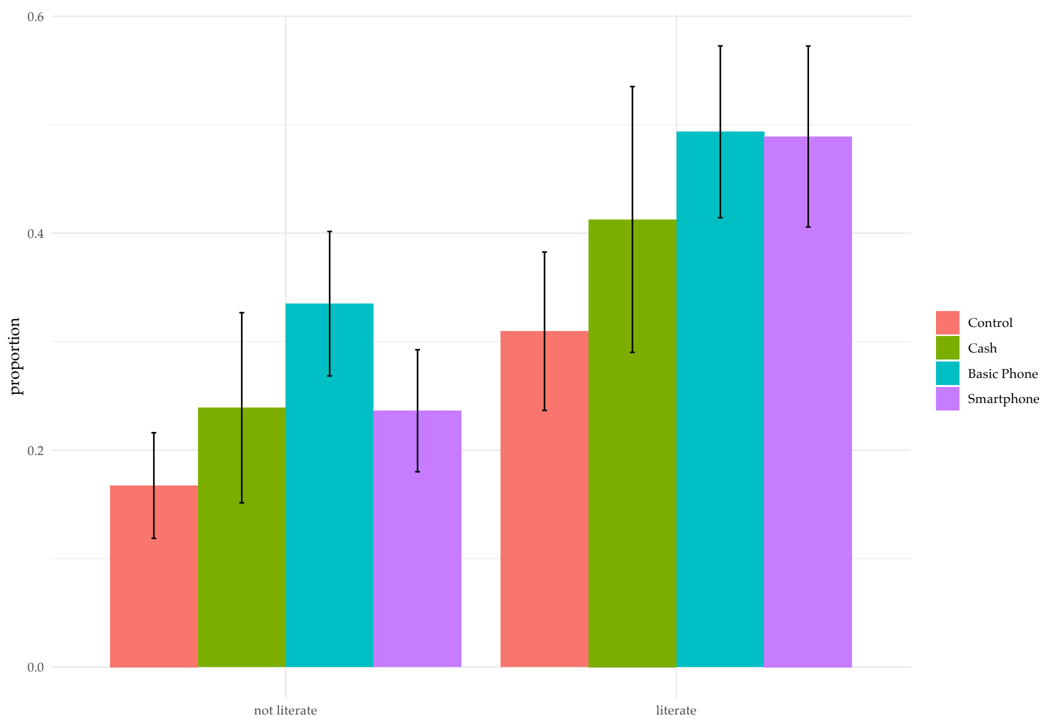

Figure A12.3: **Subgroup effects on mobile money uptake by baseline levels of literacy.** Bars indicate mean rate of choosing mobile money in the on-the-spot digital payments test and having it sent to one's own own mobile money wallet. 95% confidence intervals.

A13 Access to Financial Information, Internet and Occupational Choice

Beyond enabling use of digital financial services, mobile technology has also been found to reduce search costs and improve access to market information (Jensen, 2007). Figure A13.1 reports regression results analysing the impact of treatment conditions on access to financial information, such as prices of goods and services, that participants need for their jobs or business activities. Those without a job tended not to answer and are excluded from the analysis. This drops slightly more than 15% of participants.

Respondents in the phone conditions were no more likely to report that it was easier for them to access financial information. One potential reason to account for this, highlighted in other studies, is that broader market failures limit the informational benefits that come from mobile technology (Aker et al., 2016b; Aker and Ksoll, 2016).

In contrast, participants in the treatment groups were significantly more likely to report accessing the internet, albeit absolute levels of internet use remained quite low. In the smartphone group at endline, 86% still reported never using the internet but frequent use (more than once per week) was six fold higher in the smartphone group compared to control.

Another important change induced by mobile technology in low-income countries is occupational shifts away from farming to business and retail and via rural-to-urban migration (Suri and Jack, 2016; Batista et al., 2018; Lee et al., 2021). We analyze this dimension in our study by testing the impact of the smartphone on participants' investment in farming and market trading. Farming is measured by the self-reported number of hours per day spent on farming. Market trading is measured based on the responses to the survey question: "What is your primary source of income?" It pools those who say, selling "fruit and vegetables;" "livestock or livestock products;"

"crafts, clothes, cloth;" "cooked goods;" and "cash crops." In contrast, non-market traders are coded as those who receive income from business, wages or salaries in cash, other casual cash earnings, and cash transfers. Consistent with existing research, we do find evidence of a shift in occupations, with smartphone recipients spending less time on farming (0.14 SD less than control) and being more likely to specialize in market trading (0.14 SD more than control). One difference with existing research, however, is these occupational changes seem to be driven less by mobile money use and more by enhanced communication capabilities. This is borne out in A13.2, in which participants who report using phones more for income-generating activities state communicating with customers and clients, pointing to the benefits phones provide to market traders.

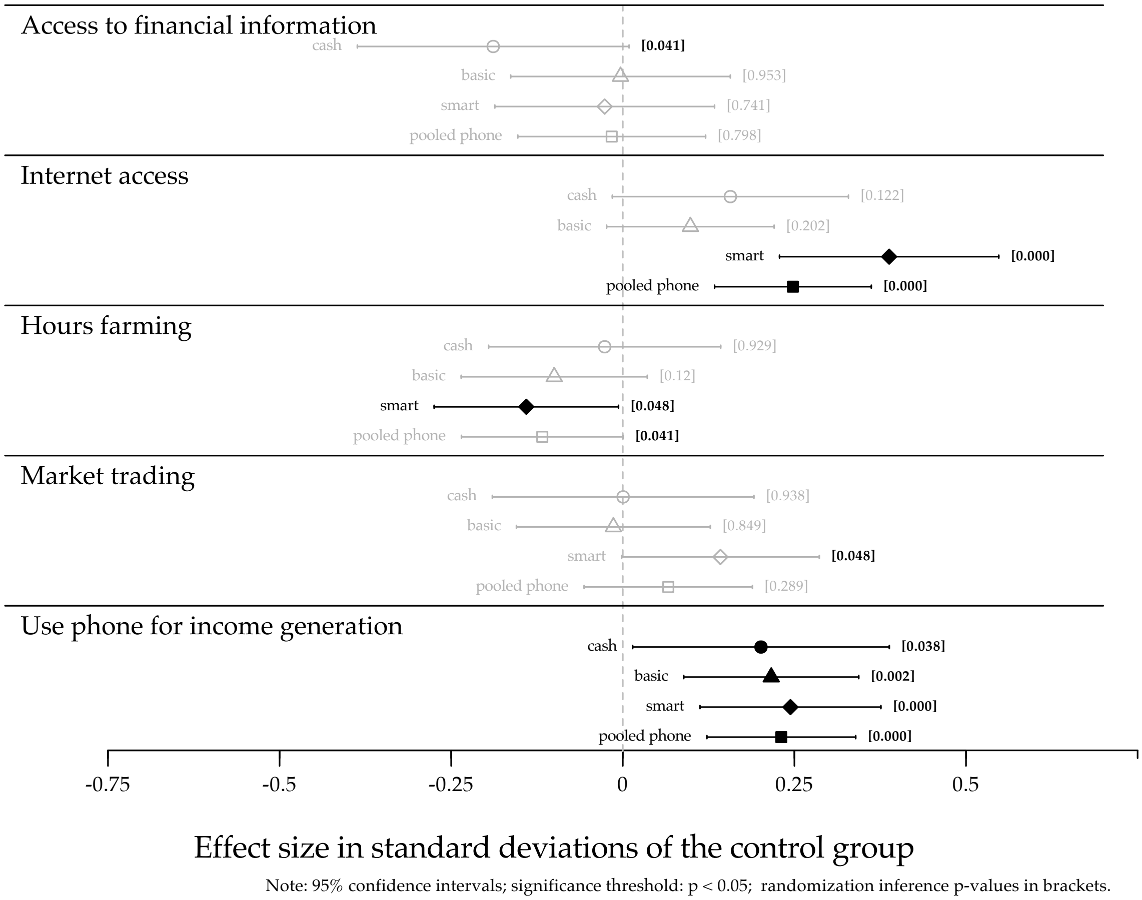

Figure A13.1: **Treatment effects on access to financial information, internet, occupational choice, and income-generation.** Access to financial information refers to participants' assessment of how hard it is to obtain financial information, such as prices of goods and services, that one needs for her job or business activities: "1" (not difficult at all) to "4" (very difficult). This variable has been inverse coded so higher values indicate less difficulty acquiring access to information. Internet access indicates how frequently participants report using the internet: "5" (every day) to "0" (never). Hours farming indicates number of hours per day spent on farming. Market trading indicates whether the primary source of income is from direct selling of goods, crops or produce. Income-generation indicates how frequently participants report using a mobile phone for income-generating purposes: "4" (every day) to "0" (never). Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

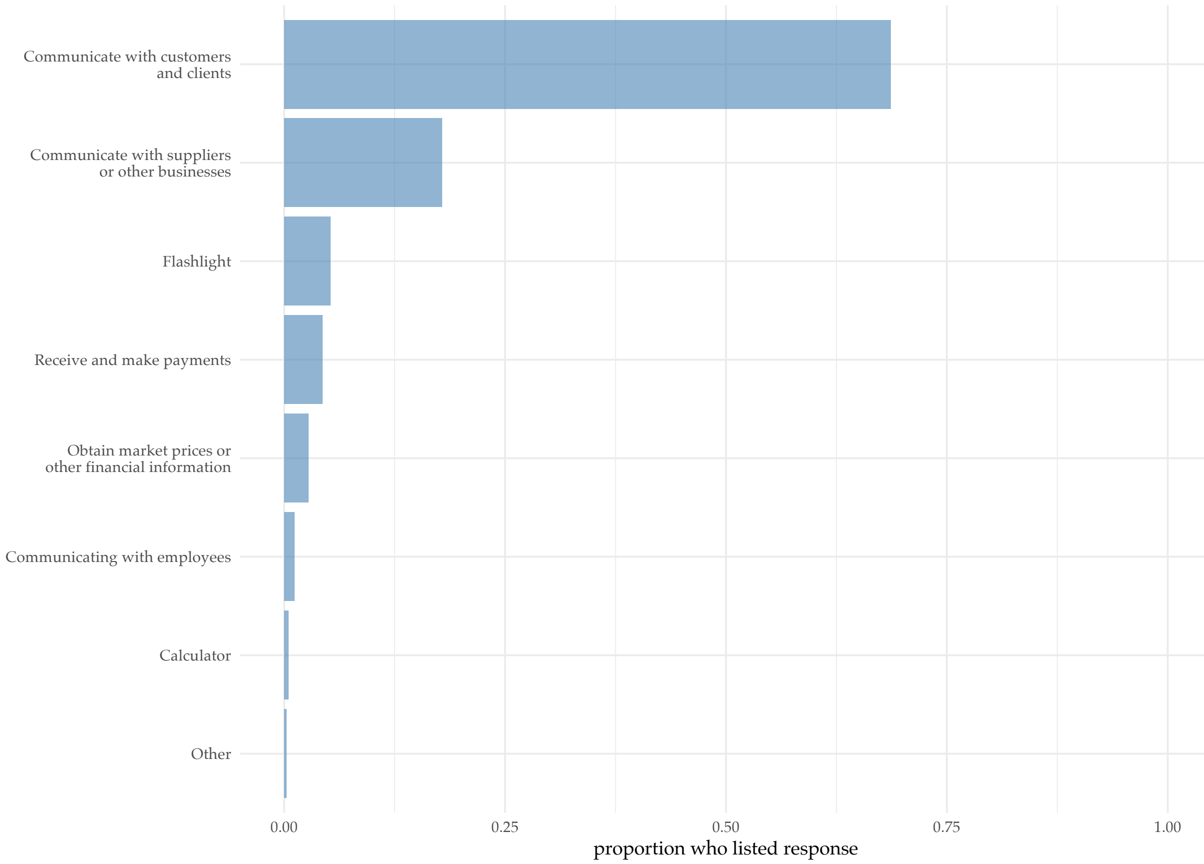

Figure A13.2: **How respondents report using a mobile phone for income-generation.**

A14 Effects of Mobile Phone Ownership on Weekly and Monthly Income

Figure A14.1 reports the effects of the treatment conditions on weekly and monthly income. Weekly and monthly income are derived from survey responses to questions about "the approximate amount of income you earned" in past week or month. Consistent with how we measure consumption, we ask respondents the amount they earned and then had the enumerators enter both the respondent's answer amount and the Tanzanian shilling range corresponding to the amount. This allows us to validate

answer responses and correct entry errors.

Figure A14.1 reports results from the weekly and monthly amounts winsorised at the 1st and 99th percentiles. The bottom two models report results from the weekly and monthly range results. Consistent with their more proficient use of mobile money, we see the strongest income effects in the basic phone condition.

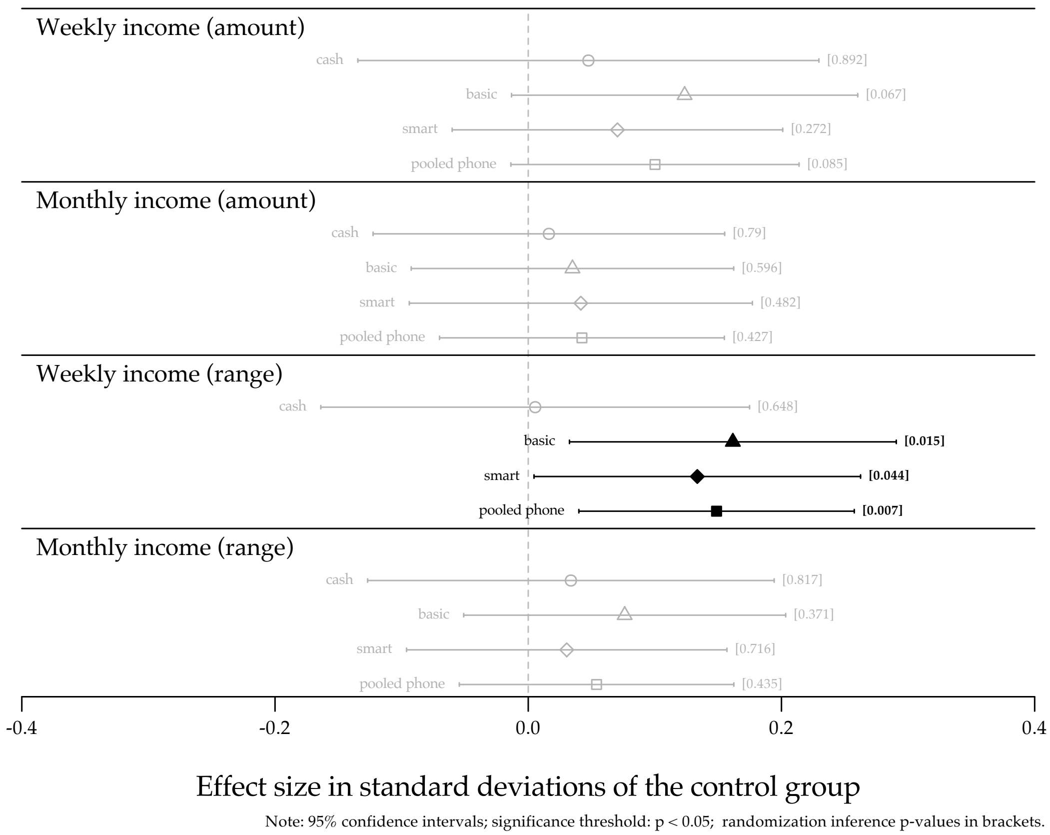

Figure A14.1: **Treatment effects on weekly and monthly income.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

A15 Robustness of Consumption Results

Here we provide more information on how we aggregate household consumption and show the results are robust to alternative aggregation rules. To reduce entry errors during the consumption module, enumerators entered both the respondent's answer amount and the Tanzanian shilling range within which this amount fell for each basket. This allows us to validate answer responses. At endline, 1.5% of responses to the consumption questions had mismatched amounts and ranges. These responses were corrected as needed following a recoding rule that minimized the number of assumptions needed to update an amount and/or range value for a given response. The most common entry errors appear to be: a.) indicating the wrong range value for a reported amount; and b.) missing a 0 from the amount answer; thus under-counting the amount by a power of 10. In our primary consumption measure, we fix these errors. We also winsorised each basket at the 1st and 99th percentiles before summing to ensure outliers are not skewing the results. The results from the cash condition are the only ones sensitive to winsorising due to extreme outliers in this group. (Compare models 3 and 4 to all others below.)

We use a similar baseline measure of consumption (summing recoded winsorised amount values for each basket). But to avoid losing additional observations, for non-reported baskets we replace the missing value with the overall baseline mean for the basket, which is consistent with the approach we use for addressing missingness for all baseline covariates.

To ensure the consumption results are robust to our aggregation decisions (summing recoded winsorised amount values for each basket and dropping missing observations), we re-run the results as follows: a.) dropping any observations with inconsistent range and amount values; b.) summing the recoded baskets but without winsorising the

values; c.) taking the row mean of the consumption components (thus, setting missing values as equivalent to the mean of the reported consumption components); and d.) coding non-reported consumption components as 0 before summing. We also impute non-reported consumption baskets employing baseline measures of overall household consumption—as reported in A9. As indicated in Figure A15.1, all aggregation measures produce highly consistent results.

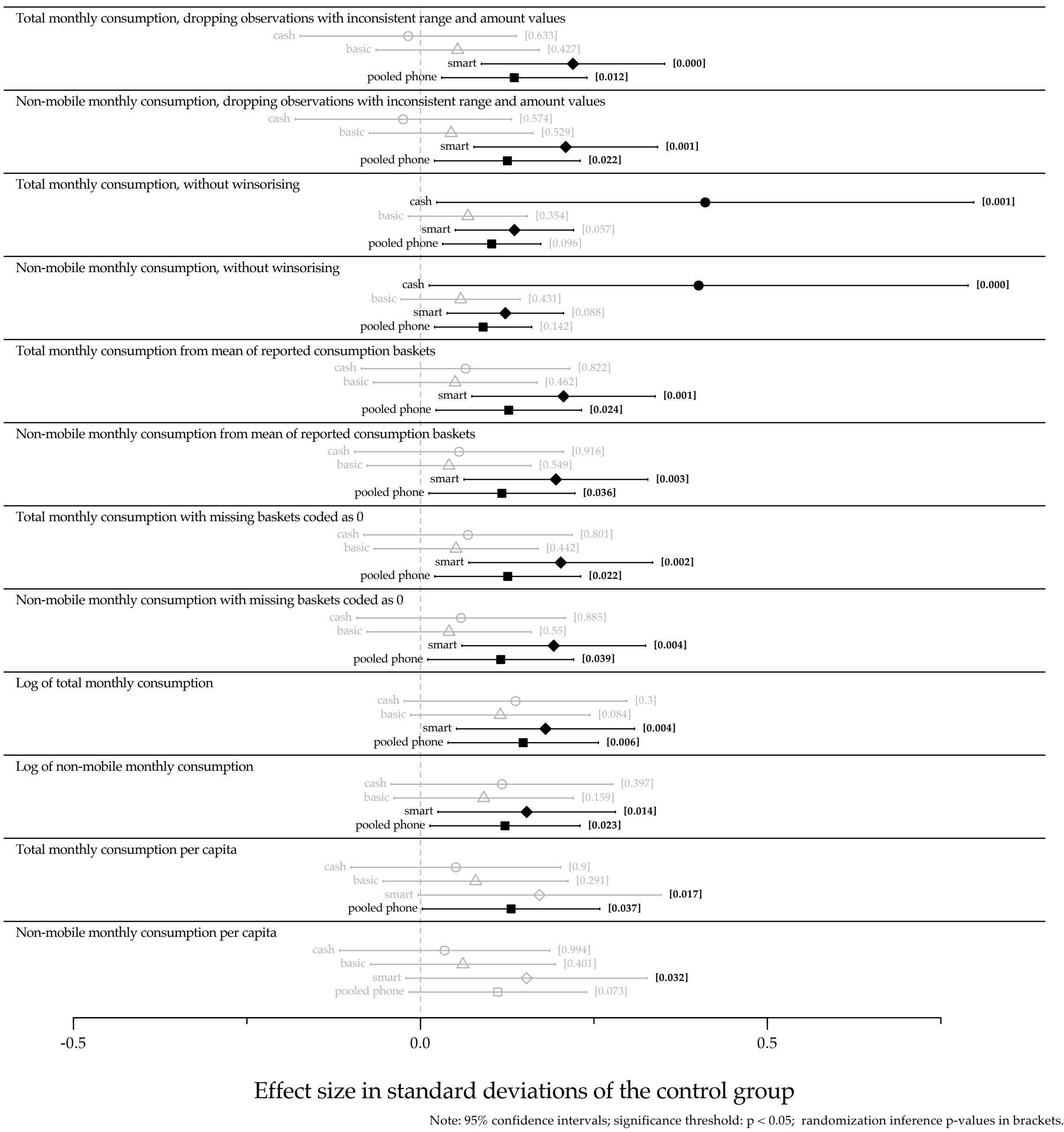

Figure A15.1: **Robustness of consumption results across alternative aggregation rules.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

A16 Analysis of Smartphone Compliance and Household Consumption

The consumption effects are driven by the smartphone condition. Here we ensure that the long-run consumption effects operate through keeping and using the smartphone rather than selling it. If it was the sale of the smartphone, then we should not observe stronger effects among the compliers (those who keep their smartphones through the end of the study). To test this, we estimate the complier average causal effect (CACE) (Gerber and Green, 2012). Using a participant's reported ownership of a smartphone at endline as compliance, we employ a two-stage model in which assignment to the smartphone condition is used as an instrument. The results are reported in Figure A16.1. The effect size for smartphone compliers is 0.7 SD, which is 3.5 times the smartphone's ITT effects. Retaining the smartphone brought much higher economic dividends than selling it.

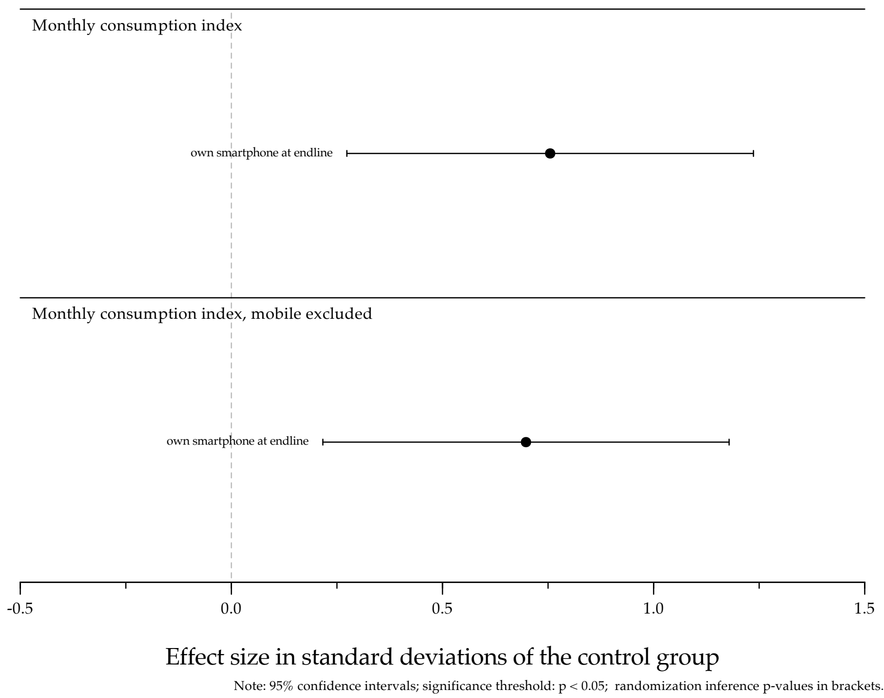

Figure A16.1: **Complier average causal effects of smartphones on household consumption.** To estimate CACE, we run a two-stage least squares (2SLS) regression analysis in which we use assignment to the smartphone condition to instrument for a participant's ownership of a smartphone at endline. (The F statistic in the first stage is 147.) Specifications include the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, blocking variables, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

A17 Analysis of Effects on Individual Consumption Baskets and Outstanding Mobile Loans

Here we report treatment effects on individual consumption baskets. In addition to mobile consumption, the smartphone significantly increased spending on transportation, schooling, community events and ceremonies, and entertainment, and led to marginally significant increases in spending on health and cooking fuel.

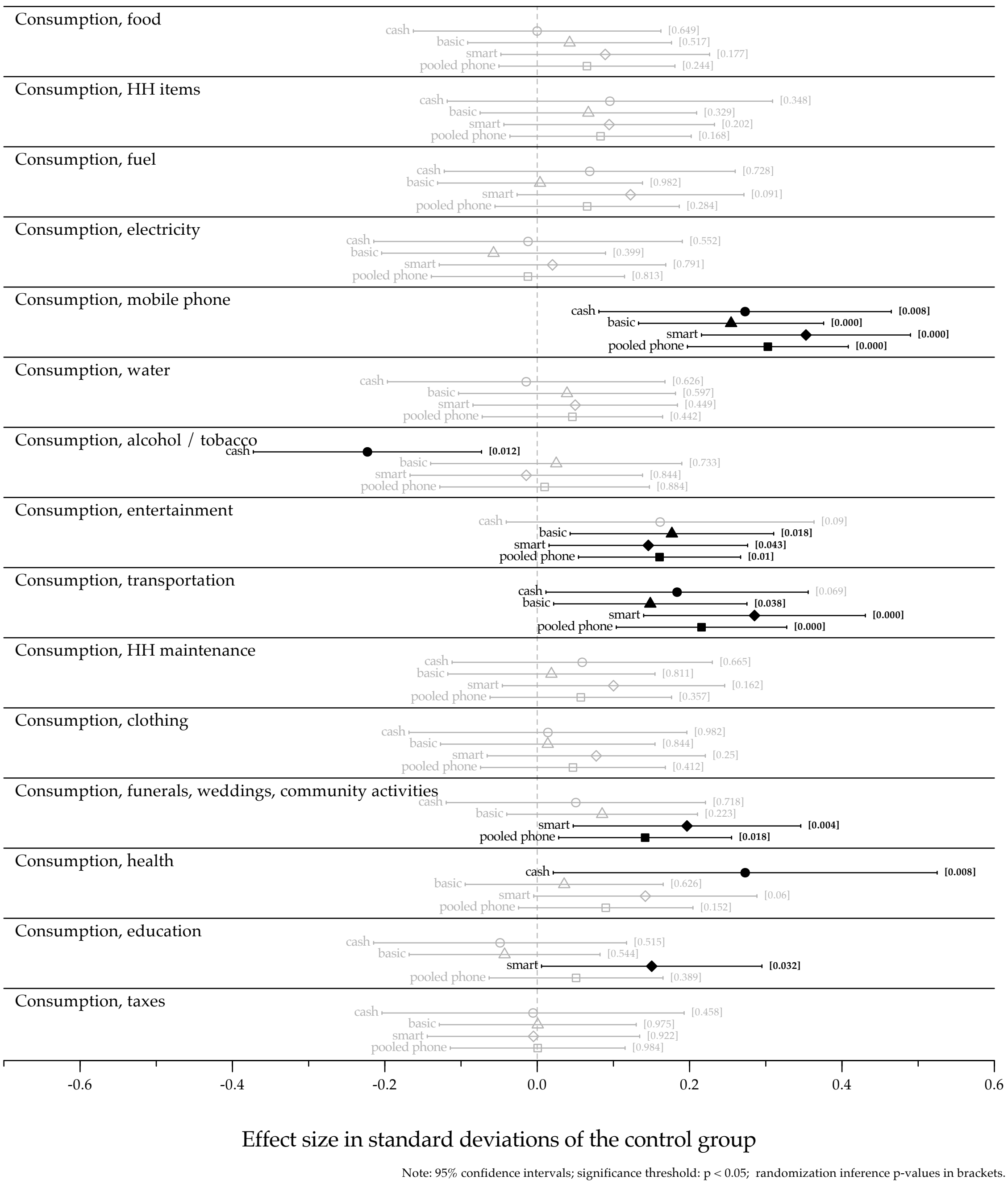

Figure A17.1: **Treatment effects on individual consumption baskets.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

Here we check whether the increased consumption gains from mobile phones caused higher-levels of indebtedness from mobile loans. While smartphone recipients were more likely to report using mobile money to receive loans (see Figure A22.3, this did not translate into significantly higher levels of outstanding loans at the end of the study as reported in Figure A17.2. Overall, levels of outstanding debt from mobile loans was very low. In the smartphone condition at endline more than 95% reported zero mobile loan debt. Among the remaining 5%, average mobile loan debt was only PPP $3.82.

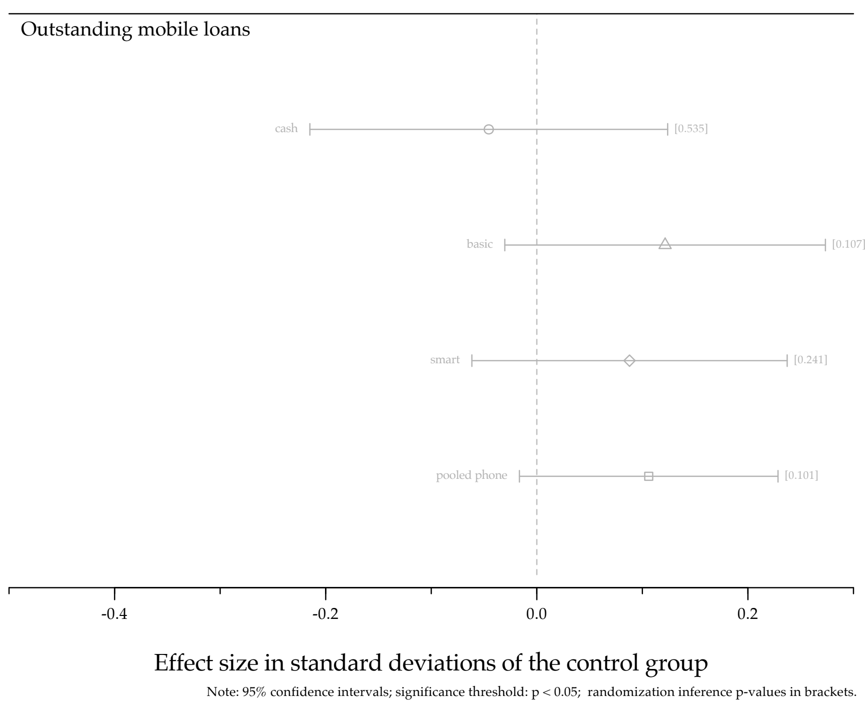

Figure A17.2: **Treatment effects on outstanding mobile loan debt.** Outstanding mobile loan amount is log transformed and standardized. Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

A18 Subgroup Effects by Baseline Literacy and Participant Program Membership

In line with our pre-analysis plan, we also report sub-group effects of mobile phone ownership by baseline literacy and participant program membership (BRAC or TASAF, one of our blocking criteria.) The former is reported in Figure A18.1. Baseline literacy was determined by respondents' abilities to read the consent form for survey participation. We distinguish between those who read the statement fluently versus those who read only part or none of the statement. As is clear, the smartphone (as well as the basic phone) treatment effects on household consumption were concentrated among literate women.

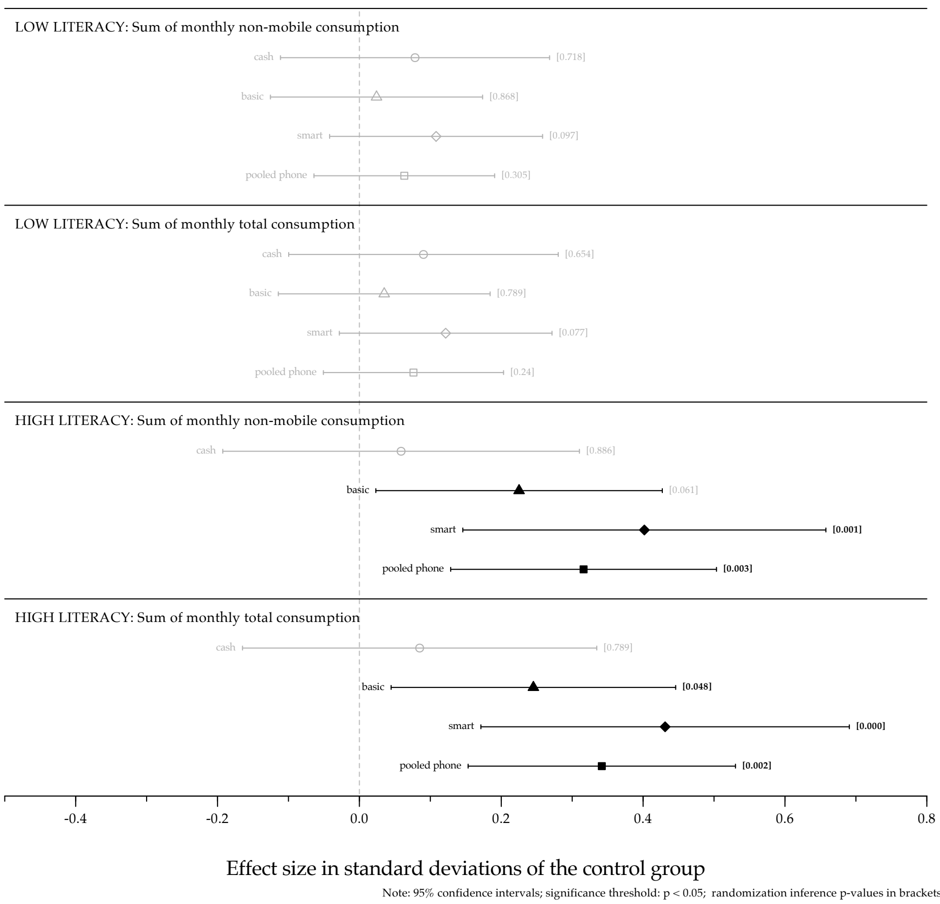

Figure A18.1: **Impact of treatment conditions on standardized measures of monthly non-mobile and total household consumption by literacy level.** Literacy was measured at baseline based on ability to read the survey consent statement. Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

We also pre-registered sub-group effects based on rural/urban locality with a focus on smallholder farmers. To check this, we compare the effects among BRAC and TASAF members as the latter tended to be more likely to reside in rural areas and practice farming (at baseline 100% of TASAF participants lived in rural areas of which 55% were farmers compared to 59% and 16% of BRAC participants, respectively). Subgroup results are reported in Figure A18.2. They confirm the importance of the smartphone treatment; it significantly boosted household consumption in both sub-groups. But the smartphone effect size on household consumption is more than 2.5 times higher among BRAC participants (the microfinance subgroup) than those in TASAF. This suggests that smartphones may be especially beneficial among households *without* full mobile phone saturation among adults, but with higher levels of capital, education, and digital literacy. Yet, we also see that among the members of TASAF (the poverty reduction program), basic phones also significantly increased consumption. The effect size is on par with smartphones among this sub-group, suggesting that, among poorer households, basic phones are a more cost-effective measure. In contrast—and accounting for its overall null effect—basic phones had no effect on consumption among microfinance clients.

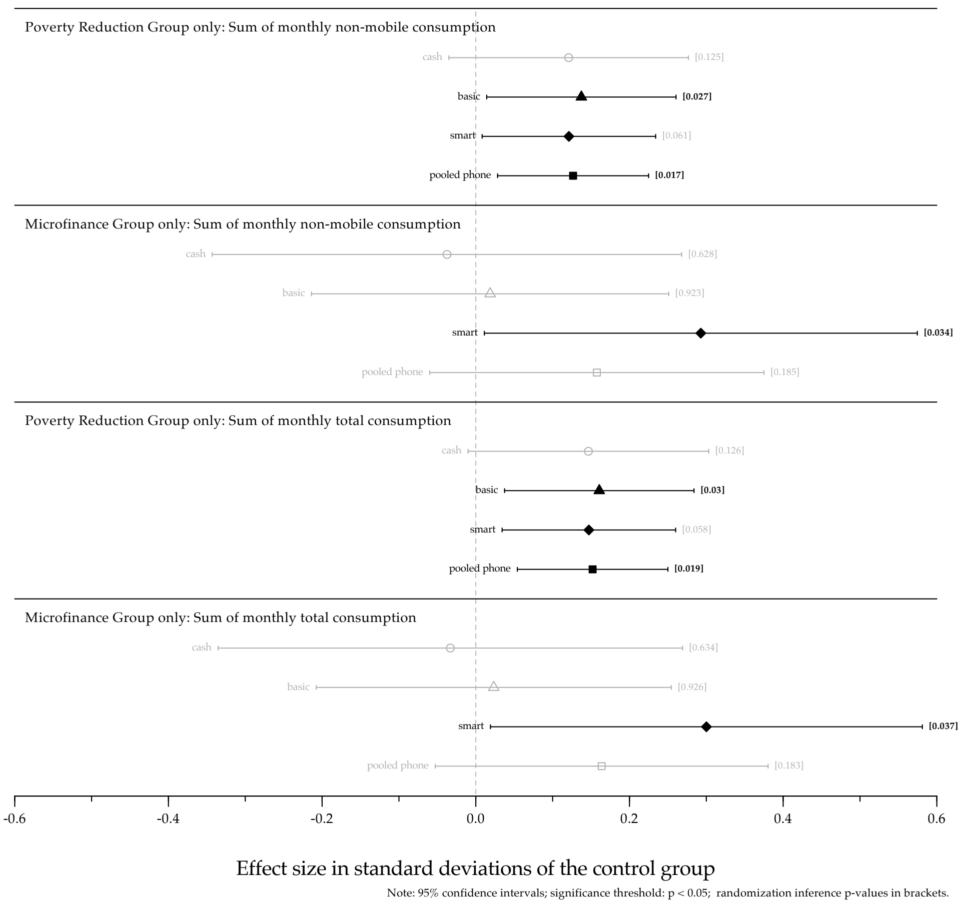

Figure A18.2: **Impact of treatment conditions on standardized measures of monthly non-mobile and total household consumption across the microfinance and poverty reduction program sub-groups.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

What accounts for the null effects of basic phones among BRAC participants? The following is an exploratory analysis looking at household mobile phone capacity. Table A18.1 reports results of a two-stage least squares regression analysis of monthly consumption using assignment to the phone conditions to instrument for household handset count. It shows that each of the conditions, except the basic phone among BRAC participants, had significant effects on consumption through the increase in number of phones in the household. For the basic phone among BRAC participants, the first stage (effect on household handset count) is significant but a weak instrument.

Consistent with these results, BRAC participants reported not retaining the basic handset we provided them compared to TASAF members. At endline only 58% of BRAC participants in the basic phone group reported still having the Samsung B110, compared to 79% of poverty reduction program participants. Strikingly, this meant that BRAC participants in the basic phone condition had lower rates of phone ownership at endline compared to their TASAF counterparts. Why microfinance participants failed to retain these handsets is a bit of a puzzle, especially as their households remained without full phone saturation among adults at endline (0.75)—and the non-retention of basic handsets appears to have come at the cost of limiting potential gains in household living standards.

	Second-stage. Standardized monthly total consumption			
	1	2	3	4
	(BRAC)	(BRAC)	(TASAF)	(TASAF)
HH handset count	0.143	0.590*	0.342*	0.233*
	(0.543)	(0.274)	(0.136)	(0.095)
	First-stage. Household handset count			
	1	2	3	4
	(BRAC)	(BRAC)	(TASAF)	(TASAF)
Basic	0.206*		0.480***	
	(0.103)		(0.097)	
Smartphone		0.512***		0.626***
		(0.113)		(0.089)
Control mean: HH handset count	1.43	1.43	0.74	0.74
F-statistic	3.99	20.35	24.45	49.34
Observations	302	312	370	368

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A18.1: **IV regression analysis of household handset count on monthly total consumption.** Results derived from a two-stage least squares regression analysis of monthly total household consumption using assignment to each of the phone conditions across program membership sub-groups on the standardized measure of monthly total consumption. Models include solar charger and voucher cross-cutting treatment conditions, a baseline measure of consumption, baseline measure of household handset count, blocking variables, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size. (Covariates not reported.) First-stage F-statistic is reported at the bottom of the table. Robust standard errors clustered at individual level.

A19 Benefit-Cost Analysis

Here we detail the costs of the mobile phone program and report the benefit-cost analysis. We focus on the overall smartphone effects and the effects on the TASAF subgroup—representing the poorest segment of the sample and of the population in Tanzania generally. Table A19.1 specifies the benefits and costs of the intervention.

ITT effects (row 1) have been calculated from the fully-saturated "long" model including the smartphone, solar charger, and voucher treatments and all of their interaction terms to most precisely isolate the effects of the smartphone. (See A25 for coefficient plots.) The models also include the normal battery of covariates and blocking variables. The ITT effects are calculated for total household consumption (including mobile-related expenditures). But we include mobile consumption per handset in the household as a cost to use the phone. In addition to mobile consumption, costs include the price of the handset, distribution and training per person, and administrative costs associated with distribution and training.

	Smartphone (overall)	Basic phone (poverty reduction program)	Smartphone (poverty reduction program)
Program benefits PPP US$ (2017)			
(1) ITT effects on total annual household consumption	$653.88	$463.97	$352.69
Program costs PPP US$ (2017)			
(2) Handset cost	$186.28	$61.36	$186.28
(3) Annual mobile-related consumption per HH handset	$72.03	$67.47	$60.28
(4) Program distribution and training costs per participant	$40.46	$40.46	$40.46
(5) Administrative costs at 15%	$6.07	$6.07	$6.07
(6) Total program costs	$304.84	$175.36	$293.09
(7) Benefit-cost ratio: (Row 1)/(Row 6)=(Row 7)	2.14	2.61	1.20

Table A19.1: **Benefit-cost analysis of mobile phone intervention on living standards of low-income households.**

The cost-effective impact of mobile technology rivals and in some cases surpasses other anti-poverty interventions. For example, based on our calculations of the results from Haushofer and Shapiro (2016), cash transfers are estimated to increase consumption roughly 1.1:1 for the cost of the cash grant (excluding programmatic costs). Banerjee et al. (2015) find that across six countries a multifaceted poverty graduation program that provided a productive asset grant plus training and other support had a benefit-cost ratio of 1.6:1 (excluding the Honduras program where many beneficiaries invested in chickens that were wiped out by an illness, the benefit-cost ratio was 2.3).

A20 Mechanisms and Mediation Analysis

The power of mobile phones stems from the technology's multidimensionality; it enables, among other things, long-distance communication, access to information and the internet, and mobile money transfers (Aker and Mbiti, 2010). As noted at the outset, traditionally the poor have faced steep barriers to each. How much does mobile phone ownership help the poor overcome these barriers? While we observe null effects on self-reported access to market information, we find mobile phones had significant causal effects on communication capabilities and the uptake and use of mobile money; and smartphones significantly boosted access to mobile internet.

To explore the degree to which these channels correlate with changes in household consumption, we undertake a mediation analysis. To do so, we employ the mediation method developed by Kohler et al. (2011) that enables us to estimate the effects of the phone treatment conditions compared to control on household consumption mediated by: *the mobile phone use index* (which captures the frequency and intensity of mobile phone use, of which the predominant reported use was making and receiving calls); *the mobile money index* (which includes both self-reported and behavioral measures of mobile money uptake); and *frequency of internet access*. Beyond these individual variables, we also include a mediator to capture household effects as measured by mobile phone capacity. The results are reported in Figure A20.1. We find participants' use of the smartphone as a communication device mediated some 24% of the effect. This corresponds with use of the phone to aid in market trading and communicate with customers and clients. In contrast, a smaller and statistically insignificant share of the consumption effect is mediated by participant mobile money use. A bit surprisingly, given how low internet use was in absolute terms, we find a more significant role for this channel (mediating some 18% of the effect). Even accounting for increased phone

use among individual recipients, we observe a substantial share of the effect (36%) stems from the increase to the household's mobile phone capacity (as measured by a count of the phones in the household). The significant effects of both individual and household channels points to the dual benefits of eliminating the mobile gender gap—it directly boosts women's mobile phone use while also reducing the costs that household members face from joint dependence on a single handset.

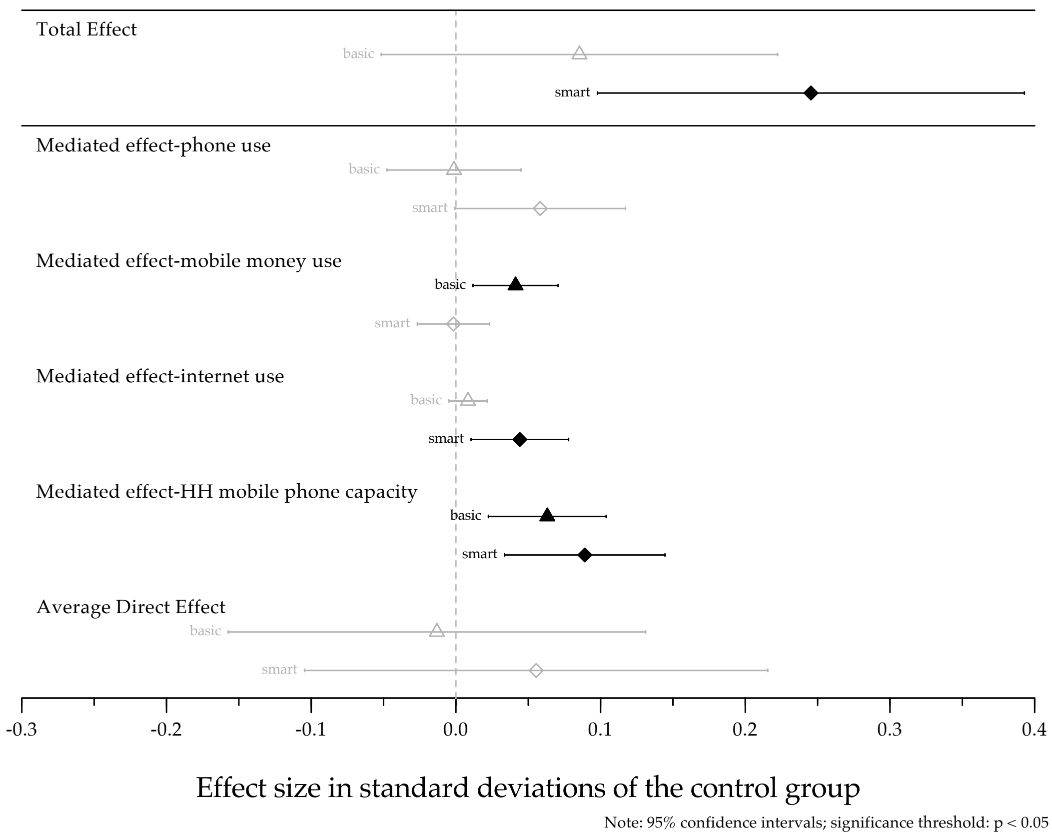

Figure A20.1: **Mediation analysis of mobile phone ownership on household consumption.** Models employ the KHB decomposition method (Kohler et al., 2011) to estimate the total, mediated and average effects of basic phone and smartphone ownership on total monthly household consumption versus control. The mediators include the mobile use index, mobile money index, frequency of internet access, and household phone count. The specifications include the solar charger and voucher cross-cutting treatment conditions, blocking variables, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

A21 Smartphone Ownership, Use by Other Family Members and Correlation with Change in Household Consumption

The mediation analysis reported in A20 suggests that the power of the smartphone was in strengthening participants' mobile phone use *and* increasing the household's overall phone capacity. Further substantiating the importance of household externalities, we observe that the strongest consumption gains accrued to those households in which participants reported their spouses also used the smartphone. This is illustrated in Figure A21.1.

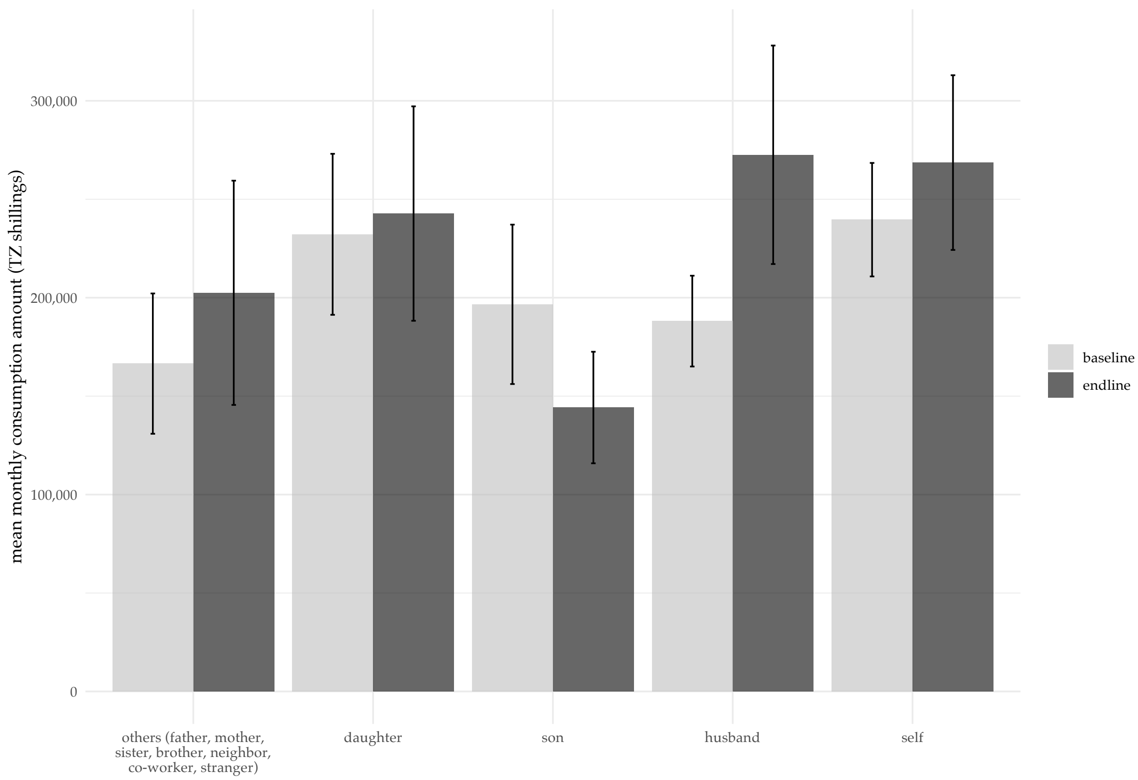

Figure A21.1: **Change in household consumption by family members' smartphone use.** Family members' smartphone use corresponds to whom the participant reported used the phone the most besides herself at the end of the endline survey. "Self" indicates when the participant reported no one else used the phone. "Self" was the modal category with 35.3%, followed by husband (22.5%), son (21.7%), daughter (13.1%), and others (7.4%). Data restricted to those in the smartphone group.

Unfortunately we do not have detailed information on other household members' phone use and economic livelihoods. Thus we are unable to precisely estimate household spillover effects—an important line of inquiry for future research. What we can discern is a potential mediating effect of household dynamics on mobile phone use. When respondents reported that their spouses, and especially their sons, appropriated the handsets, their households realized no consumption gains. If, on the other hand, the participant retained control and used the smartphone jointly with her husband, this correlated with a significant increase in household consumption. This is illustrated in Figure A21.2. These households start at similar consumption levels at the outset

of the study but then diverge based on cooperative or non-cooperative smartphone use. Caution is warranted in the conclusions we draw from these patterns as they are merely descriptive and require the comparison of small sub-groups, but they resonate with a growing body of research on the importance of household bargaining and gender norms on the impact of mobile technology (Aker et al., 2016a; Barboni et al., 2018; Riley, 2019).

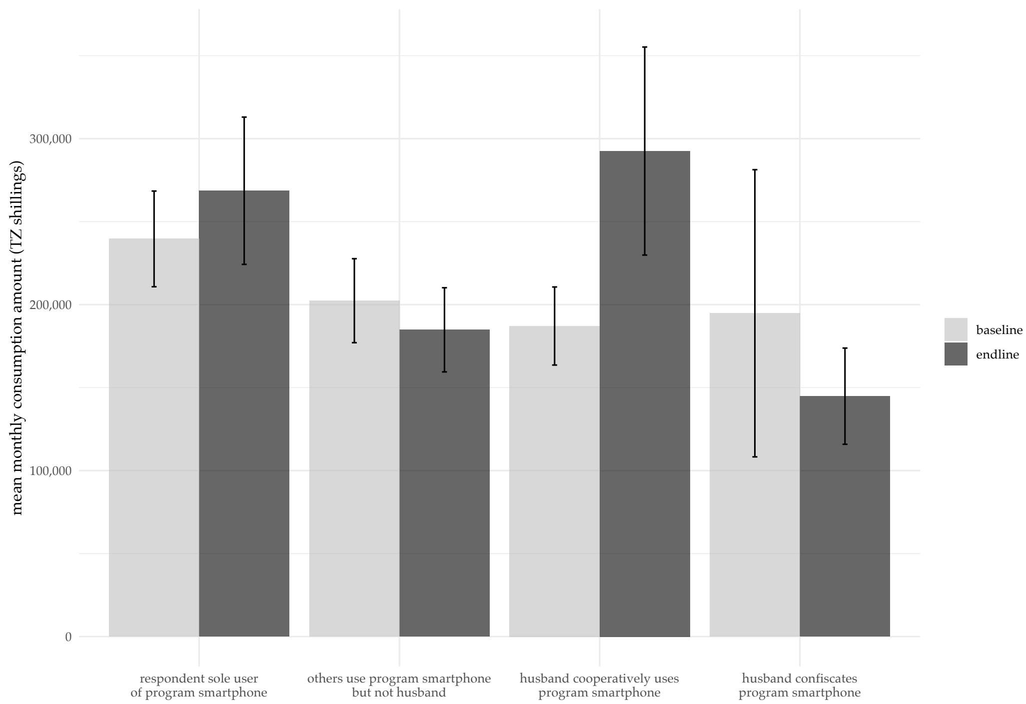

Figure A21.2: **Household use of smartphone and consumption.** Baseline and endline mean total monthly consumption across different ways the program smartphone was reportedly used. "Husband confiscates program phone" corresponds to those respondents who reported their husbands were the only ones who used the smartphone. Data restricted to those in the smartphone group.

Non-cooperative phone use may also account for the negative consumption effects observed when the participant reported that her son also used the handset. (See again Figure A21.1.) Sons were more likely to use the smartphone than the basic handset but they also were reported to be more likely to monopolize control of the former. (See Table A21.1.) It appears this use did little to improve the household's welfare

and may have made it worse off. Of course, this is merely a descriptive finding and other factors may account for both the son's use and household welfare.

	Handset Type	
Who Else Used Project Phone	Basic Phone	Smartphone
Only Participant	43.6%	35.3%
Spouse	25.1%	22.5%
Son	11.0%	21.7%
Daughter	11.9%	13.1%
Other	8.4%	7.4%

...and How? **Those reported to have appropriated the handset**	Basic Phone	Smartphone
Spouse	4.8%	12.7%
Son	5.4%	32.9%
Daughter	15%	21.7%

Table A21.1: ***Upper table*** **Project handset use by participants and other household members.** Self-reported data in response to a question at the end of the survey, "Besides you, who used your phone the most?" 'Only participant' corresponds to those who reported other family members did not use the project handset. ***Lower table*** **Non-cooperative phone use across different household members who also used handset.** Non-cooperative use corresponds to participants who reported that their husband, son or daughter was the *only one* who used the phone, suggesting the participant no longer was able to use the handset.

The analysis in the preceding two sections suggests that modes of mobile technology ownership mediate its impact on the household's economic well-being. When participants retained control of the handset and used it cooperatively with other family members, the household was much better off. In contrast when the handset was appropriated from participants by other family members, the household was worse off. This suggests that women's control of mobile technology is an important mediating factor on its economic impact—and we would expect baseline factors that predict women's control of the handset to correlate with changes in household well-being.

One potentially important determinant of women's mobile technology control is their income level. Figure A21.3 reports the sub-group results of household consumption by women's baseline income levels (one of our blocking criteria). Receipt of mobile

technology caused no household welfare gains for participants in the bottom-half of the income distribution. In contrast, household consumption gains were very large for participants in the top-half of the income distribution. While participants' income levels may affect mobile technology through a number of channels, Table A21.2 illustrates that low-income women were more than twice as likely to have reported their phone was appropriated.

	Baseline Income Level	
Who Used Project Phone Most	Below Median	Above Median
Only Me	54.2%	68.6%
Mostly me; sometimes someone else	17.2%	17.0%
Equally me and someone else	5.1%	2.8%
Mostly someone else; sometimes me	8.1%	4.0%
Only someone else	15.4%	7.6%

Table A21.2: **Project handset use by participants and other household members.** Self-reported data in response to a question at the end of the survey, "Besides you, who used your phone the most?" 'Only participant' corresponds to those who reported other family members did not use the project handset.

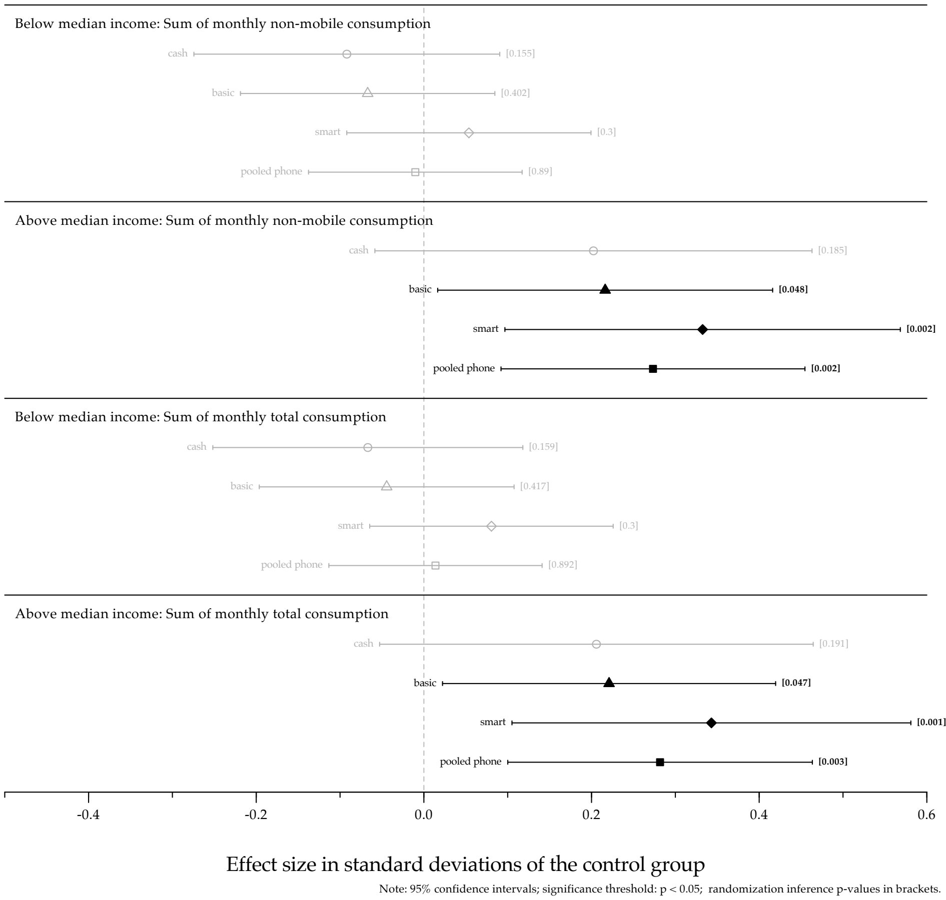

Figure A21.3: **Impact of treatment conditions on standardized measures of monthly non-mobile and total household consumption across below and above median baseline income sub-groups.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

Overall, these additional analyses underscore that mobile technology's impact on household consumption operates through the women participants—and, it seems, their capabilities and bargaining power to use and maintain control of mobile technology. Conversely, however, this suggests null effects if women lack such capabilities and

bargaining power.

A22 Impact on Women's Empowerment

Beyond the impact of mobile phones on improved economic livelihoods, does ownership lead to greater levels of empowerment—in terms of greater agency, capabilities, bargaining power, and efficacy?

Here the evidence is more mixed—at least after thirteen months. Figure A22.1 reports coefficient plots across a set of pre-registered outcomes encompassing components of empowerment. Each empowerment dimension is measured as an index: subjective welfare, participation in the formal economy, access to the internet, allocation of time, social connectedness, individual efficacy, household bargaining, intimate partner violence, and political participation. (For components of indexes, see A8. For effects on individual components, see Figs. A22.2 to A22.8 below.) Mobile phone ownership significantly affected participation in the formal economy, access to the internet (at least in the smartphone group), allocation of time (with less time spent on farming and more time spent on chores), and social connectedness. Yet, we do not see evidence that women's phone ownership catalyzed greater political engagement, boosted individual efficacy, or strengthened household bargaining or influence. Instead, we see some evidence that phone ownership caused a marginal uptick in gender-based violence—but importantly this did not originate from husbands. We observe null effects on intimate partner violence (Figure A22.1); instead, coercive acts were more likely from non-spouses in the form of reports of humiliation and threats (see Figure A22.8 below).

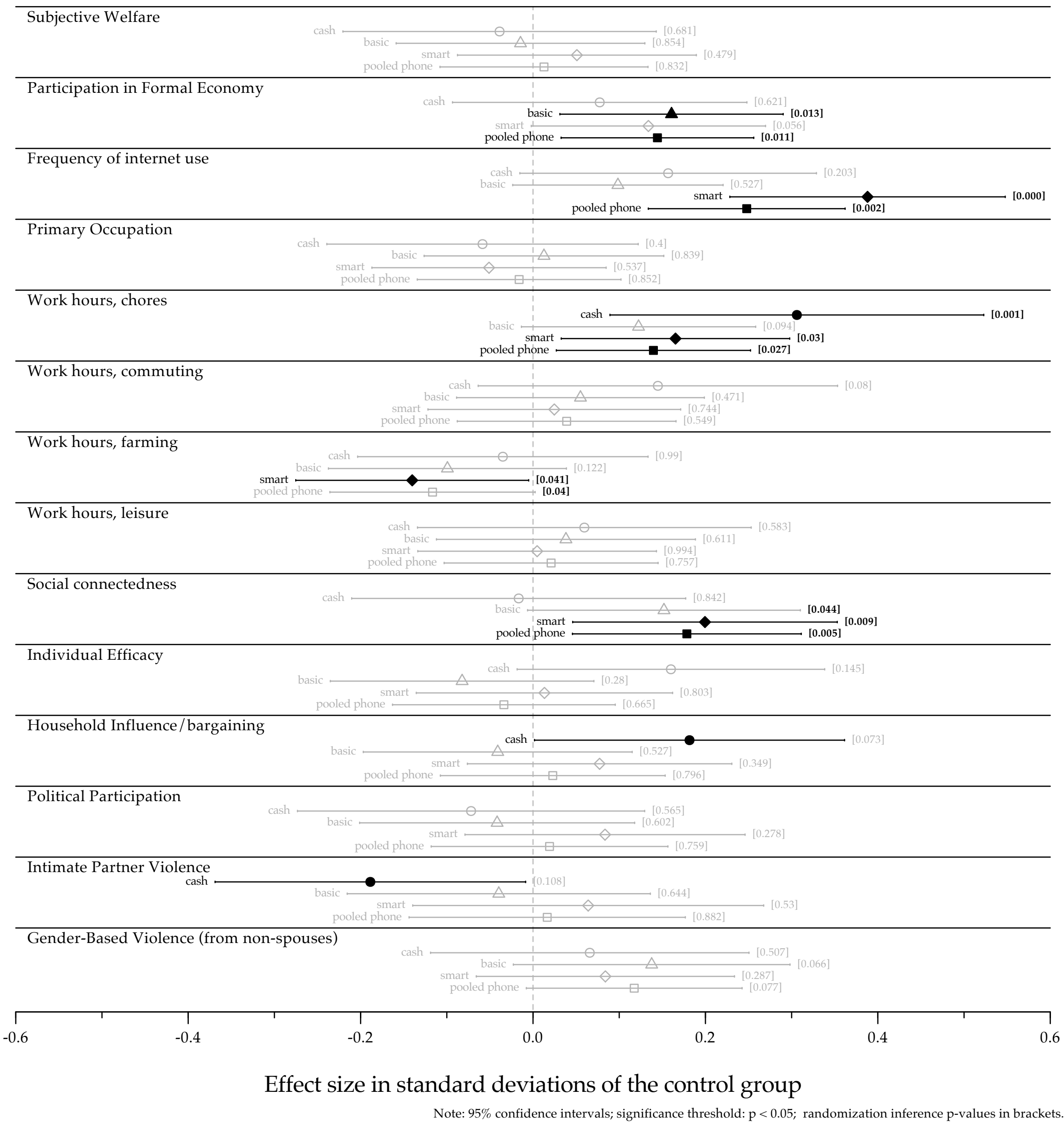

Figure A22.1: **Downstream effects of mobile phone ownership on different measures of empowerment.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

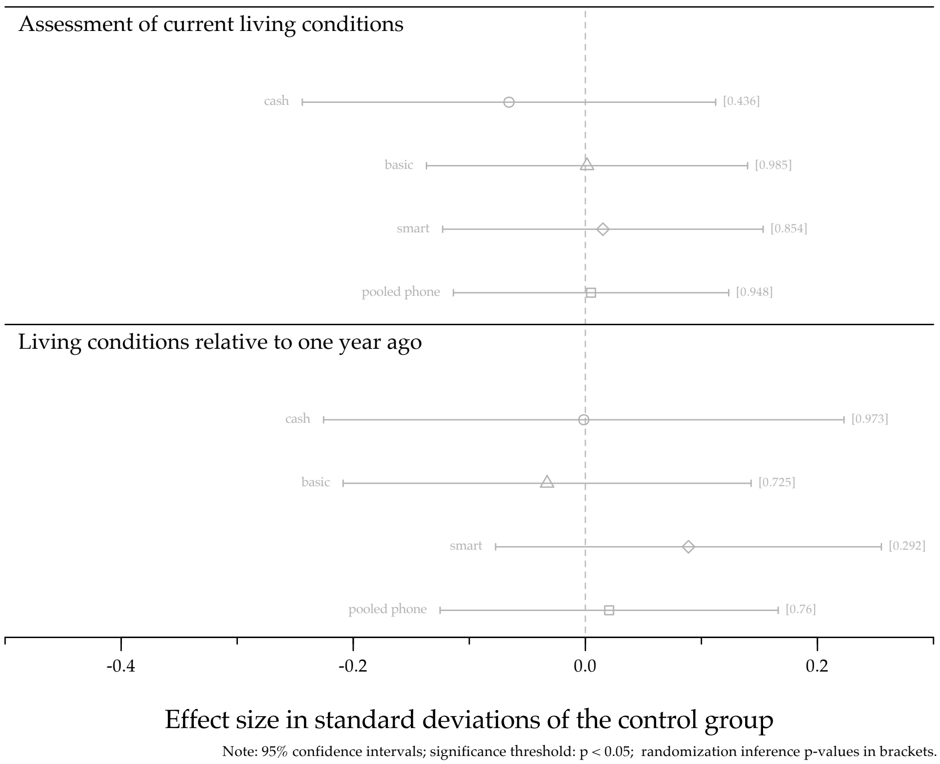

Figure A22.2: **Effects on subjective welfare components.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

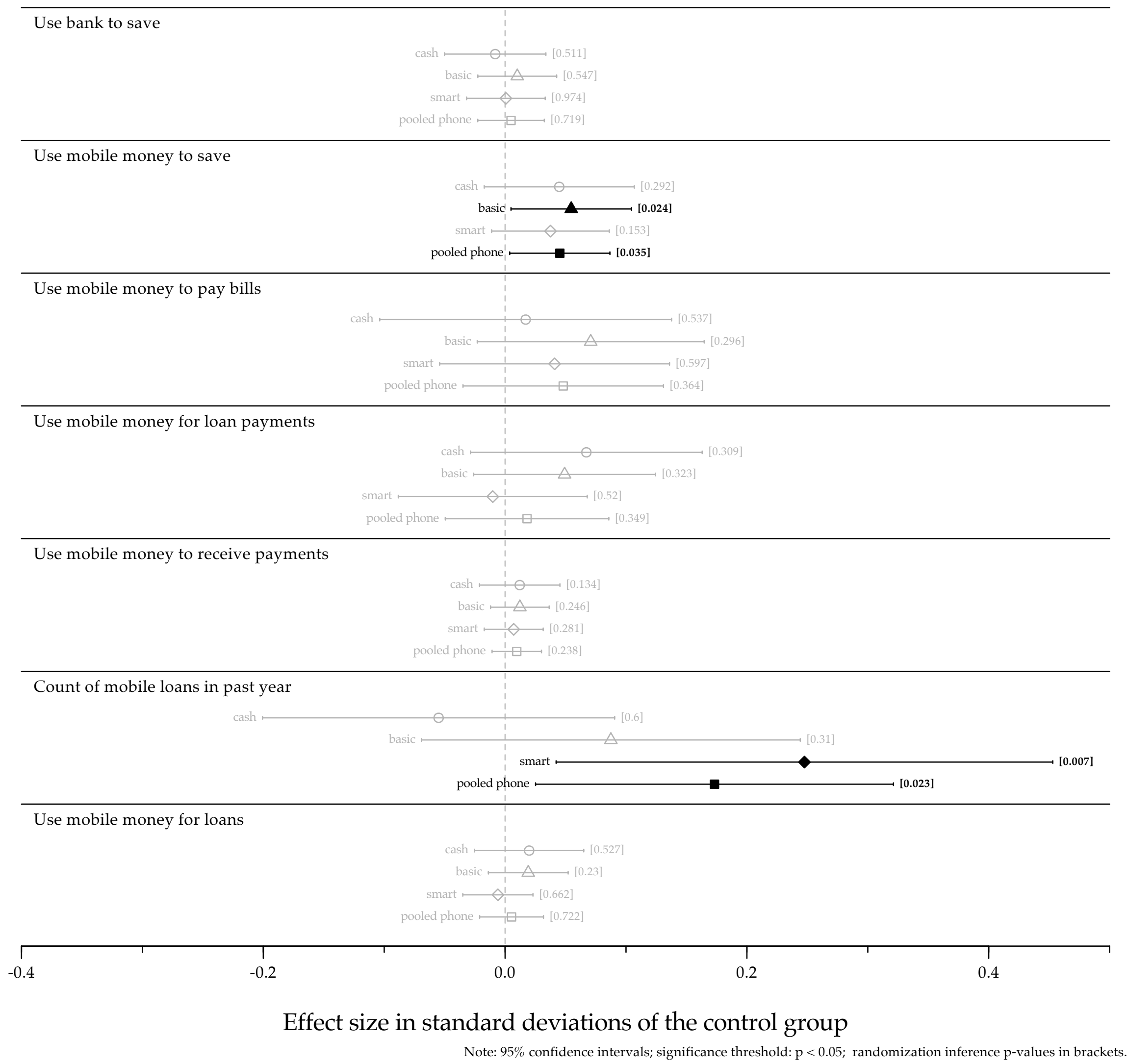

Figure A22.3: **Effects on components of participation in formal economy.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

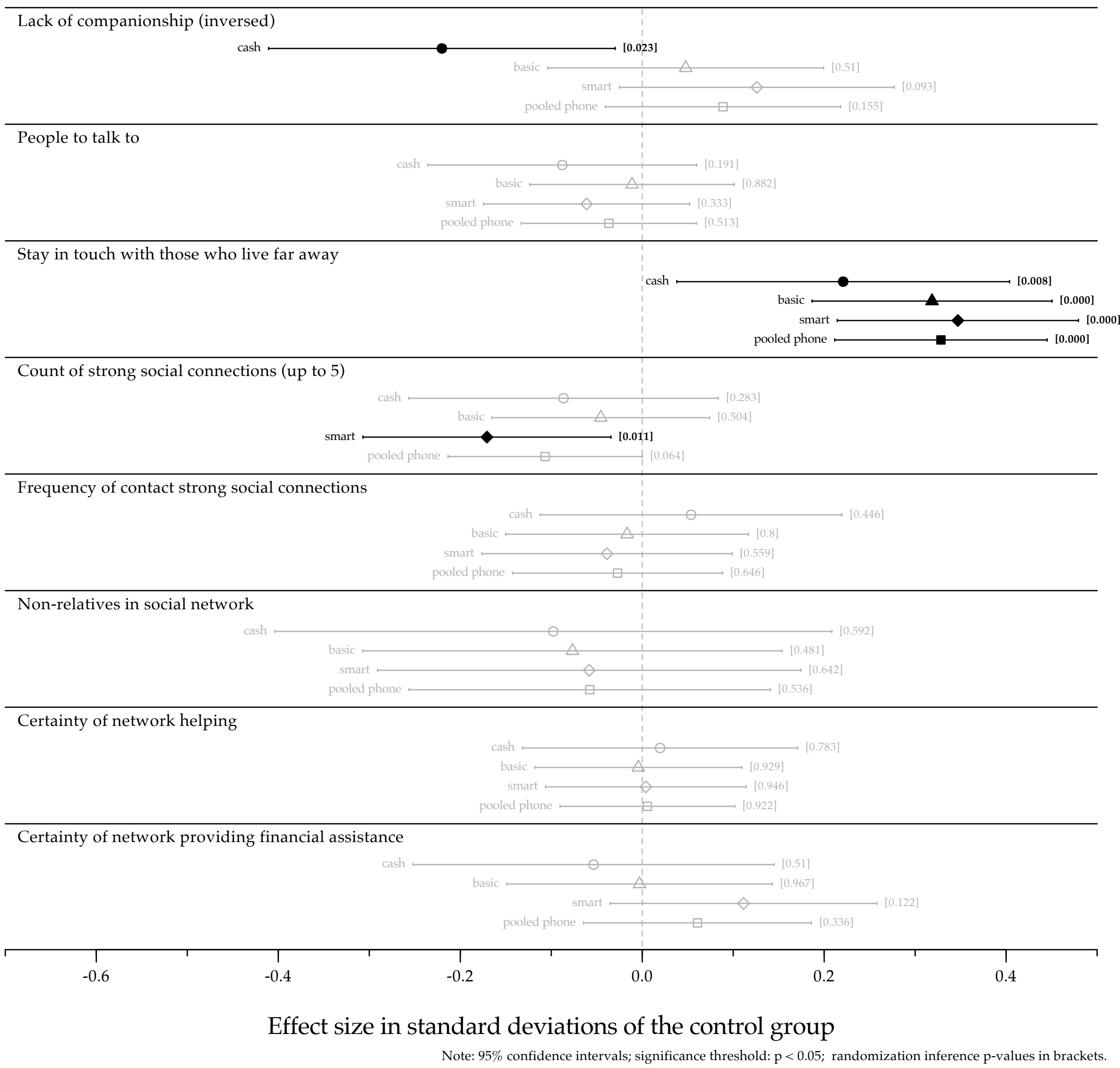

Figure A22.4: **Effects on components of social connectedness.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

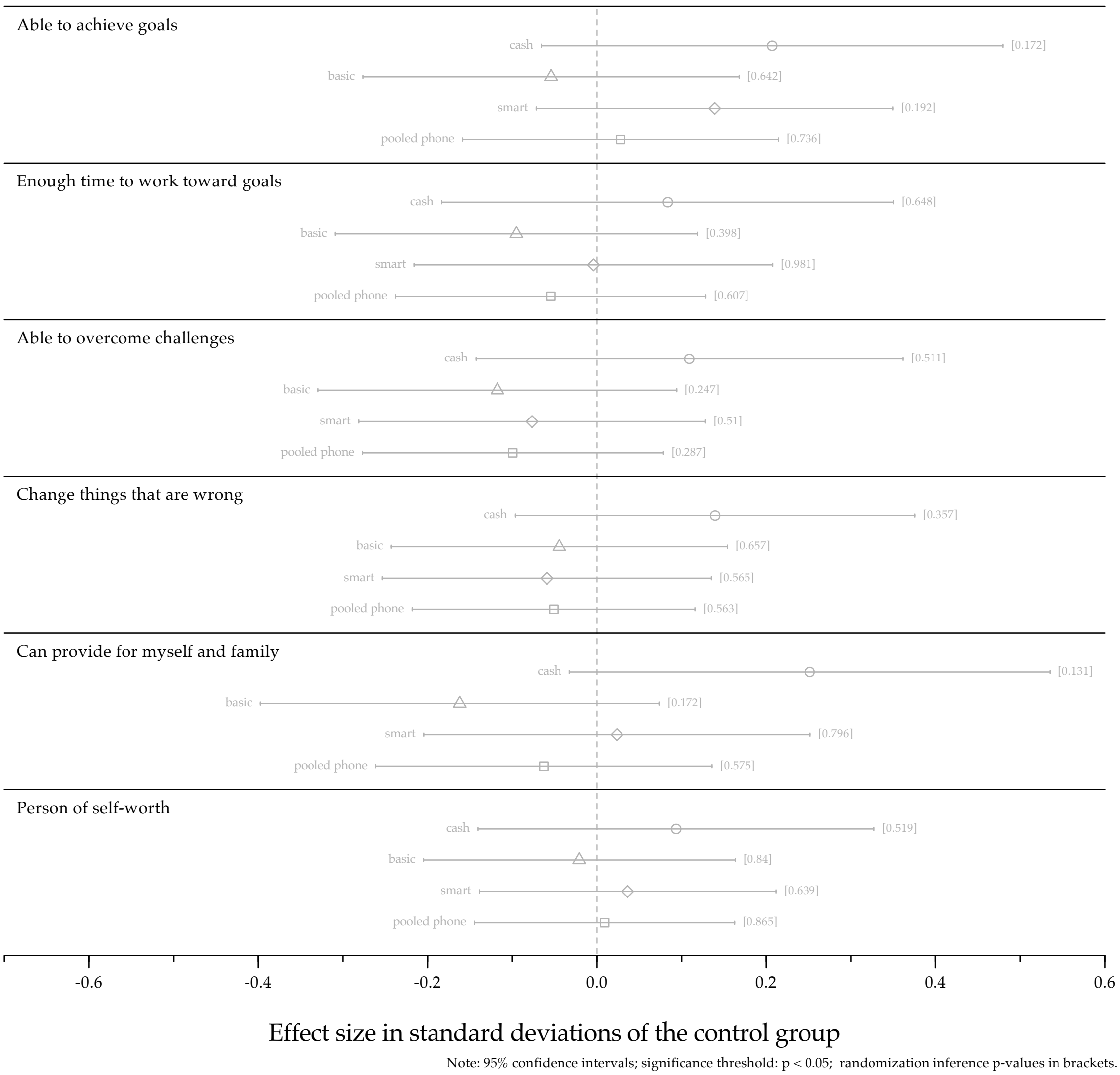

Figure A22.5: **Effects on components of individual efficacy.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

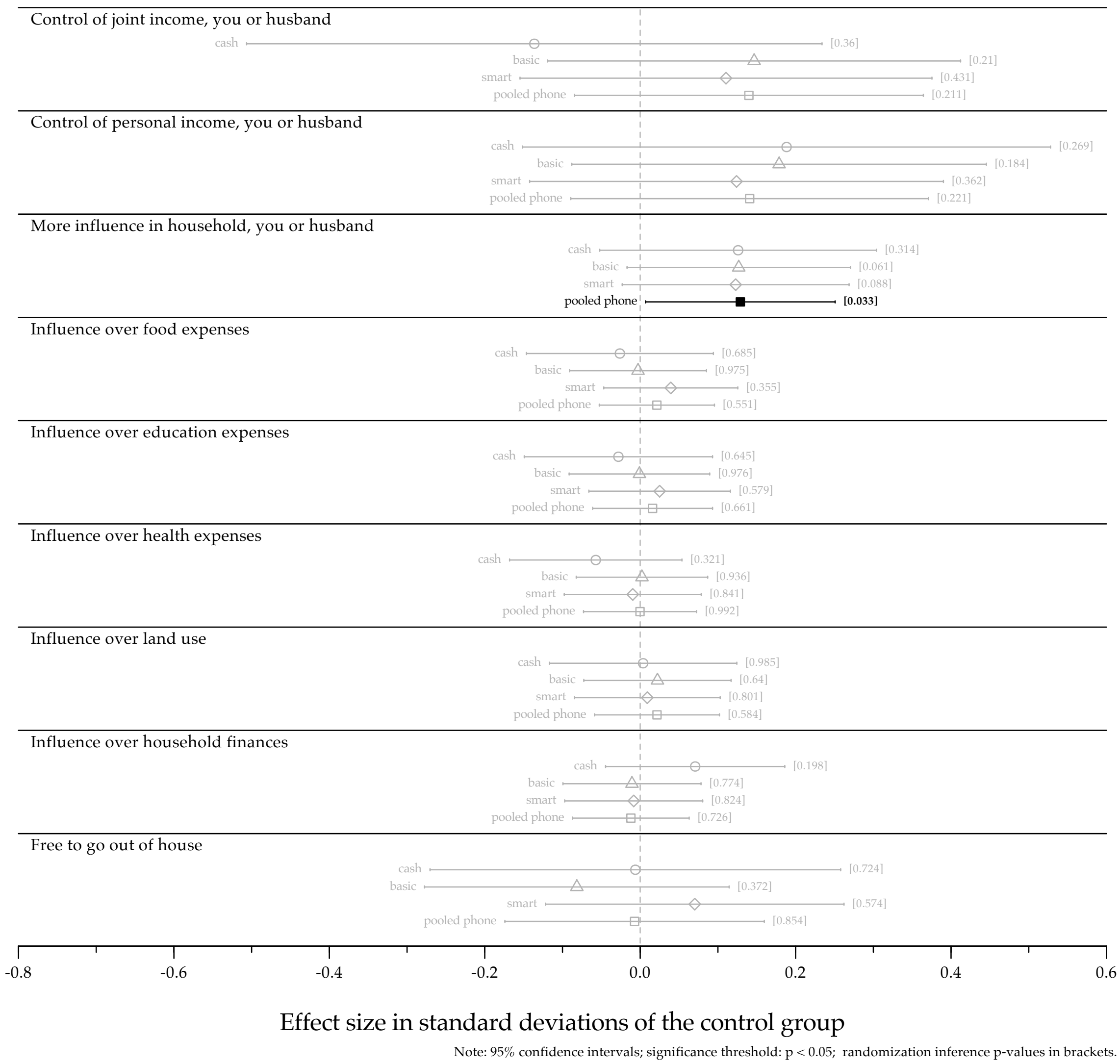

Figure A22.6: **Effects on components of household influence.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

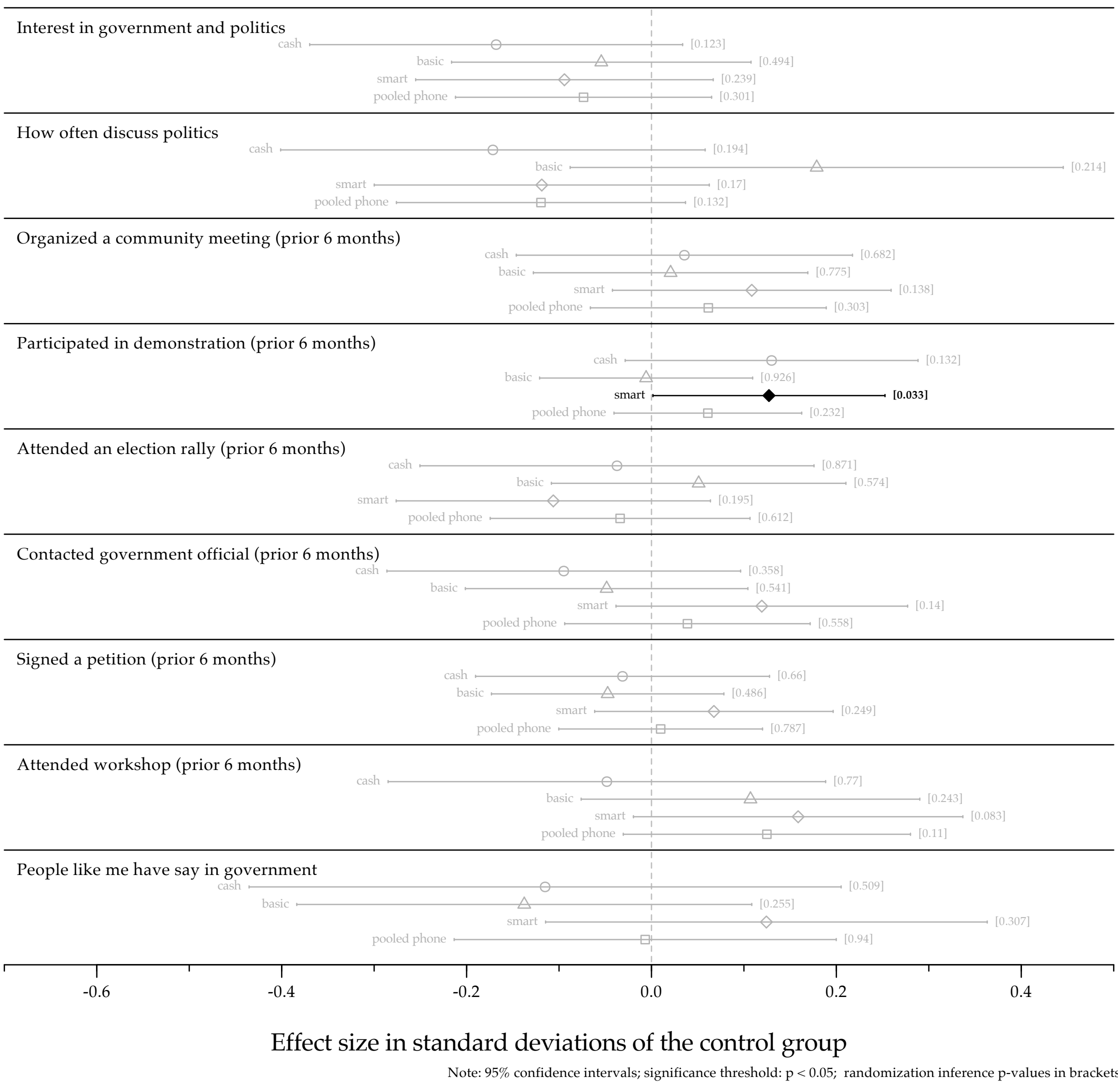

Figure A22.7: **Effects on components of political participation.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

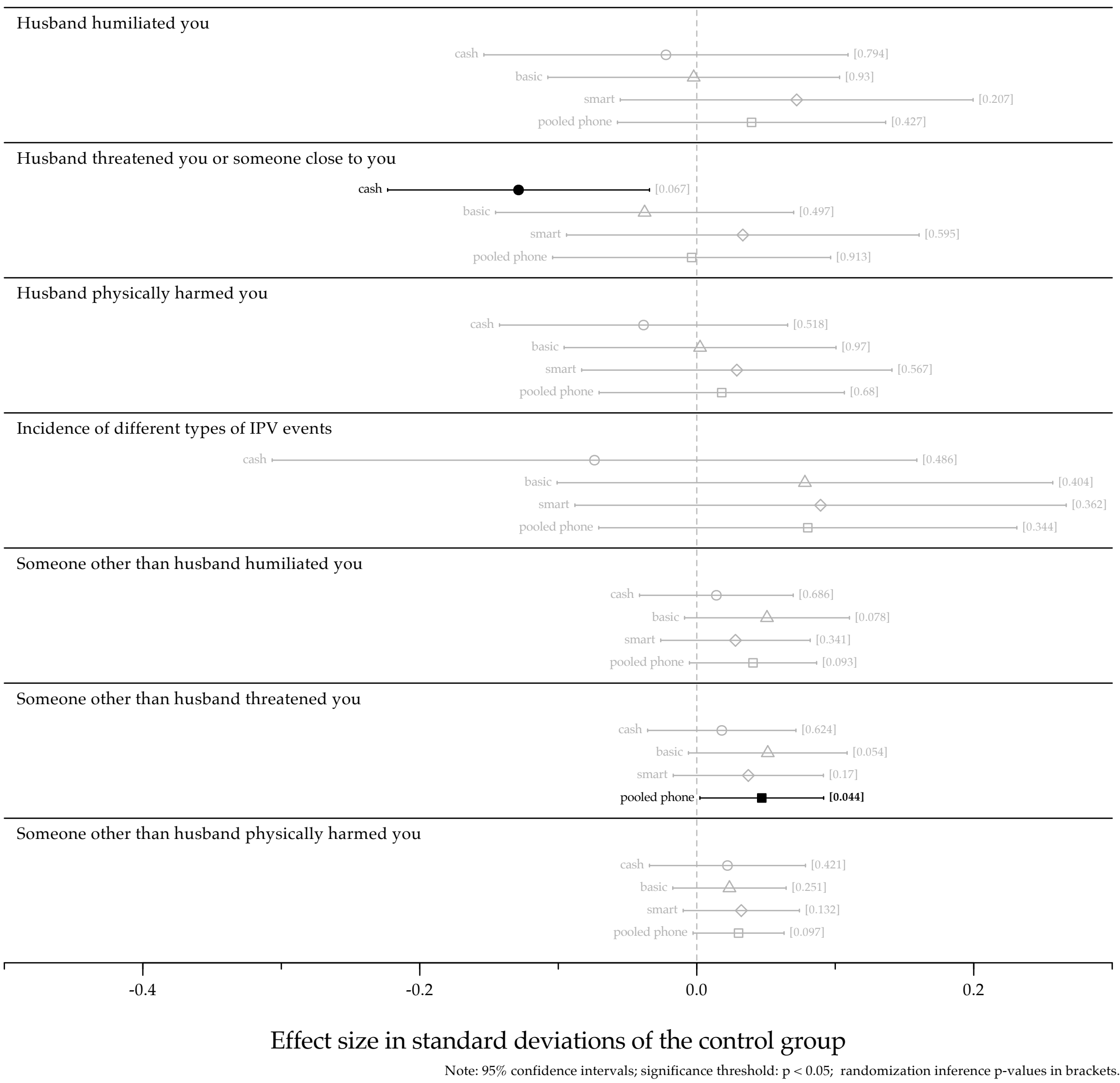

Figure A22.8: **Effects on components of intimate partner violence.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

A23 Effect of Group versus Individual Training on Phone Recipients

As noted, the main treatments were cross-cut with a weighted treatment of training. (See Table 1 in main paper). The training module delivered by our field team instructed participants on how to install a SIM card, charge the phone, turn on the phone, use the radio and flashlight, make a phone call, send SMS, use mobile money, and for smartphone recipients, how to access the internet and download an app. Training was randomly assigned to be delivered either one-on-one or among a group with a median size of 15. Figure A23.1 pools the phone conditions and reports the effects of individual versus group training. Generally we see no effects of training nor any differences between individual and group training.

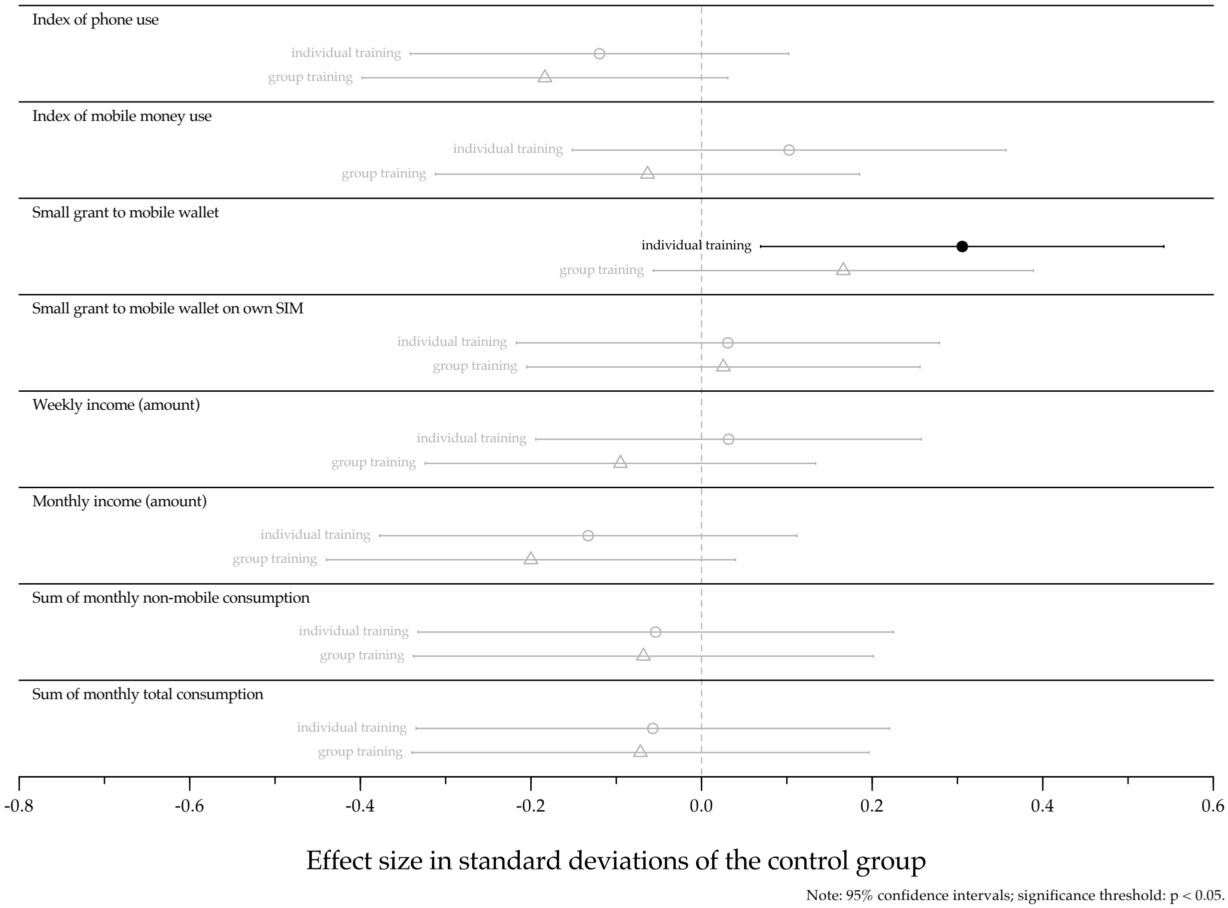

Figure A23.1: **Individual vs group training on main outcomes.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include covariates for our blocking strata (BRAC or TASAF membership, income, urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

A24 Interaction Effects of Main Conditions and Training

Here we report results re-running the analysis of the main outcomes including training and interaction terms with the main conditions. The interaction term is always insignificant. However, training may partially account for the increases in household consumption observed in the smartphone condition. We cannot rule this out. Given the limited variation in training and phone ownership, however, we should be cautious in drawing conclusions that are too strong either way. Estimating the precise impact of training independent from phone ownership—and how to improve mobile technology training for low-literacy populations—is an important step for future research.

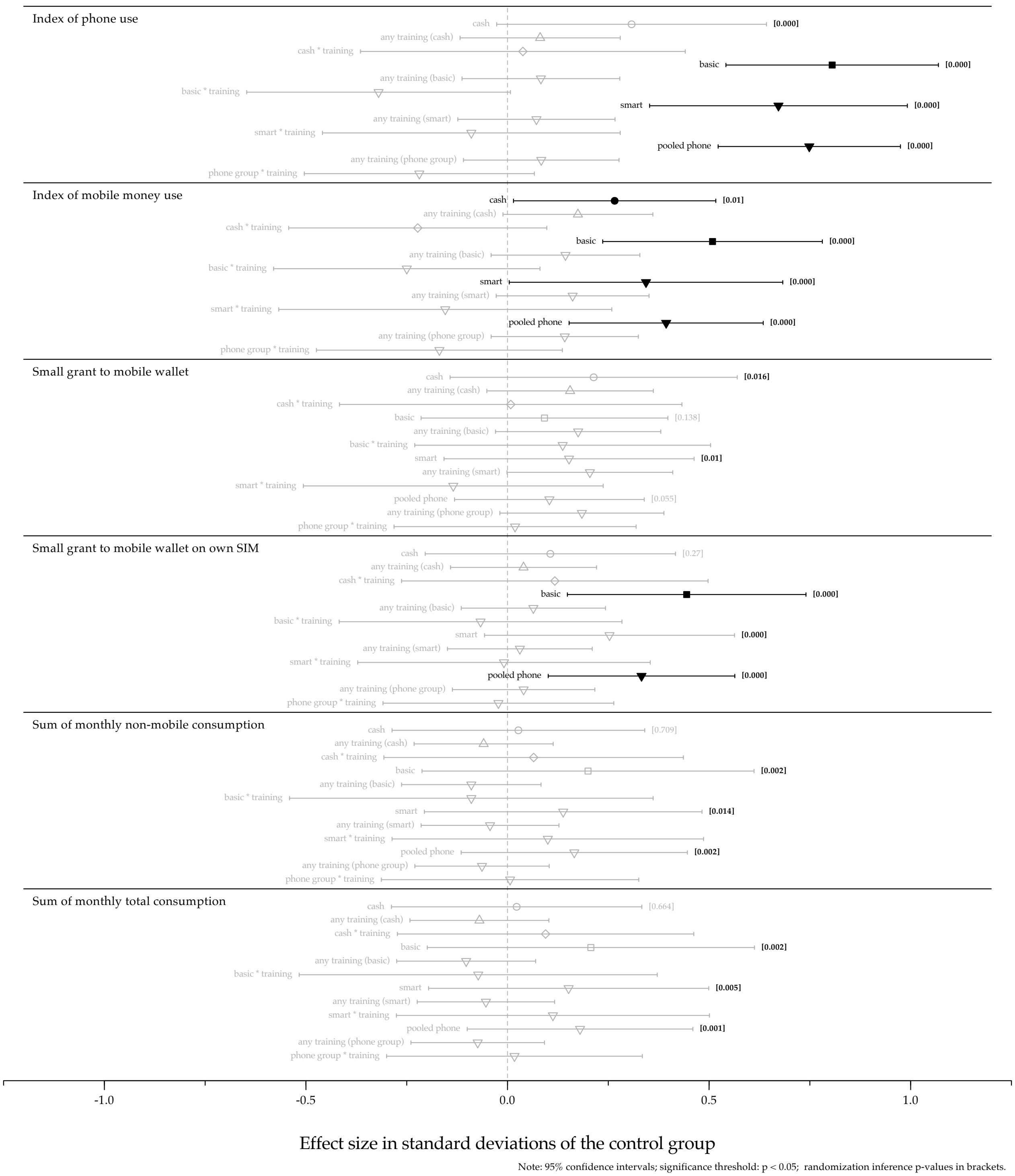

Figure A24.1: **Interaction effects of main conditions and training.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include usual covariates. The components of the interaction are reported as "cash" and "any training (cash)". The interaction as "cash*training."

A25 Results from Long-Model with Interaction Terms of Cross-cutting Conditions

In the main analysis we follow our pre-registered specification and report the "short" model (treatment conditions without interactions). Following from Muralidharan et al. (2019) we also rerun the analysis of the main outcomes employing a fully-saturated "long" model with the main treatment effects and all interactions. Including the interaction terms generally does not change the effect sizes on the phone conditions, suggesting it is the receipt of the handsets rather than the solar chargers or credit vouchers that are driving the results.

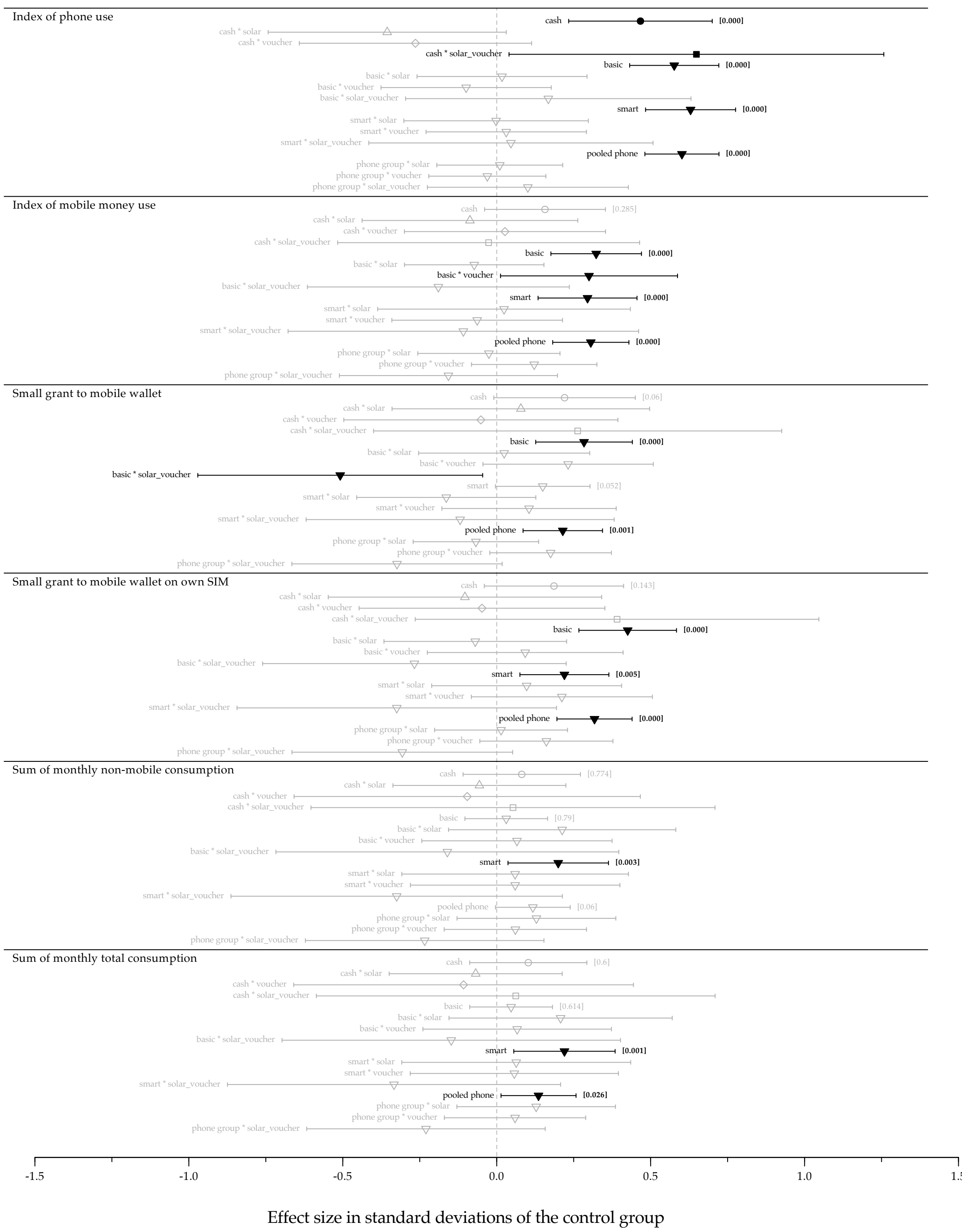

Figure A25.1: **Long-models with interaction terms of main treatment conditions.** Each estimate is derived from a separate model with treatment compared to control (the reference category). Point estimates and confidence intervals estimated using OLS regressions with robust standard errors at individual-level. We also report p-values in brackets using randomization inference that assesses the probability that any treatment effects observed could be drawn from 10,000 alternative random assignments. Model specifications include usual covariates.

A26 Results from Experiment 2 among Phone Owners

As described, we fielded two experiments: one among non-phone owners and a second among phone owners. In the second experiment of 648 women (see Table A26.1), in addition to the same set of cross-cutting treatments as in experiment 1, participants were randomly assigned to receive a smartphone (Huawei Y3C), an unconditional cash grant of 130,000 (PPP US$179), or wait-listed to receive a smartphone in year 2.

Main Conditions	**n**	**Proportion Assigned to Cross-Cutting Conditions**		
		Training	Solar Charger	Voucher
Control (SIM + waitlisted for smartphone in year 2)	257	0.37	0.24	0.22
Cash (SIM + 130,000 TZS (PPP $179))	117	0.62	0.45	0.48
Smartphone (SIM + Huawei Y3C)	274	0.79	0.37	0.37

Table A26.1: **Distribution of participants across treatment conditions in experiment 2 among phone owners.** This table illustrates the distribution of participants across the main and cross-cutting experimental conditions. Participants were block randomized into the following combination of conditions: a.) control, smartphone or cash grant; b.) training (either individual or in a group) or no training; c.) solar charger or no solar charger; or d.) vouchers or no vouchers. Column 2 indicates the total number of participants assigned to the main conditions. Columns 3-5 indicate the proportion of those in each of the main conditions who were assigned to the cross-cutting conditions. As the design was full-factorial, it was possible for participants to receive multiple cross-cutting treatments. Training was concentrated in the non-control conditions (62%-79%) to enable the pre-registered comparison of group versus individual training on phone recipients. (See A21). Given the overlap between phone/cash and training, we bundle these treatment arms. Estimations thus reflect the effect of phone/cash + some form of training compared to control (no phone/cash and few receiving training).

As reported in Table A2.1, overall participants in Experiment 2 were qualitatively different from those in Experiment 1; they tended to have higher levels of income and education, and to be a mix of small business owners and smallholder farmers (rather than predominantly farmers). Participants in Experiment 2 also tended to be much more intensive users of mobile money at baseline and to come from households with full mobile phone saturation among adults (1.17 handsets per adult in the household

versus 0.44 in Experiment 1).

We observe some mobile phone loss in experiment 2 but the rates are much lower. At endline, 82.5% in the control group, 90.7% in the cash group, and 92.5% in the smartphone group report owning a phone.

The main results of experiment 2 are reported in Figure A26.1. Other than Index of Phone Use, we see no treatment effects.

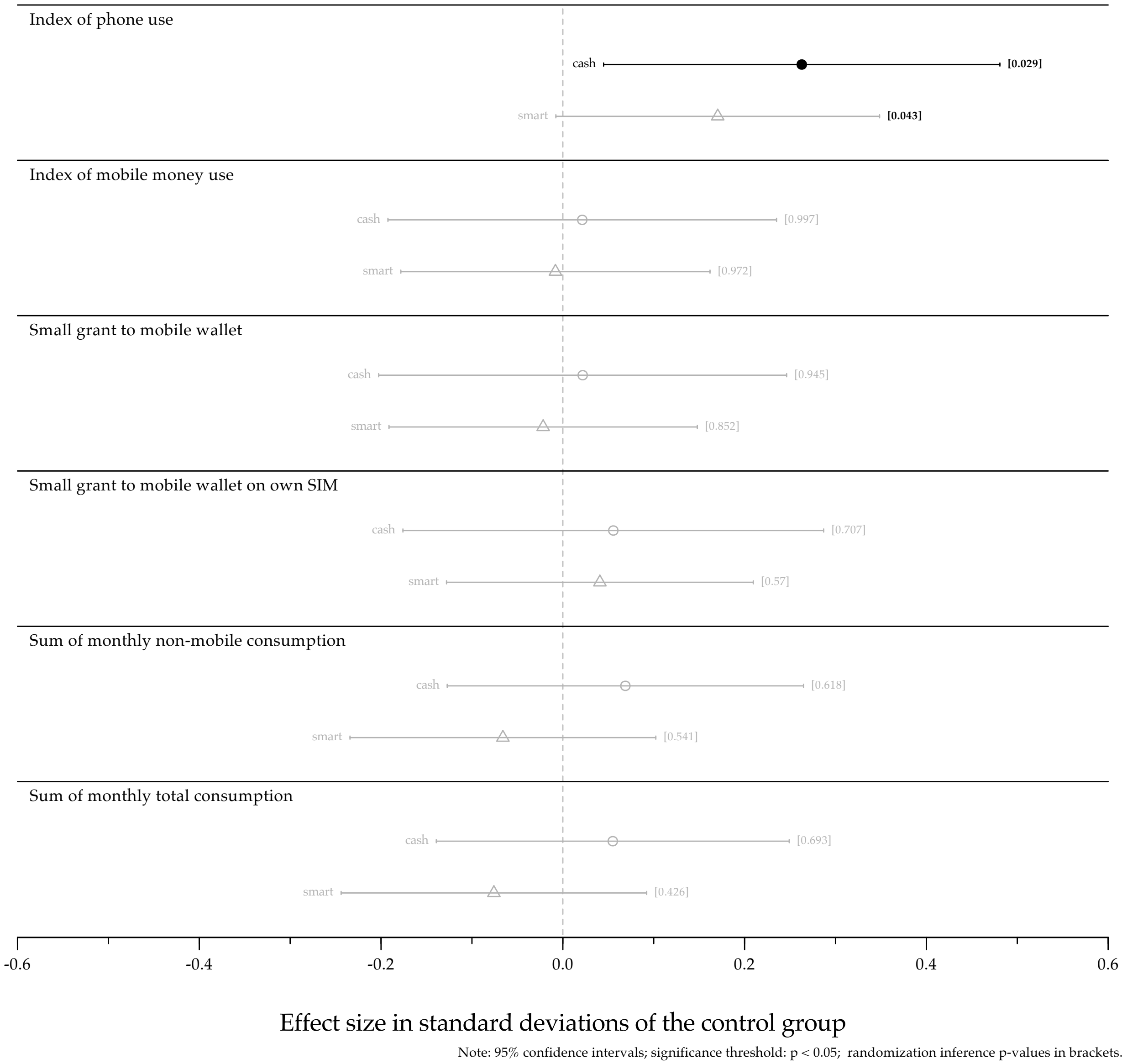

Figure A26.1: **Main results in Experiment 2 among phone owners.** Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, and household size.

We do, however, see much greater reports of internet access among those in the smartphone and cash conditions, compared to control. In the smartphone condition

40% of people report at least some internet use, compared with 11% in the control. (See Figure A26.2.) This increased use in the smartphone condition serves as a useful manipulation check. Still, overall internet use remains low with the remaining 60% of the smartphone group reporting never using it.

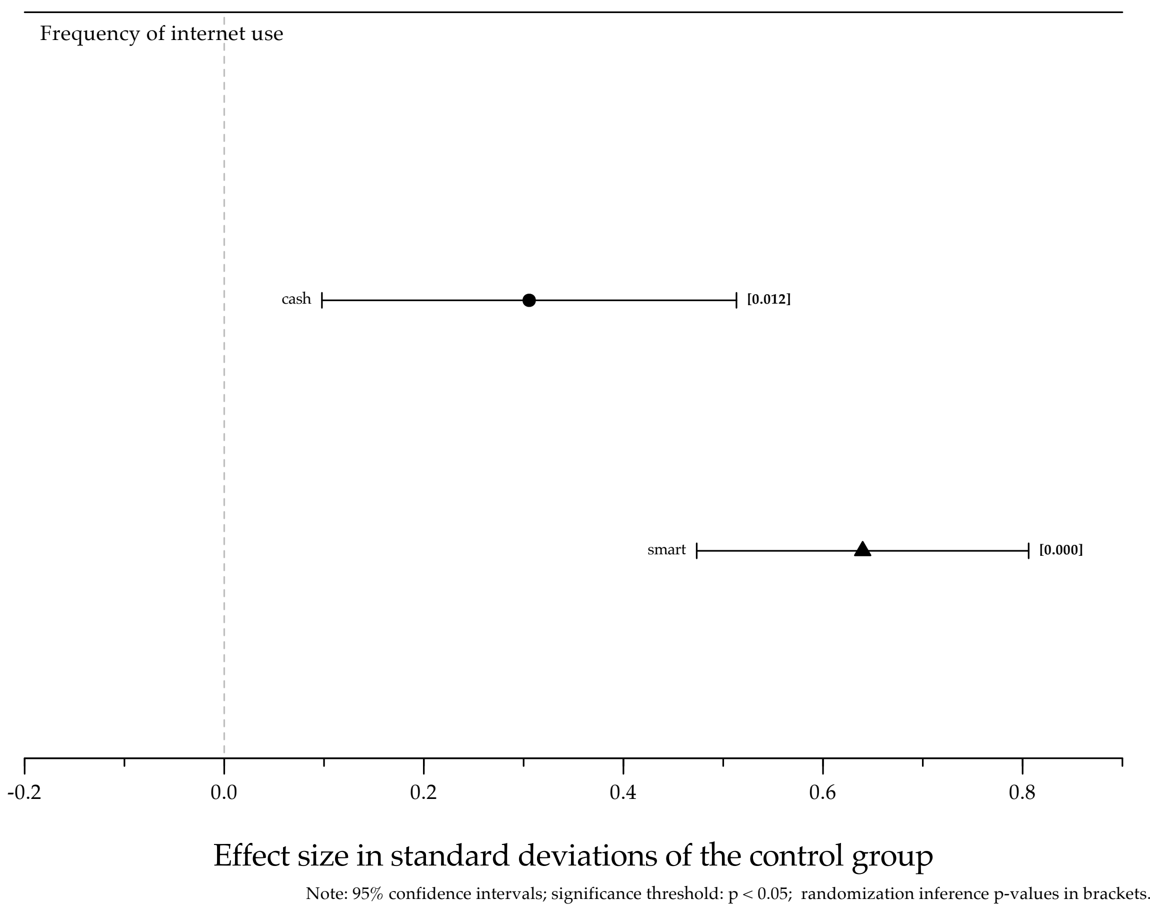

Figure A26.2: **Effects on frequency of internet use in Experiment 2.** Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, and household size.