The economic impact of mobile phones on low-income households

Philip Roessler,[1*] Peter P. Carroll,[2] Flora Myamba,[3]
Cornel Jahari,[4] Blandina Kilama,[4] Daniel L. Nielson[5]

[1]William & Mary, [2]University of Michigan, [3]Women and Social Protection in Tanzania,
[4]REPOA, [5]Brigham Young University

[*]To whom correspondence should be addressed; E-mail: proessler@wm.edu

One Sentence Summary: Mobile technology proves a cost-effective measure to boost the economic well-being of low-income households, but its impact was attenuated by handset turnover.

Abstract: We report the results of the first large-scale experimental study (n=1,352) of mobile phone ownership. We assigned no-cost basic handsets, smartphones, and a cash grant to women non-phone owners in Tanzania and compared them to control. Over thirteen months, the phone interventions caused a significant uptake in digital financial services, and powerfully boosted household consumption, especially in the smartphone condition. Smartphones improved annual household consumption per capita by 20%—an increase of PPP US$118.89 (95% CI: $26.61-$211.16). A benefit-cost ratio of more than 2:1 makes mobile technology among the most cost-effective anti-poverty interventions available. However, handset turnover, which occurred at a rate of 26%, attenuated economic gains. This highlights that mobile technology's impact is not independent of the poverty trap that constrains many low-income households.

1 Introduction

One of the most important technological advances over the last quarter-century has been the global diffusion of mobile phones (*1*). Mobile devices have transformed not just communication and access to information (*2*), but also, with the advent of mobile money and digital banking, access to financial services (*3*). While the economic benefits from the mobile phone revolution are far-reaching, the greatest potential impact holds for the poorest—those who traditionally face steep barriers to long-distance communication, acquiring market information, and utilizing financial institutions.

A burgeoning research program analyzes the impact of mobile technology on the livelihoods of the poor (*4–10*), with a particular focus on the uptake and use of mobile money (*3, 11–16*). Access to mobile money in low-income countries is found to increase household consumption; induce more efficient allocation of labour, savings, and risk; and improve health, education, and agricultural productivity (*12, 14–17*). Welfare gains from mobile money are found to be especially strong for female-headed households (*14*) and for women microfinance recipients who control their own mobile money accounts (*18*). Yet, in many emerging economies, a significant digital gender gap persists—in which women not only have less access to mobile internet and mobile money, but continue to lag in mobile phone ownership (*19–22*). This digital inequality risks compounding existing structural disparities (*23*), and, by one estimate, will represent a global welfare loss of $700 billion in GDP growth over the next five years (*21*).

Here we report on the first large-scale experimental study (n=1,352) of mobile phone ownership in an emerging economy, focusing on women non-phone owners. Beyond enabling the first causal estimates of the impact of mobile phone ownership on poverty reduction, it allows analysis of the channels through which reducing the mobile gender gap affects economic well-being. It also provides new insights into the microfoundations of mobile phone ownership—a critical but understudied arena of research.

Fielded between July 2016 and December 2017 in Tanzania, our experiment assigned no-cost basic handsets, smartphones, and a cash grant to women non-phone owners in Tanzania and compared them to a control group. Our primary pre-registered outcomes were the uptake and use of digital financial services and the downstream effects on economic well-being, as measured by household consumption.

Employing self-reported and behavioral measures of mobile money use, we find that both basic handsets and smartphones significantly increased adoption of digital financial services. Phone ownership, especially smartphones, also significantly increased household consumption. 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). With a benefit-cost ratio of more than 2:1, this makes mobile technology among the most cost-effective anti-poverty interventions available, rivaling the efficacy of cash transfers and multifaceted poverty graduation programs (*24, 25*)—at least in the short term. Mobile technology's powerful anti-poverty effects, however, emerged only in the face of substantial turnover in mobile phone ownership. By endline, 26% of those assigned to the phone groups owned no phone at all—in part due to selling the device or having it appropriated. This highlights that mobile technology's impact is not independent of the poverty trap and social structures that constrain the economic well-being of many low-income households. It also suggests mitigating handset turnover requires much greater policy attention.

How does mobile phone ownership boost household consumption? An exploratory analysis of causal mechanisms finds it operated through the participant's increased use of mobile money *and* improved efficacy as market traders from enhanced communication capabilities. But shared mobile technology use by family members also seems to have mattered. Descriptively, we observe that the strongest consumption gains accrued to those households in which female participants retained control of the smartphone but it was also used by their spouses. Conversely, when spouses, and especially sons, confiscated the handsets, the household was worse off. This dynamic suggests the potential importance of cooperative intra-household mobile technology use for poverty reduction, but requires future research.

2 Experimental Design and Results

Working across five regions in Tanzania between July 2016 and December 2017, we fielded a pre-registered experiment with 1,352 women non-phone owners. For a full description of participant recruitment and experimental protocols, see Materials and Methods. (We also fielded a smaller, parallel experiment among women phone owners to study the effects of the migration from basic phones to smartphones, see S25.) After a screening survey for mobile ownership and a baseline survey, participants were randomly assigned to one of four main conditions (Table 1): basic handsets, smartphones, a cash grant, and control. At program distribution all participants, including those in the control, were offered a SIM card from one of the three major mobile network operators (MNO) in Tanzania with strong coverage in their area. SIM cards were registered to participants on site by agents from the MNOs. Accordingly, any effects we see in our study operate through mobile phone ownership rather than SIM registration.

The experiment also included three cross-cutting conditions: mobile phone training delivered either one-on-one or in a group of roughly 15; solar chargers (Sun King Pro 2); or a \$15 monthly-credit voucher for 12 months.[1] As reported in Table 1, the assignment of training was heavily concentrated in the main treatment conditions to enable group- versus individual-training comparison (S22), and the experimental findings reflect the composite effect of phone/cash + training compared to control (for interactions, see S23). 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 (S24.1).

In our analyses we compare the phone conditions to control. While we intended the cash grant—roughly equal to the cost of a basic phone—to serve as a placebo condition, this was confounded as 55% of cash recipients reported acquiring mobile phones. Moreover, the cash

[1]Note all US\$ values are reported at purchasing power parity, using the World Bank PPP US\$ 2017 exchange rate of 725 TZS for \$1 US.

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.

group, which due to program constraints had the fewest participants, had by chance an imbalance in higher levels of consumption at baseline and higher levels of attrition at endline. For these reasons, we are cautious about the inferences we can draw from the phone-versus-cash comparison using our data, and we recommend this as an avenue for future research.

To assess the impact of the phone program on participants' uptake and usage of mobile money and their economic well-being, we conducted baseline, midline (t + 6 months) and endline surveys (t + 13 months). Table S1.1 reports descriptive statistics of key covariates from the baseline survey and Table S1.2 reports covariate balance across conditions. Figure S2.1 displays survey response rates at midline and endline. At endline, we re-surveyed 94% of participants, with no meaningful differences between control and phone groups. However, differential attrition occurred for the control group at midline. We thus focus our analysis on endline outcome measures. Following our pre-analysis plan, to estimate treatment effects we employ randomization inference (RI) (*26*).

2.1 Uptake and use of mobile money

Our primary pre-registered outcomes were the uptake and use of digital financial services and the effects on economic well-being as measured by household consumption. We first report the intention-to-treat (ITT) effect of assignment to experimental conditions on overall phone use (as a type of manipulation check) and, our primary pre-registered outcome, the 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. 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 micro-grant, varying the amount if received as cash (PPP US$5.50) or as a mobile money transfer (PPP US$11.00). 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 an on-the-spot behavioral test of the use of DFS.

Figure 1 reports the results. (For full regression tables, see S9. Results on the components of the mobile phone use and mobile money indexes are reported in S10 and S11, respectively.) Phone ownership substantially increased phone and mobile money use—on both self-reported and behavioral measures. In the behavioral test of revealed preferences for mobile money, those assigned to the basic phone and smartphone conditions chose mobile money and had it sent to their own digital wallets at a respective rate of 40.4% and 33.7% compared to a control mean of 22.8%. Beyond enabling use of digital financial services, we see more mixed effects on participants' access to market information and income-generation (S12 and S13).

2.2 Effects on household economic well-being

To measure the impact of increasing women's mobile phone ownership on the household's economic well-being (*27*), we employed a consumption module that provides an extensive picture of expenses. In Figure 1, we first 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 then report total household consumption, including mobile expenditures, to ensure we capture as complete a picture of a household's living standards as our data allow.

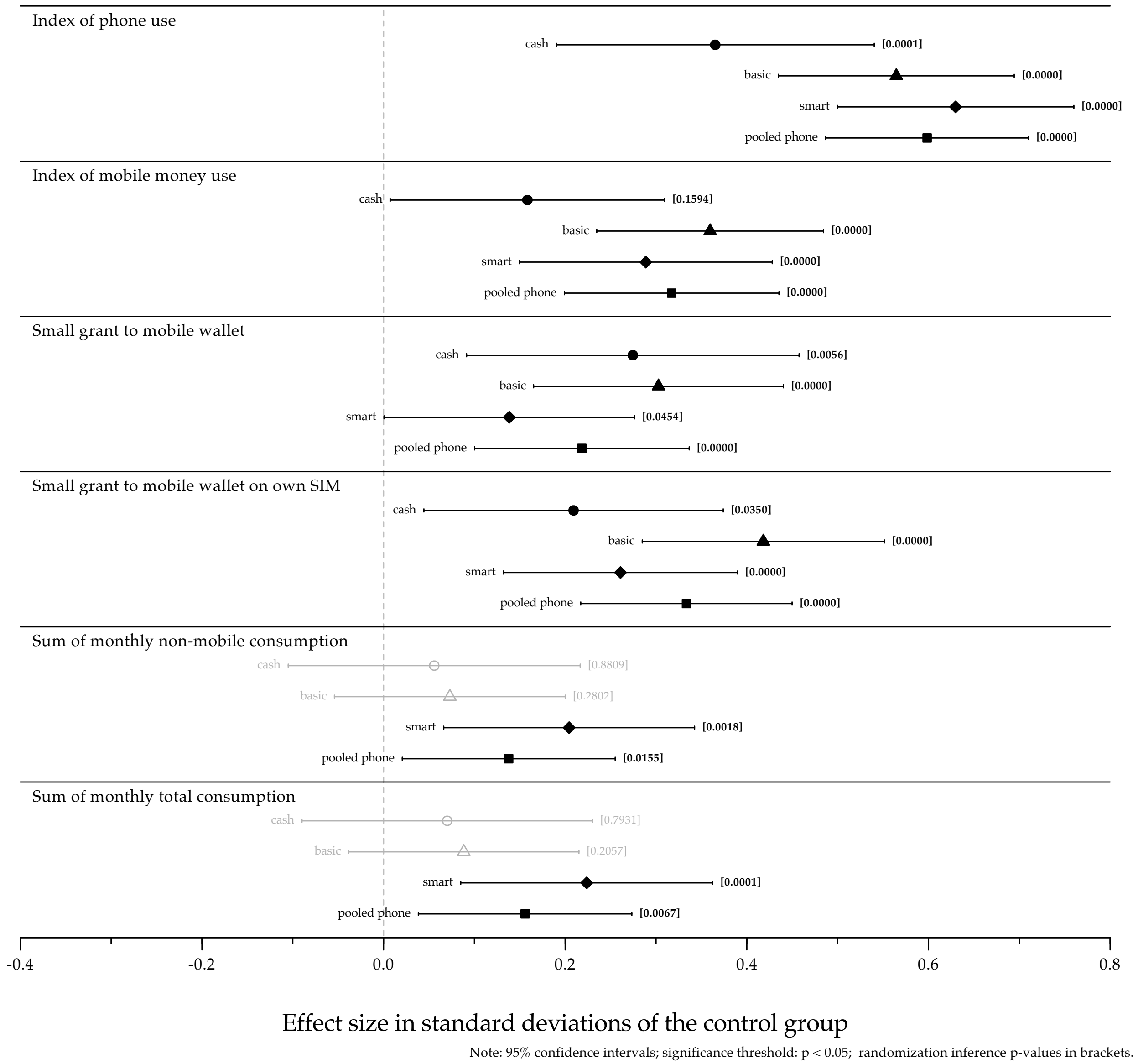

Fig. 1: Impact of treatment conditions on measured outcomes. Models estimated using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

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 1, 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). Reported *p*-values for the pooled and smartphone conditions are robust to multiple-comparisons adjustment using the Benjamini-Hochberg procedure (*28*), and the smartphone *p*-values also clear the recommended redefined threshold for statistical significance of 0.005 (*29*).

Figure S16.1 reports treatment effects on individual consumption baskets. The results illustrate that, beyond spending on the phone itself, smartphones significantly increased expenditures on several economic and social activities, including transportation, schooling, community events and ceremonies, and entertainment, and led to near significant increases in spending on health and cooking fuel. One concern is that the stronger smartphone consumption effects were driven by selling the premium handset, which on average was worth 2.5 weeks of baseline consumption. Yet, as illustrated in Figure 2, this is not the case. Consumption gains accrued most to those participants whom we verified had the project smartphone on their person at the endline survey.

This is reinforced by the strength of the complier average causal effects—a 0.66 SD and 0.71 SD increase, respectively, in non-mobile and total consumption over control—estimated by instrumenting smartphone ownership at endline with assignment to the smartphone condition (Figure S15.1). These results indicate that the welfare gains from increasing women's mobile phone ownership operate through their retention and use of the technology. Through causal mediation analysis we aim to better understand the channels accounting for the rise in household consumption (S19.1). This provides suggestive evidence that participants' increased use of mobile money *and* improved efficacy as market traders from enhanced communication capabilities independently correlated with consumption gains. Yet the analysis estimates that these two mediators account for only 34% of the consumption increase for smartphone owners, pointing to a substantial effect through other channels. One possibility is positive spillover effects within the household discussed below.

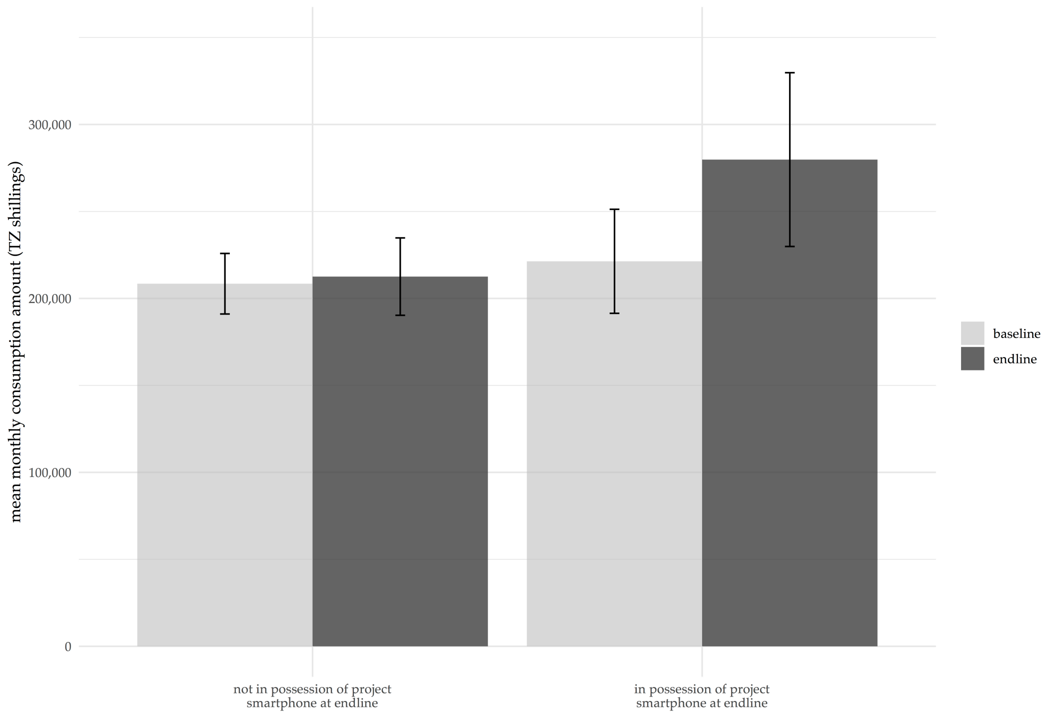

Fig. 2: Household consumption among smartphone compliers and non-compliers. Bars indicate mean total monthly consumption at baseline and endline across smartphone compliers (those verified to have the project smartphone on their person at endline) and smartphone non-compliers (those who could *not* show the project smartphone at endline). 95% confidence intervals.

In line with our pre-analysis plan, we also report subgroup effects by one of our blocking criteria: participants' organizational membership—either microfinance (45% of sample) or poverty reduction program (55% of sample). Among the latter sample, which included some of the poorest households in Tanzania (Table S1.1), both the basic handset and smartphone significantly increased household consumption. In contrast, among the microfinance sub-sample, which more closely approximated the average household in Tanzania, only the smartphone significantly boosted living standards (Figure S17.1). As reported in Table S17.1, consumption effects across the microfinance and poverty reduction subgroups track the degree to which the treatments increased the number of handsets in the household.

From a benefit-cost perspective, mobile phones are a highly efficient means to increase living standards of low-income households. Whereas cash transfers are estimated to increase consumption roughly 1.1:1 for the cost of the cash grant (excluding program costs) (*25*), the smartphone intervention increases consumption 2.1:1 (PPP US$653.88 total household consumption compared to smartphone and programmatic costs of PPP US$305) (Table S18.1). However, among the poorest households (the poverty reduction program subgroup), basic phones prove more cost-effective than smartphones: with a benefit-cost ratio of 2.6 compared to 1.2. This puts mobile phones on par with multifaceted poverty graduation programs in terms of cost-effectiveness (*24*).

3 Handset Turnover

Mobile technology's anti-poverty effects, however, emerged only in the face of substantial turnover in phone ownership. As illustrated in Figure 3, among those provided with phones at distribution, 27% of women in the basic phone group and 24% in the smartphone condition reported not owning *any* phone at endline. Handset turnover also occurred among participants in the six months prior to the study (as reported retrospectively in the baseline survey). Although overall phone ownership at endline was slightly higher in the smartphone than basic phone condition, fewer in the smartphone group retained the handset given to them at distribution. Many in the smartphone group (43%) reported trading down their premium handset for a basic one—with a quarter of the non-compliers reporting they gave the smartphone to someone else in their household (S3).

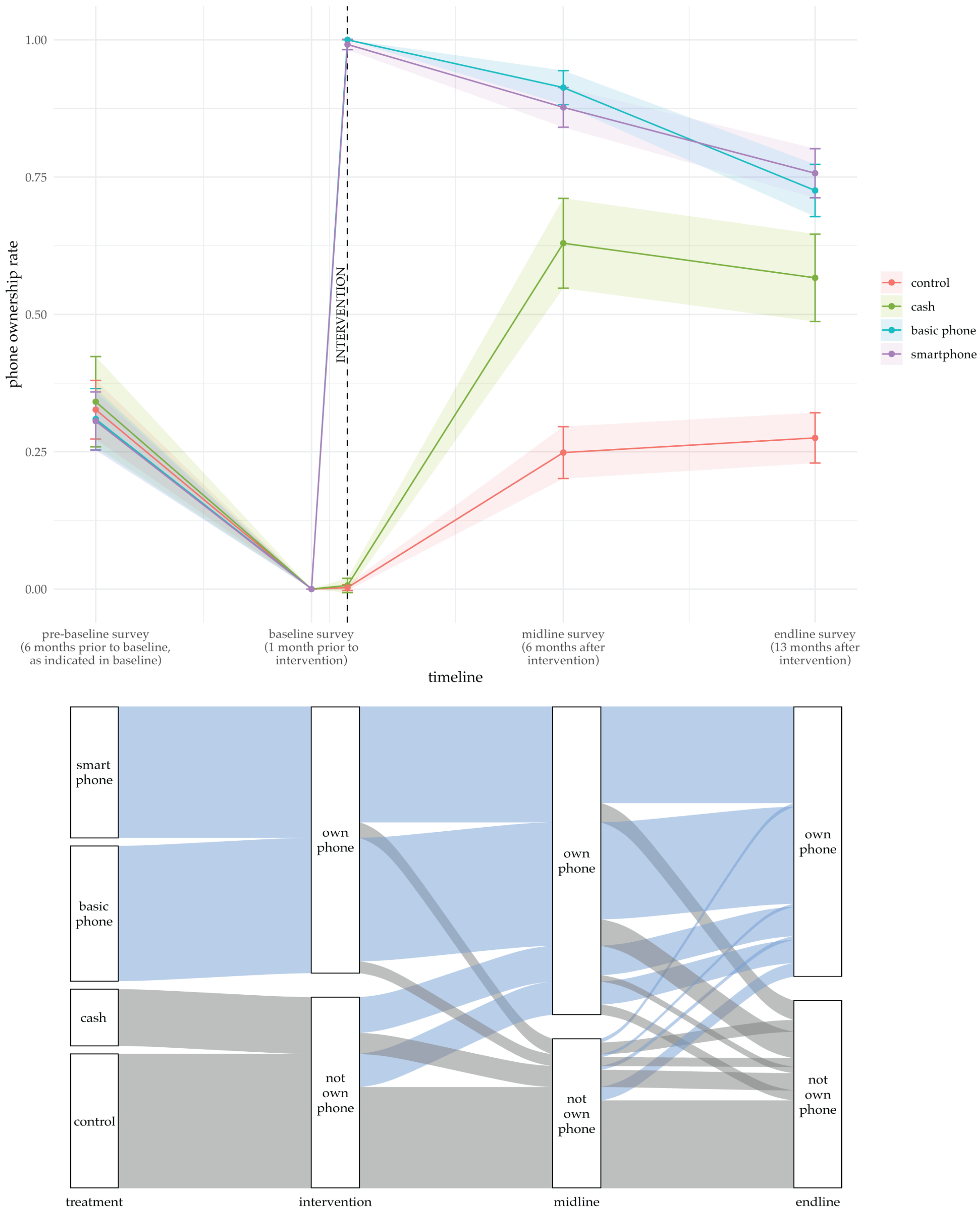

Fig. 3: *Upper panel*: Phone ownership rates over time. The figure depicts the proportion of respondents by experimental condition who reported owning any mobile phone across the study timeline. (Shading represents 95% confidence intervals.) As non-phone ownership was a criterion for the study, at baseline none owned a phone. *Lower panel*: Sankey diagram illustrating phone ownership across treatment conditions over time. "Own phone" indicates ownership of any phone, not necessarily the project phone.

This high rate of handset turnover is an important finding in and of itself, as there are few panel studies on mobile phone ownership. As illustrated in the Sankey diagram in the bottom panel of Figure 3, a high rate of handset churn is observed across *all* conditions—with participants acquiring and subsequently not retaining their phones due to selling, theft, confiscation, breakage, or loss (Figure S3.4). This suggests handset turnover was not a by-product of the distribution of cost-free phones (S4). Although irregular handset ownership attenuated the efficacy of the mobile phone distribution program, the interventions still caused a substantial increase in the count of phones in the household and especially in boosting the rate of women's mobile phone ownership (Figure S5.1-Figure S5.4.)

Why would individuals not keep their phones if they proved so valuable over the long-run? Pinning down the precise mode of handset non-retention is difficult, but some reported selling their phones. This points to the potential effects of the poverty trap on mobile technology's impact—short-term financial hardship and exigencies possibly led the poor to sell a device that over time would bring increasing and significant welfare gains.

The social structures in which mobile technology is embedded also appear to be important. We observe that the strongest consumption gains accrued to those households in which participants retained control of the smartphone but also shared their use with their spouses (Figure S20.1 and S20.2). Conversely, when spouses, and especially sons, appropriated the handsets, the household was worse off. These households start at similar consumption levels at baseline but then diverge based on cooperative or noncooperative smartphone use. The results from this ex-post analysis should be taken with caution as the patterns are merely descriptive, require the comparison of relatively small sub-groups, and are subject to potential reporting bias. Nonetheless, these exploratory findings suggest the potential importance of cooperative intra-household mobile technology use for poverty reduction—a critical line for future inquiry. The benefits of cooperative use may manifest in terms of sharing access to the smartphone and its advanced capabilities, but also knowledge transmission, increased digital literacy, and greater economic productivity among household income-earners. This implies that women's control over mobile technology mediates its welfare-enhancing effects at the household-level.

We likewise observe significant heterogeneous mobile phone effects on consumption conditional on baseline levels of participants' income (another one of our blocking criteria) (Figure S20.3). Consumption gains from the receipt of mobile technology were concentrated in households in which participants had above-median income levels at baseline. And, consistent with the possible mobile control mechanism, participants with higher incomes were less likely to report having their handset confiscated (Table S20.2) and more likely to exhibit greater influence within the household at endline. On the whole, however, women's mobile phone ownership had more mixed effects on participants' levels of empowerment—at least after thirteen months (S21). On some dimensions, such as participation in the formal economy; access to the internet (in the smartphone group); allocation of time (with less time spent on farming); and social connectedness, phone ownership exhibited significant positive effects. Yet on others, such as subjective welfare, political engagement, or individual efficacy, we find null effects.

4 Discussion

Overall, our results affirm the benefits of closing the gender gap in mobile phone ownership. Doubling mobile phone capacity for a household with two adult income-earners 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.

These findings belie the generally accepted notion that an essential threshold will be reached when all possess a mobile phone. Handset ownership for the poor is tenuous: financial hardship brings pressure to sell their phones, while lost, broken, or stolen phones cannot be easily replaced. To regain phone ownership and generate mobile-based income, such individuals must necessarily wait for a windfall or for savings to accumulate—and our data suggest that such a moment may take many months or even years. While mobile phones might indeed provide part of the answer in the struggle against global poverty, deeper consideration must be given to the challenges faced by low-income households when they confront the loss of valued assets.

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The economic impact of mobile phones on low-income households

Supplementary Materials

Materials 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). 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.

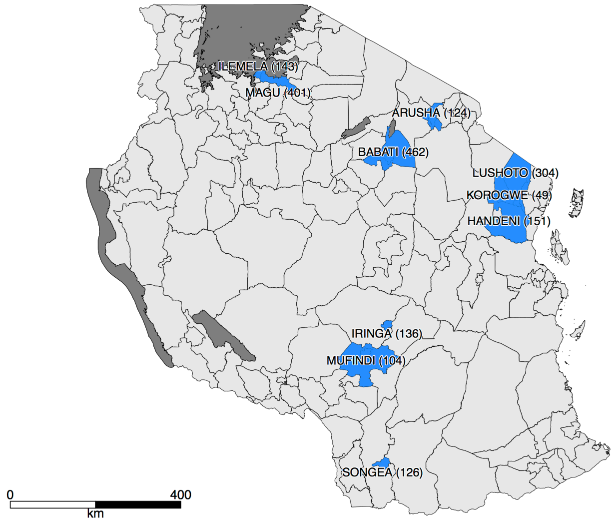

Fig. S1.1: illustrates experimental sites across Tanzania with total participants in both experiments for each district reported in parentheses.

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 S1.2 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 S25.) 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. In experiment 1, those who chose to participate in the program were eligible for a basic phone (Samsung B110, procured for PPP US$64 including VAT), a smartphone (Huawei Y3C, procured for PPP US$186 including VAT), an unconditional cash grant (PPP US$55), a solar charger (Sun King Pro 2), or monthly mobile credit vouchers. In experiment 2, participants were eligible for the Huawei Y3C smartphone, an unconditional cash grant (PPP US$179), 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 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 S3 (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 (Figure S2.1). 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.

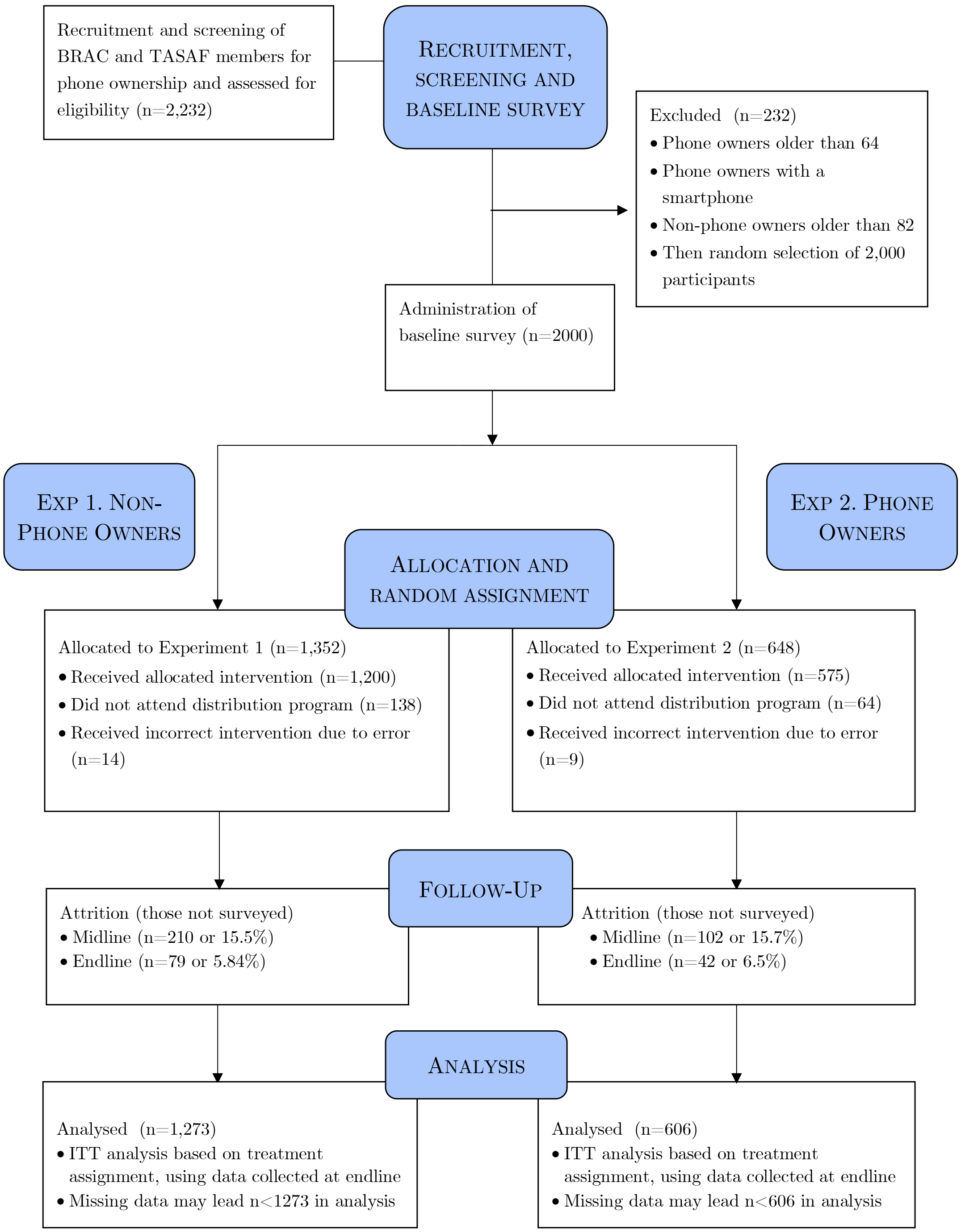

Fig. S1.2: A CONSORT diagram of participant recruitment, screening, assignment to treatment, and evaluation as part of the Mobile Phone and Livelihoods of Women Program and experiment.

Measures

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 S7.1 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 S7, missing observations are generally not correlated with treatment. Nonetheless, we report the results from all analyses imputing missing observations from baseline outcome measures. The results are highly consistent when imputing missing observations as those reported in S8.)

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). 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 an on-the-spot 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 (2017). (*3*) 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 S14) 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 S8.) 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 S7.

Analysis methods

Following our pre-analysis plan, we use treatment assignment to estimate intention-to-treat (ITT) effects, primarily employing randomization inference (RI). (*26*) 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 clustered by participant, 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 (*30*). 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 S24.)

Human subjects approval

Research approval for this study was obtained from the Tanzania Commission for Science and Technology (COSTECH) and the Institutional Review Boards at the co-authors' respective institutions prior to fielding the experiment. Informed consent was obtained from all participants ahead of each survey round and for their participation in the Mobile Phone and Livelihoods of Women Program.

Pre-registration

We pre-registered the experimental design at EGAP prior to researcher access to any outcome data. A de-identified version for review is available here:

https://www.dropbox.com/s/i3e0qzcqkybm79f/OSF_pre-registration.pdf?dl=0

Competing interests

The authors declare no competing interests for this study.

S1 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 S1.1 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.	
	BRAC		TASF				TZ 2016	
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 S1.1: Baseline summary statistics in experiment 1 and experiment 2 compared to the female population in Tanzania (2016)

Table S1.2 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 S1.2: 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 S1.3: Joint statistical significance of each treatment in experiment 1 with 12 baseline covariates reported in Table S1.2.

S2 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 Figure S2.1, 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 S2.1: 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 S2.1 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.

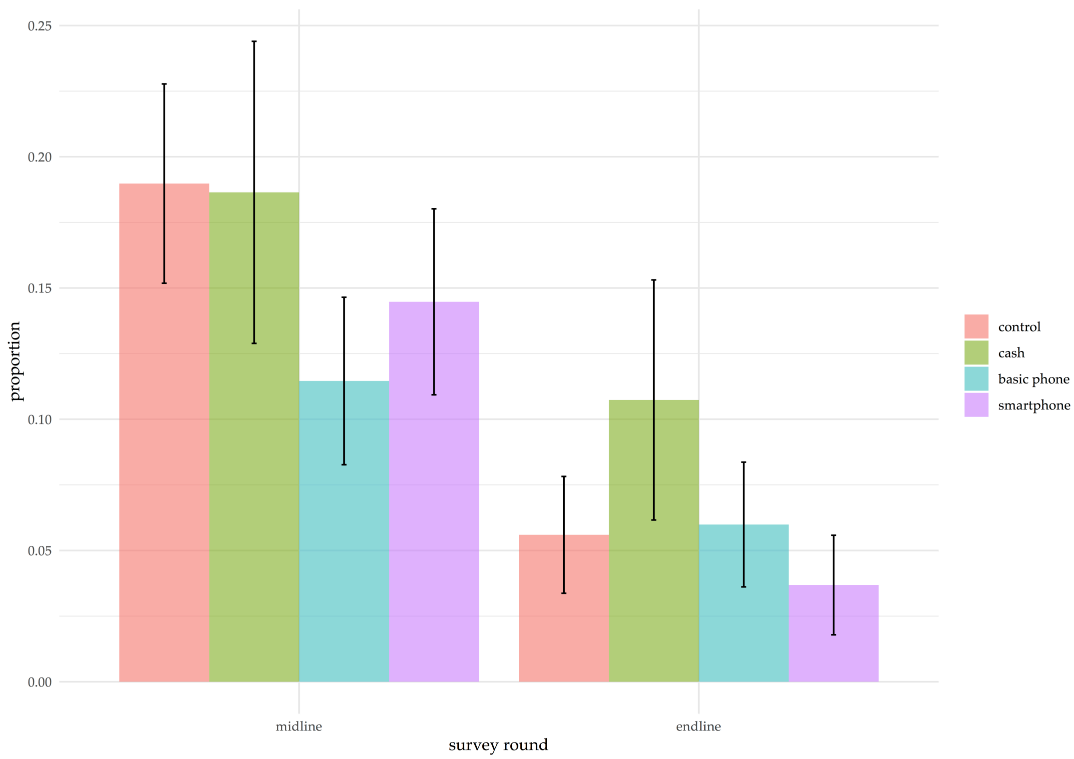

Fig. S2.1: Participant attrition at midline and endline surveys across treatment conditions. The y-axis indicates the proportion of participants who could not be surveyed for a given survey round.

S3 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 S3.1 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.

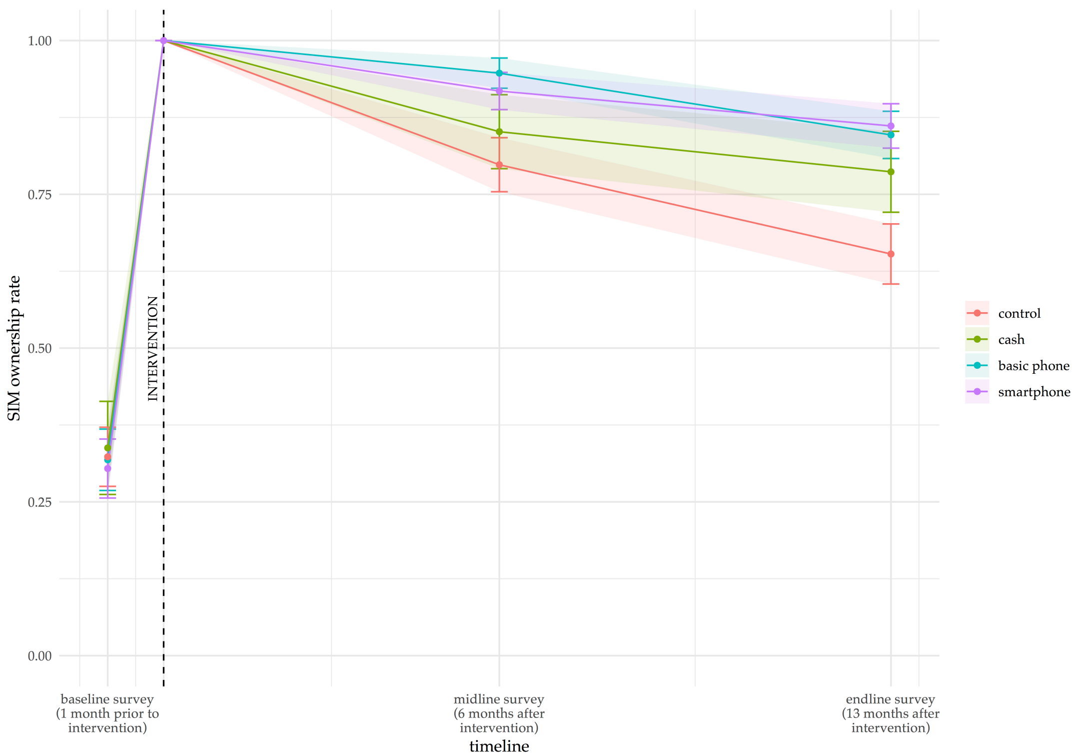

Fig. S3.1: Ownership of at least one SIM card before and throughout the study period. (Shading represents 95% confidence intervals.) This figure excludes those who did not attended program distribution.

Figure S3.2 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 S3.3 and Figure S3.4 provide additional information on handset turnover. Figure S3.3

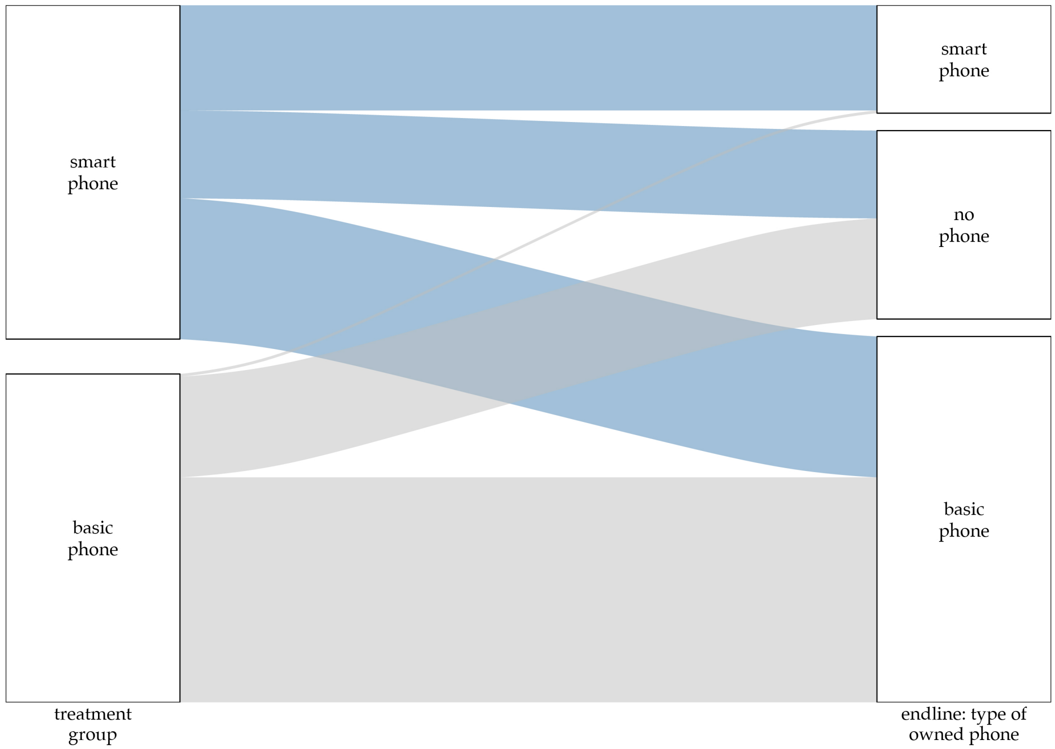

Fig. S3.2: Sankey diagram documenting change in handset type among those in the basic phone and smartphone conditions from intervention to endline. Handset type and phone ownership status are self-reports from endline survey.

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 S3.4 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.

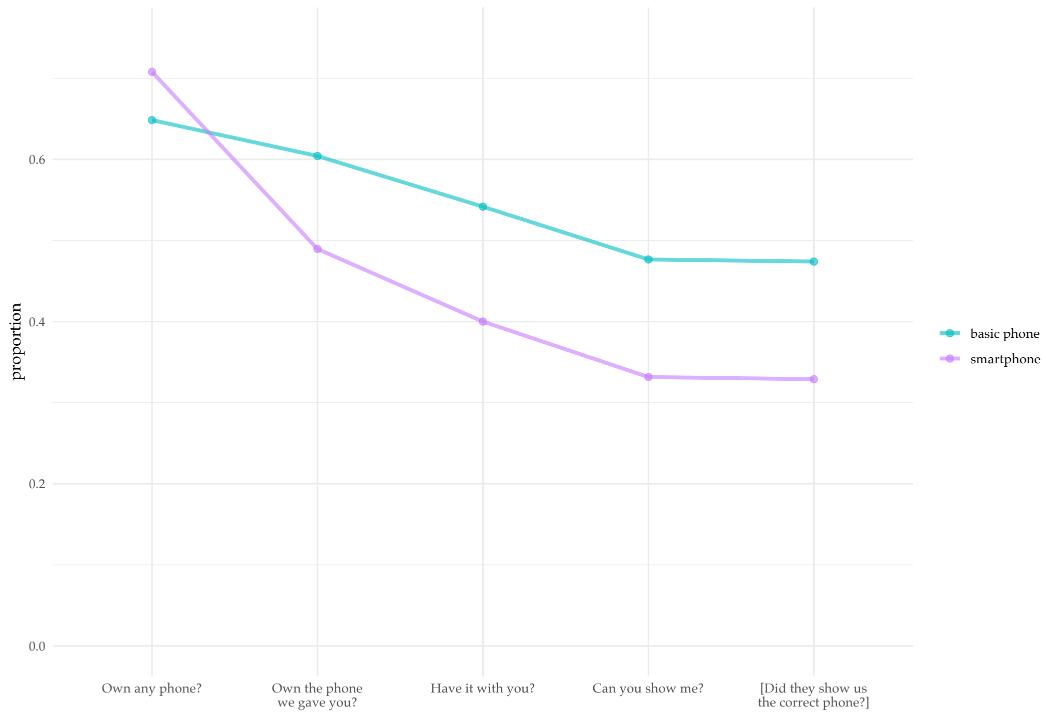

Fig. S3.3: Phone retention analysis

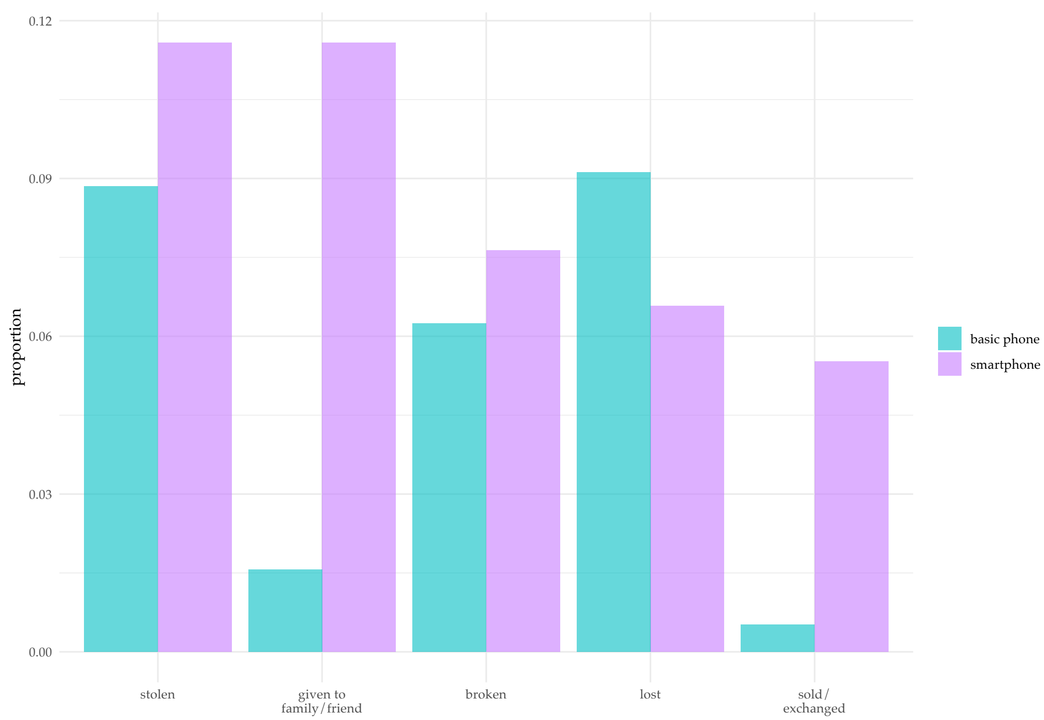

Fig. S3.4: Mode of non-retention of project phone

S4 Handset Non-retention from Midline to Endline

Consistent with the Sankey diagram in Figure 3, Figure S4.1 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.

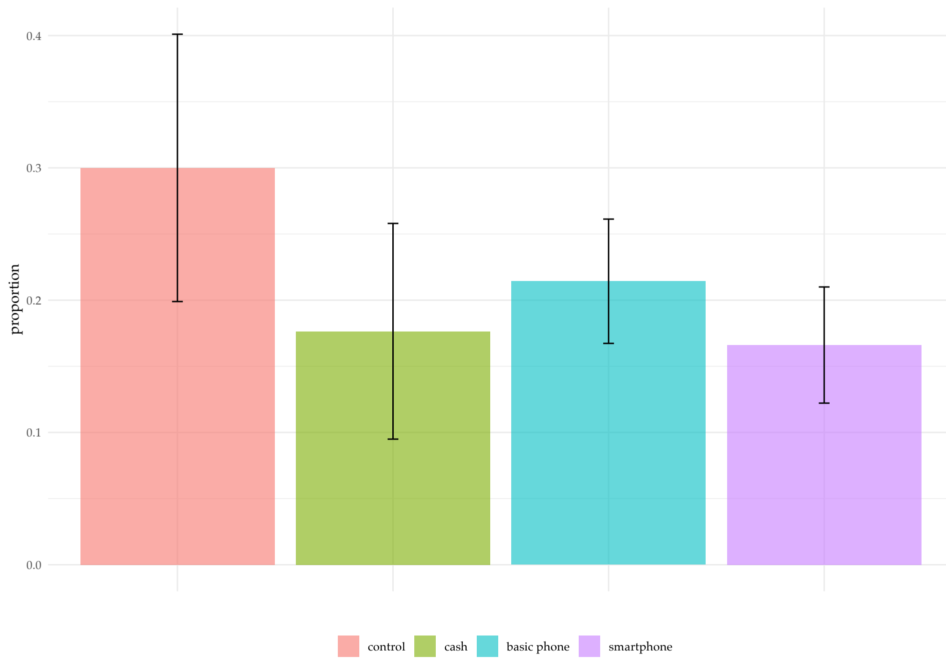

Fig. S4.1: Phone loss between midline and endline across treatment conditions. Each bar indicates the proportion of participants who reported owning any phone at midline but then did not report owning a phone at endline.

S5 Impact of Intervention on Household Mobile Phone Count

As shown in Figure S5.1, 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 S5.1, the divergence in household phone count between the basic phone and smartphone condition arose between midline and endline.

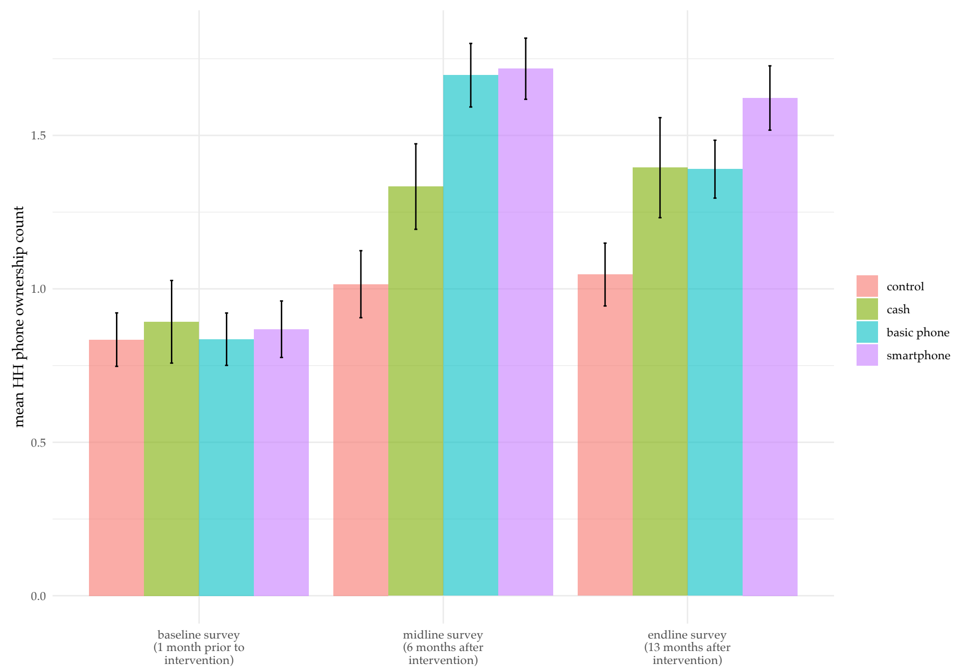

Fig. S5.1: Household mobile phone count between midline and endline across treatment conditions. Each bar indicates the average number of mobile phones reported in the household at baseline, midline, and endline.

The stacked bar chart in Figure S5.2 shows the distribution of phones per household by treatment condition. Whereas in the control condition three-quarters of households 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

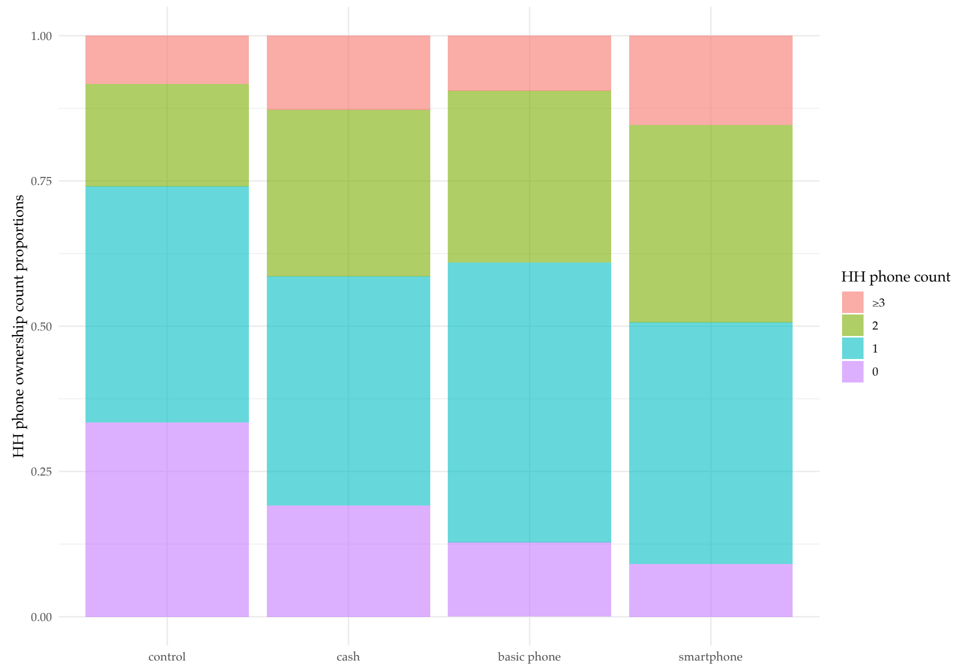

Fig. S5.2: Distribution of phone count per household across treatment conditions at endline.

and thus may have sold or exchanged them). (See Figure S5.3.) 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.

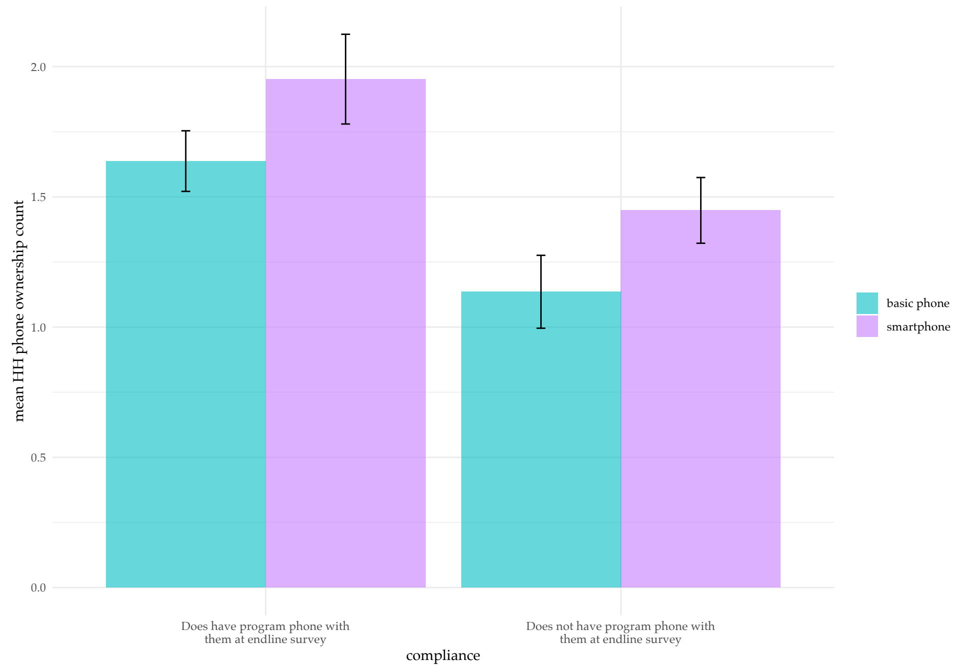

Fig. S5.3: Household handset count across compliers (those who kept their project phones throughout the duration of the study and had them in their possession during the endline survey) and non-compliers in basic and smartphone conditions.

Finally, Figure S5.4 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.

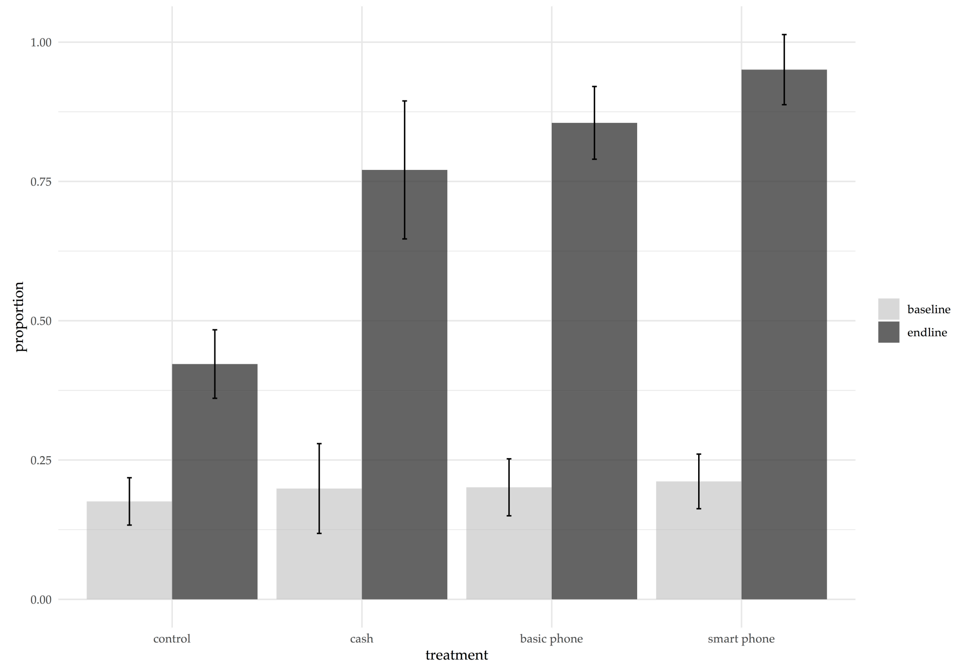

Fig. S5.4: Mean women's phone ownership in the household, by treatment (95% confidence intervals).

S6 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.

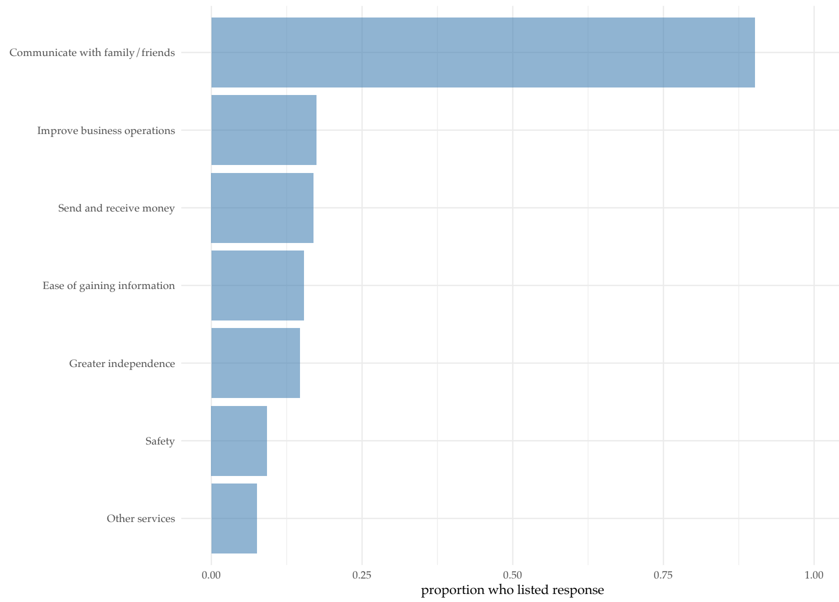

Fig. S6.1: Participants' baseline preferences for mobile phone use

S7 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 S7.1 to S7.3 provide a description of each variable and summary statistics. Following from Anderson (*31*), indexes were created using inverse covariance weighting.[2] 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 S7.4 to S7.6. Generally, missing observations on 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

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

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 S7.7) 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 S7.1: 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 S7.2: 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 S7.3: 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 S7.4: 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 S7.5: 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 S7.6: 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 S7.7: 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.

S8 Re-Analysis of Main Outcomes in Figure 2 Imputing Missing Observations

As explained in S7, 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 1 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.

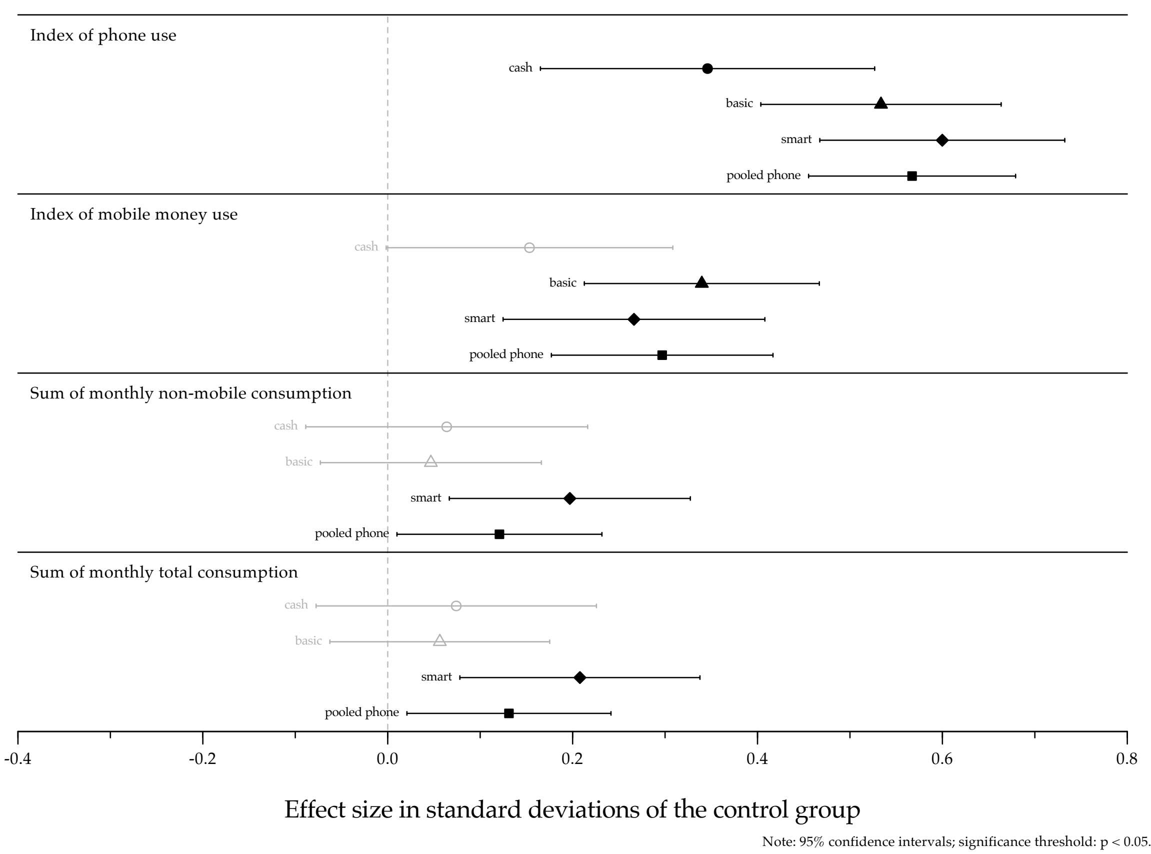

Fig. S8.1: Phone use, mobile money use and household consumption with missing observations imputed from baseline dependent variables. OLS regressions include the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, blocking variables, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

S9 Full Regression Results of Main Outcomes

S9.1 Full Regression Results of Main Outcome: Index of Phone Use

	(1)	(2)	(3)	(4)
Pooled Phone	0.598^{***}			
	(0.057)			
Basic		0.565^{***}		
		(0.066)		
Smart			0.630^{***}	
			(0.066)	
Cash				0.365^{***}
				(0.094)
Phone Use (baseline)	0.013	0.018	0.011	0.025
	(0.023)	(0.027)	(0.028)	(0.033)
Solar	0.094	0.141	0.098	0.083
	(0.072)	(0.092)	(0.094)	(0.112)
Voucher	-0.003	-0.037	0.016	-0.017
	(0.071)	(0.093)	(0.091)	(0.112)
Age	0.027^{*}	0.021	0.030^{*}	0.026
	(0.010)	(0.013)	(0.012)	(0.015)
Age^2	-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
	(0.065)	(0.081)	(0.080)	(0.096)
Education	0.034^{***}	0.035^{***}	0.036^{***}	0.046^{***}
	(0.008)	(0.010)	(0.010)	(0.011)
Own Phone In Past	0.248^{***}	0.238^{**}	0.319^{***}	0.337^{***}
	(0.061)	(0.074)	(0.073)	(0.086)
HH Size	0.008	0.001	0.008	-0.007
	(0.010)	(0.013)	(0.013)	(0.015)
Urban	0.184^{*}	0.201	0.203	0.305^{*}
	(0.088)	(0.110)	(0.109)	(0.136)
TASAF	-0.356^{***}	-0.408^{***}	-0.341^{***}	-0.377^{***}
	(0.077)	(0.095)	(0.093)	(0.110)
Income (H/L)	0.085	0.025	0.105	0.066
	(0.064)	(0.080)	(0.078)	(0.095)
Control mean	-0.380	-0.380	-0.380	-0.380
Observations	1077	717	727	519

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

Table S9.1: 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.

S9.2 Full Regression Results of Main Outcome: Index of Mobile Money Use

	(1)	(2)	(3)	(4)
Pooled Phone	0.317^{***}			
	(0.057)			
Basic		0.360^{***}		
		(0.065)		
Smart			0.289^{***}	
			(0.072)	
Cash				0.158^{*}
				(0.079)
MM Index (baseline)	0.189^{***}	0.224^{***}	0.169^{***}	0.194^{***}
	(0.032)	(0.033)	(0.040)	(0.037)
Solar	−0.102	−0.156	−0.061	−0.124
	(0.076)	(0.080)	(0.105)	(0.092)
Voucher	0.041	0.135	−0.088	−0.022
	(0.076)	(0.093)	(0.103)	(0.105)
Age	0.011	0.014	0.004	0.013
	(0.010)	(0.013)	(0.012)	(0.014)
Age^2	−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
	(0.067)	(0.071)	(0.084)	(0.077)
Education	0.026^{**}	0.030^{***}	0.026^{**}	0.035^{***}
	(0.008)	(0.009)	(0.010)	(0.009)
Own Phone In Past	0.189^{**}	0.128^{*}	0.284^{***}	0.259^{***}
	(0.059)	(0.064)	(0.074)	(0.069)
HH Size	0.022^{*}	0.011	0.039^{**}	0.036^{*}
	(0.011)	(0.013)	(0.013)	(0.015)
Urban	0.223^{*}	0.057	0.354^{**}	0.313^{*}
	(0.101)	(0.105)	(0.127)	(0.121)
TASAF	-0.285^{***}	-0.343^{***}	-0.227^{*}	-0.240^{*}
	(0.079)	(0.089)	(0.097)	(0.094)
Income (H/L)	0.024	0.030	0.001	0.004
	(0.062)	(0.066)	(0.079)	(0.075)
Control mean	0.0383	0.0383	0.0383	0.0383
Observations	1033	684	701	502

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

Table S9.2: 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.

S9.3 Full Regression Results of Main Outcome: Small Grant to Mobile Wallet

	(1)	(2)	(3)	(4)
Pooled Phone	0.109^{***}			
	(0.030)			
Basic		0.150^{***}		
		(0.035)		
Smart			0.069^{*}	
			(0.035)	
Cash				0.136^{**}
				(0.047)
MM Index (baseline)	0.039^{*}	0.040^{*}	0.030	0.038
	(0.016)	(0.019)	(0.019)	(0.022)
Solar	−0.063	−0.036	−0.049	0.061
	(0.036)	(0.045)	(0.047)	(0.057)
Voucher	−0.004	−0.027	−0.026	−0.061
	(0.036)	(0.046)	(0.047)	(0.058)
Age	0.003	0.002	0.002	0.006
	(0.006)	(0.007)	(0.007)	(0.008)
Age^2	−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
	(0.033)	(0.041)	(0.041)	(0.051)
Education	0.010^{*}	0.011^{*}	0.010	0.008
	(0.004)	(0.005)	(0.005)	(0.006)
Own Phone In Past	0.075^{*}	0.041	0.119^{**}	0.122^{*}
	(0.034)	(0.041)	(0.042)	(0.048)
HH Size	0.010	−0.002	0.015^{*}	−0.004
	(0.006)	(0.007)	(0.007)	(0.009)
Urban	0.130^{**}	0.141^{**}	0.136^{**}	0.124^{*}
	(0.041)	(0.049)	(0.051)	(0.061)
TASAF	-0.095^{*}	−0.099	-0.128^{*}	-0.135^{*}
	(0.042)	(0.052)	(0.050)	(0.060)
Income (H/L)	0.008	0.037	−0.021	0.007
	(0.034)	(0.041)	(0.041)	(0.050)
Control mean	0.49	0.49	0.49	0.49
Observations	1106	742	749	542

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

Table S9.3: 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.

S9.4 Full Regression Results of Main Outcome: Small Grant to Mobile Wallet on Own SIM

	(1)	(2)	(3)	(4)
Pooled Phone	0.156^{***}			
	(0.026)			
Basic		0.195^{***}		
		(0.032)		
Smart			0.122^{***}	
			(0.030)	
Cash				0.098^{*}
				(0.042)
MM Index (baseline)	0.061^{***}	0.081^{***}	0.038^{*}	0.062^{**}
	(0.016)	(0.019)	(0.018)	(0.021)
Solar	-0.042	-0.059	-0.018	-0.004
	(0.034)	(0.042)	(0.043)	(0.049)
Voucher	0.008	-0.015	0.007	-0.011
	(0.035)	(0.043)	(0.044)	(0.049)
Age	0.006	0.009	0.004	0.011
	(0.005)	(0.006)	(0.006)	(0.007)
Age^2	-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
	(0.030)	(0.038)	(0.034)	(0.042)
Education	0.014^{***}	0.015^{**}	0.015^{**}	0.018^{***}
	(0.004)	(0.005)	(0.004)	(0.005)
Own Phone In Past	0.127^{***}	0.097^{**}	0.180^{***}	0.141^{***}
	(0.031)	(0.037)	(0.035)	(0.039)
HH Size	0.000	-0.008	0.008	-0.001
	(0.006)	(0.007)	(0.006)	(0.007)
Urban	0.172^{***}	0.198^{***}	0.169^{**}	0.162^{*}
	(0.045)	(0.054)	(0.054)	(0.063)
TASAF	0.036	0.048	-0.017	-0.058
	(0.038)	(0.047)	(0.045)	(0.052)
Income (L/H)	0.020	0.027	-0.004	-0.001
	(0.031)	(0.037)	(0.035)	(0.041)
Control Mean	0.23	0.23	0.23	0.23
Observations	1106	742	749	542

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

Table S9.4: 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.

S9.5 Full Regression Results of Main Outcome: Standardized Monthly Non-mobile Consumption

	(1)	(2)	(3)	(4)
Pooled Phone	0.138^{*}			
	(0.057)			
Basic		0.073		
		(0.064)		
Smart			0.204^{**}	
			(0.072)	
Cash				0.056
				(0.082)
Consumption (baseline)	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
	(0.088)	(0.113)	(0.113)	(0.124)
Voucher	-0.092	-0.126	-0.153	-0.236^{*}
	(0.079)	(0.096)	(0.098)	(0.114)
Age	0.039^{***}	0.033^{**}	0.046^{***}	0.028^{*}
	(0.010)	(0.012)	(0.012)	(0.014)
Age^2	-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
	(0.066)	(0.077)	(0.085)	(0.088)
Education	-0.001	-0.004	-0.002	-0.003
	(0.008)	(0.009)	(0.010)	(0.008)
Own Phone In Past	0.130^{*}	0.109	0.128	0.136
	(0.061)	(0.072)	(0.073)	(0.079)
HH Size	0.019	0.001	0.025	0.008
	(0.013)	(0.013)	(0.016)	(0.016)
Urban	0.367^{**}	0.299^{*}	0.433^{**}	0.374^{**}
	(0.114)	(0.121)	(0.145)	(0.144)
TASAF	-0.408^{***}	-0.371^{***}	-0.494^{***}	-0.383^{***}
	(0.083)	(0.103)	(0.096)	(0.108)
Income (H/L)	0.055	0.040	-0.058	-0.067
	(0.065)	(0.083)	(0.072)	(0.080)
Control mean	$-.082$	$-.082$	$-.082$	$-.082$
Observations	1012	679	689	504

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

Table S9.5: 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

S9.6 Full Regression Results of Main Outcome: Standardized Monthly Total Consumption

	(1)	(2)	(3)	(4)
Pooled Phone	0.156^{**}			
	(0.057)			
Basic		0.088		
		(0.064)		
Smart			0.224^{**}	
			(0.073)	
Cash				0.070
				(0.082)
Consumption (baseline)	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
	(0.088)	(0.112)	(0.113)	(0.124)
Voucher	-0.087	-0.115	-0.149	-0.227^{*}
	(0.079)	(0.096)	(0.099)	(0.113)
Age	0.039^{***}	0.033^{**}	0.047^{***}	0.030^{*}
	(0.010)	(0.012)	(0.012)	(0.014)
Age^2	-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
	(0.067)	(0.077)	(0.086)	(0.088)
Education	-0.000	-0.004	-0.001	-0.003
	(0.008)	(0.009)	(0.010)	(0.008)
Own Phone In Past	0.118	0.087	0.116	0.116
	(0.061)	(0.071)	(0.073)	(0.078)
HH Size	0.019	0.001	0.026	0.008
	(0.013)	(0.013)	(0.016)	(0.016)
Urban	0.365^{**}	0.300^{*}	0.428^{**}	0.376^{**}
	(0.114)	(0.120)	(0.145)	(0.143)
TASAF	-0.418^{***}	-0.387^{***}	-0.509^{***}	-0.404^{***}
	(0.083)	(0.102)	(0.096)	(0.107)
Income (H/L)	0.063	0.051	-0.048	-0.054
	(0.065)	(0.082)	(0.072)	(0.079)
Control mean	$-.093$	$-.093$	$-.093$	$-.093$
Observations	1003	673	681	499

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

Table S9.6: 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

S10 Components of Phone Use Index

Figure S10.1 reports regression results from individual components of the phone use index on treatment conditions.

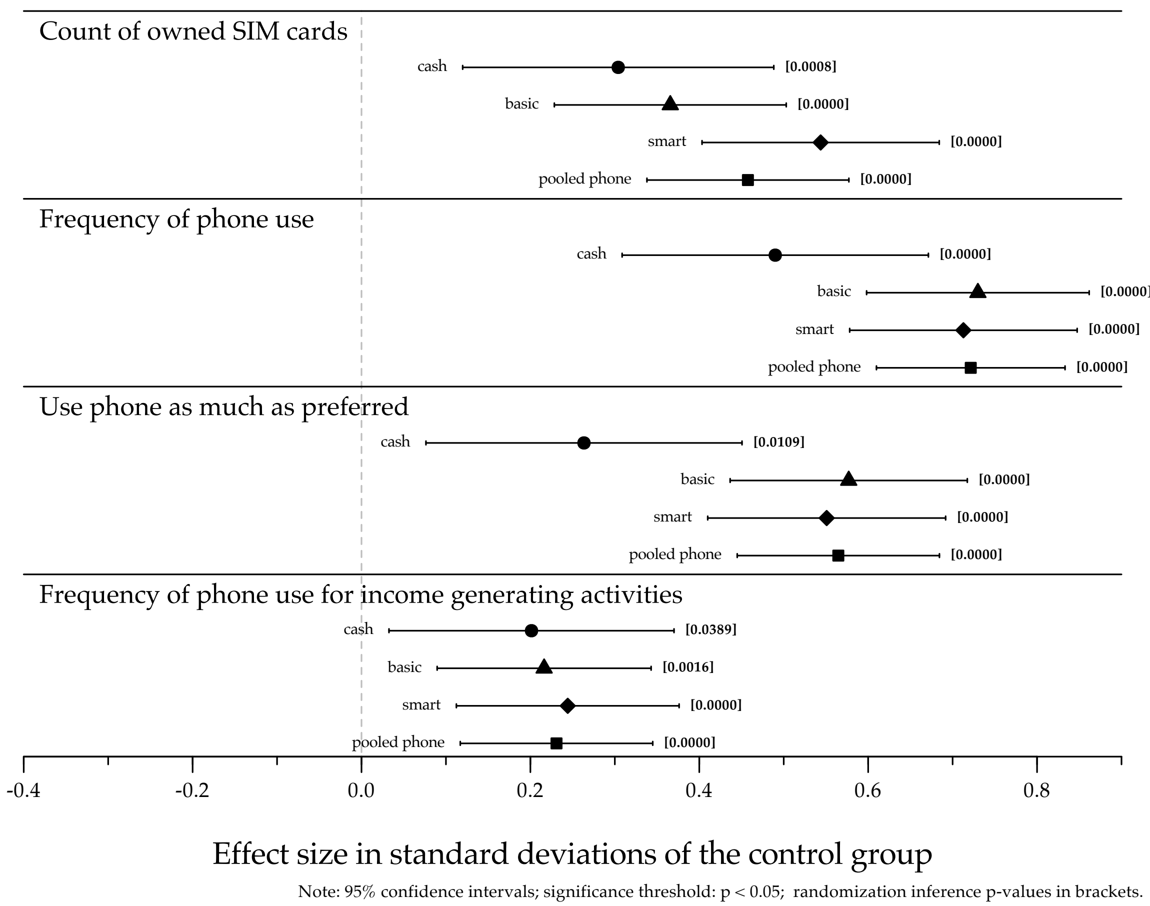

Fig. S10.1: Treatment effects on individual components of phone use. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

S11 Components of Mobile Money Use Index

Figure S11.1 reports regression results from individual components of the mobile money use index on treatment conditions.

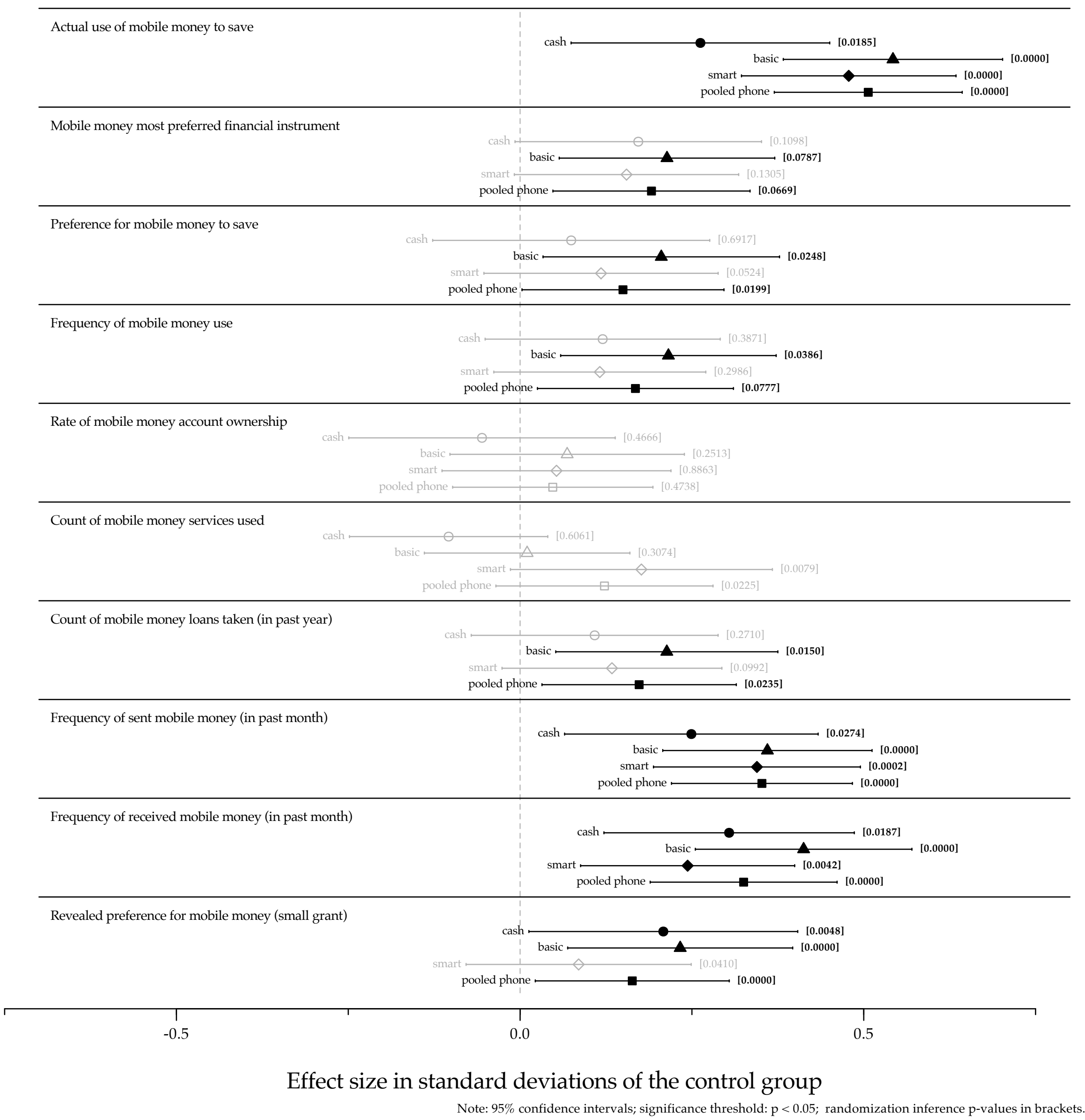

Fig. S11.1: Treatment effects on individual components of mobile money use. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

S12 Access to Financial Information and Use of Phone for Income-Generation

Beyond enabling use of digital financial services, mobile technology has also been found to reduce search costs and improve access to market information (*2*). Figure S12.1 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 (*8, 9*).

In contrast, participants in the treatment groups are significantly more likely to report using a phone for income-generation. As reported in S12.2, phones were predominantly used for communicating with customers and clients, pointing to the benefits phones provide to market traders.

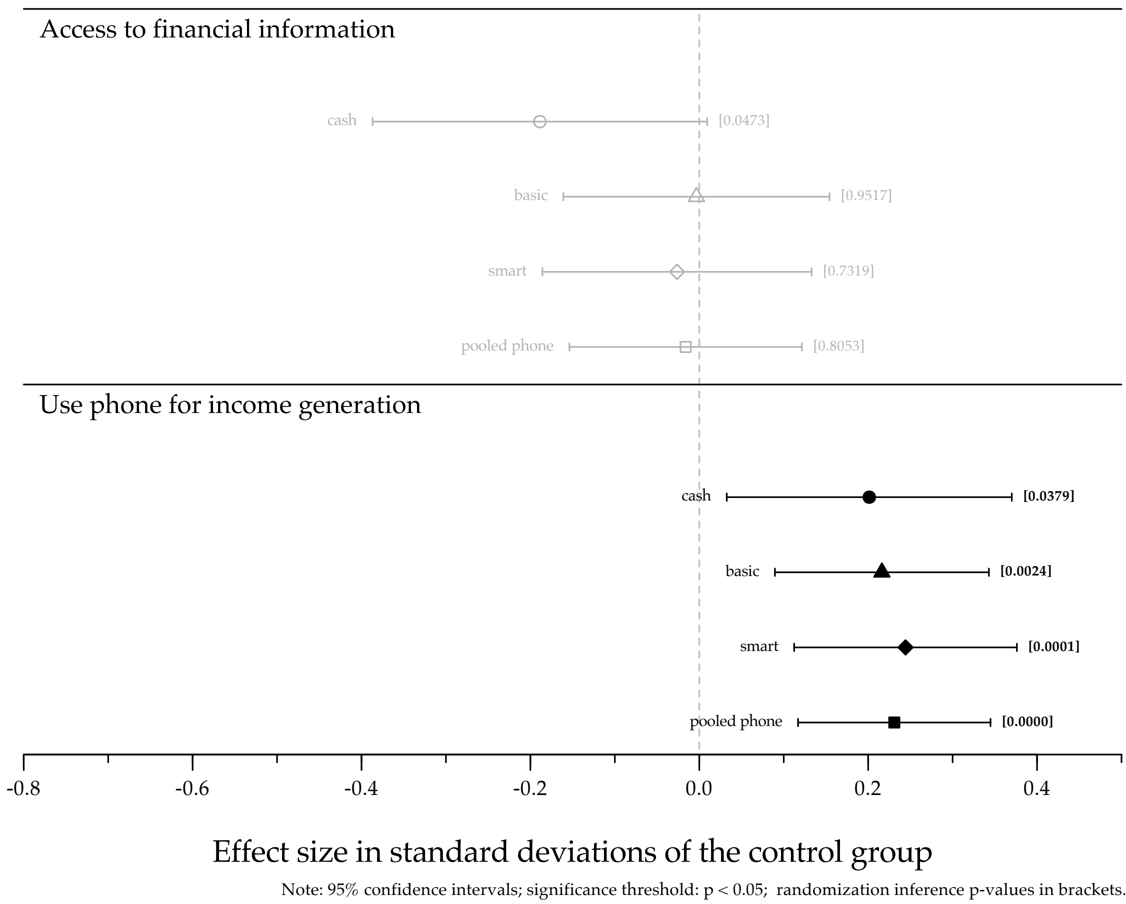

Fig. S12.1: Treatment effects on access to financial information and income-generation. Access to financial information refers to participants' assessment of how hard it is to obtain financial information, such as prices of goods and services, that one needs for her job or business activities: "1" (not difficult at all) to "4" (very difficult). This variable has been inverse coded so higher values indicate less difficulty acquiring access to information. Income-generation indicates how frequently participants report using a mobile phone for income-generating purposes: "0" (never) to "4" (every day). Analysis run using randomization inference. 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.

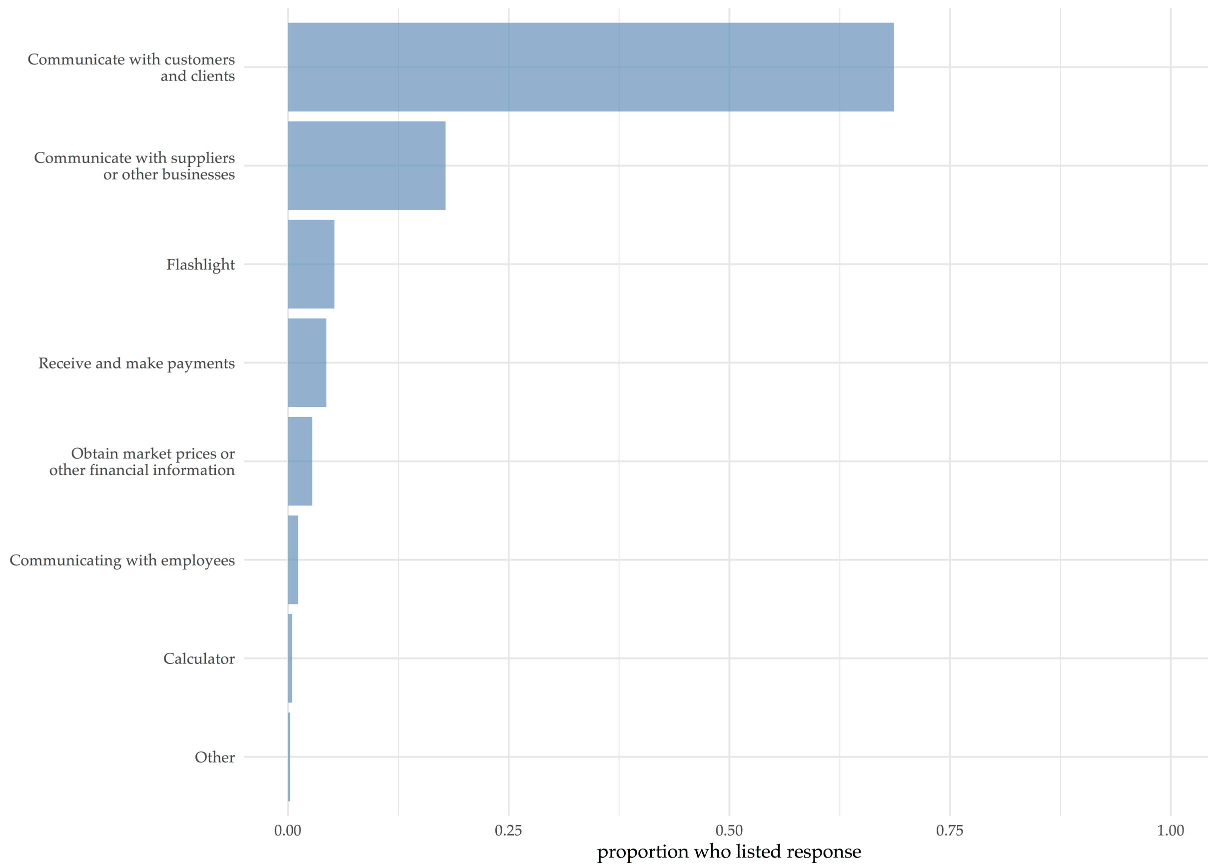

Fig. S12.2: How respondents report using a mobile phone for income-generation.

S13 Effects of Mobile Phone Ownership on Weekly and Monthly Income

Figure S13.1 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 S13.1 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.

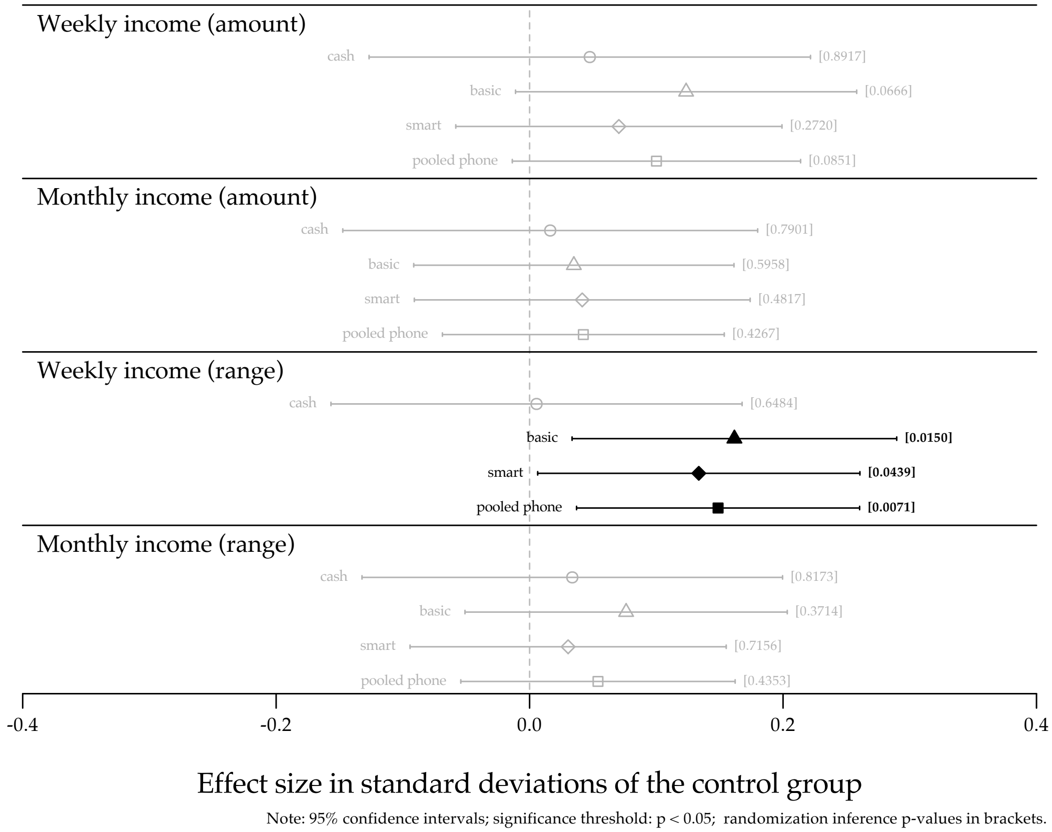

Fig. S13.1: Treatment effects on weekly and monthly income. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

S14 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 S8. As indicated in Figure S14.1, all aggregation measures produce highly consistent results.

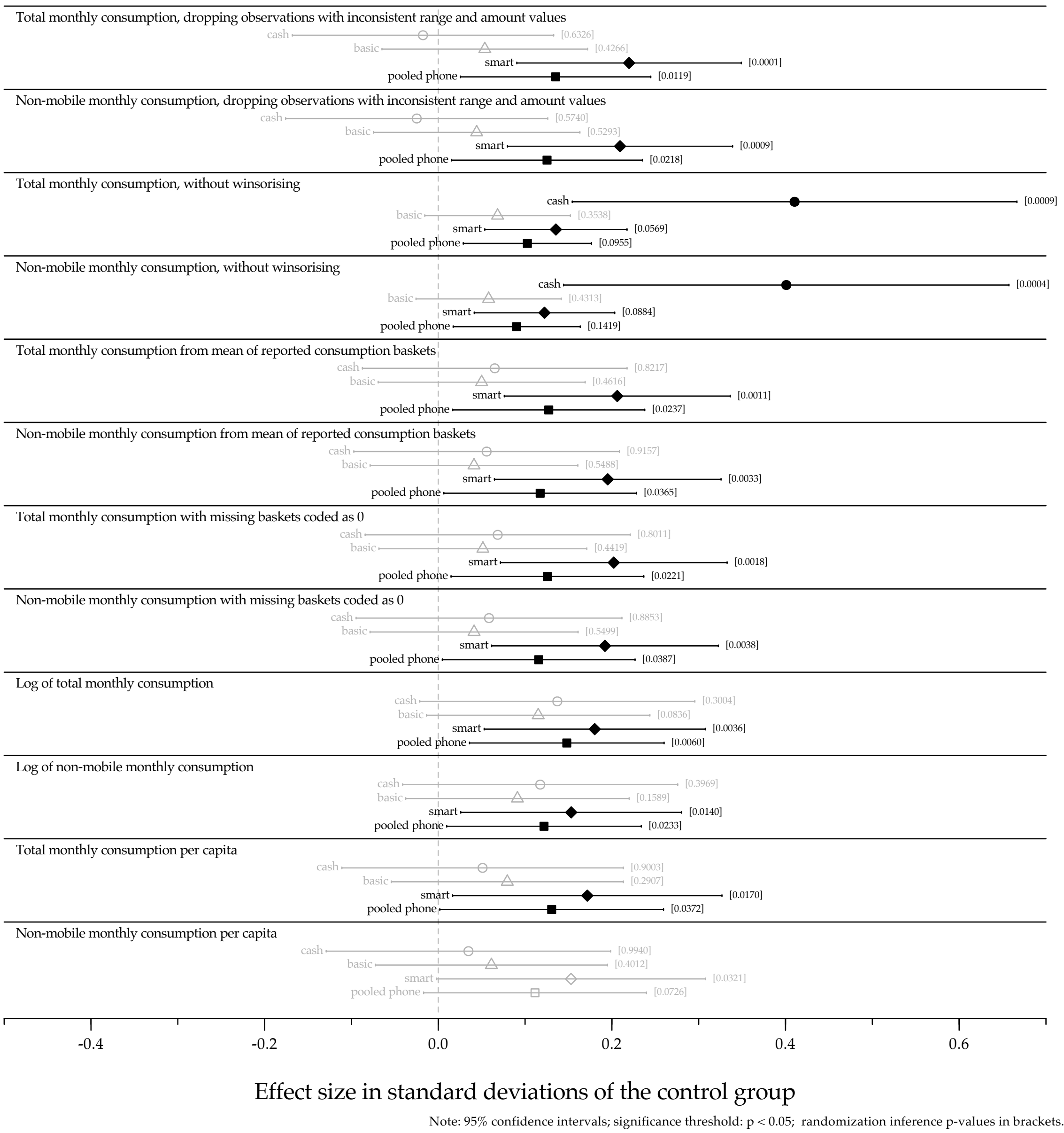

Fig. S14.1: Robustness of consumption results across alternative aggregation rules. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

S15 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—which may have been tempting for many participants given the high asset value of the smartphone (PPP US$186) compared to the basic handset (PPP US$64). 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). (*26*) 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 S15.1. 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 non-compliance.

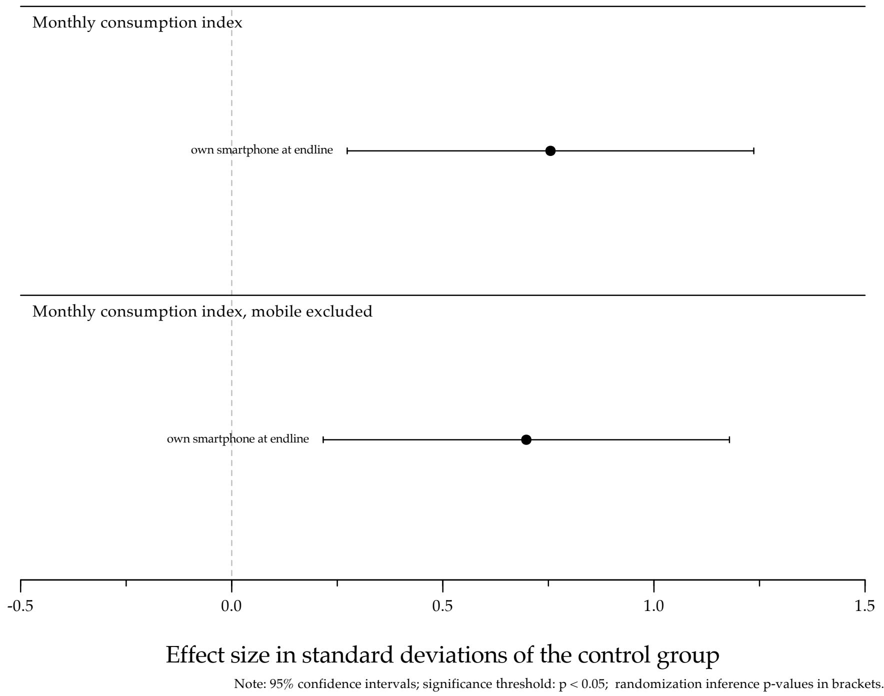

Fig. S15.1: Complier average causal effects of smartphones on household consumption. To estimate CACE, we run a two-stage least squares (2SLS) regression analysis in which we use assignment to the smartphone condition to instrument for a participant's ownership of a smartphone at endline. (The F statistic 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.

S16 Analysis of Effects on Individual Consumption Baskets

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.

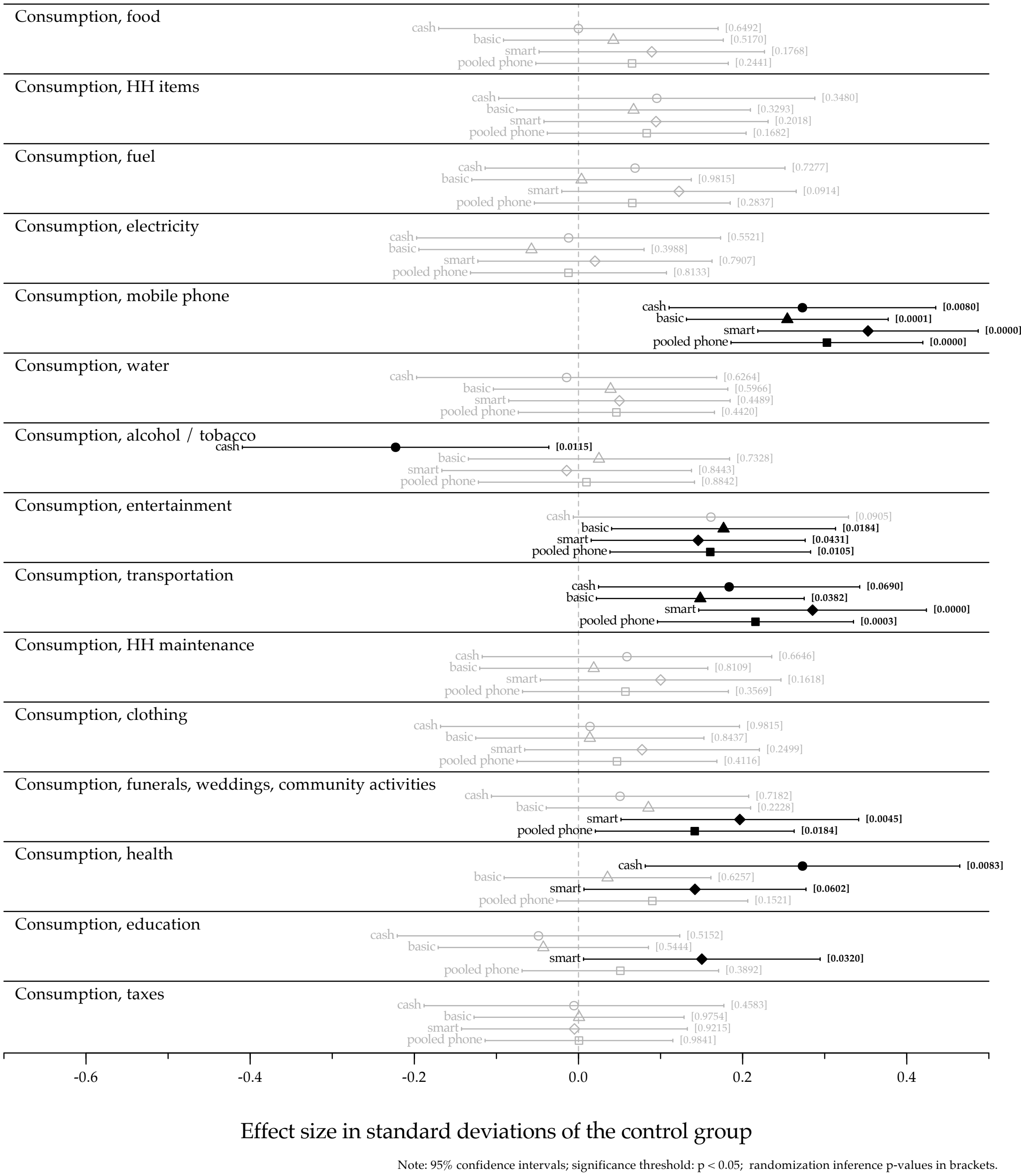

Fig. S16.1: Treatment effects on individual consumption baskets. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

S17 Subgroup Effects by Participant Program Membership

In line with our pre-analysis plan, we also report sub-group effects of mobile phone ownership among microfinance clients and members of the poverty reduction program, one of our blocking criteria. At baseline, the former had significantly higher levels of education, literacy, urbancity and household consumption, and they tended to be relatively wealthier traders and business owners rather than very poor smallholder farmers (See Table S1.1). These results 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 in the microfinance subgroup than those in the poverty reduction program. 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 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.

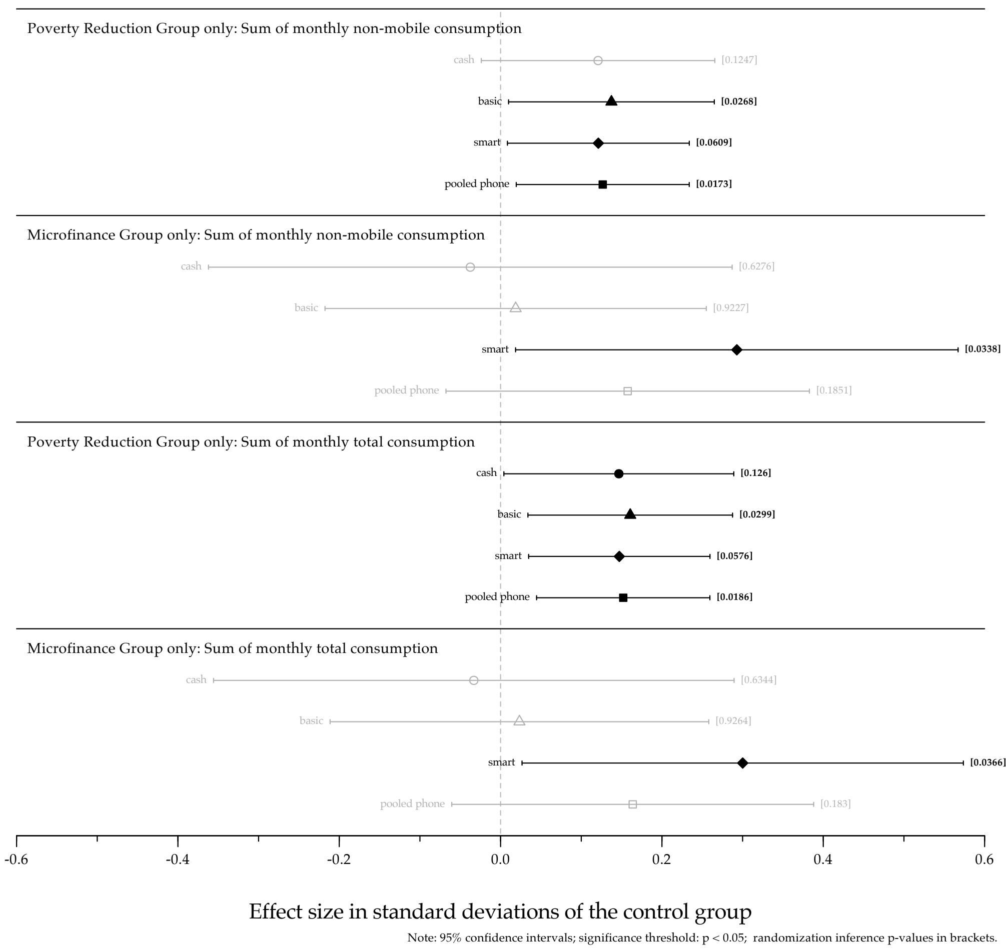

Fig. S17.1: Impact of treatment conditions on standardized measures of monthly non-mobile and total household consumption across the microfinance and poverty reduction program sub-groups. Models run using randomization inference. Specifications include our income blocking strata (urban/rural and development group membership drop out as perfectly correlated with sub-groups), 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.

Table S17.1 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 microfinance participants, had significant effects on consumption through the increase in number of phones in the household. For the basic phone among microfinance participants, the first stage (effect on household handset count) is significant but a weak instrument.

Consistent with these results, microfinance participants reported not retaining the basic handset we provided them compared to members of the poverty reduction program. At endline only 58% of microfinance participants in the basic phone group reported still having the Samsung B110, compared to 79% of poverty reduction program participants. Strikingly, this meant that microfinance participants in the basic phone condition had lower rates of phone ownership at endline compared to their counterparts from the poverty reduction program. 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 S17.1: 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.

S18 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 poverty reduction program subgroup—representing the poorest segment of the sample and of the population in Tanzania generally. Table S18.1 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 S24 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 S18.1: 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, cash transfers are estimated to increase consumption roughly 1.1:1 for the cost of the cash grant (excluding programmatic costs). (*25*) Banerjee et al. (*24*) 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).

S19 Mechanisms and Mediation Analysis

The power of mobile phones stems from the technology's multidimensionality; it enables long-distance communication, access to information, and money transfers. (*1*) 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—improving women's efficiency as market traders—and the uptake and use of mobile money.

To ensure that these mechanisms contribute to the observed increase in household consumption, we run a two-stage least squares regression analysis of monthly household consumption using assignment to the pooled phone condition to instrument for standardized measures of mobile-based income-generation activities (market trading) and frequency of weekly mobile money use. The results are reported in Table S19.1. Both the first and second stages are strong. The F statistic in the first stage is 16.1 for mobile-based income-generation activities and 18.7 for frequency of weekly mobile money use. In the first stage, mobile phone ownership increases the frequency of mobile-based income-generation by 0.22-0.24 SD over control, and in the second stage, is estimated to cause a 0.62 SD and 0.65 increase in monthly non-mobile and total consumption, respectively. Likewise, mobile phone ownership increases the frequency of weekly mobile money use by 0.27 SD over control and, in the second stage, this is estimated to cause a 0.52 SD and 0.59 increase in monthly non-mobile and total consumption, respectively.

	Second-stage. Standardized monthly total consumption			
	1	2	3	4
	Non-mobile Consumption	Total Consumption	Non-mobile Consumption	Total Consumption
Mobile-based income-generation activities (weekly)	0.622^{*}	0.653^{*}		
	(0.279)	(0.262)		
Mobile money use (weekly)			0.523^{*}	0.589^{*}
			(0.234)	(0.244)
	First-stage. Mobile income generation act. or mobile money use			
	1	2	3	4
	Mobile-based income gen	Mobile-based income gen	Mobile money use	Mobile money use
Pooled phone	0.220^{***}	0.238^{***}	0.271^{***}	0.266^{***}
	(0.057)	(0.057)	(0.060)	(0.060)
First-stage F-statistic	14.86	17.57	20.35	19.38
Observations	1004	995	989	981

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

Table S19.1: IV Regression Analysis of Mobile-based Mechanisms on Household Consumption. Results derived from a two-stage least squares regression analysis of standardized monthly non-mobile and total household consumption using assignment to the pooled phone condition to instrument for standardized measures of mobile-based income-generation activities (reported in columns 1 and 2) and frequency of weekly mobile money use (reported in columns 3 and 4). Models include solar charger and voucher cross-cutting treatment conditions, a baseline measure of consumption, baseline measure of mobile phone use or mobile money use, blocking variables, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size. (Covariates not shown.) First-stage F-statistic is reported at the bottom of the table. Robust standard errors clustered at individual level.

We further undertake a mediation analysis to estimate the degree to which these mechanisms contribute to increases in household consumption. To do so, we undertake KHB decomposition models (*32*) estimating the effects of the phone treatment conditions compared to control on household consumption mediated by standardized measures of mobile money use and using the phone for income-generating activities.

In both models, mobile money use and mobile income-generation are independently significant, suggesting the multifaceted ways mobile technology affects livelihoods. Jointly the mechanisms account for about the same standard deviation increase in household consumption over control: 0.07 (95% CI: 0.034-0.12) in the smartphone condition and 0.06 (95% CI: 0.026-0.10) in the basic phone condition. This points to the benefits of increasing women's phone ownership for household welfare.

However, only in the smartphone condition does the phone have a significant non-mediated effect—pointing to the effects of the smartphone on consumption beyond the participant's use. In fact, our estimations suggest in the smartphone model the mediators only account for 34% of the overall household consumption effect; 66% appears to arise from other uses of the phone. This suggests that the power of the smartphone was in strengthening participants' mobile money use and market trading *and* the other economic effects it brought to the household.

The results reported in Figure S19.1 are consistent with the differences in how smartphones and basic handsets were reported to be used in the household. The smartphone tended to be more of a household good than the basic handset—it was more likely to be exchanged or shared and thus used by other family members. Also, as reported in S6, smartphones tended to complement the household's basic handset and boosted overall mobile phone capacity at a higher rate. Taken together, we would expect broader productivity gains at the household level to arise from smartphones than from basic handsets.

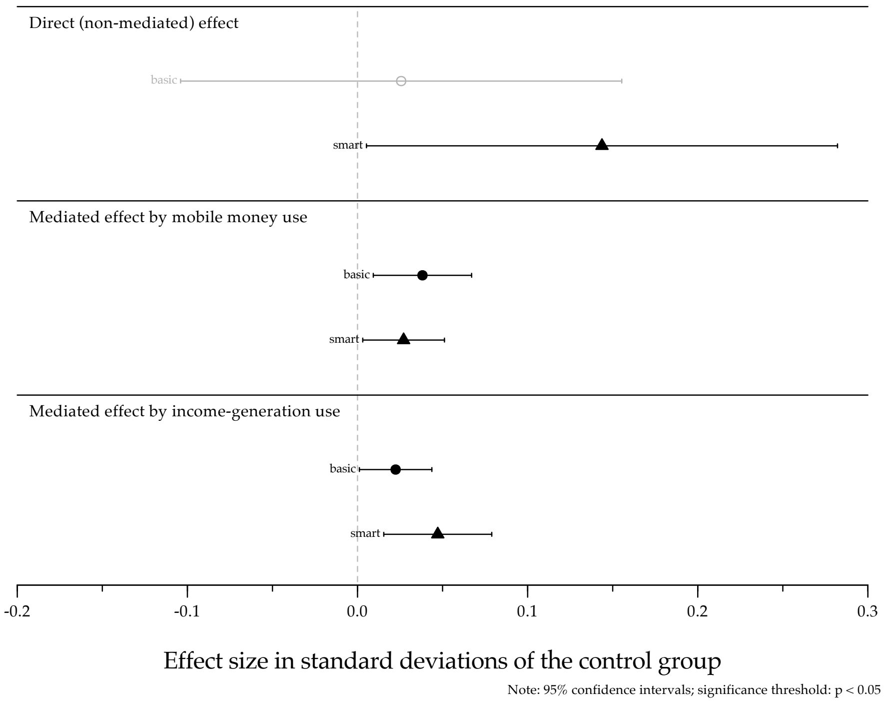

Fig. S19.1: Mediation analysis of mobile phone ownership on household consumption. KHB decomposition models (*32*) estimating the direct and mediated effects of basic phone and smartphone ownership, respectively, on total monthly household consumption versus control. The mediators include the participant's weekly use of mobile money and weekly use of a phone for income generation. 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.

S20 Smartphone Ownership, Use by Other Family Members and Correlation with Change in Household Consumption

The mediation analysis reported in S19 suggests that the power of the smartphone was in strengthening participants' mobile money use and market trading *and* the other economic effects it brought to the household. In line with this, we observe descriptively that the largest increases in household consumption occurred among those participants who reported that beyond themselves it was their husbands who used the smartphones. This is reported in Figure S20.1.

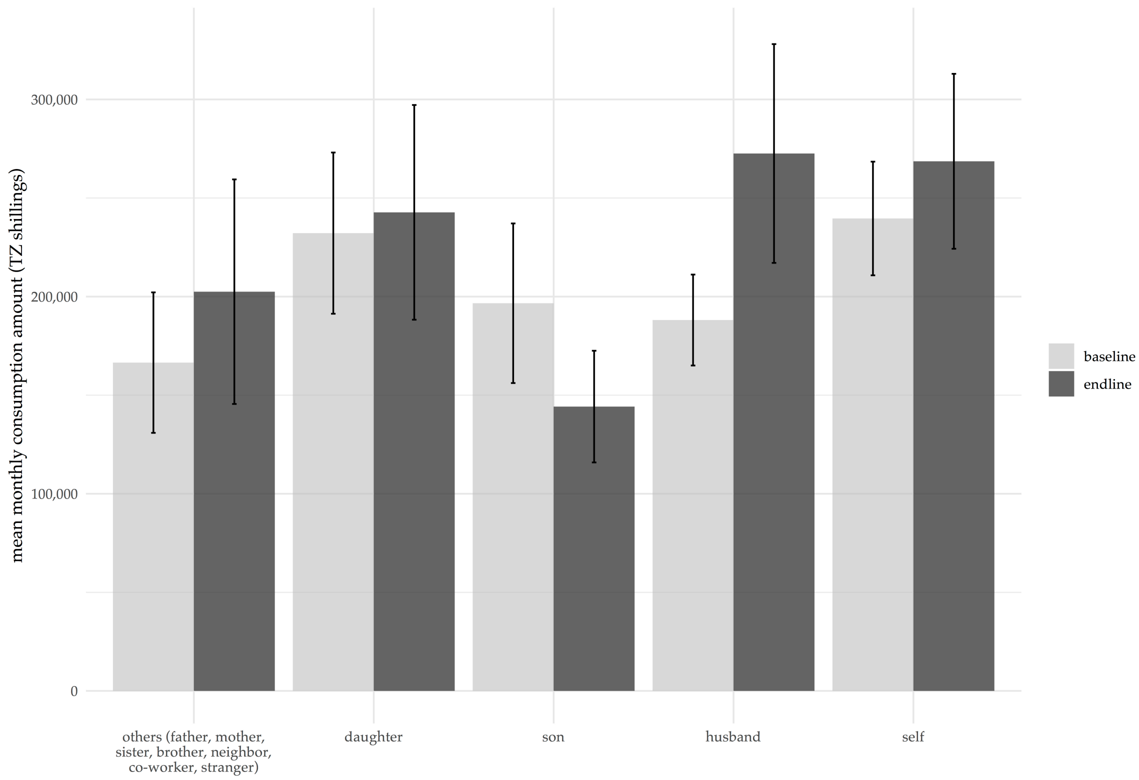

Fig. S20.1: Change in household consumption by family members' smartphone use. Family members' smartphone use corresponds to whom the participant reported used the phone the most besides themselves at the end of the endline survey. "Self" indicates when the participant reported no one else used the phone. "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.

How the husband is reported to have used the phone seems to matter, however. If the participant reported her husband appropriated the smartphone (i.e., was the only one who used it), this correlated with a significant reduction in household consumption. 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 S20.2. These households start at similar consumption levels at the outset of the study but then diverge based on cooperative or non-cooperative smartphone use.

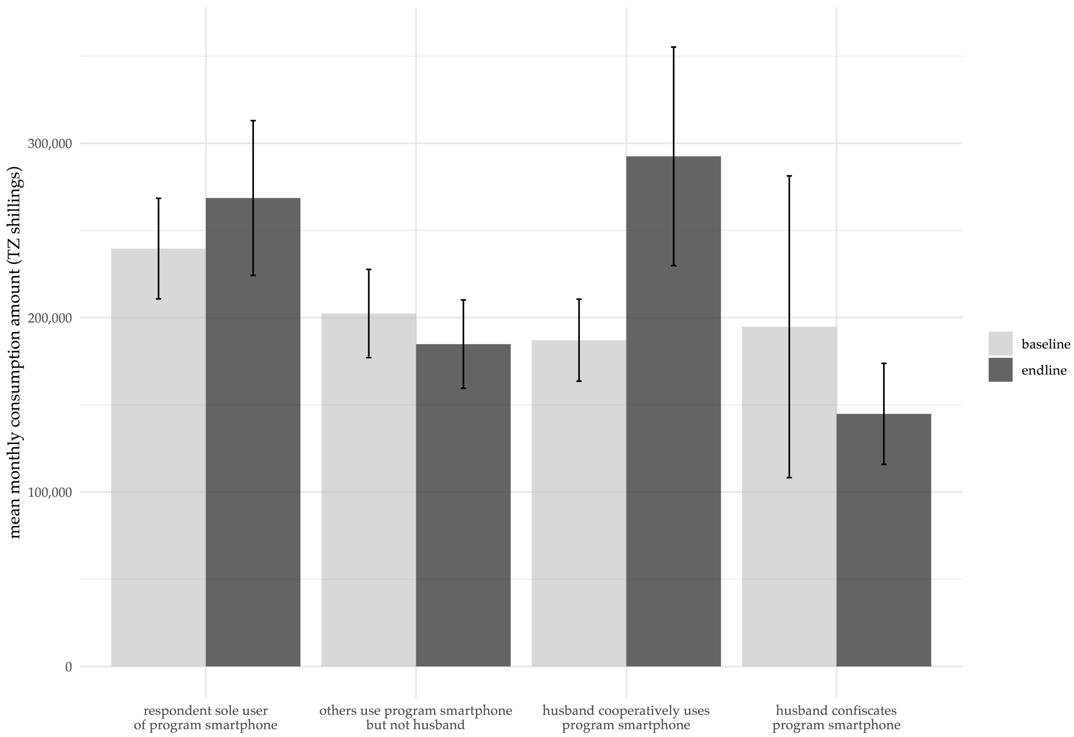

Fig. S20.2: Household use of smartphone and consumption. Baseline and endline mean total monthly consumption across different ways the program smartphone was reportedly used. "Husband confiscates program phone" corresponds to those respondents who reported their husbands were the only ones who used the 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 S20.1.) 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 S20.1.) 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.

Who Else Used Project Phone	Handset Type 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 S20.1: *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 S20.3 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 S20.2 illustrates that low-income women were more than twice as likely to have reported their phone was appropriated.

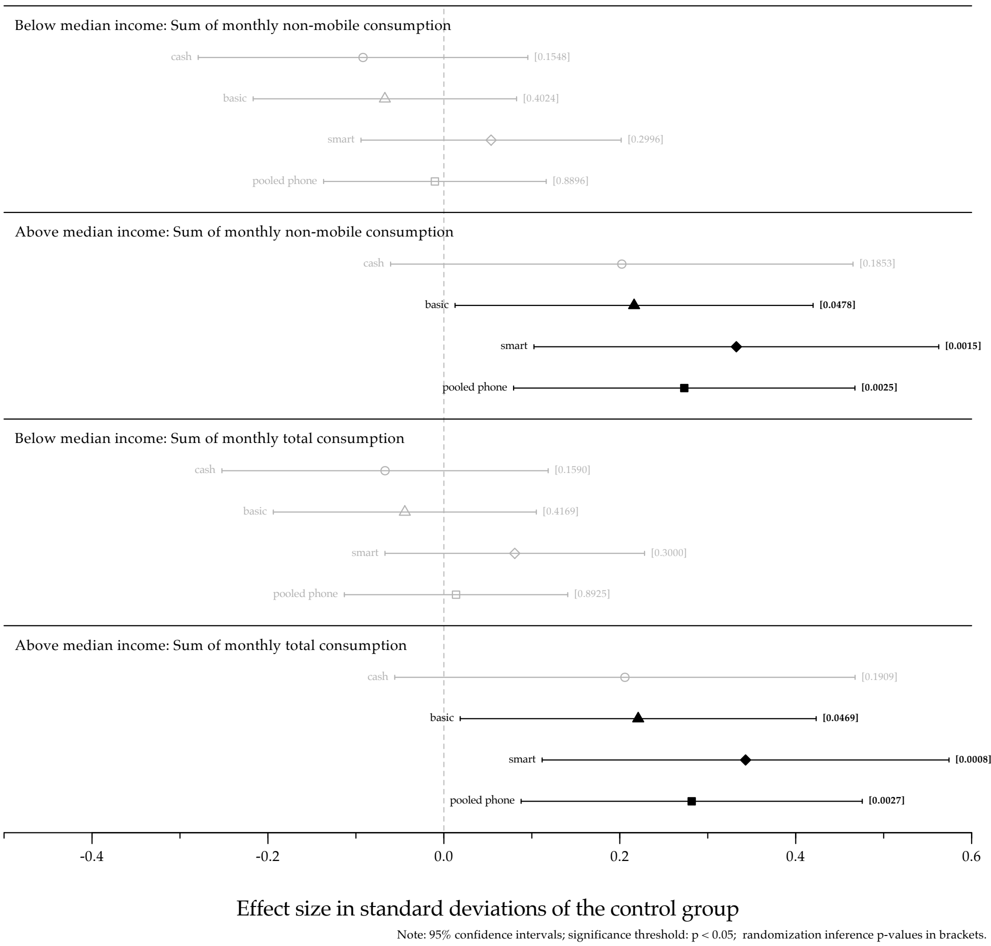

Fig. S20.3: Impact of treatment conditions on standardized measures of monthly non-mobile and total household consumption across below and above median baseline income sub-groups. Models run using randomization inference. Specifications include our income blocking strata (the income block drops out as perfectly correlated with sub-groups), 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.

	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 S20.2: 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.

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.

S21 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 S21.1 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 S7. For effects on individual components, see Figs. S21.2 to S21.8 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 exerted somewhat of a backlash in terms of an uptick in intimate partner violence—but importantly this did not originate from husbands (see null effects on IPV index of spouse measures—Figure S21.1); it was more likely among non-spouses (perhaps boyfriends or other family members) in the form of reports of humiliation and threats (see Figure S21.8 below).

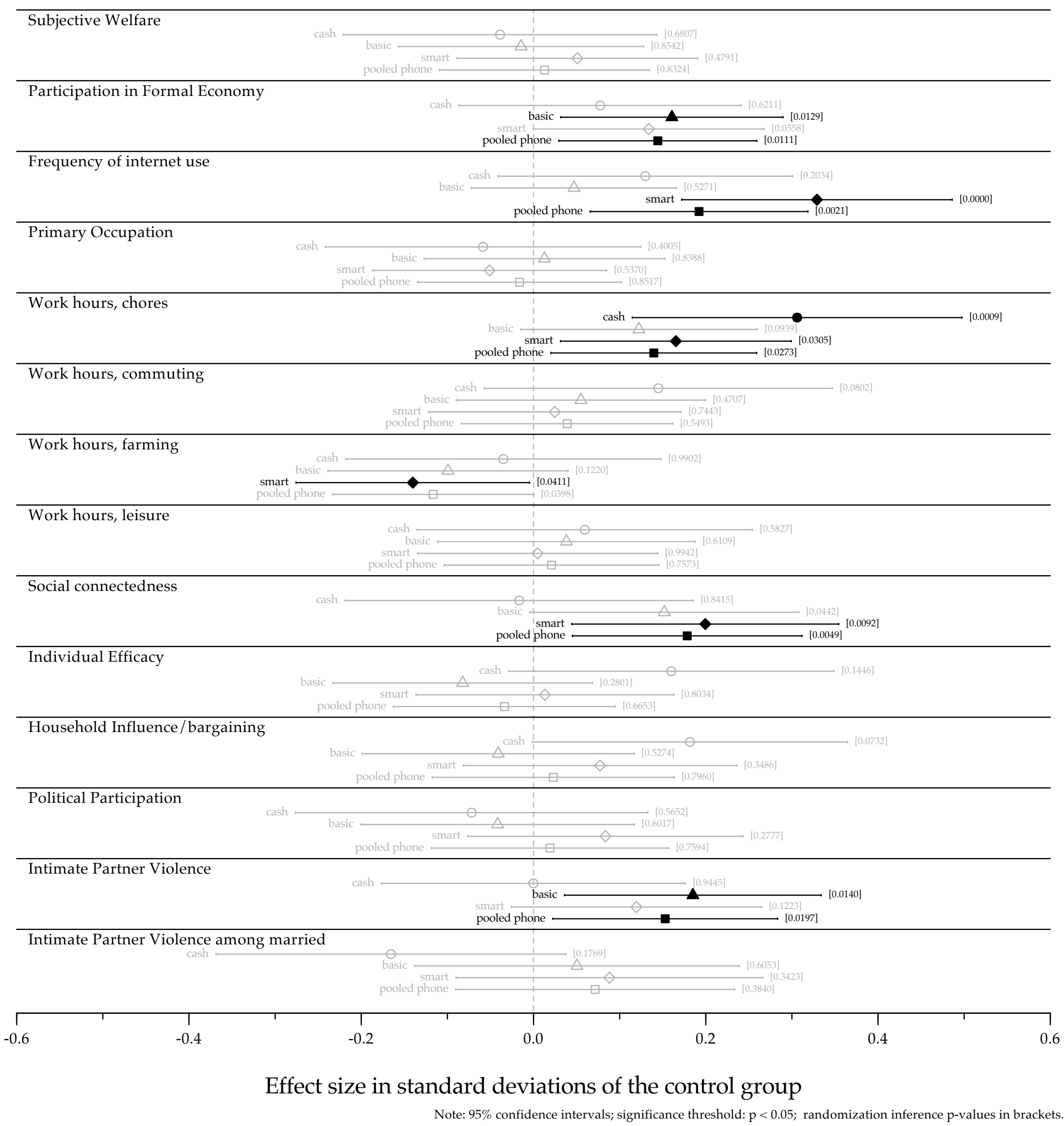

Fig. S21.1: Downstream effects of mobile phone ownership on different measures of empowerment. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

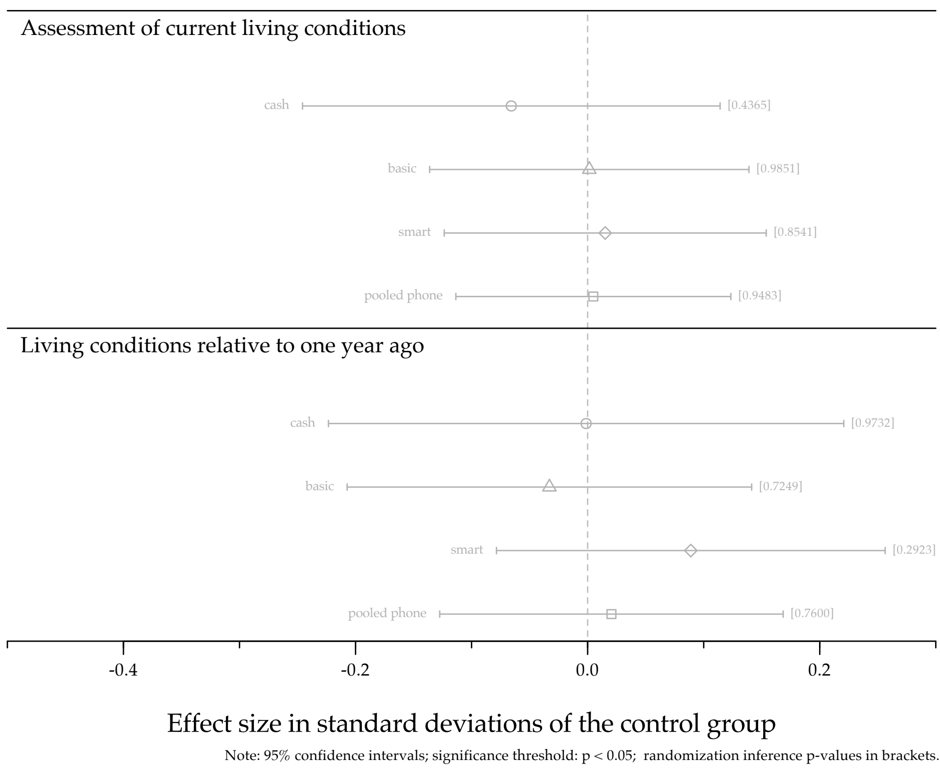

Fig. S21.2: Effects on subjective welfare components. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

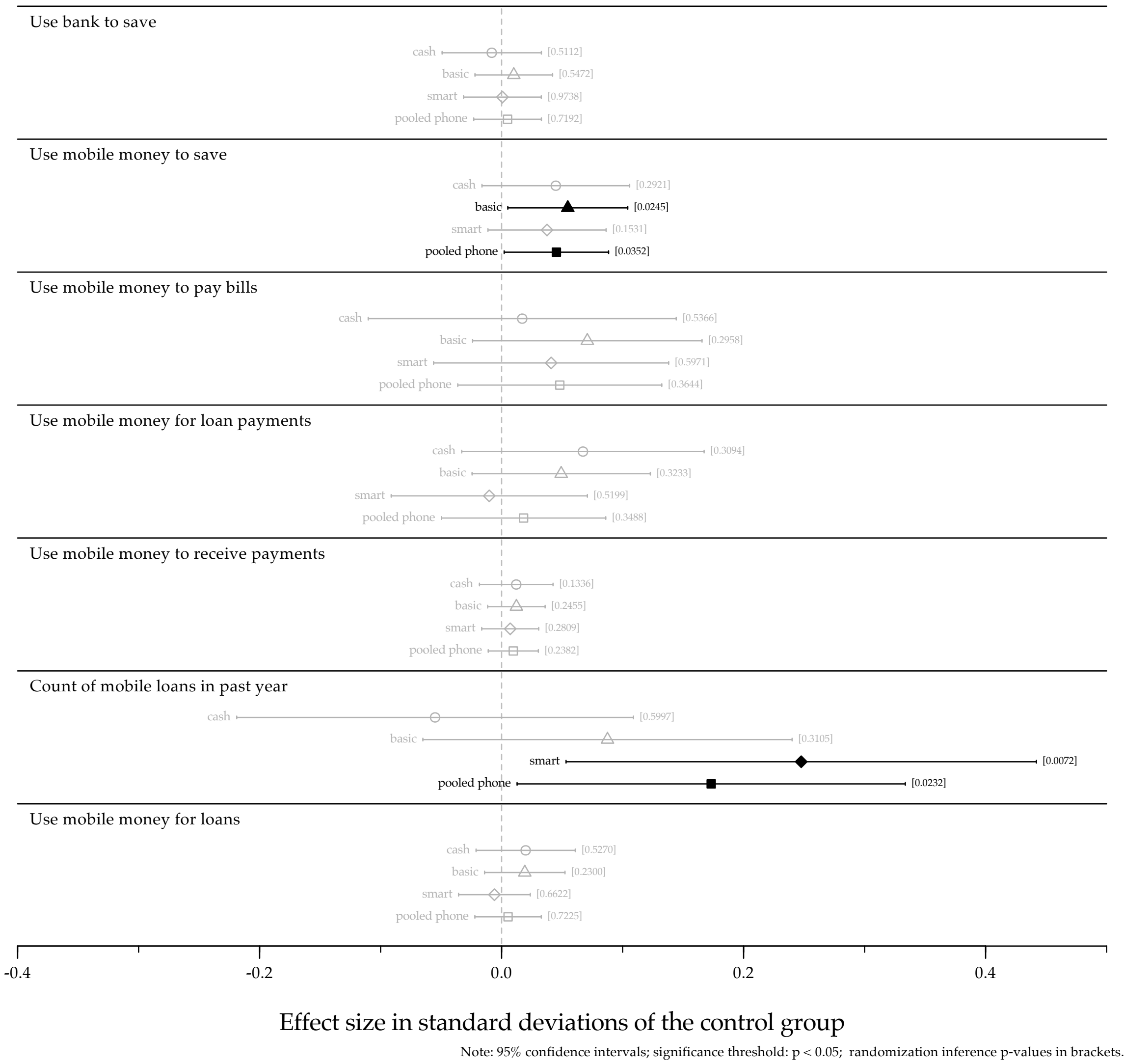

Fig. S21.3: Effects on components of participation in formal economy. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

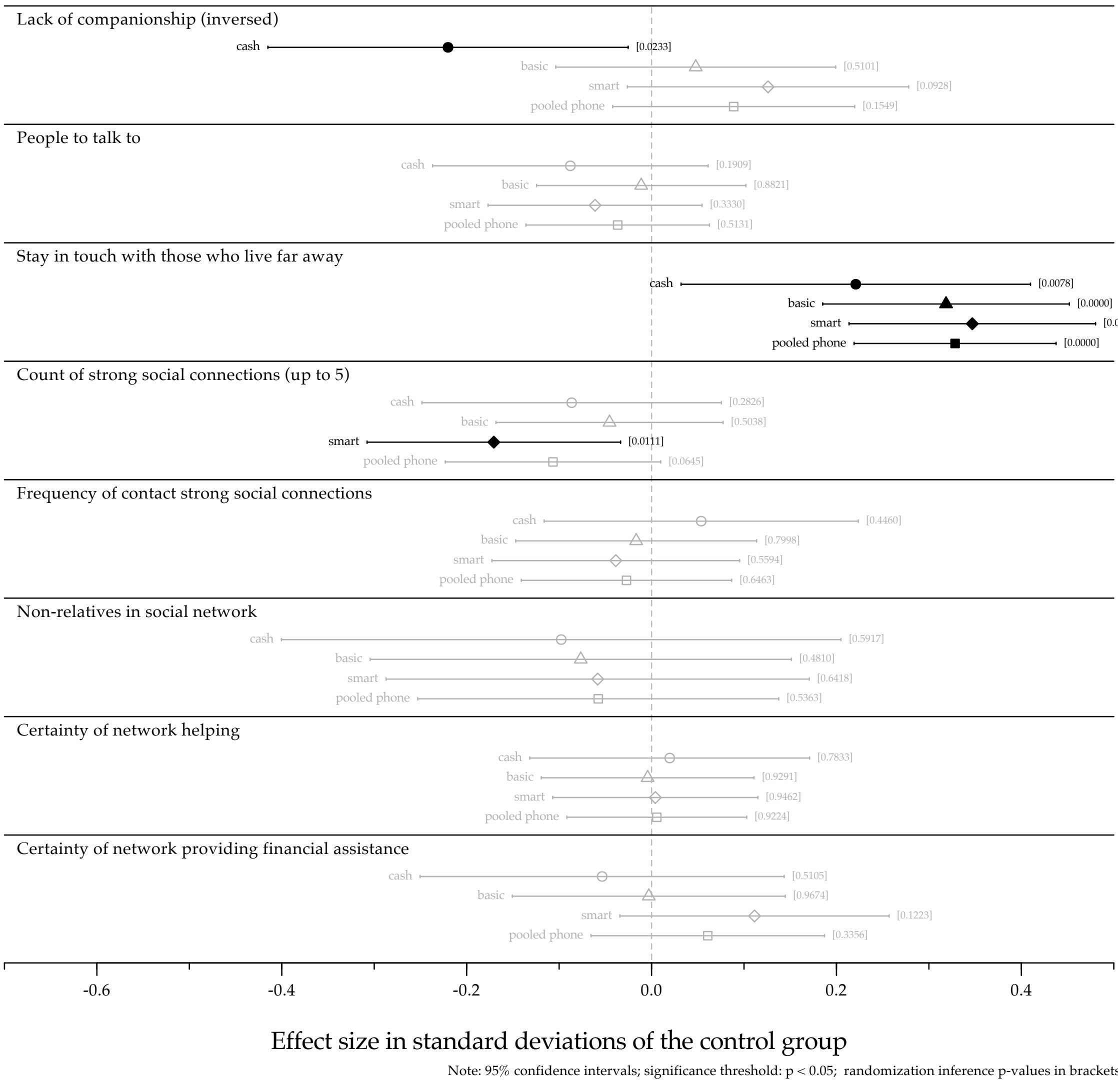

Fig. S21.4: Effects on components of social connectedness. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

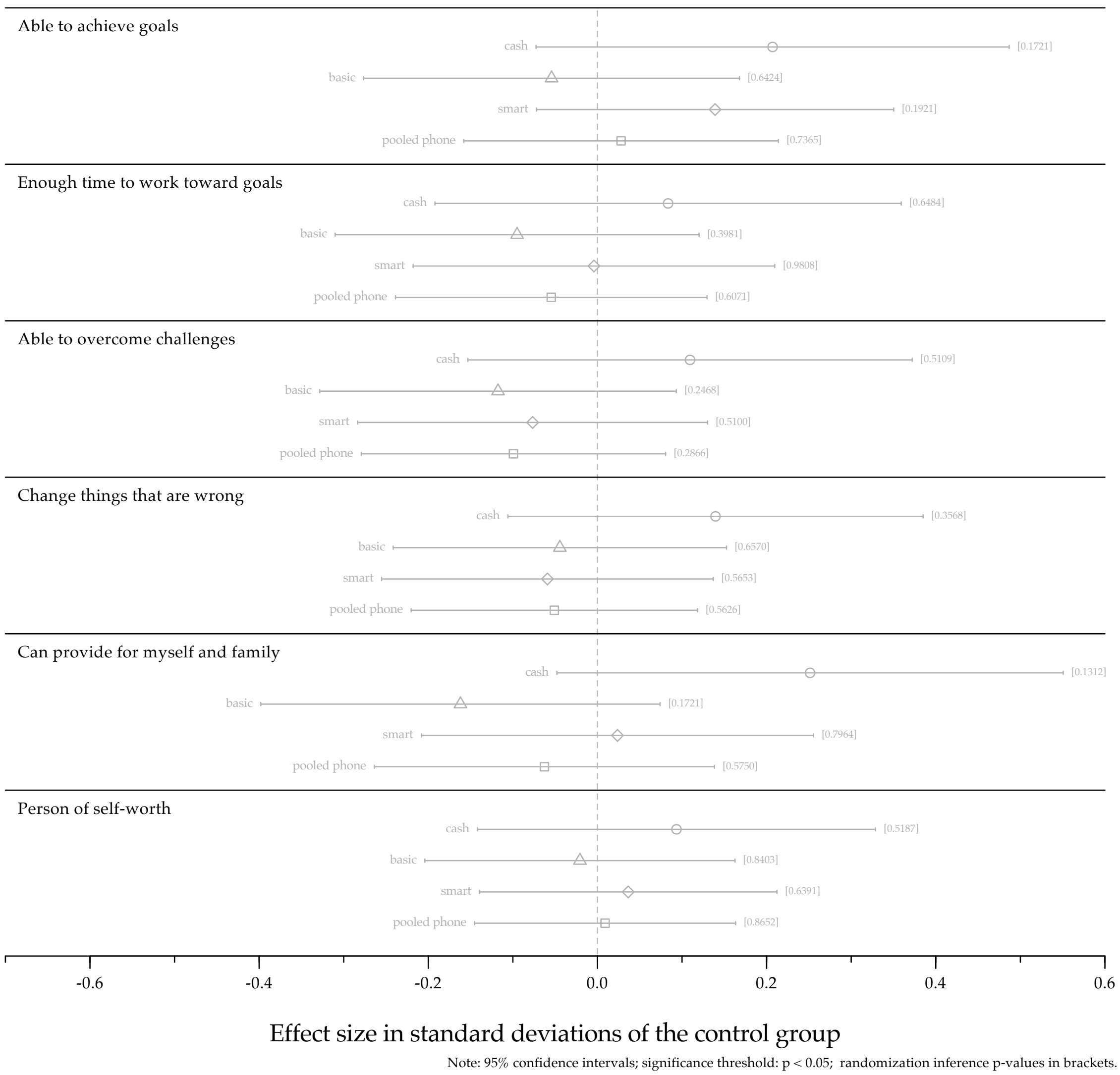

Fig. S21.5: Effects on components of individual efficacy. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

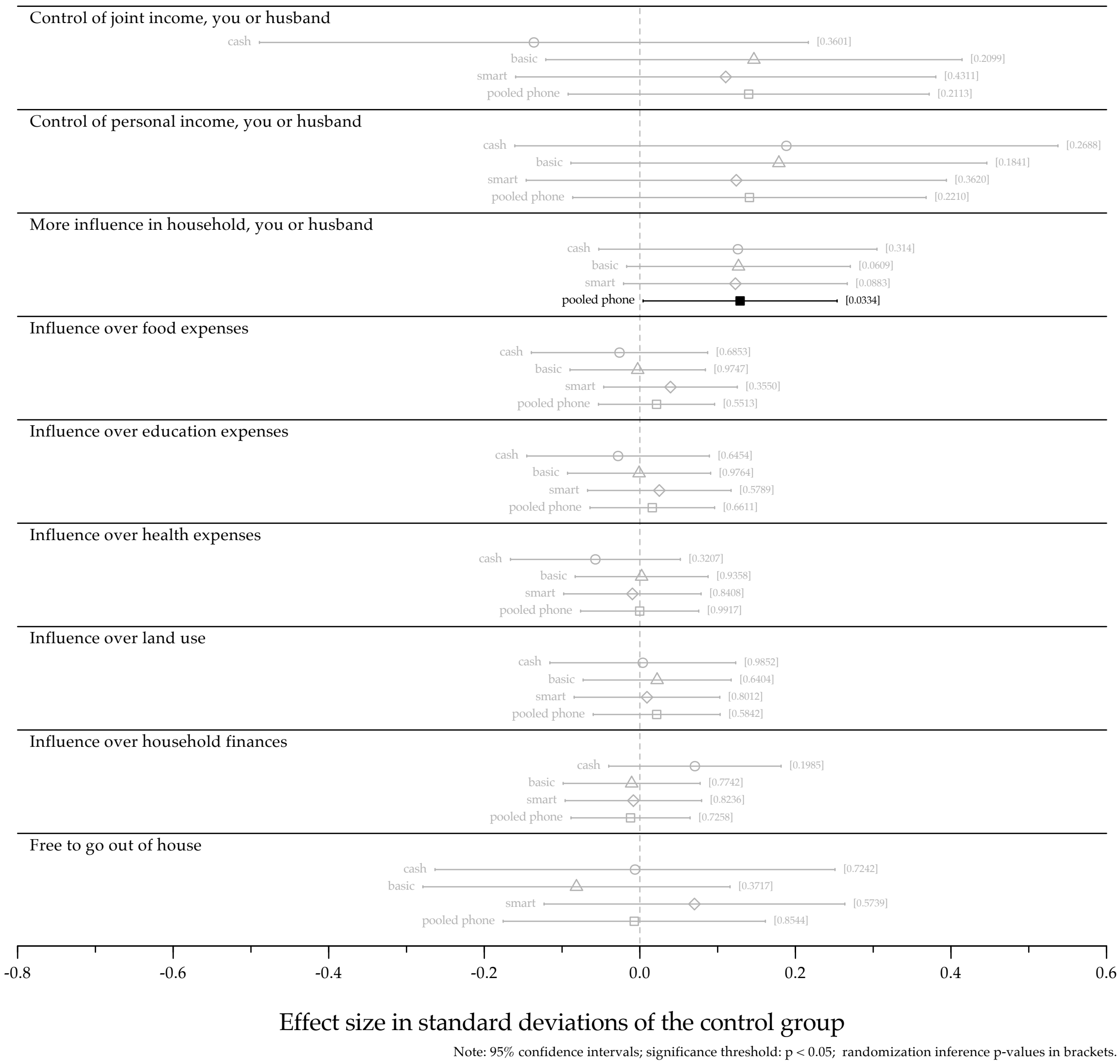

Fig. S21.6: Effects on components of household influence. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

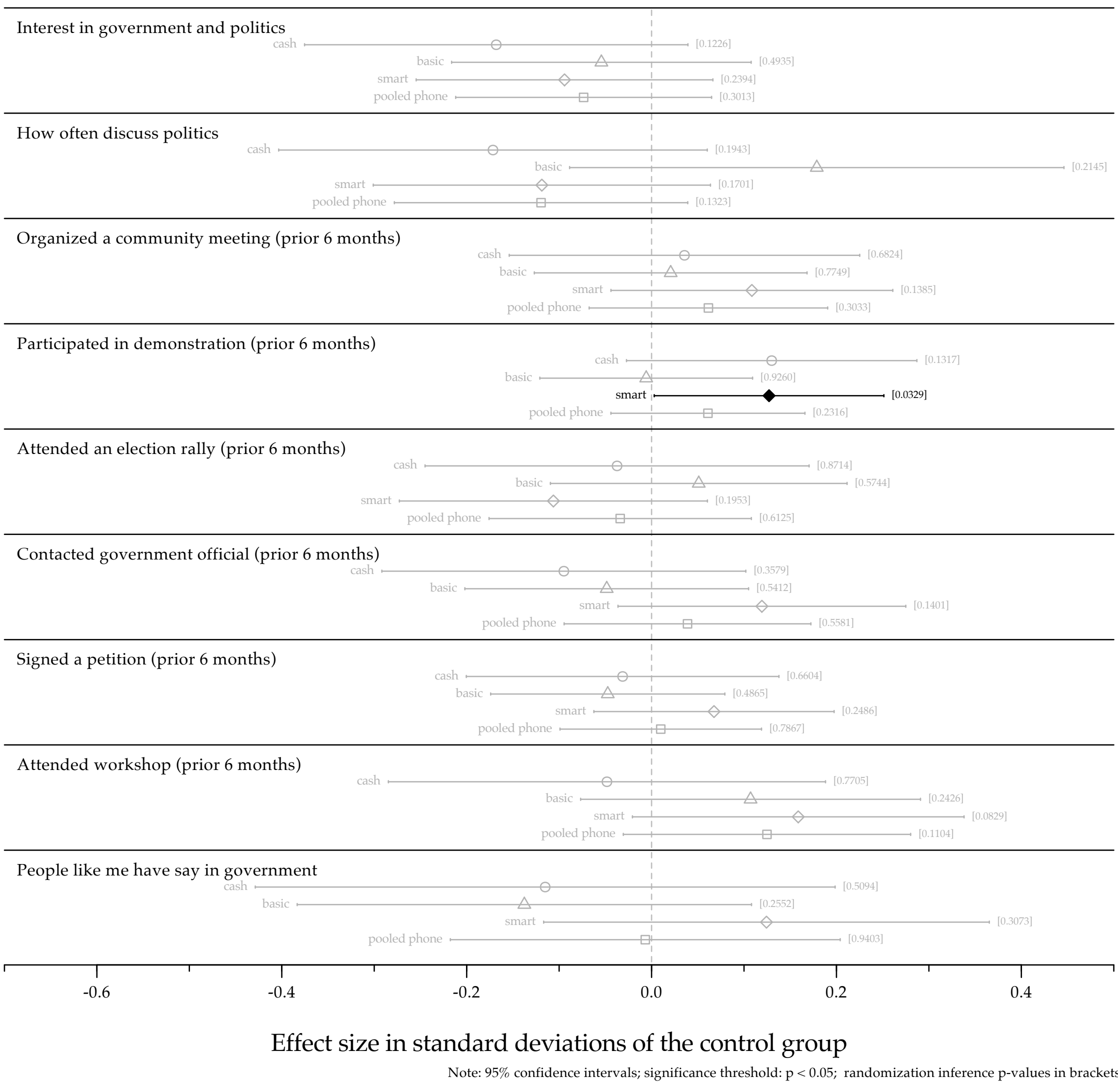

Fig. S21.7: Effects on components of political participation. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

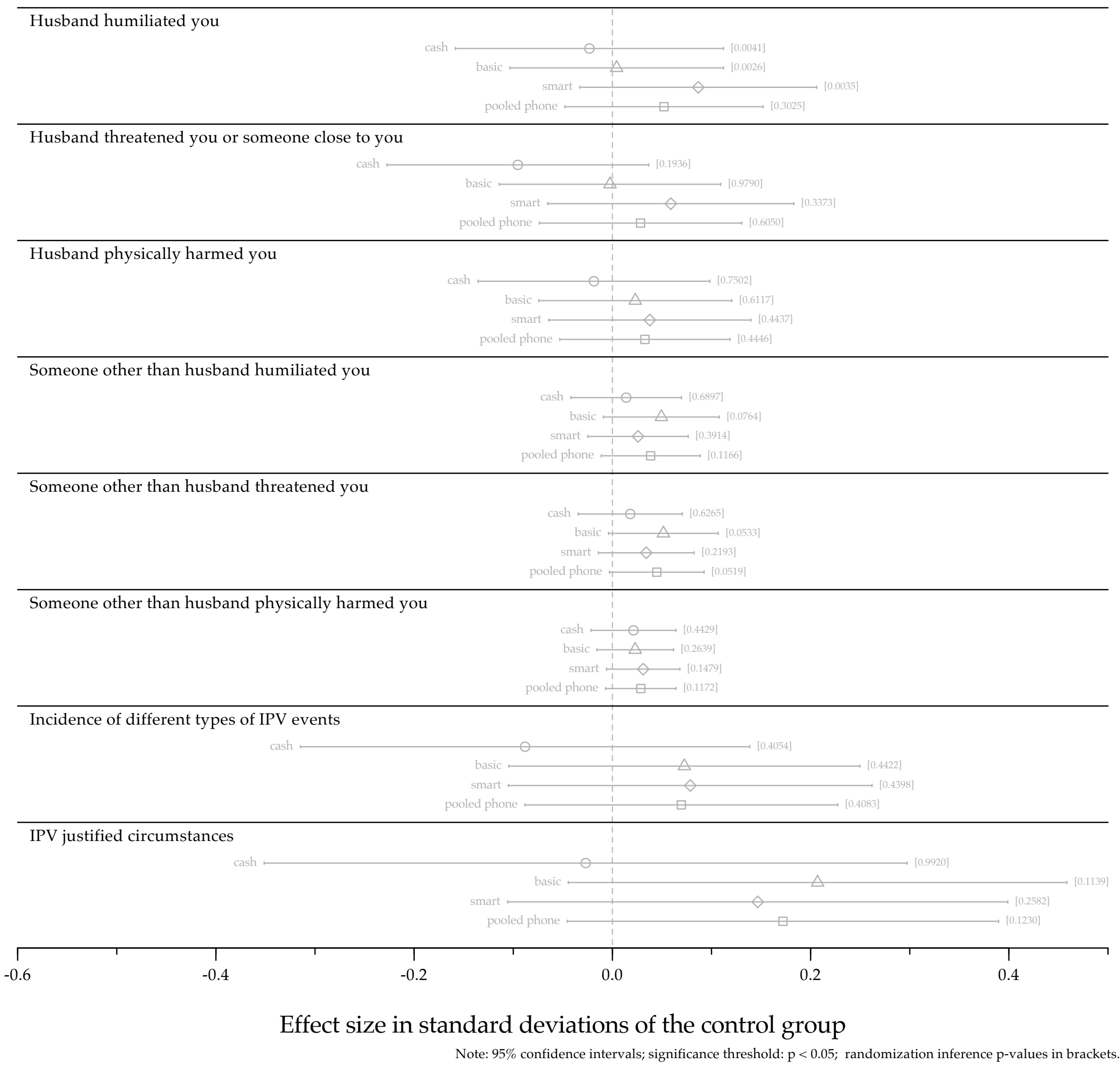

Fig. S21.8: Effects on components of intimate partner violence. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

S22 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 S22.1 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.

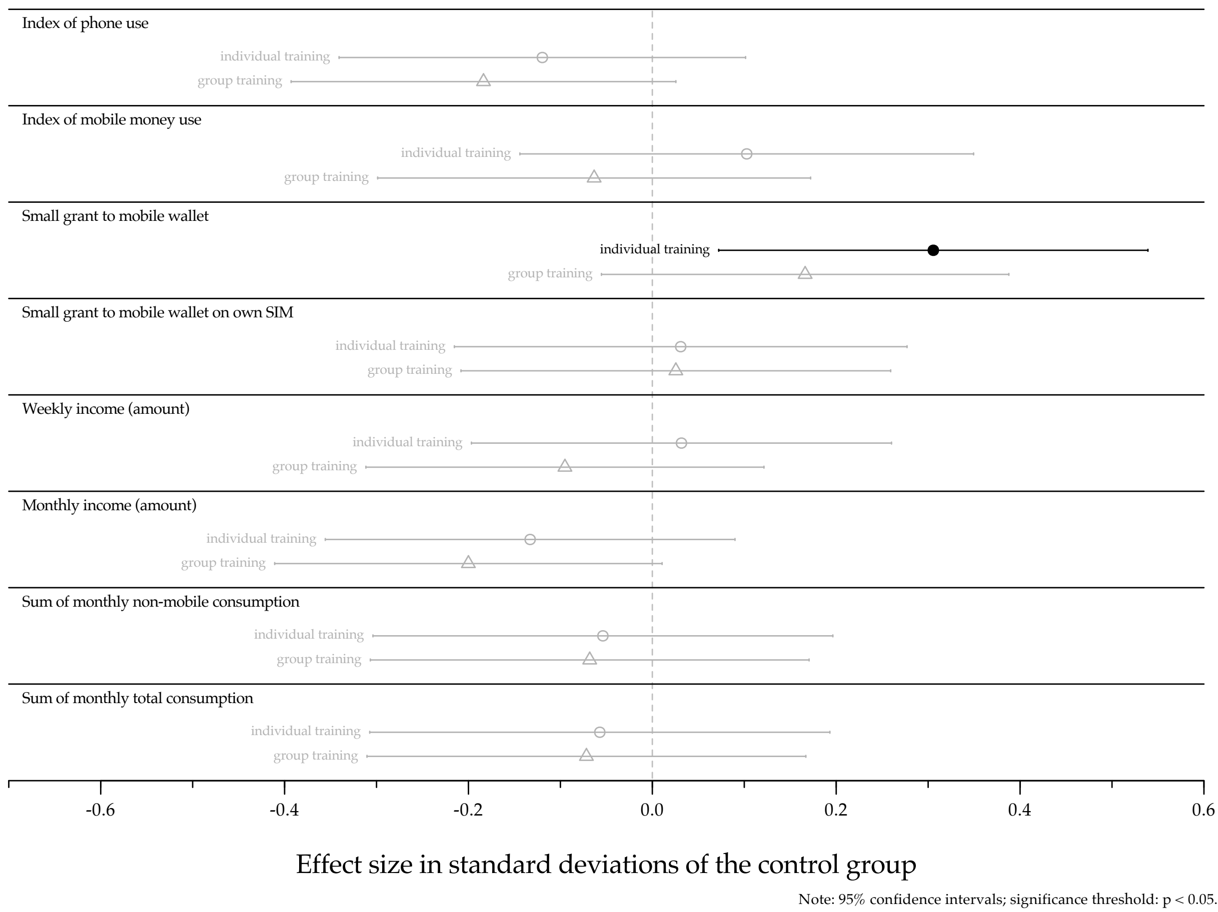

Fig. S22.1: Individual vs group training on main outcomes. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

S23 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.

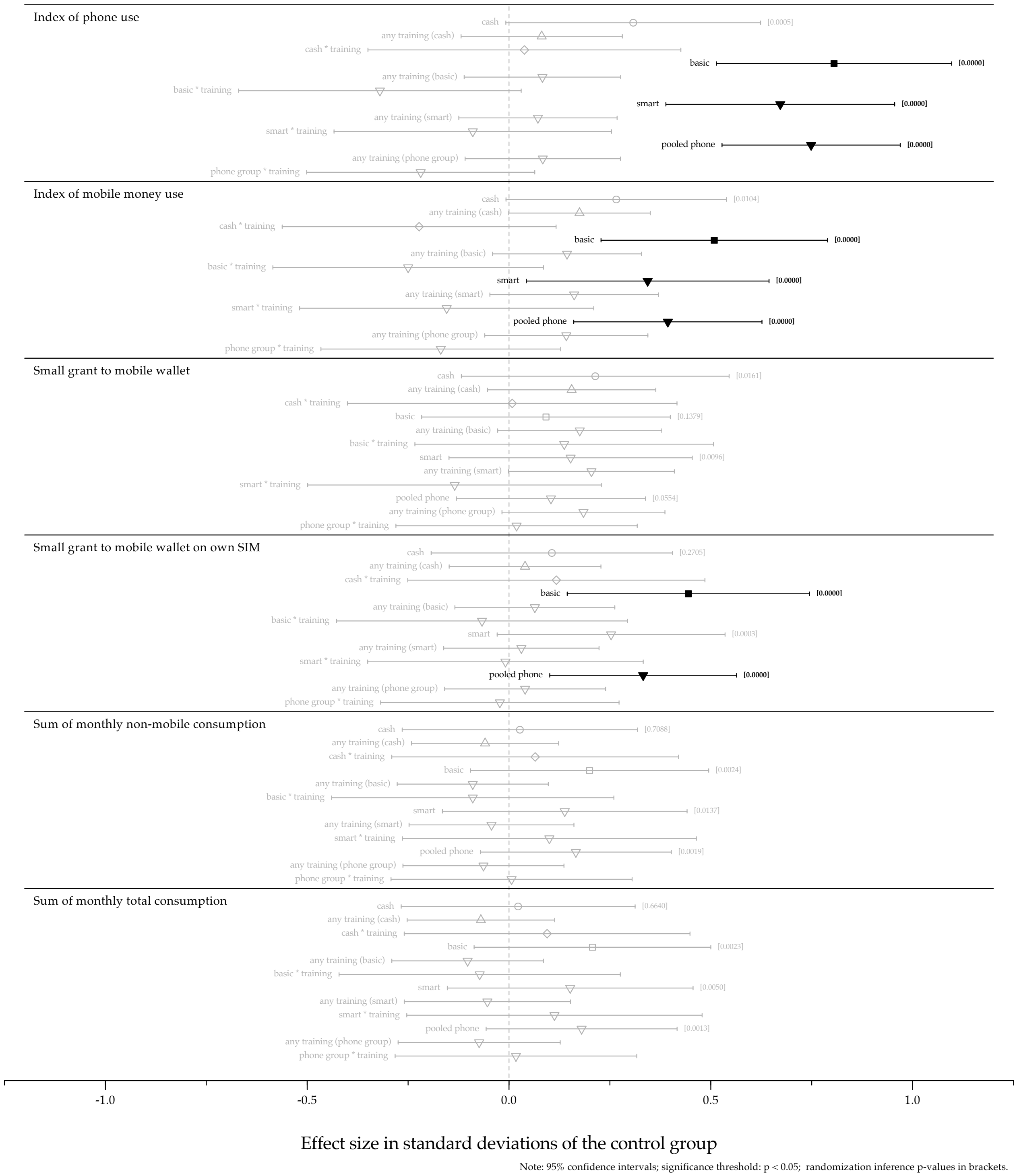

Fig. S23.1: Interaction effects of main conditions and training. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size. The components of the interaction are reported as “cash” and “any training (cash)”. The interaction as “cash*training.”

S24 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, Romero and Wüthrich, (*30*) 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.

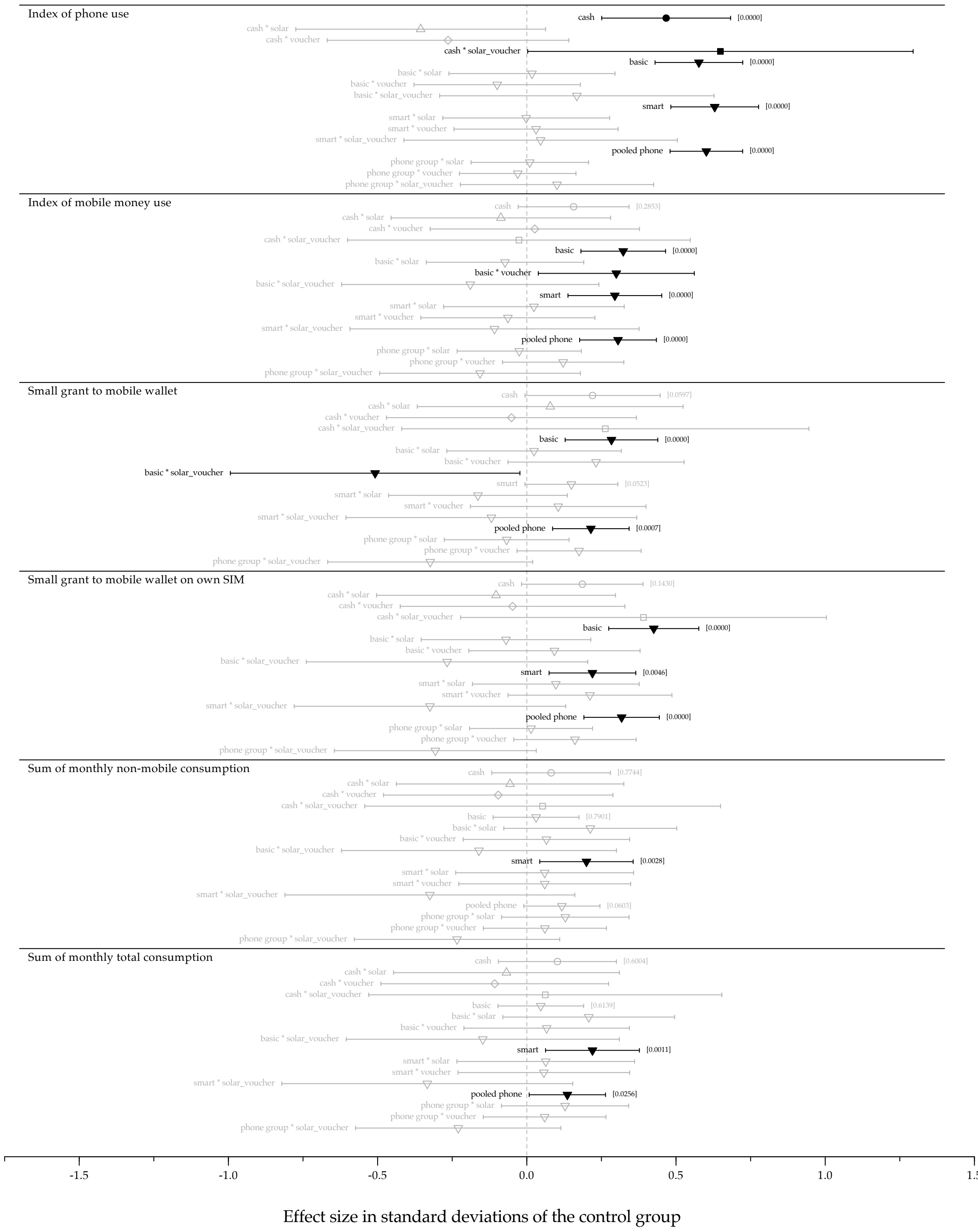

Fig. S24.1: Long-models with interaction terms of main treatment conditions. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, previous phone ownership, and household size.

S25 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 S25.1), 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 ($59))	117	0.62	0.45	0.48
Smartphone (SIM + Huawei Y3C)	274	0.79	0.37	0.37

Table S25.1: 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 S20). 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 S1.1, 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 S25.1. Other than Index of Phone Use, we see no treatment effects.

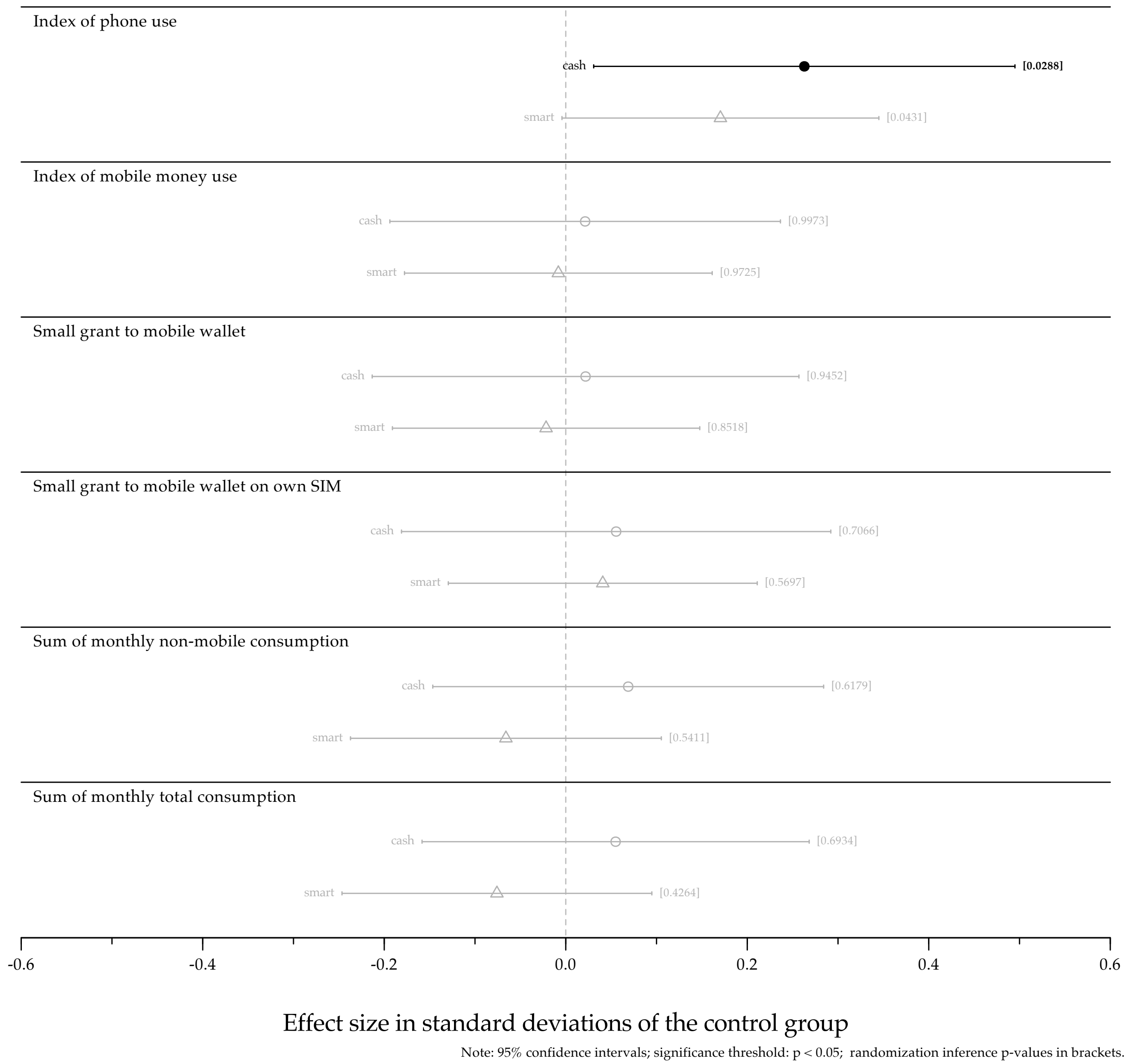

Fig. S25.1: Main results in Experiment 2 among phone owners. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, 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 S25.2.) 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.

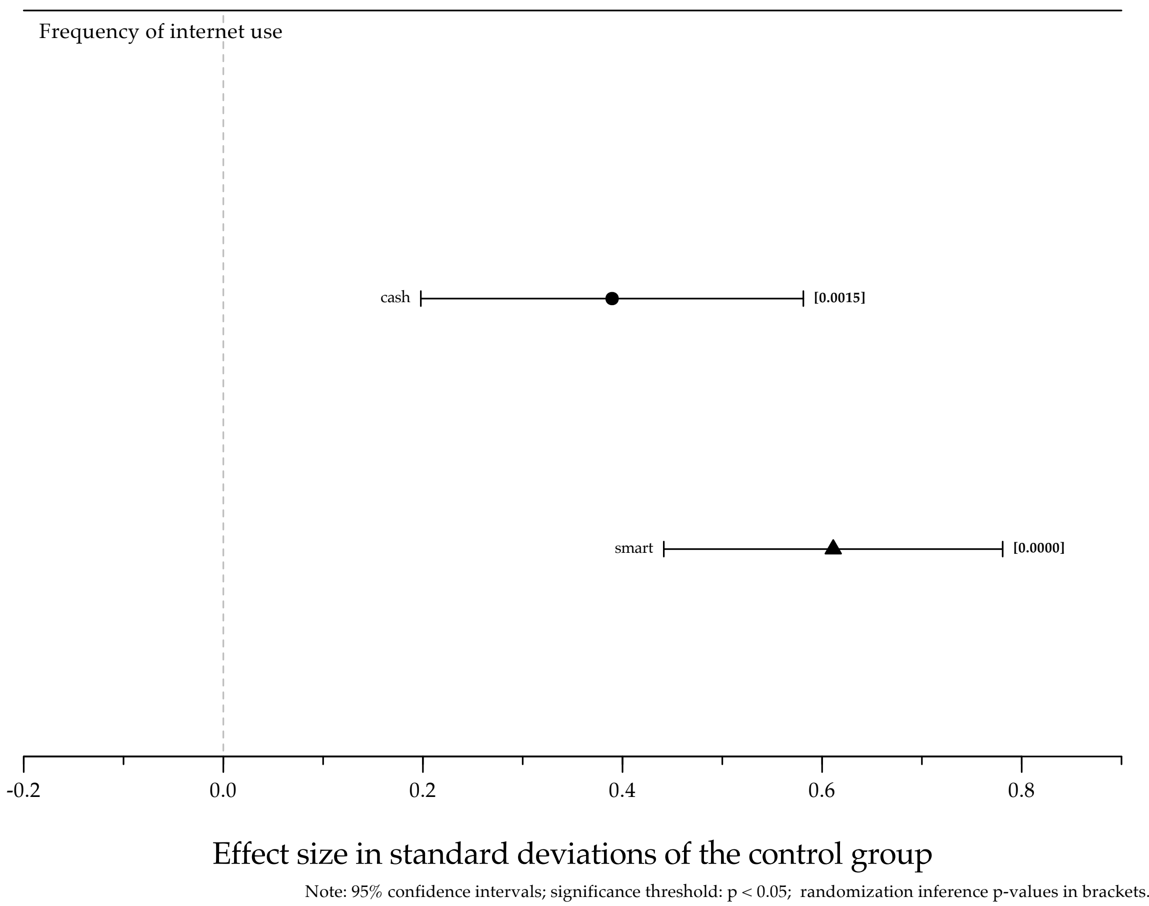

Fig. S25.2: Effects on frequency of internet use in Experiment 2. Models run using randomization inference. Specifications include our blocking strata (program membership; income; urban or rural), the solar charger and voucher cross-cutting treatment conditions, a baseline measure of the dependent variable, and baseline covariates measuring age, age squared, marriage status, level of education, and household size.