The Digital Lives of the Poor: Entertainment Traps and Information Isolation

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Smartphones have enabled the delivery of life-improving information services to base-of-the-pyramid (BOP) consumers. However, little is known about how the poor interact with the digital world. Through a novel app we developed to investigate real-time smartphone usage, we identify an unnoticed barrier to digital information access by the poor – data shortages. By analyzing over 9.4 million minutes of smartphone usage data from 929 residents of a Mumbai settlement, we find that entertainment consumes 61% of their phone time. Our data reveal that under universally adopted monthly data plans, low-income individuals binge on YouTube and social media, resulting in data shortages and information isolation in the late-plan period. We offer a practical operational solution to this problem – shorter data replenishment cycles – which serve as a commitment device to curb binge usage. We randomly assign participants to a 'capped plan' – with daily data usage caps – or a standard (monthly) plan. Assignment to the capped plan increases late-plan access of invites to health camps sent via WhatsApp, increases attendance at these in-person camps by 27%, and reduces social media binge usage. Most participants (particularly those with low self-control and high fear of missing out) prefer the capped plan, even when costlier – clearly signaling demand. Because capped plans are inherently cheaper to provide, offering them could enable providers to increase BOP customer value and expand access. Our results suggest an opportunity to amplify the impact of life-improving services targeted at the poor by leveraging users' interactions with smartphone technology.

Key words: base-of-the-pyramid value creation; mobile data replenishment; information isolation; commitment devices; field experiment

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1. Introduction

In base-of-the-pyramid (BOP) markets, digital platforms offered via mobile phones have enabled the poor to access life-improving information and services in areas as diverse as education (Aker and Mbiti 2010), financial inclusion (Acimovic et al. 2020), savings (Karlan et al. 2016), health (Kazi 2017) and agriculture (Parker et al. 2016), often via innovative business models (Lee and Tang 2018). The UN's SDGs emphasize expanding mobile phone access to alleviate poverty, and governments and aid organizations subsidize them (UN 2015, Karlsson et al. 2017). Meanwhile, policy makers in the West are debating a serious negative consequence of mobile technology – addictive entertainment usage (Alter 2017), and the US senate has recently considered a bill about placing restrictions on social media usage.[1] Yet, digital entertainment binge usage (i.e., excessive, or uncontrolled consumption) has received little attention in the poverty alleviation agenda – perhaps because little is known about the digital lives of the poor Kremer et al. (2019) highlight the need for research on how the poor use internet-enabled mobile phones.

Understanding how the poor engage with the digital world is crucial to meeting their information needs. In Africa, mobile broadband costs up to 32% of consumers' monthly average income, and in most developing countries, data costs exceed global affordability targets set by UNESCO and the International Telecommunication Union's (ITU's) broadband commission.[2] BOP consumers buy prepaid plans for a month (ITU 2017), and cannot afford to top up when data runs out. Therefore, bingeing on entertainment and later facing data stockouts – or continuing to prioritize entertainment when data is scarce – could stifle access to life improving information. Because the usefulness of real-time online information – e.g., on market prices – often diminishes quite quickly, longer data stockouts are more deleterious. If low-income smartphone users struggle to restrain their consumption of digital entertainment and therefore face late-plan data shortages, an offering that enables them to curb impulsive data overuse could be attractive to them.

In this research, we investigate the smartphone usage patterns of low-income individuals and examine how universally adopted monthly data plans affect information access.[3] To objectively measure smartphone usage, we developed an app to collect real-time smartphone usage data. Our novel approach enables access to otherwise inaccessible data, providing a unique window into the everyday lives of the poor. We used our app to operationalize an experiment we conducted in a Mumbai settlement. To our knowledge, we are the first to develop an app to collect individual smartphone time use data for research purposes.

In a randomized experiment, we examine the effects of offering a commitment device[4] to curb binge usage, in the form of a monthly duration phone plan with daily data usage caps. In this "capped" plan, data perishes and replenishes daily over the plan's duration. The shorter data replenishment cycles curb binge usage, and, analogous to the case of inventory replenishment, reduce the chances of long data stockouts. To examine how data availability impacts access to life-improving information, we randomly assigned low-income individuals to a capped or a standard data plan, and invited them to attend in-person health camps organized by us, via WhatsApp

[1] Available at `https://www.congress.gov/bill/116th-congress/senate-bill/2314/text?r=239`

[2] Available at `https://www.itu.int/en/ITU-D/Statistics/Documents/publications/prices2020/ITU_A4AI_Price_Briefing_2020.pdf`

[3] Defined as whether or not users open messages sent to them.

[4] A commitment device is an instrument that helps curb self-control problems by increasing the relative cost of undesirable choices (Bryan et al. 2010).

messages. These WhatsApp invites to our health camps serve as a metaphor for *any* mobile data-enabled information intervention. Policy makers and researchers frequently deploy such information notifications.

We uncover three key findings. 1) Our highly granular data reveal that participants randomly assigned to a standard plan binge on digital entertainment early in the plan, and show a significant drop in usage of data-consuming apps later on. 2) We find that this late-plan usage drop comes hand-in-hand with significantly lower access to life-improving information and significantly less time spent on online communication apps. 3) Participants randomly assigned to a plan with daily data caps do not face a late-plan drop in information access. This paced access to life-improving information translates into real-life behavior – health camp attendance – suggesting that such plans could expand the effectiveness of the myriad internet-enabled mobile phone-based information interventions increasingly used to reach the poor.

Naturally, the practicality of this approach would depend on the demand for such an instrument. We find that many poor smartphone users are willing to pay for the capped plan, especially those who report facing self-control problems or fear of missing out (FOMO).

In October – December 2019, we recruited a large sample ($n = 929$) of smartphone users in Mankhurd, the Mumbai neighborhood with the lowest human development index, (HDR 2010), for our experiment. About half of the sample were daily-wage workers or unemployed. We enrolled participants through telecom stores, at the point of mobile data recharge. After conducting a baseline survey, a surveyor downloaded our usage tracking app into a participant's smartphone and randomly assigned the participant to one of two equally priced data plans. Participants in the control arm received three consecutive, automatically recharged 4-week, 14GB standard phone plans – with unlimited calls and text messages and no data rollover. Those in the treatment arm received an otherwise identical plan, which imposed a 0.5GB daily data usage cap. Payment was upfront. Throughout the 12-week experiment, our app continuously tracked participants' mobile app usage. With this high-frequency data, we constructed daily individual time use and count measures for different apps and app categories, with a view to examining usage patterns.

Our data reveal that in this highly underprivileged sample, most of whom lack access to home Wi-fi or a TV, average daily smartphone use is 4.4 hours (vs. 3.1 hours in the US[5]). Users in the top decile of our sample spend 9.2 hours (about 54% of their self-reported waking day) on their smartphones. YouTube alone accounts for 20.3% of daily smartphone time use. Contacts/Phone (the inbuilt direct voice calling app) and WhatsApp account for 9.9% and 9.1%, respectively, followed by TikTok and Facebook at 4.9% and 4%, respectively. The average user visits 15 unique

[5] Available at `https://www.emarketer.com/content/us-time-spent-with-mobile-2019`

apps and checks social media 11 times a day (45 times a day in the top decile). Remarkably, while governments and development organizations view smartphones as a source of life-improving information, we find that entertainment-related apps comprise 61% of phone time use. Our subjective responses further confirm that the poor perceive their smartphones primarily as a source of entertainment (e.g., social media). Frequent (or very frequent) smartphone usage was reported by 74% of participants for entertainment purposes, by 78% for communication purposes and by only 21% for accessing information.[6] If users prefer to spend scarce data on entertainment, this could crowd out access to vital information, markets, and online platforms. Tellingly, over a third of our sample report facing data stockouts. While available, add-on data packs are expensive and only 0.7% of participants report that they purchase add-on data if their data runs out.

To examine how entertainment use affects access to life-improving information, we used an information dissemination method similar to that of interventions that governments, NGOs, and researchers commonly use. We invited each participant to four topical health camps (e.g., on dental hygiene or nutrition) held in Mankhurd, via WhatsApp. Each invitation message included a relevant health infographic, and the timing and location of the upcoming health camp. Because participants were enrolled on different dates, each message – sent simultaneously to all participants on a randomly picked date – reached participants on different days in each (4-week) data plan. We used WhatsApp's inbuilt 'read receipt' feature to measure whether participants had accessed each message, and if so, how long after it was sent.

We find that standard data plans exacerbate the information isolation of the poor. Only in the standard plan arm, we see a significant drop in the likelihood and timeliness of users accessing our information messages late in a data plan. Beyond lower access of our messages, we observed a similar significant late-plan drop in WhatsApp usage and online communication apps usage in this arm, indicative of a wider barrier to information dissemination. Importantly, the improved information dissemination unlocked by the capped plan impacted real-life behavior. Participants randomly assigned to the capped plan were 27% more likely to attend our health camps. Interventions similar to our health camps, advertised via data-using apps, are common in BOP markets. Our results indicate that standard data plans reduce the effectiveness of internet-enabled smartphone information interventiond, including many that have been tested by researchers and adopted by policy makers (see Online Appendix Table 8).[7]

[6] We leveraged Google PlayStore's app categories to classify apps and verified this categorization scheme by asking participants to report their top two apps for different purposes. Participants' stated top entertainment app was categorized by us as entertainment 97.2% of the time. Also, actual entertainment usage was higher for participants who reported frequent or very frequent entertainment usage. Details in Online Appendix Section 3.

[7] Data-enabled interventions in BOP markets are ubiquitous, and are becoming increasingly more common. Aside from the research papers cited in Table 8, there is ample evidence from the World Bank that smartphones are

To examine to what extent the poor might value the opportunity to constrain their data usage, prior to randomization we asked participants' preferences over the two plan types. Remarkably, 75% of participants prefer the capped plan, and 44% prefer it even at a 10% price premium, indicating a clear willingness to pay for data caps.

Our evidence suggests that, as in the developed world, smartphone users at the global base of the pyramid perceive mobile data as a temptation good, and are aware of their behavioral tendencies. Importantly, they exhibit positive willingness to pay to constrain their temptation towards mobile data binge usage. The unusually high demand for the commitment device we offered (see John 2020, Table 1 for benchmarks)[8] suggests that a simple operational policy intervention – making such data-saving instruments available on the market – could increase poor users' access to life-improving information services.

A key objective of telecom providers in BOP markets is to increase low-income users' internet usage and bring the 'last billion' online.[9] Our discussions with telecom executives[10] indicate that from a provider's perspective, capped plans are inherently cheaper to provide because overall bandwidth requirements shrink and unused data perishes. Capped plans would enable providers to create value for BOP consumers both by helping them curb their self control problems, and by expanding access (via cheaper prices).

2. Background

Our results contribute to three interrelated streams of literature. The first is the recent empirical and data-driven literature in operations management on improving the effectiveness and efficiency of life-improving products and services in emerging markets – such as rechargeable light bulbs (Uppari et al. 2020), online agri-platforms (Levi et al. 2020), solar panels (Kundu and Ramdas 2021), mobile money (Acimovic et al. 2020, Suri and Jack 2016), or HIV early infant diagnosis networks (Jónasson et al. 2017). Similarly, we examine an operational instrument that improves

being used for internet-enabled information dissemination. Data-consuming smartphone-based interventions ranges from educational apps to use of social media ads against malaria and youth skill training programs. As another example, the WHO used WhatsApp as a tool to disseminate information about the COVID-19 pandemic to many developing countries. Note, however, that interventions that use no data may be affected differently. Available at https://www.worldbank.org/en/events/2021/06/10/using-smartphones-to-strengthen-the-human-capital-of-online-and-offline-populations-new-evidence-and-collaborations-for- and https://www.who.int/news-room/feature-stories/detail/who-health-alert-brings-covid-19-facts-to-billions-via-whatsapp.

[8] For instance, Ashraf et al. (2006) find that 28% of low-income households in the Philippines adopt a savings account commitment device. Similar to our plan preference question with a 10% premium, 32% of Indian rickshaw drivers prefer a costly sobriety commitment device (Schilbach 2019).

[9] Available at
`www.bcg.com/publications/2020/plan-to-bring-high-speed-internet-access-to-two-billion-people`.

[10] We spoke with 14 telecom executives and managers who had experience serving poor users in Africa, the Middle East and India.

information dissemination to the poor. Unlike prior research in development economics, medicine and operations management that investigates the impact of specific mobile-phone-based information interventions (see Online Appendix Table 8), we create new knowledge on how low-income smartphone users' interaction with the digital world governs the effectiveness of such information services. Our intervention amplifies the effect of other smartphone-based interventions, by increasing poor users' access to online information, markets and platforms.

Second, we contribute to the empirical literature in operations management and behavioral economics on customer behavior in service systems (e.g., see Donohue et al. 2020). This literature has examined topics including operational transparency (Buell and Norton 2011), discrimination in online marketplaces (Cui et al. 2020, Mejia and Parker 2021), and customer behavior in queues (Aksin et al. 2020, Ülkü et al. 2020). We investigate the socioeconomic impact of designing service systems that account for the behavioral tendencies of poor users. Behavioral economists have shown that a savings account serves as a commitment device, enabling the poor to curb impulse spending (Ashraf et al. 2006). However, real-life evidence of their effectiveness – particularly on take-up and positive willingness to pay – is scant (Laibson 2015). Yet, such evidence is critical to feasibility. While financial incentives could help ameliorate the potential self-control problems that afflict low-income populations (Schilbach 2019), they are infeasible at scale. In this backdrop, our study adds an operational perspective to the research on commitment devices, by focusing on their design and delivery. Experimental evidence from scarcity theory suggests that when resources are limited, people tend to overborrow from the future, and this borrowing is counterproductive (Shah et al. 2012). In BOP markets, where data stockouts are common and top ups unaffordable, daily data usage caps prevent borrowing from tomorrow's data. We investigate the impact of such caps on digital entertainment usage and information access. Note that this unique aspect of investigating different usage purposes of mobile data differentiates our work from the extensive literature on phone tariff design (e.g., Lambrecht and Skiera 2006, Kim et al. 2010). Our findings indicate that financial liquidity constraints are not the only explanation for poor consumers' observed preference for smaller serving sizes (Prahalad 2006, Uppari et al. 2019). Self control problems are an alternative explanation.

Third, we contribute to the OM literature on managing scarce resources and inventory, by identifying a new and potentially fruitful application area for theory and model building. While recent research has examined end consumers' inventory costs (e.g., for groceries, Belavina 2021), at a conceptual level, end consumers also manage inventories of some experiential service resources - mobile data being a case in point. In this context, the user decides both which plan to buy and how it will be consumed. Thus, the inventory ordering policy could potentially affect the demand function. Also, stockouts of such experiential service resources can be costly, particularly to poor users, and cost increases with duration of stockout. Rechargeable light bulbs that are out of power leave users in darkness (Uppari et al. 2019). Data stockouts block access to market prices, riders' locations in ride hailing apps, securtiy and health alters, and other valuable information. We highlight that service delivery constraints that shorten the service replenishment cycle allow poor users with behavioral tendencies to avoid long stockout periods – by enabling 'lean consumption' (Jones and Womack 2005) in an experiential services context.

Finally, we also make two methodological contributions to empirical research. First, our approach to collecting objective app usage data is broadly applicable to other settings. Prior research that has used digital trace data obtained from phone operators (e.g., Nevo et al. 2016) lacks the contextual richness we obtain by combining surveys and app-based data. Our data collection approach is replicable and is also ethically more acceptable because we gathered data with informed consent. Also, it enables collecting data on the population of interest, not restricted to the consumer base of a particular operator.[11] Our second methodological contribution is our innovative app-based approach to running field experiments.

3. Experimental Design

Between October and December 2019, we recruited 929 urban settlement dwellers in Mankhurd, a highly underprivileged urban community in Mumbai, to participate in a 12-week experiment. Figure 1 illustrates the timeline of the experiment for participants who are randomly assigned to either arm of the experiment, on two different start dates.

Executing this study in the Mankhurd settlement required acceptance of the researchers by the local community. One of the authors spent a total of eighteen weeks in the field, for this purpose. To facilitate replication, we provide the full study protocol, including details of our smartphone app's features and variable construction details, in Online Appendix Section 1.

Recruitment and Baseline. We recruited participants amongst customers who came to one of our partner multi-brand telecom stores to buy a new prepaid plan.[12] This eliminates concerns about selection based on telecom provider or any confounding due to residual data from prior plans. Of 1,275 individuals approached, 235 rejected to join and another 111 were ineligible. (see Online Appendix Table 2 for enrollment details and Figure 16 for robustness checks on external validity). At the baseline, we asked participants' preferences over the two plans, and obtained measures including demographics, sleep, and perceived self-control. Next, the surveyor downloaded our usage tracking app into a participant's smartphone and randomly assigned each participant

[11] In the case of our Mankhurd sample, many of whom had no official address, using nighttime geolocation to determine residence location would create both selection and privacy concerns.

[12] Prepaid plans are often the only available option for the poor, who typically have no official address, proof of regular income or bank account (ITU 2017). Over 95% of Indian SIM cards are prepaid (TRAI 2020).

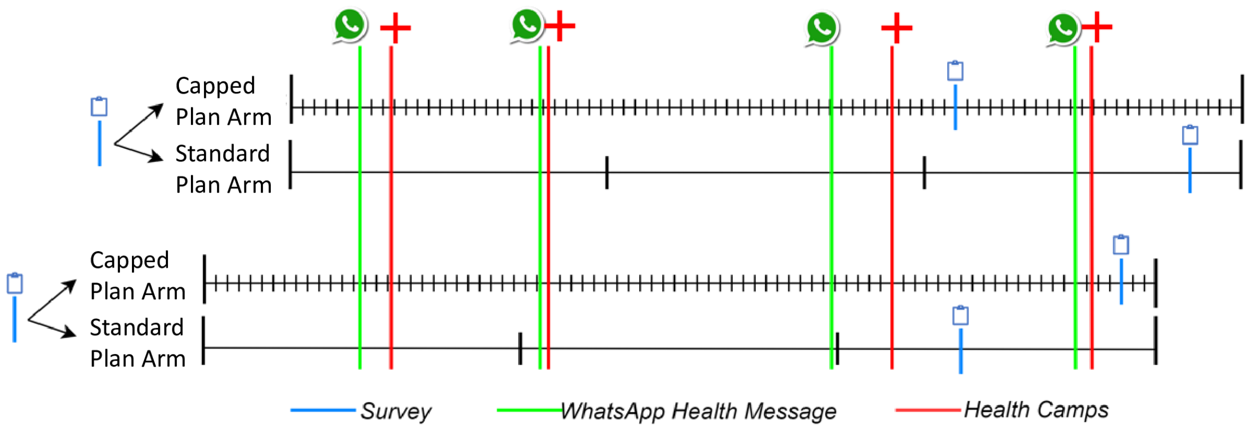

Figure 1 **12-week experimental period**

Notes: In the capped plan arm and standard plan arm, the black vertical bars separate the automatic daily and 4-weekly data replenishment cycles, respectively.

to the capped or standard plan with equal probability. We operationalized the daily data caps by buying larger-than-needed data plans, and consuming the extra data through our app (see Online Appendix section 1.2). Throughout the experiment, our app tracked each time the participant used a mobile app and sent this data to our secure servers. If a participant deleted our app we received a deletion notification.

Health Messages and Camps. To measure access to life-improving information, we sent each message recipient four unique health infographics combined with camp invitation messages on WhatsApp. We used WhatsApp because of its widespread adoption (93% of our sample had WhatsApp) and its inbuilt message read receipt function, which allows to record when a user accesses a message (Online Appendix Figure 6 contains an illustration). Smartphones are widely used for internet-enabled information dissemination. For example, Manji et al. (2021) provide a review of WhatsApp-based health systems, and Fabregas et al. (2019) describe the value of video-based interventions in agriculture.

Due to staggered enrollment, individuals enrolled on different dates would receive the same message at different points in a 4-week data plan. Ten days after each message was sent, we checked the message read receipts to measure whether participants had accessed the message, and if so, when. Camp topics were chosen with guidance from a local social enterprise, with a view to educating attendees on basic preventive self care. During the camps, which were conducted in a rented office in Mankhurd by our field staff, we shared further information on the camp topic and provided participants with a relevant kit: mosquito window nets (preventative healthcare), toothpaste and toothbrush (dental hygiene), soap and shampoo sachets (washing and bathing), and soybeans (nutrition). We designed our messages and health camps to mimic events that are run by NGOs and governments (e.g., see Kazi 2017, Dizon-Ross et al. 2017). We offered no incentive in the form of cash or snacks.

Endline. To capture temporal shifts in participants' perceptions, we conducted the endline survey in our field office or through a phone call on a randomly picked day in each participant's last 4-week plan. Measures gathered at the endline enabled supplementary analyses including heterogeneous treatment effects analysis. (See Online Appendix Section 1 for details and data collection timeline).

4. Empirical Strategy and Results

4.1. Estimation

We preregistered our experiment and committed to the construction of process and outcome variables, regression specifications and moderators in our pre-analysis plan.[13] We adopt the following regression specification to estimate the effect of random allocation to treatment (i.e., to the capped plan).

$$Y_{it} = \alpha + \beta T_i + \psi H_{it} + \tau T_i H_{it} + \gamma Z_i + \delta_t + \nu_{it} \tag{1}$$

where Y_{it} denotes the main outcome measures for individual i on day t, T_i is an indicator for the capped plan, H_{it} is an indicator for the second half of a 4-week data plan (both capped and standard), Z_i is a vector of control variables including demographics and other baseline measures as defined in our pre-analysis plan, and δ_t is a vector of date fixed effects.[14] The coefficients β and $\beta + \tau$ capture the treatment effect in the first and second half of a data plan, respectively; ψ and $\psi + \tau$ capture the late-plan usage drop in the standard and capped plan arms, respectively. Standard errors are clustered at the respondent level. Sample balance checks can be found in Online Appendix Section 2.

We examine three sets of outcome variables, each involving a different unit of analysis. First, through our usage-tracking app, we collect objective daily time use and count data for different apps and app categories, for each participant-day. To eliminate bias due to pre-enrollment usage, the first day of the first 4-week plan is excluded. Second, we measure whether or not a participant accessed a health message within ten days of our sending it, time to access the message, and health camp attendance, for each participant-message. Finally, we collect baseline and endline data on an array of measures, for each participant.

[13] Pre-registration was done in the American Economic Association RCT Registry. (Registry id AEARCTR-0004594)

[14] In the WhatsApp health message access, health camp attendance and app data usage regressions, we control for age, gender, education, an income proxy (measured as low, medium or high based on ownership of specific items – a water filter, TV, PC, motorbike and car), number of household members, an indicator for sharing the mobile phone with others, prior mobile phone ownership duration, a home wi-fi connection indicator, occupation, telecom provider indicators and date fixed effects. In the endline regressions we include all of the above controls (except for the telecom provider indicators – which we drop as they are irrelevant to the outcomes of interest), as well as surveyor, day-of-week and week-of-year fixed effects.

4.2. Evidence of an Entertainment Trap

The model free evidence in Figure 2 suggests that users in the standard plan fall prey to an entertainment trap. They binge on social media and YouTube (but not on communication apps) when data is abundant (weeks 1-2 of a 4-week plan), and later significantly reduce their time online.

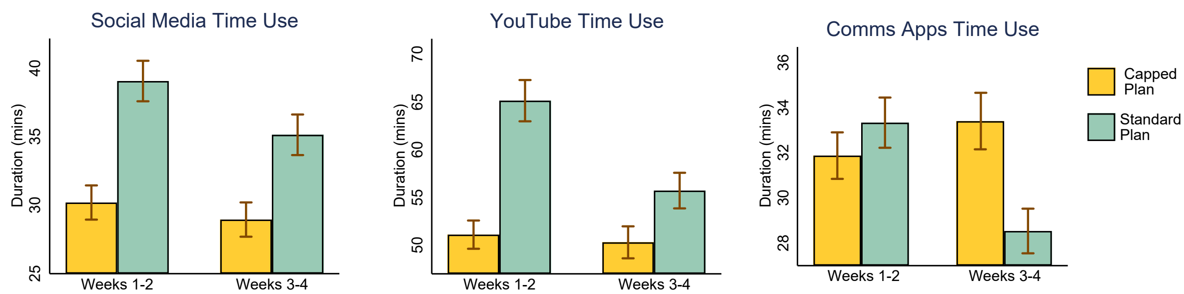

Figure 2 Participants randomly assigned to the standard plan binge on entertainment early in a plan, and later face a significant usage drop.

Notes: This panel shows usage on overall communication and major entertainment-related apps in the capped plan arm – with daily data caps – and in the standard plan – without caps. Note that the starting value and scale of the Y-axis is different for each plot.

Regression results presented in Table 1, Panel A, support the model free evidence. The estimates in Column 1 indicate that daily social media time use is 35% (10.2 minutes) higher in Weeks 1-2 of the standard plan ($p < 0.05$), and 26% higher (7.3 minutes, $p < 0.1$) even in weeks 3-4,[15] when data is more scarce, indicating prioritized usage.[16] Interestingly, in Column 2, we see that the daily-capped plan curbs – possibly compulsive – social media checking by 28% (about 3.5 fewer instances a day) in weeks 1-2, and this effect persists through weeks 3-4 ($p < 0.05$), suggesting that participants in the treatment arm are less distracted by social media. YouTube usage is almost 17% higher ($p < 0.05$) in weeks 1-2 of the standard plan, and drops by 16% in the late plan ($p < 0.01$), while it is stable and low in the treatment arm (Column 3). We observe a 6% (15 minute) late-plan drop in total daily time use in the standard plan ($p < 0.01$), whereas the daily plan smooths overall consumption (Column 4).[17,18]

[15] This coefficient is computed as the sum of β (-10.2 minutes) and τ (2.9 minutes), and the standard error is computed accordingly.

[16] For many users, there were some no-usage days during which our servers captured no data, possibly because the user's phone broke down or was not used. No-usage day occurrences do not differ systematically across the arms (Online Appendix Section 6 contains robustness checks).

[17] There are no systematic differences in app diversity, defined as the daily number of unique apps visited, over time or across the plans.

[18] Incurring data shortages may catalyze data withdrawal effects and reduce some aspects of well-being. In the endline survey, only in the standard plan arm, we observe a statistically significant increase in time impatience towards

Table 1 Effect of Daily Data Caps on Smartphone Usage

	Smartphone Usage				
	Social media time use (mins) (1)	Social media count (2)	YouTube time use (mins) (3)	Total time use (mins) (4)	App Diversity (5)
Panel A. Effects of the Capped Plan on Early vs. Late-plan					
Capped plan	-10.161**	-3.533**	-10.726**	1.885	-0.428
	(4.336)	(1.612)	(4.863)	(10.905)	(0.355)
Weeks 3-4	-3.089*	-0.552	-10.129***	-15.755***	-0.105
	(1.589)	(0.413)	(2.871)	(4.795)	(0.125)
Capped plan	2.851	0.651	8.542**	18.733***	0.101
× Weeks 3-4	(1.952)	(0.591)	(3.321)	(5.975)	(0.170)
Standard Plan Weeks 1-2					
Mean	39.061	12.585	65.181	268.18	15.519
Std. dev	[74.837]	[28.538]	[109.639]	[201.71]	[6.509]
Panel B. Main Effects of the Capped Plan					
Capped plan	-8.824**	-3.229**	-6.710	10.623	-0.381
	(4.118)	(1.497)	(4.387)	(10.785)	(0.363)
Standard Plan					
Mean	29.621	9.838	50.745	265.99	15.191
Std. dev	[59.762]	[16.999]	[74.97]	[194.383]	[6.301]
Observations	35,953	35,953	35,953	35,953	35,953

Notes: Each regression includes a vector of control variables and time fixed effects. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

If early-plan binge usage drives the late-plan usage drop, one should expect to observe a larger late-plan drop for heavy users, and little – or no – drop for light users. To investigate such distributional effects, within each trial arm we estimate a quantile regression with total time use as the dependent variable, and a binary variable indicating weeks 3-4 as the independent variable, for the 10^{th} through the 90^{th} quantiles.[19] Figure 3 reveals that the late-plan total smartphone time use drop in the standard plan is more severe in the higher quantiles of total time use. This uneven distributional effect in the standard plan arm is consistent with the late-plan usage drop being mainly driven by early-plan binge usage.

4.3. Access to Life-improving Information Services

During the experiment, we sent 3,232 health camp invitation messages via WhatsApp (4 camp announcements, each sent to 808 respondents who had non-private WhatsApp accounts – i.e.,

mobile data (measured as present bias, a binary variable indicating a higher temporal discounting at present than in the future), from early to late plan ($p < 0.05$). As a comparison, we also captured time impatience towards money, for which we see no evidence of time impatience. For standard plan users, we see a concomitant mildly significant late-plan decrease in happiness ($p < 0.1$). There is no effect on sleep, loneliness or depression. Results are in Online Appendix Table 11.

[19] The equation takes the form: $Y_{it} = \theta H_{it} + \varepsilon_{it}$. Within each arm, we run 9 regressions, one each for the 10^{th} through the 90^{th} quantile.

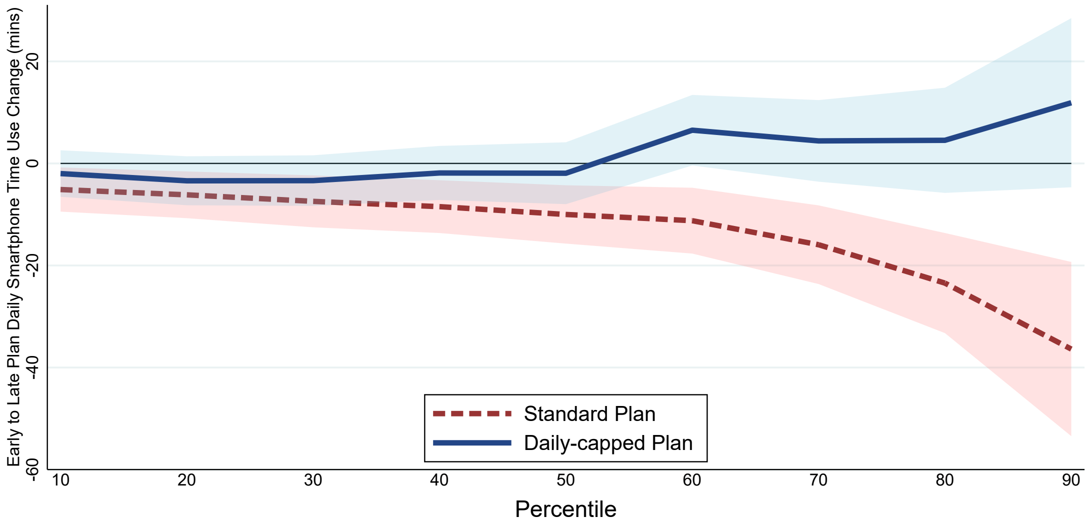

Figure 3 Quantile treatment effects on total time use show late-plan usage drop is mainly driven by a drop in binge usage

Notes: The shaded area depicts the 95% confidence intervals.

accounts that generated a read receipt), containing educational content on the camp topic. About 57% of the messages were accessed within 3 hours of being sent and 85% within 10 days. Also, 26% of the messages resulted in the recipient attending a health camp.

The model-free evidence presented in Figure 4 indicates that data shortages curtail the potential of internet-enabled information services. We see a late-plan (weeks 3-4) reduction in message access and an increase in time to access messages, in the standard plan arm, while camp attendance is lower throughout in this arm.

Regression results in Table 2, Panel A, confirm these patterns. In Column 1, the likelihood of accessing our WhatsApp information messages is statistically indistinguishable across the trial arms, in weeks 1-2 of a 4-week plan. However, for those randomly assigned to the standard plan, access drops significantly in the late-plan period (weeks 3-4) – from 89% to 80% ($p < 0.01$). In contrast, in the treatment arm, access remains stable and high, at around 88%.[20] In Column 2, conditional on being accessed, message access occurs 4.2 hours (25%) earlier in the daily-capped plan arm, largely because users in the standard plan display a 53% late-plan increase in the time time to access a message ($p < 0.01$). This result is robust to a survival model specification (column 3).

[20] Online Appendix Figures 7 and 8 present non-parametric Kaplan-Meier curves and WhatsApp message access rates for each day-in-plan, respectively.

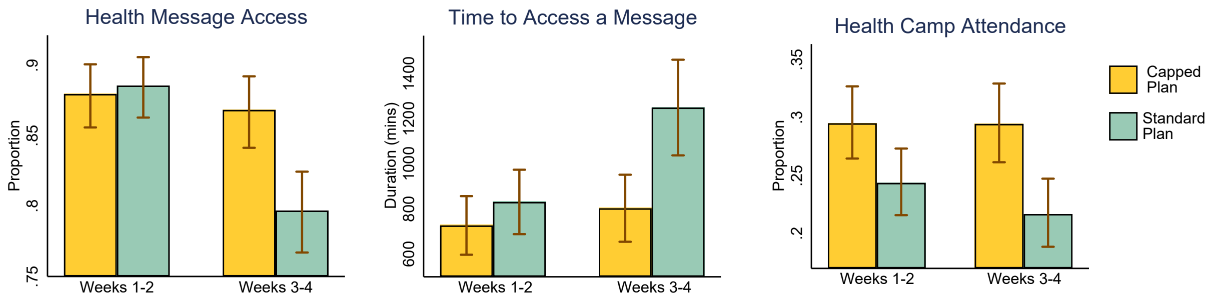

Figure 4 Information Access early vs. late in a data plan in the capped vs. standard plan arms

Notes: Bars reflect means and bands reflect 95% confidence intervals. Note that the starting value and scale of the Y-axis is different for each plot.

Expanding mobile phone access increases the number of people who *can* access any piece of information. In contrast, our results so far suggest that daily data caps can increase the number of people who actually *do*, and in a timely fashion.

Next, we examine how information access translates into real-life behavior. Because smartphone-based interventions are intended to provide access to resources – e.g., health camps, we focus on camp attendance, rather than on eventual health outcomes. Our camps were advertised exclusively via our WhatsApp messages. As only 8 camp attendances were associated with unread messages, camp attendance captures our effect of interest. Column 4 of Table 2, Panel B, highlights a 27% (6.4 percentage point) treatment effect on camp attendance ($p < 0.01$). As in Figure 4, we observe a mild early-plan treatment effect (followed by a significant late-plan treatment effect), perhaps attributable to the 1 to 5 day gap between sending health camp invitation messages and running the camps, distractions caused by online entertainment consumption,[21] or reduced memory cues due to not checking WhatsApp when data is low. We see a similar temporal treatment effect when restricting our analysis to the sample of messages which had been accessed (Table 2, Column 5).

Columns 6 and 7 in Table 2, Panel A, indicate that the late-plan drop in WhatsApp message access for standard plan users coincides with a 9% (4.5 minute) drop in daily communication apps usage ($p < 0.01$) and a 19% drop in daily WhatsApp usage ($p < 0.01$), while usage remains stable over time in the treatment arm. These results highlight a wider barrier to information access. To examine whether these observed effects were due to data shortages, we separately examined the most-used offline communication app, Contacts/Phone, which uses no data. This app's usage is stable over time for standard plan users (Column 8), indicating that the observed usage decreases are likely driven by data shortages. Interestingly, for Contacts/Phone, we see a positive treatment

[21] For instance, Mendoza et al. (2018) show that having a mobile phone nearby distracts students and worsens their performance.

Table 2 Effect of Daily Data Caps on Information Access

	Health Camp Message Access and Camp Attendance					Smartphone Usage		
	WhatsApp health camp message access	Time to access a message (mins)	Time to access a message *Survival Model*	Health camp attendance	Health camp attendance [if pre-camp msg. access]	Communi-cation apps time use (mins)	WhatsApp time use (mins)	Contacts (or Phone) time use (mins)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Effects of the Capped Plan on Early vs. Late-plan								
Capped plan	-0.009	-54.495	-0.009	0.051*	0.058*	5.471	-0.979	8.393***
	(0.019)	(110.845)	(0.053)	(0.027)	(0.030)	(4.270)	(1.815)	(2.610)
Weeks 3-4	-0.079***	415.602***	-0.284***	-0.017	0.015	-4.543***	-3.723***	-0.314
	(0.016)	(131.477)	(0.055)	(0.017)	(0.020)	(1.506)	(0.982)	(0.797)
Capped plan	0.074***	-341.049**	0.264***	0.028	0.002	7.979***	5.446***	1.020
× Weeks 3-4	(0.022)	(171.994)	(0.077)	(0.025)	(0.030)	(1.987)	(1.183)	(1.163)
Standard Plan Weeks 1-2								
Mean	0.886	833.07	-	0.244	0.285	58.425	20.987	20.587
Std. dev	[0.318]	[2043.08]	-	[0.430]	[0.452]	[78.494]	[39.596]	[46.063]
Panel B. Main Effects of the Capped Plan								
Capped plan	0.026	-204.573**	0.119***	0.064***	0.059**	9.170**	1.552	8.863***
	(0.018)	(93.129)	(0.040)	(0.024)	(0.027)	(4.294)	(1.820)	(2.635)
Standard Plan								
Mean	0.844	1019.2	-	0.231	0.287	56.139	19.180	20.514
Std. dev	[0.363]	[2368.8]	-	[0.421]	[0.452]	[75.637]	[34.934]	[45.691]
Observations	3,232	2,776	3,224	3,232	2,656	35,953	35,953	35,953

Notes: Each regression includes a vector of control variables and time fixed effects. The estimates reported in column 4 is from the Cox proportional hazard model, in which 8 messages which were accessed within a minute of being sent are dropped as their time to access was zero. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

effect starting early-plan, which might indicate an online-to-offline substitution effect driven by data caps.

Several mechanisms can explain the late-plan information access drop in the standard plan and smoothed usage in the capped plan. First, heavy data users who face self-control problems or are unaware of how intensively they consume data may face late-plan data stockouts in the standard plan arm, whereas daily caps prevents lengthy stockout periods. Consumers who are tempted to overuse data on entertainment apps could borrow data from the future in the standard plan, leaving less data allotment for both entertainment and information apps later in a plan. Second, daily caps might asymmetrically disable certain types of data-intensive entertainment usage (e.g., watching a 2 GB movie in one sitting), which results in more data allotment for information purposes. Third, entertainment usage might allow for a higher degree of temporal substitution, compared to information. For instance, accessing market price information requires daily connectivity, whereas one can watch a movie by borrowing from tomorrow's data. Fourth, standard plan users might prioritize entertainment over information usage even when they are short on data. Fifth, daily plans might enable easier management of time spent online, due to their shorter time horizon.

4.4. Robustness Checks

Our main results in Tables 1 and 2 are robust to various alternative econometric specifications, including removing the control variables; using logit specification for binary, negative binomial for count, ordered logit for ordered categorical and natural log transformation for duration variables, with weekly (rather than bi-weekly) indicators. For the right-censored measure of time to access a message, we drop messages that are never accessed. To account for the potentially unimodal shape of the hazard function (e.g., if recipients systematically wait before opening a message), we employ a log-logistic accelerated failure time model. (See Online Appendix Tables 9 to 13.) Adversarial bounds analysis (following Horowitz and Manski 2000) confirms that our results are robust to potential selection bias (see Online Appendix Figure 16).

5. Demand for Constrained Data Access

"I prefer the daily [capped] plan because every morning I wake up happy, knowing I have the internet." – *A Mankhurd resident in our study*

Prior to randomization, we elicited preferences over a lump sum monthly amount vs. an equal and equally priced amount in daily-servings of data.[22] For comparison, we conducted the same exercise for another temptation good (chips), and a non-binge-worthy good (shampoo).[23] We showed the goods in random order. Then, we raised the chosen option's price by 10%, to assess stickiness. astonishing 75% of our sample preferred daily data caps when both options were identically priced (Figure 5), 60% in the case of chips, but only 46% for shampoo.[24] Moreover, 44% of participants were willing to pay a 10% premium to incur daily data caps, a clear indicator of positive willingness to pay for a means to control their behavioral tendencies. For chips and shampoo, less respondents were willing to pay for the daily-capped option. Mobile data enables access to *both* a temptation good (entertainment) and life-improving information. The binge-worthiness of mobile data likely drives the observed positive willingness to pay for data caps. Because 44% of participants had at most 2 years of smartphone experience and 51% did not know how to check their remaining data amount, data caps could have be chosen for their insurance effect (Lambrecht and Skiera 2006). In a follow-up study, we elicited incentive compatible preferences by assigning respondents to the plans they preferred, 20% of the time. Results remain similar (see Online Appendix Figure 11 for further details).

[22] Daily-capped monthly plans with hefty daily allowances have been introduced in India. Such plans provide ample data while conserving required bandwidth and lowering price per GB, but are not easily affordable by the poor – nearly 50% of Indians do not have internet access (TRAI 2020). Monthly plans with equivalent amounts of data are not offered at all, likely due to the exorbitant bandwidth requirements.

[23] We also asked a similar question about income, which is neither a temptation good nor one where temptation is irrelevant. Income is hard to compare with the other items as it is a much more salient measure. Income was least preferred in daily portions. See Online Appendix Figure 11.

[24] The proportion preferring caps was significantly higher for data and chips than for shampoo ($p < 0.01$).

Financial liquidity constraints can make smaller package sizes attractive to BOP consumers (Prahalad 2006). That our participants preferred daily caps on data and chips despite equal pricing suggests that they wished to curb their behavioral tendencies. Consistent with this argument, low self-control predicts preferences for daily caps on data and chips consumption ($p < 0.05$), as does high FOMO (see Online Appendix Figure 12).[25]

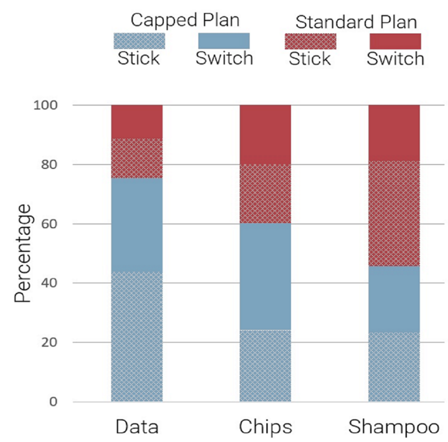

Figure 5 A majority of users prefer the capped plan, even at a higher price

Notes: Sample percentage that stick to the lump sum and daily-capped serving options, and percentage that switch, if the unchosen option's price drops.

6. Conclusion

We document binge usage of addictive digital entertainment by smartphone users at the global base of the pyramid, and show that an operational instrument that alters how mobile data is replenished – daily data usage caps – can amplify the effect of information services targeted at BOP consumers. Even when data is scarce, participants prioritize social media usage. By curbing participants' entertainment usage, data caps smooth data consumption, improve information access and impact real-life behavior. Our findings on reduced late-plan information access in the standard plan arm may help explain the fairly modest effects of prior mobile-phone-based information interventions (Fabregas et al. 2019).

Our results speak to three key stakeholders: low-income smartphone users, telecom companies, and policy makers (i.e., governments, international organizations, and NGOs). First, from a user's standpoint, we see clear evidence of demand for data caps. This finding suggests that BOP consumers value being protected against extended data stockouts, and highlights the practical relevance of offering capped plans. Second, customers' preference for these plans could increase uptake and brand loyalty, which are important metrics for telecom companies. Capping usage also helps

[25] We use standard self-reported measures of self-control and FOMO, see Online Appendix section 1.1 for further details.

telecom companies better manage network capacity, by dampening surges in aggregate cellular data traffic, and unused data perishes faster in a capped plan. Thus, offering such plans is attractive from a cost-benefit standpoint. Third, from a policy perspective, internet-enabled smartphone information interventions are a common policy tool. Furthermore, some developing country governments already subsidize mobile data plans (Karlsson et al. 2017). Beyond subsidizing smartphones and data plans, governments could subsidize capped data plans, as a non-obvious path to amplifying the impact of digitally-led BOP services. The benefits of capped plans should scale with more widespread dissemination of information to underprivileged communities.

Our innovative app-based data collection method enables to access new and rich data sources of objective digital trace data. Our app-based approach also provides unique flexibility to experimentally investigate behavior on mobile app platforms.

This study has three main limitations. Because we do not observe data consumption, we are unable to distinguish data shortages from data stockouts. Second, we are unable to detect within app changes in behavior, e.g., prioritization of texting over video calls on WhatsApp. Third, a dedicated research project investigating the underlying mechanisms discussed in section 4.3 would further extends knowledge, but is beyond the scope of this paper.

Our results indicate a broader opportunity for leveraging operational strategies to amplify the value of smartphones in alleviating poverty by enabling users to better manage their day-to-day interactions with mobile technology. Daily data caps are only one of many possible solutions. Future research could investigate other data delivery options, incentives, interface design features, and use of data analytics to design personalized data plans, e.g., with app-specific usage limits. With universal internet coverage within sight (UN 2015), our findings have immediate, far-reaching implications for how much the global BOP consumers can eventually benefit from the mobile phone technology.

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The Digital Lives of the Poor:

Entertainment Traps and Information Isolation

(Kamalini Ramdas and Alp Sungu)

Online Appendix

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1 Materials and Methods

1.1 Outcome Variables

We have three avenues for data collection: WhatsApp health messages and health camp attendance, our usage-tracking app, and baseline and endline surveys. We provide further details on our survey-based outcome variables.

Time impatience (Data and Money). Assignment to the daily-capped plan could plausibly affect participants' degree of temptation towards immediate mobile data reward. To measure time impatience towards mobile data, we use the a standard present bias test. We first asked participants to choose between receiving 1GB today or varying higher amounts in 5 days, and next between the same pairs of options 30 days vs. 35 days from now. We then repeated these questions using monetary rewards (using 1GB = Rs. 100). Participants were identified as time impatient if they displayed more patience in distant trade-offs than in current trade-offs. Individuals who crave instant gratification are likely to display time impatience, i.e., to be less patient towards delayed rewards in the present state, than in a future state. For further details, and theoretical foundations, see O'Donoghue and Rabin (2015) and the references therein.

Preference for Capped Plans. At the baseline (prior to randomization) and the endline, we elicited participants' preferences over the standard plan (14GB for 28 days) and the daily-capped plan (the same plan with daily 0.5GB usage caps) under identical pricing and payment scheme. We then increased the price of the chosen plan by 10%, to assess stickiness. At the baseline, we also elicited participants' preferences over monthly vs. daily serving sizes for chips, shampoo and income. To minimize inter-question spillover effects, we randomized the question order.

Self-control and FOMO. To elicit the behavioral mechanisms underlying plan choice, we included a 13-question perceived self-control test (Tangney et al., 2004) at the baseline. The measure we adopted is referred in the psychology literature as the Brief Self-Control Scale (BSCS), which includes a set of questions about participants' perceptions on their own self-control. This measure is widely used, and has been shown to predict indulgence behaviors including food bingeing and alcohol consumption (Tangney et al., 2004). Also, at the endline, we measured long-term fear of missing out (FOMO), as in Przybylski et al. (2013). FOMO is defined as "a worried feeling that you may miss exciting events that other people are going to, especially caused by things you see on social media".[1] The FOMO test we adopted consists of 11 questions on self-reported fear of missing out. For the full list of questions see Przybylski et al. (2013).

Sleep and Subjective Well-Being. At baseline and endline, we measured sleep quality using PSQI (Buysse et al., 1989) and sleep disorders using ISI scores (Bastien et al., 2001). The subjective PSQI sleep score consists of seven components: quality, latency, duration and habitual efficiency of sleep, daytime dysfunction, use of sleep medication and sleep disturbances. We adopted a version of this survey instrument that excluded the sleep medication and sleep disturbances components. A shortened PSQI measure has been used in a recent research study on sleep quality in an urban low-income Indian population in Chennai (Bessone et al., 2021). The authors report a strong correlation between PSQI-based self-reported sleep quality and objective actigraph measurements. We measured self-reported subjective well-being through measures of happiness, loneliness and depression (Kahneman and Krueger, 2006).

1.2 Flow of the Experiment and Data

The experiment consists of 7 parts: enrollment, baseline survey, 12-week mobile data plan period, WhatsApp health messages during the 12-week period, health camps during the 12-week period, endline survey, and lottery event.

Enrollment Through Partner Telecom Stores. The nature of our research involves recharging

[1] Available at `https://dictionary.cambridge.org/us/dictionary/english/fomo`

Table 1: Data Collection Timeline

Variables	Baseline Survey	12-week Experiment Period	Endline Survey[a]
Preference for Capped Plans	✓		✓
Self-Control	✓		
Time impatience (Data and Money)			✓
Smartphone App Time Use		✓	
WhatsApp Health Messages		✓	
Health Camp Attendance		✓	
Sleep (PSQI and ISI)	✓		✓
Subjective Well-Being			✓
Fear of Missing Out (FOMO)			✓
Stated Outcomes Related to Smartphone Usage	✓		✓

[a] Endline surveys were conducted on a random day in the last 4-week plan of the 12-week experiment period.

data plans. In order to ensure that participants have a new data plan at the start of the experiment, we partner with local telecom stores. Recruiting participants through telecom stores allows us to capture individuals at the point of recharge. A partner telecom store provides various mobile-phone-related services, including recharging data plans, selling phone cases and phone maintenance. It is important to highlight that these stores are not associated with a particular telecommunication service provider and provides recharges for various providers. A research assistant waits at a partner telecom store, and invites customers who come to recharge their mobile data plans, to learn about the experiment. Among the customers who are willing to join the experiment, we exclude dual-sim phone and IOS operating system users.

Baseline Data Collection and Randomization. Once a participant agrees to join the experiment, we conduct a baseline survey. During the baseline survey we collect main process and outcome variables. If an individual rejects to join the experiment, we ask why they reject to join without asking any further questions (Table 2). One key measure we collect is a choice question between standard and capped data plans: we ask participants to choose between the intervention schemes we offer. Once we elicit their choices and finish the baseline survey, we randomly allocate participants to one of the two plans, with equal probabilities. Upon completion of the baseline data collection, we download our smartphone usage-tracking app into the participant's phone.

12-week Mobile Data Plan Period and Usage-Tracking App. Our two randomized intervention schemes are data plans with different flexibility levels– data replenishment cycles. Participants in the standard plan gets 14GB of data for 28 days along with unlimited calls and text messages. At the end of the 4-week plans, unused data perishes. Participants in this arm will receive 3 of these 4-week data plans. Participants in the capped plan get 0.5GB of data per day along with unlimited calls and text messages. At the end of each day, unused data perishes. Participants in this arm will receive 84 of these 1-day data plans. Throughout the experiment, all data plan recharges are done automatically. We price both standard and capped data plans equally in order to eliminate financial liquidity effects in the participants' choices and in their enrollment into the study. We also provide a modest incentive of Rs. 40 for joining the experiment.

During this 12-week data collection period, we collect data on smartphone usage of participants through the smartphone application software (i.e., an app) that we developed and installed into participants' devices. We chose a name familiar to participants, MANKHURD (name of the urban settlement in which we conducted the experiment), for our app. A 5-digit user id is asked for only once, when the app is installed. A research assistant enters a predetermined user id, which activates the app. The MANKHURD app has four key features. First, it collects second-by-second app level time use data, which allows us to construct daily time use information. From the same data we also construct two types of daily count information: app diversity, i.e., unique number of apps used each day, and social count – number times that a participant opens a social media application. Second, every 10 hours, our app wakes itself up, shares this data with the server and also stores it in the internal memory of the device. If the data has not been shared with the server due to network problems, an attempt is made to share it the next time the app wakes itself up. This allow us to capture participants' full interaction with their smartphone, including usages that occur when there is no internet connection. This process is done automatically in the background and is not visible to the participants. There is also a sync button in the app. Clicking this button immediately shares all the internally stored data with the server. We sync all the devices during the endline visit. Third, in order to ensure that everyone gets data plans according to the intervention arm they belong to, we include a data eat-up feature. This feature allows us to tailor data plans of arbitrary size by buying enough data (e.g., buying a 600MB daily data plan and burning up 100MB of it). This process is done overnight, only when the phone is under gsm connectivity. It is done in the background, and thus, is not visible to participants. Fourth, if a participant deletes the MANKHURD app, it sends a termination notification with the participant's user id to the server. Therefore, we know which participants delete our app.

WhatsApp Health Messages. We used WhatsApp's inbuilt 'read receipt' feature to measure whether participants had accessed the message, and if so, when (see Figure 6). Each message was a topical 3MB-sized JPEG educative infographic with time and location information for an upcoming health camp (see Figure 6). Each message was sent simultaneously to all participants who had non-private WhatsApp accounts, on a random day 5 or fewer days prior to the camp date. Thus, individuals enrolled into the experiment on different dates would receive the same message at different points in a 4-week plan. We go back to each message 10 days after we send it and record the information on WhatsApp read receipt. We sent each message to all intended recipients simultaneously. As our needs exceeded the 256 contacts limit for WhatsApp broadcasting,[2] we individually sent messages to all recipients offline (i.e., airplane mode) and

[2]Details can be found at `https://faq.whatsapp.com/en/30046788/`.

then went online at the planned message send time. An important thing to note is that a message access time appears only if the participant opens the chat page. However, WhatsApp text messages can be read on lock-screen without going to the chat page. Thus, a participant may receive the WhatsApp text information without generating a message access time. To prevent this from happening, we send all the messages in a picture format, which can be read only via opening the chat page. We send our health information messages over WhatsApp. WhatsApp is the most commonly used communication app in India[3], and about 93% of our participants had a WhatsApp account (balanced across the two trial arms, please see Online Appendix Table 4). We were unable to send our messages to those who did not use WhatsApp. We use the 'read receipt' feature (i.e., the blue tick in WhatsApp, see Online Appendix Figure 8 for an example) to measure access of our health information messages. Users can change the privacy setting in WhatsApp to disable the read receipt function. This feature disables message senders (i.e., us) from observing the read receipt.[4] About 5% of our WhatsApp users had private accounts (balanced across the two arms, see Online Appendix Table 4). This leaves us with 3,232 messages sent to 808 participants, in our analysis.

Topical Health Camps. Camp topics were chosen with guidance from a local social enterprise, with a view to educating attendees on basic preventive self care. During the camps, which were conducted in a rented office in Mankhurd by our field staff, we shared topical information and provided participants with an unannounced relevant kit worth under $0.5: mosquito window net (preventative healthcare), toothpaste and toothbrush (dental hygiene), soap and shampoo sachets (washing and bathing), and soybeans (nutrition).

Endline Data Collection. We conduct the endline survey data collection on a random day in the last 28-day plan cycle of a participant's experiment period. If a participant is not available to administer the endline survey on the assigned date, we either schedule another date within the same week or after the completion of the 84-day experiment period. We collect this data in our office or through phone calls.

Lottery Event. In order to compensate participants for a potential inconvenience of installing our app, and responding to the endline survey, we conduct a lottery event two weeks after the date of completion of the last enrolled participant's 84-day experiment period. In the lottery

[3]Country-level most common communication app usage statistics are available at `https://www.similarweb.com/corp/blog/research/market-research/worldwide-messaging-apps/`

[4]Further details are available at this link: `https://faq.whatsapp.com/android/security-and-privacy/how-to-check-read-receipts`

event, we provide various gifts: water jug, iron, clock, trolley bag, pressure cooker, kettle, mobile phone and a laptop. Every participant, upon successful completion of the experiment, receives a unique lottery id to be used in the lottery event. On the day of the lottery event, we ask partner telecom store managers to draw lottery ids from a box and provide gifts to winning participants.

2 Enrollment and Sample Balance Checks

Table 2: Baseline Sample Selection, Eligibility, and Response at the Endline

STAGE	Number of Individuals
I. Baseline Survey Screening	N = 1,275
Interested in the study	1,040
Preferred a different phone plan (includes all non-smartphone users)	169
Not interested in the next stage (did not state a reason)	33
Not willing to conduct survey	21
Did not want to download usage-tracking app	12
II. Eligibility Checks	N = 1,040
Eligible to join the experiment	929
Dual SIM phone users	49
Did not have a phone that supported the usage-tracking app	39
Younger than 18 years old	22
III. Enrolled in the Experiment	N = 929
IV. Endline Survey Responses	N = 881
Endline in office	574
Endline via phone call	307
Conducted endline during last 4-week plan	824

Notes: This table shows the enrollment process and response rate for the endline survey.

- Stage I provides a breakdown of the 1,275 participants screened. Of these, 235 participants were not interested in joining the experiment. Most individuals who preferred a different phone plan had come to recharge a plan that did not include data (i.e., it included only calls and text messages), and a few to top up a data plan. This group includes all individuals who use non-smart mobile phones.
- Stage II provides a breakdown of the 1,040 participants who underwent eligibility checks. Among these, there were 111 ineligible participants. Dual SIM phone users were excluded to prevent spillovers from usage via a participant's second SIM card. Our usage-tracking app runs on Android version 5.1 and above – this covered 89.6% of the Indian Android market in October 2019 (GSCounter, 2020). Only 39 participants were excluded due to not having a smartphone that supported our usage tracking app. We did not observe any IOS users during the enrollment period.
- In stage III, 929 participants were enrolled into the experiment.
- Stage IV provides a breakdown of endline survey responses. We attempted to conduct the endline surveys on participant's last 4-week plan. If a participant was unavailable on the assigned date, we made two more attempts, one within the same week and the other after completion of the participant's 12-week experimental period.

Table 3: Balance Across the Trial Arms (Variables Used in the Analysis)

	Standard Plan Arm (1)	Capped Plan Arm (2)	Balance test difference [p-value] (1) - (2)
Age	28.172	27.253	-0.919
	(8.744)	(8.509)	[0.105]
Age squared	869.965	814.949	-55.016
	(597.710)	(562.258)	[0.150]
Female	0.199	0.208	0.009
	(0.400)	(0.406)	[0.737]
Marital status	1.469	1.508	0.039
	(0.512)	(0.514)	[0.248]
Number of children	2.676	2.408	-0.268*
	(1.449)	(1.319)	[0.055]
Number of people living in the same house	6.079	5.772	-0.307
	(3.215)	(2.875)	[0.126]
Interstate migrant	0.647	0.673	0.026
	(0.478)	(0.470)	[0.402]
Self control	3.161	3.154	-0.007
	(0.514)	(0.452)	[0.832]
FOMO	1.237	1.270	0.033
	(0.577)	(0.559)	[0.386]
Baseline time-inconsistent preferences (Money)	0.257	0.248	-0.009
	(0.438)	(0.433)	[0.754]
Baseline sleep (PSQI)	-3.442	-3.421	0.021
	(2.106)	(2.160)	[0.879]
Baseline sleep duration	7.234	7.189	-0.045
	(1.550)	(1.541)	[0.658]
Baseline sleep insomnia (ISI)	-7.515	-7.526	-0.011
	(3.857)	(3.773)	[0.964]
Last time check phone before sleep (mins)	15.105	14.461	-0.644
	(15.420)	(12.036)	[0.494]
First time check phone after wake up (mins)	27.643	25.950	-1.693
	(32.309)	(28.750)	[0.416]
Preference for data capped plan	0.778	0.729	-0.049*
	(0.416)	(0.445)	[0.085]
Preference for chips capped plan	0.600	0.604	0.004
	(0.490)	(0.490)	[0.890]
Preference for shampoo capped plan	0.459	0.459	0.000
	(0.499)	(0.499)	[0.997]
Preference for income capped plan	0.348	0.307	-0.041
	(0.477)	(0.462)	[0.187]
Owns Wifi at home	0.164	0.186	0.022
	(0.371)	(0.389)	[0.383]
Baseline finish data plan	7.384	6.582	-0.802
	(25.270)	(23.922)	[0.620]
Share phone	0.359	0.291	-0.068**
	(0.480)	(0.455)	[0.027]
Communication apps stated usage frequency	3.095	3.074	-0.022
	(0.980)	(0.962)	[0.742]
Information apps stated usage frequency	1.792	1.777	-0.015
	(0.835)	(0.811)	[0.794]
Entertainment apps stated usage frequency	2.901	2.981	0.079
	(0.988)	(0.988)	[0.246]
Observations	482	447	929

Notes: This table presents the balance checks between the treatment and the control arm. Columns 1 and 2 shows the mean and standard deviations for individuals in capped plan and the standard plan arm, respectively. Column 3 shows the mean difference between two arms. Corresponding p-value of test for the equality of means are in brackets. Note that for 'share phone' (a dummy for sharing of the phone), the treatment and control arms are not balanced. Results remain similar after controlling for phone sharing (and for an array of other controls). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Balance Across the Trial Arms (Data Verification Variables)

	Standard Plan Arm (1)	Capped Plan Arm (2)	Balance test p-value (1) - (2)
WhatsApp user	0.927	0.924	0.841
	(0.260)	(0.265)	
Private WhatsApp account	0.062	0.049	0.389
	(0.242)	(0.217)	
App deletion	0.052	0.047	0.732
	(0.222)	(0.212)	
Fraction of participants for whom the first spell of app data lasted less than 2 days	0.006	0.009	0.632
	(0.079)	(0.094)	
Fraction of participants for whom the first spell of app data lasted less than 28 days	0.432	0.438	0.831
	(0.496)	(0.497)	
Fraction of participants for whom the first spell of app data lasted less than 84 days	0.838	0.832	0.807
	(0.369)	(0.374)	
Completed endline (total)	0.936	0.962	0.071^{*}
	(0.246)	(0.191)	
Completed endline within last 4-week cycle	0.878	0.913	0.081^{*}
	(0.328)	(0.283)	
Endline survey type (in office or phone call)	1.479	1.452	0.404
	(0.500)	(0.498)	
Observations	482	447	929

Notes: This table presents balance checks for WhatsApp usage, usage-tracking app and variables related to endline survey administration across the commitment plan and the standard plan arms. Note that the first spell starts on the first day of the experiment, and lasts until the first no-usage day occurs. Columns 1 and 2 contain the means and standard deviations for individuals in each arm. Column 3 contains p-values for the test for equality of means between the commitment plan and standard plan arms. Note that a slightly higher percentage of participants in the capped plan arm complete the endline survey. However, this percentage is high (around 95%) in both arms, and the difference is not statistically significant at the 5 percent significance level. Also, this does not affect our main analyses (on smartphone time use and WhatsApp information access), which use no endline survey data. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Covariate Balance Table: WhatsApp Users *vs.* Non (or private) Users

	Private WhatsApp Users (or no WhatsApp) (1)	Non-private WhatsApp Users (2)	Balance test difference (1) - (2)
Age	29.471	27.469	-2.002**
	(10.041)	(8.385)	0.017
Age squared	968.529	824.769	-143.760**
	(713.983)	(556.800)	0.011
Female	0.215	0.202	-0.013
	(0.412)	(0.402)	0.738
Marital status	1.463	1.491	0.029
	(0.501)	(0.515)	0.569
Number of children	2.760	2.524	-0.236
	(1.238)	(1.416)	0.264
Number of people living in the same house $_sharers$	6.140	5.900	-0.241
	(2.976)	(3.071)	0.420
Interstate migrant	0.736	0.649	-0.087*
	(0.443)	(0.478)	0.060
Self control	3.180	3.154	-0.026
	(0.495)	(0.484)	0.585
FOMO	1.216	1.258	0.042
	(0.538)	(0.572)	0.482
Baseline time-inconsistent preferences (Money)	0.248	0.254	0.006
	(0.434)	(0.435)	0.892
Baseline sleep (PSQI)	-3.430	-3.432	-0.002
	(2.140)	(2.131)	0.992
Baseline sleep insomnia (ISI)	-7.802	-7.478	0.324
	(3.968)	(3.792)	0.384
Last time check phone before sleep (mins)	15.587	14.684	-0.902
	(15.142)	(13.700)	0.534
First time check phone after wake up (mins)	26.333	26.886	0.553
	(27.599)	(31.024)	0.864
Preference for capped plan for data	0.760	0.754	-0.007
	(0.429)	(0.431)	0.875
Preference for capped plan for chips	0.595	0.603	0.008
	(0.493)	(0.490)	0.872
Preference for capped plan for shampoo	0.512	0.450	-0.062
	(0.502)	(0.498)	0.203
Preference capped plan for income	0.347	0.325	-0.022
	(0.478)	(0.469)	0.637
Owns Wifi at home	0.157	0.177	0.020
	(0.365)	(0.382)	0.590
Baseline finish data plan	12.628	6.155	-6.473***
	(32.630)	(23.093)	0.007
Share phone	0.331	0.325	-0.005
	(0.472)	(0.469)	0.912
Communication apps stated usage frequency	3.019	3.094	0.075
	(0.981)	(0.970)	0.452
Information apps stated usage frequency	1.774	1.786	0.012
	(0.849)	(0.820)	0.898
Entertainment apps stated usage frequency	2.726	2.971	0.245**
	(1.083)	(0.970)	0.017
Observations	121	808	929

Notes: Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3 App Categorization and Smartphone Time Use Summary Statistics

We leveraged Google PlayStore's app categories to classify apps. However, in BOP markets, some apps could serve for different purposes than they originally intended. For instance, Maps & Navigation might be essential business apps for rickshaw drivers, WhatsApp could be mainly used for business communication by local retailers, or Facebook videos could be the main source of news for the illiterate folks. We tested the accuracy of our categorization of apps by asking participants to list the top app they use for various purposes. We found that the stated top entertainment app mapped into the entertainment category in our scheme 97.2% of the time, and communication into 99.3% of the time. Users in our sample use YouTube and social media apps primarily for entertainment, while WhatsApp and Contacts/Phone are primarily used for communication (Table 5). To further validate our time use measures, we linked participant's self-reported usage frequency for entertainment and communication apps to their actual time use in these apps under our categorization scheme. Participants who reported frequent (or very frequent) entertainment usage exhibit higher actual entertainment time use than those who did not. Stated and actual usage of communication apps follows a similar pattern (Figure 1).

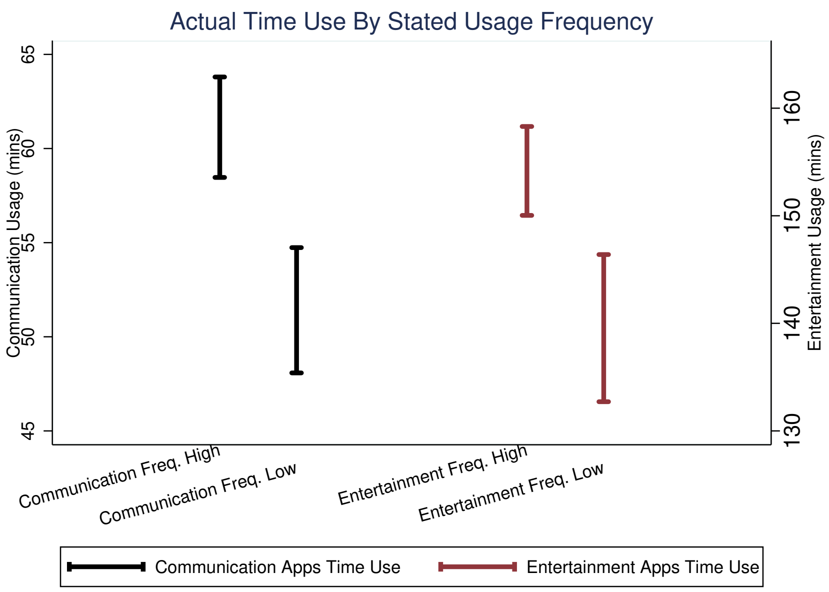


Figure 1: Actual Time Use by Stated Usage Frequency

Notes: This figure presents a comparison between participants' actual time use and stated usage frequency, for communication apps and entertainment apps. For each category, we present the 95% confidence intervals of individual participants' average time use in our sample, separately for those who stated frequent (or very frequent) usage, and those who did not.

Table 6: Comparison of Stated App Usage Category and Our App Categorization

Communication			Entertainment		
Stated Most Frequently Used App	Count	Classified Correctly as Communication	Stated Most Frequently Used App	Count	Classified Correctly as Entertainment
WhatsApp	793	Yes	YouTube	378	Yes
Contacts/Phone	47	Yes	TikTok	70	Yes
Facebook Messenger	6	Yes	Facebook	57	Yes
IMO	5	Yes	PUBG	42	Yes
Instagram Direct Message	4	No	Music Player	33	Yes
No Answer	74	N/A	Candy Crush Saga	27	Yes
Total	929	**Match Rate: 99.3%**	Ludo King	26	Yes
			VidMate	20	N/A
			Like	14	Yes
			Car Room King	11	Yes
			Subway Surfer, Gaana, Free Fire, Hot Star	8	Yes
			Bubble Game	7	Yes
			Clash Of Clans	6	Yes
			Vigo	4	Yes
			Instagram, Ace TeenPatti	3	Yes
			Helo, JioSaavn, Gunship, TempleRun, Hill Climb Racing, Action Bus	2	Yes
			Asphalt, Balloon Fly, Snapchat, FM Radio, Football, Mini Milita, Plumbergame, Pool,Video Player, Rummy, Cricket, Super Bino, Tom Hero, Cell Break Game, Wynk	1	Yes
			Puzzle, Twitter	1	No
			Motorbike	1	N/A
			No Answer	168	N/A
			Total	929	**Match Rate: 97.24%**

Notes: This table presents the success rate for the mapping of participants' stated top communication app and top entertainment app to our communication and entertainment categories. We report participants' stated top app for communication and for entertainment. Then, for each of these stated top apps, we list whether it was classified correctly. (Note that Motorbike and VidMate are not on Google PlayStore.)

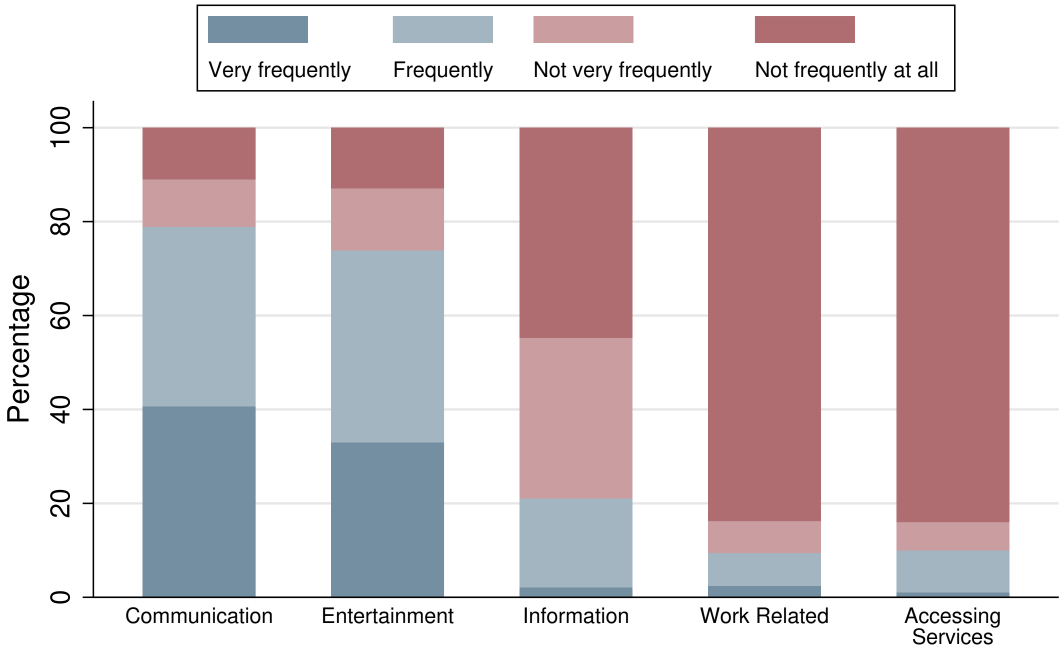


Figure 2: Perceived App Usage Frequency

Notes: This figure presents participants' stated usage frequency for an array of purposes. Light blue bars reflect frequent usage and light reddish bars reflect not very frequent usage. Dark blue and dark reddish bars indicate very frequent and not frequent at all, respectively. For communication and entertainment purposes 679 and 618 participants respectively reported frequent (or very frequent) usage. This number drops down to 165, for usage frequency for information purposes (e.g., Google), 74, for work-related purposes (e.g., maps for rickshaw drivers), and 76, for accessing services (e.g., mobile money services). Among 929 participants, 68 did not respond to the communication time use frequency question. This number is 93 for usage frequency for entertainment purposes, 145 for information, 164 for work-related and 166 for accessing services.

Table 7: App Category Description

Classified as Entertainment	Usage Purpose	Google Play Store Categories	Description	Examples
Yes	*Media & Entertainment*	Art & Design, Comics, Entertainment, Music & Audio, Hedonic Games, Photography, Video Players & Editors	Apps for leisure activities, and visual and auditory content design	Netflix, Spotify, Candy Crush Saga, Adobe Photoshop
Yes	*Lifestyle - General Interest*	Auto & Vehicles, Beauty, Events, Food & Drink, Health & Fitness, House & Home, Lifestyle, Shopping, Sports, Travel & Local, Personalization, Wearables	Apps for personal hobbies and designing the way of living	Tripadvisor, Zomato, Cricket Exchange, Flipkart
Yes	*Social Connectivity*	Dating, Social	Apps for social networking	Facebook, Tiktok, Tinder
No	*Communication*	Communication	Apps for contacting other people	WhatsApp, Messenger
No	*Activity Assistance*	Maps & Navigation, Productivity, Tools, Weather	Apps for organizing or helping with specific tasks	Gmail, Calendar, Calculator, Alarm, Flashlight
No	*News & Information*	Books, Library, News & Magazine	Apps for accessing information	Hindi News, Audible, Oxford Dictionary
No	*Self Enhancement*	Business, Education, Finance, Medical, Parenting, Utilitarian Games	Apps for work or skill development	Uber Driver, Hello English, M-Pesa, Math Master
No	*Off Market*	-	Apps that are not inbuilt or in Google Play Store	-

Notes: We used Google PlayStore primary and secondary app categories to create 8 app usage purposes: media & entertainment, lifestyle – general interest, social connectivity, communication, activity assistance, news & information, self enhancement and off market. (As a part of this step, we classified action, adventure, arcade, card, casino, racing, sports, simulation, casual, board, music and strategy game categories under *hedonic* games. Brain, education, trivia, puzzle and word game categories are coded under *utilitarian* games.) Then, we labeled media & entertainment, lifestyle – general interest and social connectivity as 'entertainment'. To identify 'communication' apps, we relied on Google PlayStore's communication apps category. We excluded internet browsers, which PlayStore includes this category, since they can be used for non-communicative purposes. We labeled apps that are not inbuilt or that do not appear in Google Play Store, as 'off market'.

We present objective daily smartphone time use measures for subgroups defined based on five characteristics – gender, employment, migration status, education and age – in Figure 3. We find that WhatsApp time use is significantly higher, and YouTube time use significantly lower, in the below median age group. Although not statistically significant at the 5% level, we find that on average, migrants spend less time and students more time on WhatsApp, compared to non-migrants and other employment types, respectively.

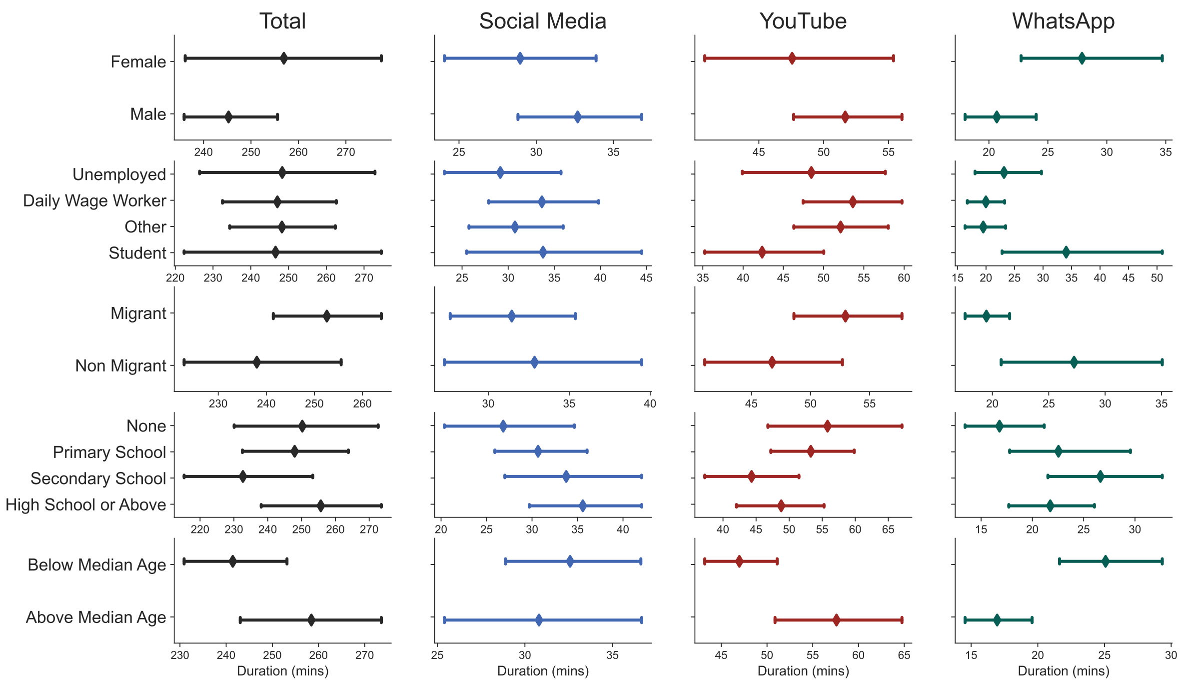


Figure 3: Daily smartphone time use by sociodemographic characteristics

Notes: This figure presents several objective daily smartphone time use measures collected using our usage-tracking app. We calculate the average daily time use of each participant over the study period and calculate subpopulation level confidence intervals with each individual as an observation. Bands reflect the 95% confidence intervals. We report four outcome variables: total (black), social media (blue), YouTube (red), and WhatsApp (green) time use. From top panel to the bottom, we investigate gender, employment status, migration status (whether or not an interstate migrant), education level, and age.

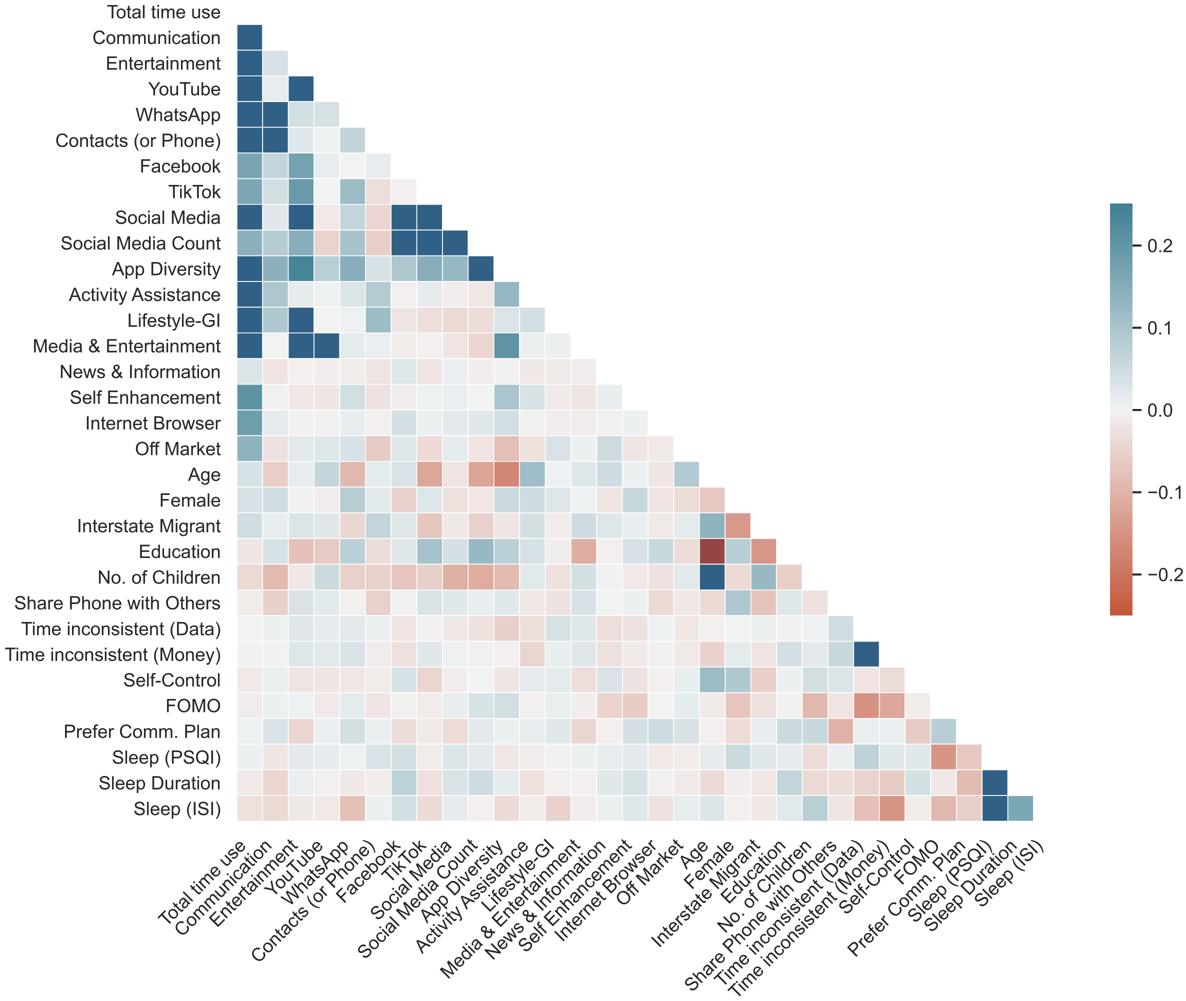


Figure 4: Correlations Among Smartphone Usage Measures and User Characteristics

Notes: This figure depicts the correlations between mobile phone usage measures and participant characteristics. Blue and reddish colors reflect a positive and negative correlation, respectively. Estimates over 0.25 or below -0.25 are colored as dark blue (e.g., total time use and communication), and dark red (e.g., education and age). The first 18 variables, until age, are measured through our usage-tracking app, and the remaining variables are measured in the baseline and endline surveys, respectively. The daily social media count and app diversity variables are measured as counts. All other mobile phone usage outcomes are daily time use variables, measured in seconds. Note that the presented set of mobile phone usage variables is not mutually exclusive. For instance, WhatsApp is included in communication apps and similarly, TikTok and Facebook are included in social media apps.

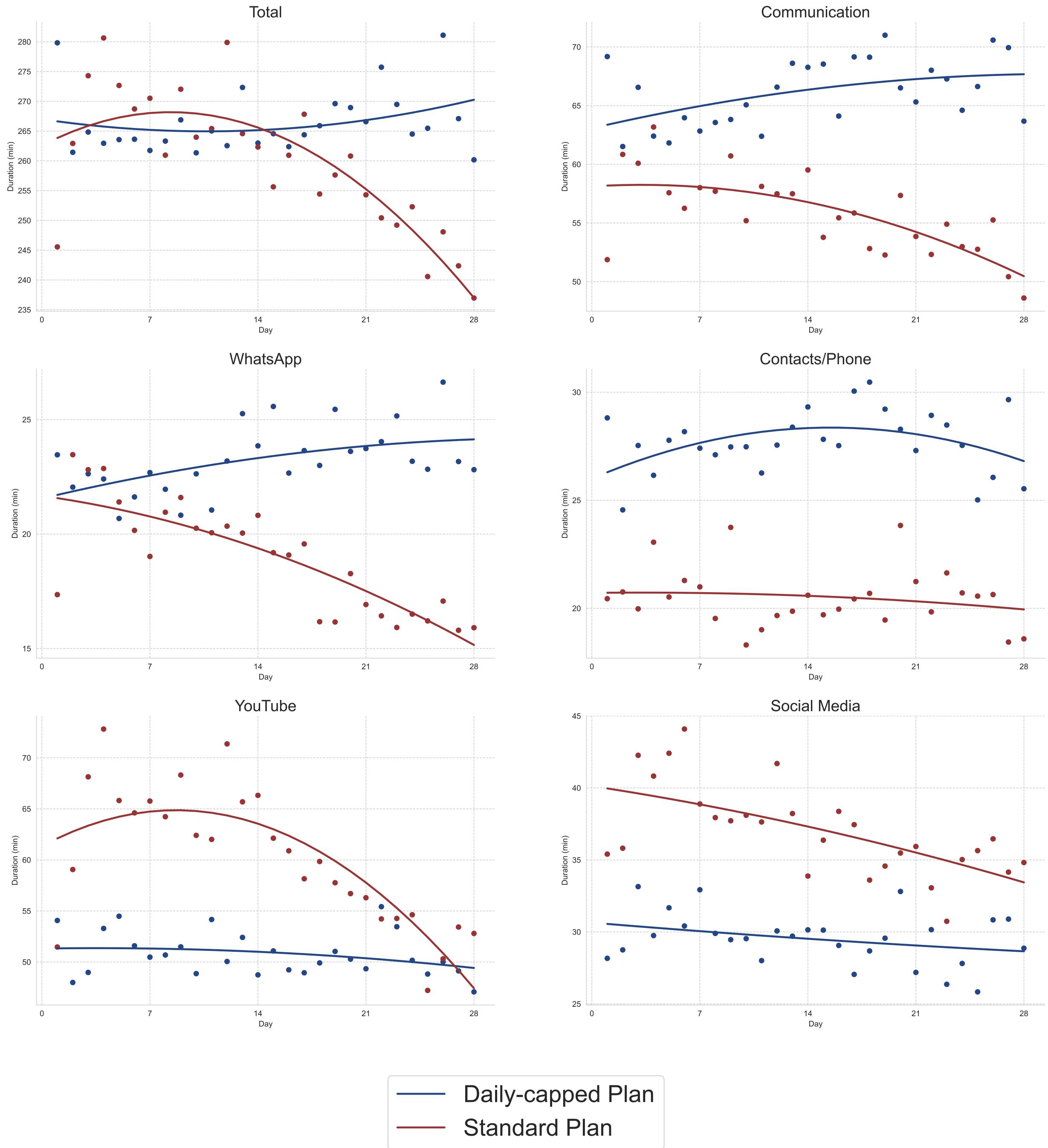


Figure 5: Smartphone Time Use

Note: Points present the daily average time use within a data plan in the capped plan arm (blue) and the standard plan arm (red). The points in each plan are fitted to a quadratic polynomial. The top panel, from left to right, shows total and communication apps time use. The panel in the middle depicts the most used online and offline communication apps time use, namely WhatsApp and Contacts (or Phone). The bottom panel reflects YouTube and social media apps time use.

4 Information Dissemination

Table 8: Prior Research in Medicine, OM and Economics on Mobile Phone-based Life-Improving Services

Author (Year)	Country	Intervention	Observed Effect
Health Behavior Change			
Lester et al. (2010)	Kenya	Text message reminders for HIV clinic visits	Improved antiretroviral treatment adherence and rates of viral suppression.
Zurovac et al. (2011)	Kenya	Malaria case management text messages to health workers	Improved adherence to national guidelines for the management of malaria.
Jamison et al. (2013)	Uganda	Text-message-based sexual health information	Increased promiscuity and no shift in perception of norms.
Gibson et al. (2017)	Kenya	Text message reminders to vaccine camps	Improved immunization coverage and timeliness when combined with financial incentives.
Banerjee et al. (2020)	India	Video messages on Covid-19 health-preserving behavior	Increased practice of suggested behaviour (e.g., social distancing and hand washing).
Food and Agriculture			
Jensen (2007)	India	Adoption of mobile phones	Reduced price dispersion and elimination of waste.
Muto and Yamano (2009)	Uganda	Increased mobile phone coverage	Increased market participation in remote areas.
Parker et al. (2016)	India	Text-message-based market price information	Reduced price dispersion and increased rate of price convergence.
Cole and Fernando (2020)	India	Voice-message-based agricultural information	Change in seed choices following the recommendations.
Financial Decision Making			
Jack and Suri (2014)	Kenya	Adoption of mobile money services	Increased household risk sharing behavior.
Karlan et al. (2016)	Bolivia, Peru and Phillippines	Text message reminder for financial savings	Increased commitment attainment for savings accounts.
Suri and Jack (2016)	Kenya	Adoption of mobile money services	More houses lifted out of poverty.
Political			
Manacorda and Tesei (2020)	Africa	Adoption of mobile phones	Mass political polarization.
Other			
Jensen and Miller (2018)	India	Adoption of mobile phones	Increased market concentration, exit of low-quality players.
Chong et al. (2015)	Peru	Pro-recycling text messages	No significant increase in recycling behavior.
Casaburi et al. (2019)	Kenya	Hotline service for farmers	Improved supply chain efficiency.

Notes: For a review of mobile phones and agricultural development, see Fabregas et al. (2019); and for a review of mobile phone-based health interventions in emerging markets, see Déglise et al. (2012).

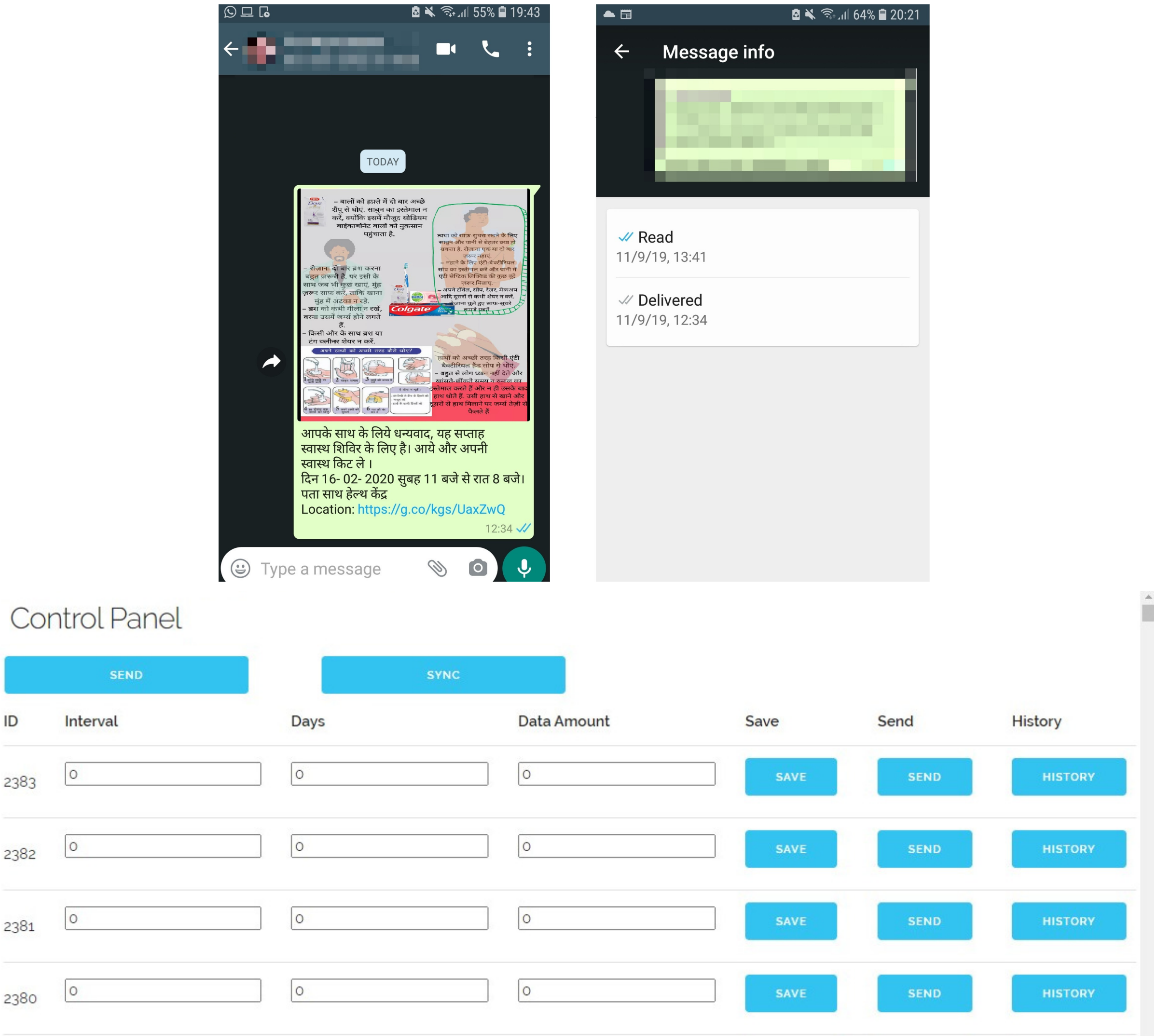


Figure 6: WhatsApp Health Messages and Usage-Tracking App Control Panel

Notes: This figure presents the WhatsApp health messages and the control panel of the usage tracking app.

- The top left panel shows an illustrative example of a health message. The image inside the WhatsApp message includes a brief infographics related to the camp's topic. The text in the message further informs about the camp, including it's date, time and location. Top right figure illustrates the inbuilt 'read receipt' function of WhatsApp. 'Read' with two blue ticks to the left indicates that the health message has been accessed and the time stamp underneath is used to measure time to access a message.
- The figure in the bottom depicts the control panel of our usage-tracking app. 'ID' reflects the participant number. The 'Interval', 'Days' and 'Data Amount' boxes are used to set the data usage caps. 'Interval' presents the duration of the cap (e.g., 1 day for the daily-capped plan and 28 days for the standard plan). 'Days' is the total period in which the data caps will be imposed (e.g., 84 days for all participants). 'Data Amount' is the actual amount of data cap per interval (e.g., 0.5GB for the daily-capped plan and 14GB for the standard plan). The 'Save' button saves the defined setting. The 'Send' button activates the defined setting on the participant's mobile device (the other 'Send' button above the panel does the same for all participants simultaneously). 'History' button shows the smartphone usage history of the participant. 'Sync' button shows participants who deleted our usage-tracking app.

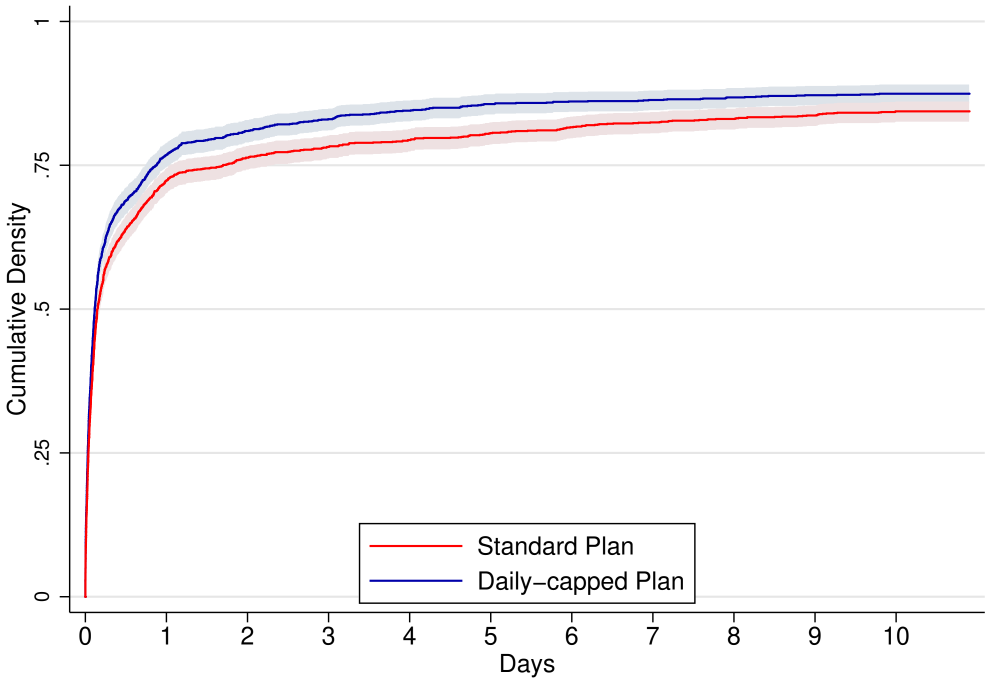
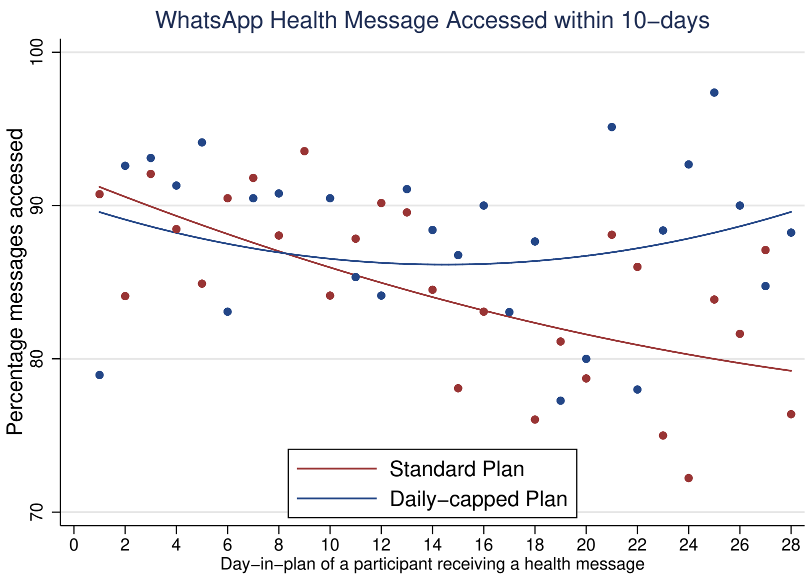
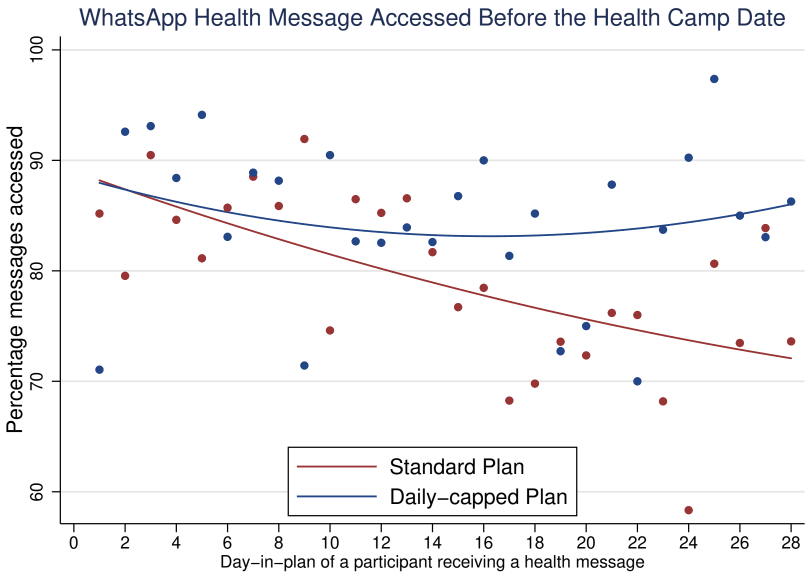


Figure 7: Kaplan-Meier Failure Curves for Time to Access WhatsApp Messages

This figure presents the Kaplan-Meier failure plots of time to access WhatsApp health camp invite messages for participants in the capped (blue) and standard plan (red) arms. Shaded areas depicts 95 percent confidence intervals.



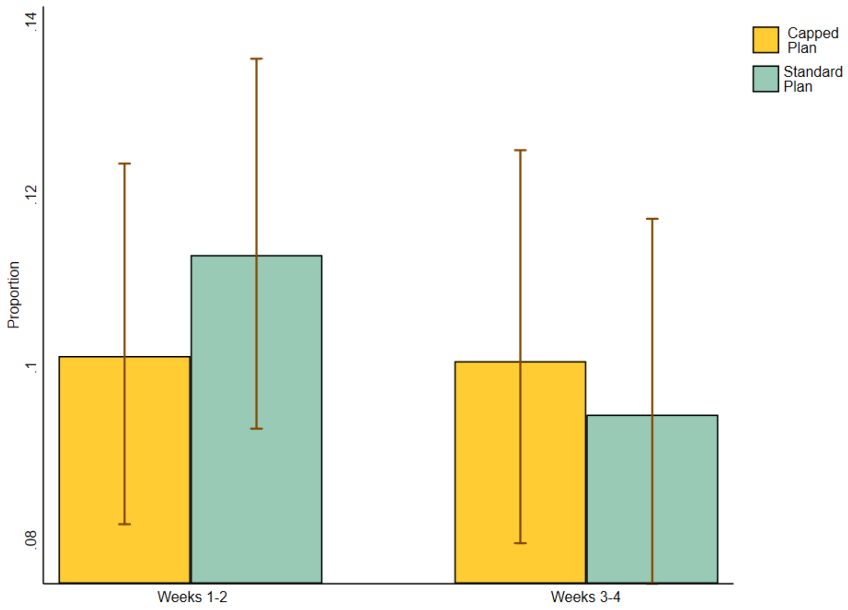
(a) Percentage of messages accessed in 10 days



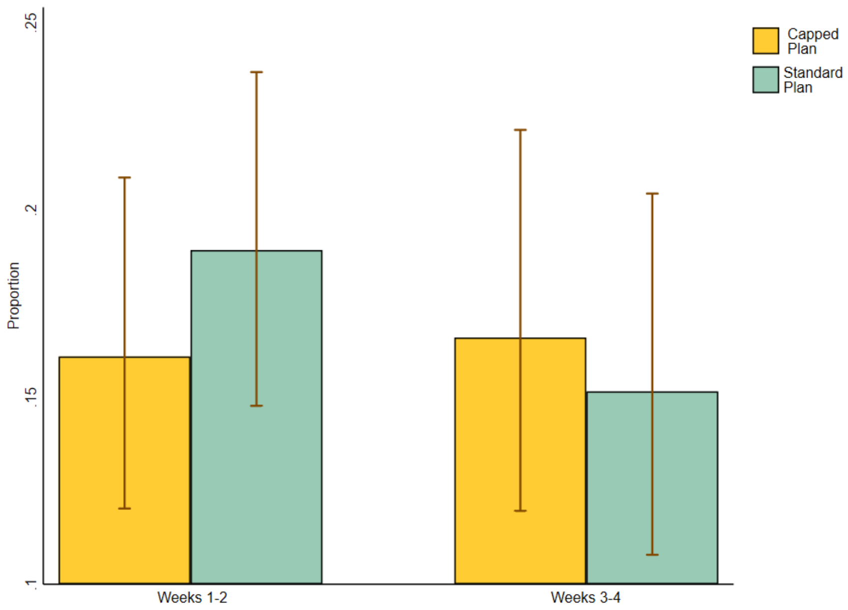
(b) Percentage access before the health camp

Figure 8: WhatsApp Health Message Access

Notes: This figure depicts the percentage of participants that access our WhatsApp health messages each day-in-plan, in the two trial arms. The data points in each plan are fitted to a quadratic polynomial. The figures in top and bottom panels present the percentage of participants who access a health message within 10-days of receiving it and before the date of the health camp, respectively. Health messages are sent on a random day 5 or fewer days prior to the camp date.



(a) Percentage of messages accessed the next morning (full sample)



(b) Percentage of messages accessed the next morning (messages sent after 20.00)

Figure 9: Effect of Caps on Propensity to Access Information Next Morning

Notes: This figure depicts the percentage of participants that access our WhatsApp messages on the morning after they were received. We define a message as having been accessed the next morning if it has been accessed the next day between 04.00 AM and 12.59 PM.

Table 9: Effect of Caps on Information Access– without controls

	Health Camp Message Access and Camp Attendance					**Smartphone Usage**		
	WhatsApp health camp message access	Time to access a message (mins)	Time to access a message *Survival Model*	Health camp attendance	Health camp attendance [if pre-camp msg. access]	Communi-cation apps time use (mins)	WhatsApp time use (mins)	Contacts (or Phone) time use (mins)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Effects of the Capped Plan on Early vs. Late-plan								
Capped plan	-0.005	-102.528	0.015	0.053**	0.060**	6.073	1.365	6.741***
	(0.019)	(109.542)	(0.051)	(0.027)	(0.030)	(4.455)	(2.348)	(2.607)
Weeks 3-4	-0.080***	417.525***	-0.275***	-0.013	0.022	-4.766***	-3.622***	-0.486
	(0.016)	(131.197)	(0.055)	(0.017)	(0.021)	(1.549)	(1.050)	(0.779)
Capped plan	0.074***	-347.632**	0.253***	0.023	-0.007	8.048***	5.543***	0.958
× Weeks 3-4	(0.023)	(170.815)	(0.077)	(0.026)	(0.030)	(2.028)	(1.209)	(1.191)
Standard Plan Weeks 1-2								
Mean	0.886	833.07	-	0.244	0.285	58.425	20.987	20.587
Std. dev	[0.318]	[2043.08]	-	[0.430]	[0.452]	[78.494]	[39.596]	[46.063]
Panel B. Main Effects of the Capped Plan								
Capped plan	0.030*	-255.367***	0.137***	0.063***	0.057**	9.811**	3.944	7.185***
	(0.018)	(92.714)	(0.038)	(0.024)	(0.027)	(4.576)	(2.452)	(2.644)
Standard Plan								
Mean	0.844	1019.2	-	0.231	0.287	56.139	19.180	20.514
Std. dev	[0.363]	[2368.8]	-	[0.421]	[0.452]	[75.637]	[34.934]	[45.691]
Observations	3,232	2,776	3,224	3,232	2,656	35,953	35,953	35,953

Notes: Standard errors are clustered at the individual level. The estimates reported in column 4 is from the cox proportional hazard model, in which 8 messages which were accessed within a minute of being sent are dropped as their time to access was zero. The Column 5 sample excludes messages accessed on or after the camp date. In Columns 6, 7 and 8, day one of the first 4-week plan is excluded (to eliminate bias due to pre-enrollment usage). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Effect of Caps on Information Dissemination (Week-in-Plan)

	Health Information Access and Camp Attendance						**Smartphone Usage**		
	WhatsApp health message access (1)	Time to access a message (mins) (2)	Time to access (mins) *excl. over 10 days* (3)	Time to access (mins) *survival model* (4)	Health camp attendance (5)	Healt camp attendance *excl. no access* (6)	Communication apps time use (mins) (7)	WhatsApp time use (mins) (8)	Contacts (or Phone) time use (mins) (9)
Week 2	-0.0101	-0.164	-0.187	-0.134	-0.166	-0.183	-0.0352	-0.0378	-0.0573
	(0.205)	(0.153)	(0.147)	(0.194)	(0.145)	(0.149)	(0.0362)	(0.0568)	(0.0474)
Week 3	-0.688***	0.505***	0.130	0.549***	-0.278*	-0.129	-0.0936**	-0.145**	-0.0568
	(0.184)	(0.158)	(0.155)	(0.205)	(0.143)	(0.148)	(0.0426)	(0.0678)	(0.0598)
Week 4	-0.549***	0.619***	0.353**	0.704***	-0.0797	0.125	-0.171***	-0.261***	-0.0841
	(0.202)	(0.173)	(0.164)	(0.217)	(0.150)	(0.156)	(0.0490)	(0.0794)	(0.0624)
Capped Plan	0.0534	-0.187	-0.205	-0.187	0.273	0.251	0.0347	-0.182	0.381**
	(0.249)	(0.182)	(0.161)	(0.202)	(0.179)	(0.184)	(0.0871)	(0.137)	(0.148)
Capped Plan × Week 2	-0.257	0.384*	0.323	0.366	-0.0126	0.0585	0.0634	0.0575	0.0653
	(0.273)	(0.208)	(0.200)	(0.274)	(0.194)	(0.202)	(0.0506)	(0.0762)	(0.0660)
Capped Plan × Week 3	0.408	-0.439**	-0.193	-0.522*	0.175	0.102	0.176***	0.173*	0.161**
	(0.269)	(0.221)	(0.208)	(0.286)	(0.199)	(0.209)	(0.0584)	(0.0950)	(0.0802)
Capped Plan × Week 4	0.457	-0.317	-0.0497	-0.380	0.136	-0.0428	0.277***	0.322***	0.178**
	(0.302)	(0.243)	(0.229)	(0.304)	(0.203)	(0.213)	(0.0656)	(0.105)	(0.0856)
Observations	3232	3232	2776	3224	3232	2656	35953	35953	35953

Notes: This table presents the results on information dissemination. Each regression includes an indicator for the treatment arm, and indicators for different weeks in a 4-week plan, their interactions, a vector of control variables and time fixed effects. Standard errors are clustered at the individual level. The regressions reported in columns 1, 5 and 6 are logit specifications. The sample in column 6 consists of participant-messages accessed prior to the camp date. In columns 2, 3, 7, 8 and 9 the dependent variable is the natural log transformation of 1 plus the outcome of interest. In column 2, messages that were never accessed are assigned a duration of 10 days, and in column 3, these messages are excluded. The estimates reported in column 4 is an loglogistic hazard specification, in which 8 messages which were accessed within a minute of being sent are dropped as their time to access was zero. The shape parameter of the survival model is 1.65. In columns 7, 8 and 9, the first day of the first 4-week plan is excluded (to eliminate bias due to pre-enrollment usage).

5 Time Use and Well-being Outcomes, Heterogeneity, and Demand for Capped Plans

Table 11: Effect of Caps on Smartphone Usage and Well-being Outcomes – without controls

Observations = 35,953 in panel A. Observations = 822 in panel B.	Daily-capped data plan (1)	Weeks 3-4 (2)	Daily-capped data plan × Weeks 3-4 (3)	*Standard Plan Weeks 1-2* Mean [Std. dev]
Panel A. Smartphone Usage				
Social media time use (mins)	-8.78** (3.889)	-3.23** (1.595)	2.78 (2.010)	39.061 [74.837]
Social media count	-2.64* (1.602)	-0.68 (0.434)	0.68 (0.601)	12.585 [28.538]
YouTube time use (mins)	-14.0*** (5.056)	-9.59*** (2.853)	8.64*** (3.310)	65.181 [109.639]
Total time use (mins)	-3.54 (11.154)	-16.2*** (4.853)	18.9*** (5.988)	268.18 [201.71]
App diversity	-0.32 (0.368)	-0.15 (0.131)	0.15 (0.173)	15.519 [6.509]
Panel B. Endline Measures				
Sleep (ISI)	-0.15 (0.353)	-0.52 (0.363)	0.43 (0.482)	-5.659 [3.643]
Sleep (PSQI)	0.053 (0.125)			-3.119† [1.944]
Sleep duration (hours)	-0.0044 (0.079)			6.899† [1.125]
Happiness	0.0095 (0.093)	-0.17* (0.096)	0.11 (0.127)	2.992 [0.947]
Loneliness	-0.11 (0.118)	0.044 (0.121)	0.080 (0.161)	1.031 [1.180]
Depression	0.0099 (0.113)	0.041 (0.116)	0.013 (0.154)	1.291 [1.148]
Time-inconsistent preferences (Data)	0.047 (0.040)	0.088** (0.042)	-0.11* (0.055)	0.144 [0.351]
Time-inconsistent preferences (Money)	0.032 (0.039)	0.0073 (0.040)	0.0095 (0.053)	[0.124] [0.329]
Preference for the daily-capped plan	-0.074* (0.045)	0.016 (0.046)	-0.056 (0.061)	0.751 [0.433]

Notes: Each regression includes time fixed effects. As time fixed effects, we include indicators for each date in the experiment in panel A, and for day of week and week of year in panel B. Standard errors are clustered at the individual level. In panel A, the first day of the first 4-week plan was excluded, to eliminate bias due to pre-enrollment usage. Among the smartphone usage measures, social media count and app diversity are count measures, and the other measures are durations, measured in minutes. The sample for panel B is users who were surveyed on a random date in the last 4-week plan. In panel B, baseline measures are controlled for in the regression for preference for daily-capped data plan and in all sleep regressions. The rightmost column presents the mean and standard deviation of standard plan weeks 1-2 for each outcome variable. † Sleep PSQI and sleep duration were self-reported measures of sleep over the last 30 days, so we report the average results of all four weeks. Sleep ISI and PSQI measures are multiplied with -1 (higher values indicate better sleep). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Effect of Caps on Smartphone Usage and Well-being Outcomes (Week-in-Plan)

Observations = 35,953 in panel A. Observations = 822 in panel B.	Week 2 (1)	Week 3 (2)	Week 4 (3)	Capped (4)	Capped Plan × Week 2 (5)	Capped Plan × Week 3 (6)	Capped Plan × Week 4 (7)
Panel A. Smartphone Usage							
Social media time use	0.014	-0.17**	-0.21**	-0.43**	0.027	0.17	0.16
	(0.069)	(0.087)	(0.095)	(0.213)	(0.094)	(0.120)	(0.125)
Social media count	-0.0088	-0.041	-0.042	-0.26**	0.023	0.063	0.0074
	(0.030)	(0.039)	(0.039)	(0.101)	(0.044)	(0.058)	(0.058)
YouTube time use	-0.0052	-0.077	-0.28***	0.024	-0.043	-0.064	0.073
	(0.070)	(0.083)	(0.097)	(0.171)	(0.102)	(0.119)	(0.134)
Total time use	-0.037*	-0.050**	-0.11***	-0.012	0.058**	0.057*	0.13***
	(0.021)	(0.024)	(0.029)	(0.049)	(0.028)	(0.034)	(0.039)
App diversity	-0.0098	-0.0077	-0.017	-0.024	-0.0083	-0.0015	0.0070
	(0.008)	(0.010)	(0.011)	(0.023)	(0.011)	(0.014)	(0.015)
Panel B. Endline Measures							
Sleep (ISI)	-0.091	-0.41	-0.13	0.024	-0.21	0.22	-0.23
	(0.299)	(0.301)	(0.309)	(0.342)	(0.416)	(0.422)	(0.415)
Sleep (PSQI)				-0.0095†			
				(0.130)			
Sleep duration (hours)				0.0034†			
				(0.010)			
Happiness	-0.35	-0.58*	-0.52	-0.17	0.27	0.54	0.21
	(0.317)	(0.323)	(0.325)	(0.329)	(0.417)	(0.420)	(0.434)
Loneliness	0.066	0.034	0.14	-0.071	-0.061	0.39	0.099
	(0.323)	(0.325)	(0.322)	(0.317)	(0.416)	(0.408)	(0.403)
Depression	-0.34	-0.13	-0.43	0.030	-0.12	-0.029	0.31
	(0.307)	(0.314)	(0.325)	(0.321)	(0.415)	(0.408)	(0.418)
Time-inconsistent preferences (Data)	-0.49	0.47	0.43	-0.067	0.71	-0.52	-0.27
	(0.440)	(0.417)	(0.405)	(0.433)	(0.578)	(0.558)	(0.562)
Time-inconsistent preferences (Money)	-0.58	-0.29	-0.043	-0.16	0.70	0.49	0.41
	(0.444)	(0.438)	(0.425)	(0.439)	(0.598)	(0.581)	(0.573)
Preference for the daily-capped plan	0.97**	0.77**	0.72*	-0.31	-0.28	-0.70	-0.13
	(0.389)	(0.381)	(0.390)	(0.365)	(0.514)	(0.487)	(0.494)

Notes: This table presents the results on smartphone time use and subjective endline survey measures. Each regression includes an indicator for the treatment arm, and indicators for different weeks in a 4-week plan, their interactions, a vector of control variables and time fixed effects. As time fixed effects, we include indicators for each date in the experiment in panel A, and for day of week and week of year in panel B. Standard errors are clustered at the individual level. In panel A, the first day of the first 4-week plan was excluded, to eliminate bias due to pre-enrollment usage. The sample for panel B is users who were surveyed on a random date in the last 4-week plan. In panel B, baseline measures are controlled for in the regression for preference for daily-capped data plan and in all sleep regressions. † Sleep PSQI and sleep duration were self-reported measures of sleep over the last 30 days, so we report the average results of all four weeks. Sleep ISI and PSQI measures are multiplied with -1 (higher values indicate better sleep). All time use measures in panel A, and sleep duration in panel B are the natural log transformation of 1 plus the outcome of interest. Social media count and app diversity regressions are negative binomial specification. In panel B, happiness, loneliness, depression, sleep ISI and PSQI regressions are ordered logit specification, and capped plan preference and time impatience regressions are logit specification. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: Effects of Data Caps on Usage in Other App Categories

	Media & Entertainment	Lifestyle General Interest	Social Connectivity	Communi-cation	Activity Assistance	News & Info.	Self Enhancement	Offmarket
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Week 2	-0.040	-0.050	0.014	-0.035	-0.076*	0.023	-0.025	-0.048
	(0.038)	(0.056)	(0.069)	(0.036)	(0.044)	(0.025)	(0.058)	(0.034)
Week 3	-0.030	-0.079	-0.172**	-0.094**	-0.075	0.037	-0.012	-0.056
	(0.047)	(0.072)	(0.087)	(0.043)	(0.055)	(0.029)	(0.074)	(0.039)
Week 4	-0.110**	-0.053	-0.215**	-0.171***	-0.013	0.031	-0.042	-0.050
	(0.056)	(0.078)	(0.095)	(0.049)	(0.057)	(0.029)	(0.081)	(0.045)
Daily-capped plan	0.048	-0.043	-0.427**	0.035	-0.036	-0.031	0.001	-0.244**
	(0.086)	(0.154)	(0.213)	(0.087)	(0.109)	(0.076)	(0.155)	(0.111)
Daily-capped plan	0.039	0.012	0.027	0.063	0.021	-0.078**	0.073	0.034
× Week 2	(0.055)	(0.085)	(0.094)	(0.051)	(0.062)	(0.036)	(0.084)	(0.047)
Daily-capped plan	-0.010	0.012	0.168	0.176***	0.016	-0.093**	0.240**	-0.009
× Week 3	(0.068)	(0.099)	(0.120)	(0.058)	(0.075)	(0.040)	(0.105)	(0.056)
Daily-capped plan	0.070	0.047	0.164	0.277***	0.022	-0.117***	0.191*	-0.082
× Week 4	(0.073)	(0.102)	(0.125)	(0.066)	(0.077)	(0.044)	(0.111)	(0.065)
Observations	35,953	35,953	35,953	35,953	35,953	35,953	35,953	35,953

Notes: This table presents the results on app usage using the finer categories specified in Table 7. Each regression includes an indicator for the treatment arm, and indicators for different weeks in a 4-week plan, their interactions, a vector of control variables and date fixed effects (see section 1.3). Standard errors are clustered at the individual level. In all columns the dependent variable is the natural log transformation of 1 plus the outcome of interest. The first day of the first 4-week plan is excluded (to eliminate bias due to pre-enrollment usage). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 14 presents the results for the two primary moderators we registered in our pre-analysis plan. First, we divide the sample into two subgroups based on preferred plan, as elicited in the baseline. From a policy perspective, eliciting participants' preferences for the capped plan before randomization – and before collection of other baseline measures – allows us to obtain counterfactual estimates of the treatment effect on participants who did not prefer a capped plan. By design, commitment devices – such as our capped data plans – help people overcome their self-control problems by limiting or penalizing undesirable behaviors. Therefore, individuals who are unaware of their self-control problems – i..e, those with naïveté – would view them as costly, and will not exhibit a demand for it.

Our results show that random assignment to the capped plan also improves the outcomes of participants who did not exhibit a preference for this plan. This raises the question, should commitment devices be made more attractive, to expand their benefits to more users? In Panel A of Table 14, we find no evidence that individuals who exhibit a preference for the capped plan obtain greater access to information through random assignment to this plan type than those who do not prefer it (other than a mild delayed capped plan effect on camp attendance). Panel B of Table 14 indicates that for most smartphone usage measures, the treatment effect was similar in both groups. However, notably, we see a larger drop in the second half in total time use and social media usage for those who prefer the standard plan.

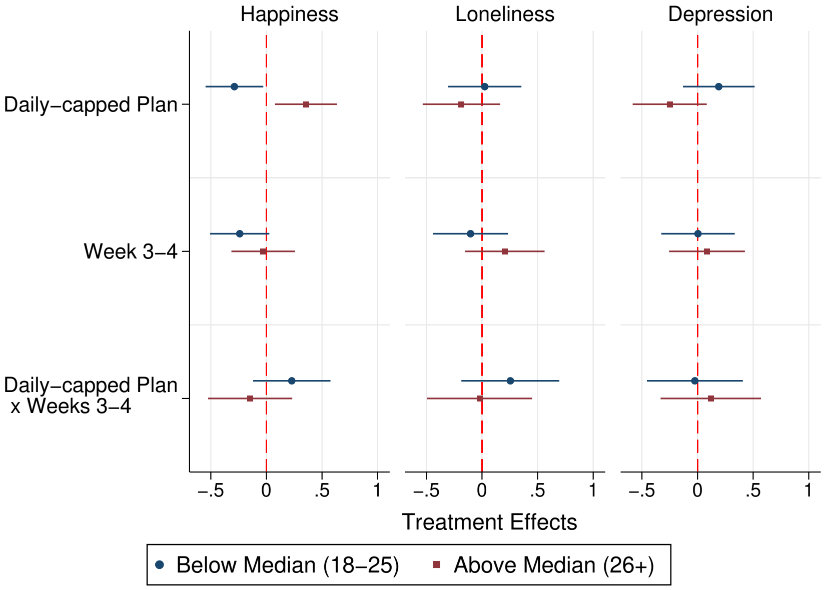
Second, we divide the sample into groups based on above and below median FOMO scores. We observe no differential treatment effect on subjective well-being across high and low FOMO participants. However, at the 10 percent level, we fail to reject a reduction in loneliness for high FOMO participants starting from the first half of the capped plan.

Our secondary moderators are migration and age (see Fig. 10 for the results), for which we had no strong *a priori* beliefs about effect sizes. There are two noteworthy patterns. First, for happiness, we observed a significant negative treatment effect for participants of below-median age, and a similar sized significant positive treatment effect for participants of above-median age. This heterogeneity may in part be related to differential usage of apps, such as social media (see Fig. 4 for correlations between user characteristics and time use in different app categories). Second, the daily-capped plan lead to a significant drop in the loneliness of interstate migrants.

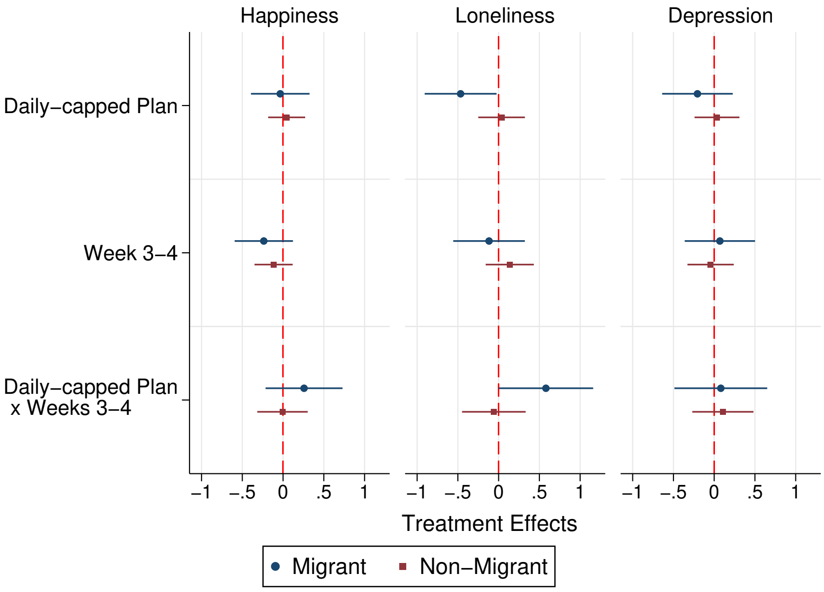
Table 14: Heterogeneous Treatment Effects

	Subgroup 1			Subgroup 2		
	Capped Plan	Weeks 3-4	Capped Plan × Weeks 3-4	Capped Plan	Weeks 3-4	Capped Plan × Weeks 3-4
	(1)	(2)	(3)	(4)	(5)	(6)
	Standard Plan Preferrers			Capped Plan Preferrers		
Panel A. Information Dissemination						
WhatsApp health message access	-0.22	-0.73***	0.36	-0.024	-0.61***	0.63***
	(0.371)	(0.256)	(0.384)	(0.224)	(0.149)	(0.225)
Time to access a message (mins)	374.2	1456.4***	-751.9	-73.27	1378.7***	-1379.8**
	(632.37)	(515.83)	(713.83)	(308.014)	(268.75)	(360.6)
Health camp attendance	0.21	-0.38*	0.35	0.29*	-0.021	0.11
	(0.303)	(0.231)	(0.290)	(0.164)	(0.115)	(0.161)
Panel B. Smartphone Usage						
Communication apps time use (mins)	-5.21	-6.62**	6.14	9.37*	-4.29**	8.68***
	(9.162)	(2.637)	(3.910)	(4.898)	(1.781)	(2.269)
Total time use (mins)	-9.00	-27.5***	22.3*	5.36	-12.3**	18.4***
	(21.951)	(9.479)	(11.740)	(12.654)	(5.536)	(6.966)
Social media time use (mins)	-8.74	-8.51*	7.39	-11.0**	-1.33	1.57
	(9.192)	(4.976)	(5.910)	(4.479)	(1.518)	(1.930)
Social media count	-4.50**	-1.44*	0.010	-3.53*	-0.20	0.87
	(2.016)	(0.829)	(1.125)	(2.075)	(0.470)	(0.683)
App diversity	0.23	-0.25	0.15	-0.48	-0.057	0.13
	(0.768)	(0.268)	(0.337)	(0.400)	(0.138)	(0.194)
	Below Median FOMO			Above Median FOMO		
Panel C. Subjective Well-Being						
Happiness	-0.041	-0.13	0.0013	0.15	-0.22	0.13
	(0.123)	(0.124)	(0.167)	(0.146)	(0.157)	(0.199)
Loneliness	0.064	-0.088	0.19	-0.30*	0.10	0.12
	(0.135)	(0.136)	(0.183)	(0.183)	(0.195)	(0.248)
Depression	0.053	-0.11	0.14	-0.16	0.084	0.036
	(0.153)	(0.155)	(0.207)	(0.168)	(0.180)	(0.229)

Notes: This table presents heterogeneous treatment effects for preference over data plans and FOMO. For each outcome variable, we divide the sample into two subgroups and run separate regressions. Each regression includes an indicator for the treatment arm, an indicator for the second half (Weeks 3-4) of a plan, their interactions, a vector of control variables and time fixed effects. Standard errors are clustered at the individual level. The standard (capped) plans were chosen by 228 (701) respondents. The sample sizes of two subgroups were 796 and 2,436 for Panel A and 8,179 and 27,774 for Panel B, respectively. Sample for panel C. are the users, whom were surveyed on a random date in the last data plan cycle with 381 below and 434 above median FOMO. WhatsApp health message access and health camp attendance regression are logit specifications. Time to access a message in panel A. and all time-use measures in panel B. are measured in minutes. Social media count and app diversity are count measures. In panel B., the first day of the first cycle was excluded (to eliminate bias due to pre-enrollment usage).



(a) Age



(b) Interstate Migrant

Figure 10: Heterogeneous Treatment Effects – Age and Migration

This figure presents the treatment heterogeneity results on happiness, loneliness and depression measures. We run the same regression separately for above median (26+) and below median (18-25) age groups as well as for interstate migrants and non-migrants. Error bars reflect the 95 percent confidence intervals. The sample sizes for the above and below median age groups are 440 and 432, respectively. The sample sizes for interstate migrants and non-migrants are 502 and 370, respectively. Note that in our pre-analysis plan, we defined four age groups. However, in our sample, we only had 23 individuals who were 50+, and 420, 330 and 156 in the 18-24, 25-34, 35-49 age groups, respectively. Therefore, we redefined age cut-offs as above and below the median age.

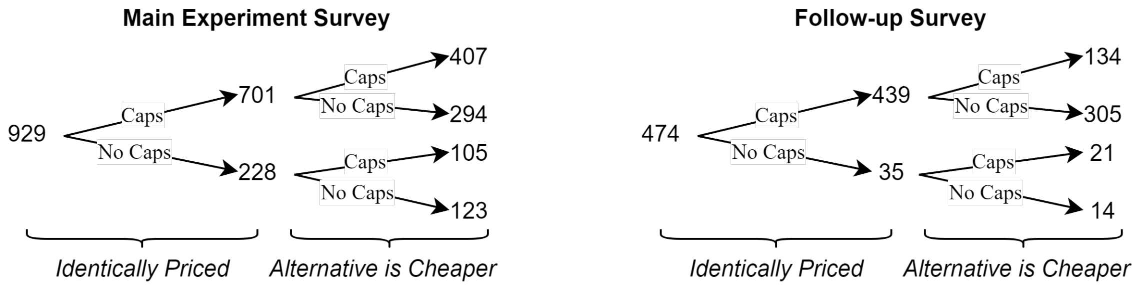


Figure 11: Data Plan Choice. Baseline and Incentive Compatible Follow-up Surveys

Notes: This figure depicts participants' choice for the daily-capped data plans.

- The leaf diagram on the left is from the baseline survey we conducted as a part of the main experiment. At the baseline, participants were offered a choice between receiving lump sum amounts of data at the beginning of the month or in proportionally smaller — daily — installments at an identical per unit price. Next, to measure stickiness to their preferred option, we increased the price of the preferred option by 10% and again elicited preferences. Arrows represent the number of people choosing each plan type.
- In our experiment, allocation to the two data plans was random. Therefore, participants' preferred plans did not affect the data plan they received. To elicit their revealed preferences, we conducted a short follow up study in March 2021. In this follow-up survey, we offered the exact same data plans as in the main study. However, this time participants were told they would receive the plan they preferred with 20% probability (either the identically priced version or the 10% increased price with equal probabilities). There was a 20 Rupees cash reward for participation. Providing the preferred plan with some probability ensures incentive compatibility of the survey questions. We conducted this study with 474 participants.
- The diagram on the right is from the follow-up study. The percentage of participants who choose the capped plan is higher in the follow up study. However, percentage of those who stick to the capped plan is relatively lower. It is unclear whether the increase for capped plan (or decrease in stickiness) is due incentive compatibility of the surveys or whether there was systematic change in preferences from main study period to the follow-up study period. Importantly, in between these studies the Covid-19 pandemic had emerged. Continuity of internet connection might became more valuable during the pandemic, which could explain the increase in demand for capped plan. Similarly, in the same period low-income consumers might became more price sensitive, which might explain the lowered stickiness.

  All in all, both studies indicate that consumers exhibit a considerable demand for the capped plans.

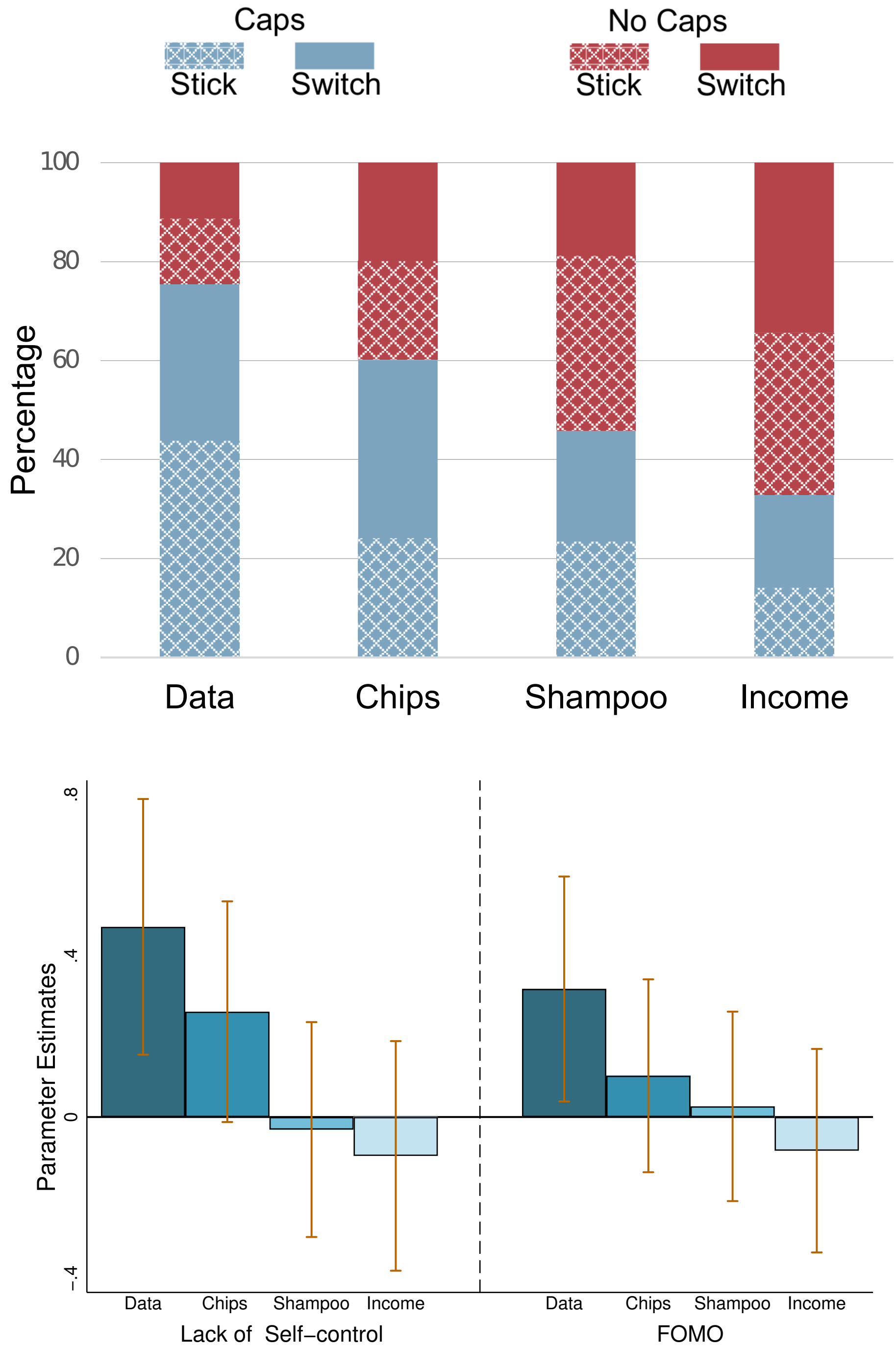


Figure 12: Preference for the Capped Plan and Correlation with Self-control and FOMO

Left Panel: Sample percentage that stick to the lump sum and daily-capped serving options, and percentage that switch, if the unchosen option's price drops. (7 stated no choices for income.) *Right panel:* Parameter estimates from eight logit regressions, 2 for each product offered (dependent variable: indicator of preference for daily-caps, independent variable: self-control or FOMO.) Bars correspond to estimates and error bands reflect 95% confidence intervals.

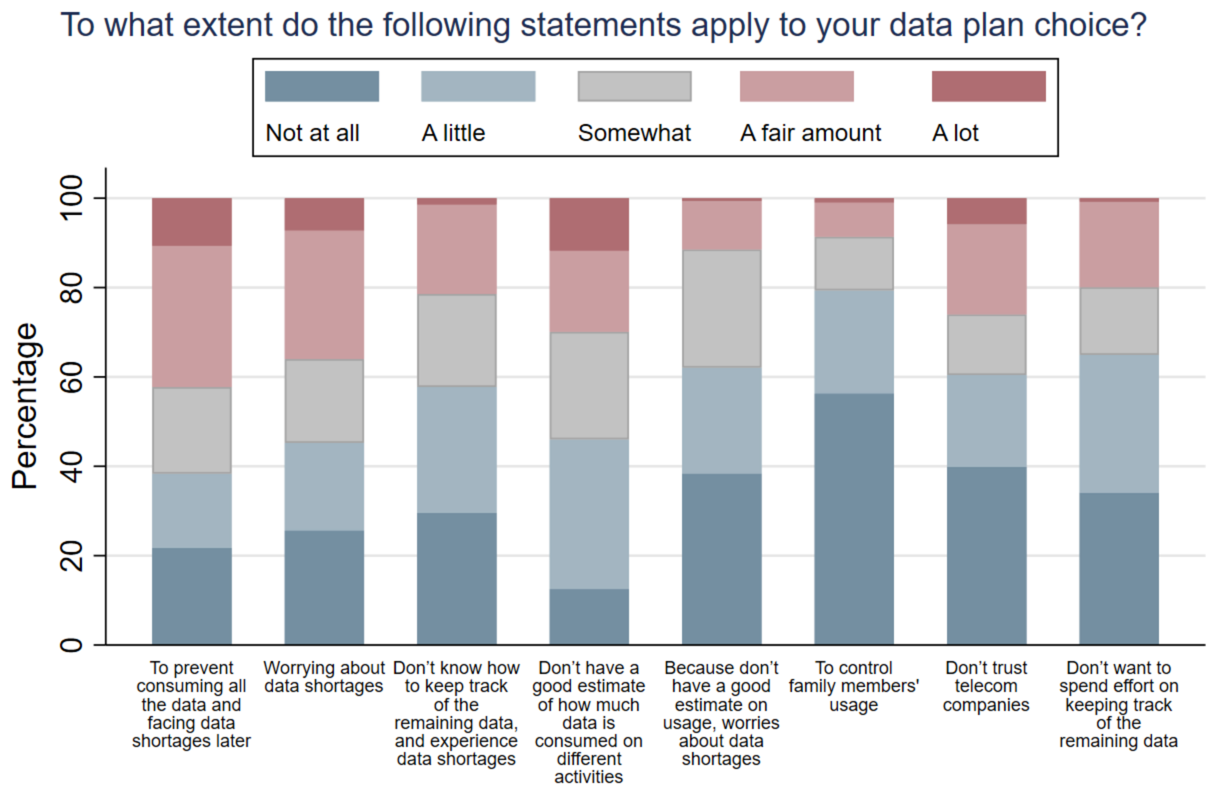


Figure 13: Stated Reasons for Preference of the Capped Plan

Notes: In the endline survey, we presented participants with 8 potential reasons why they might choose the capped plan, and asked to what extent these statements applied to their plan choice. The figure above displays the results, for those who preferred the capped plan.

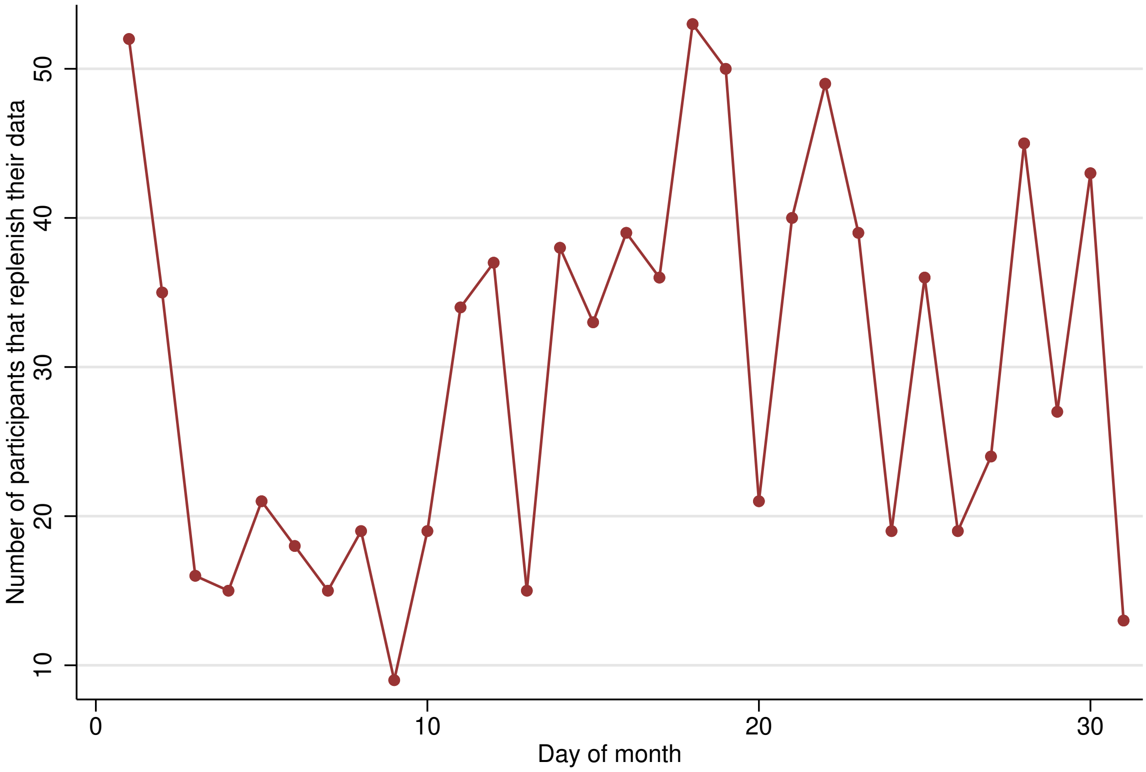
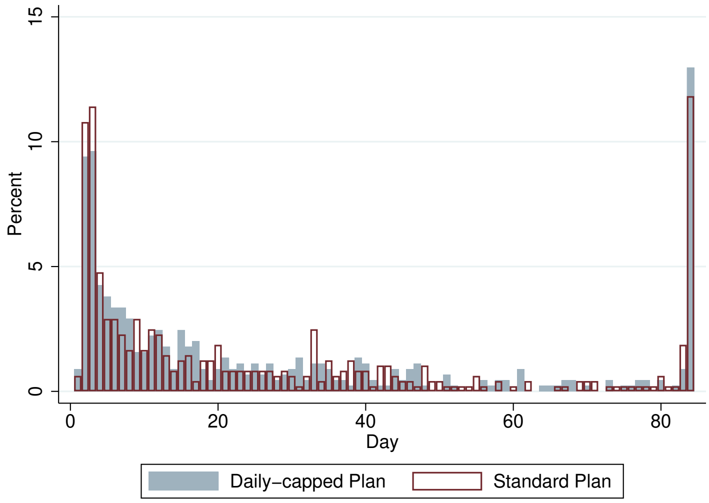


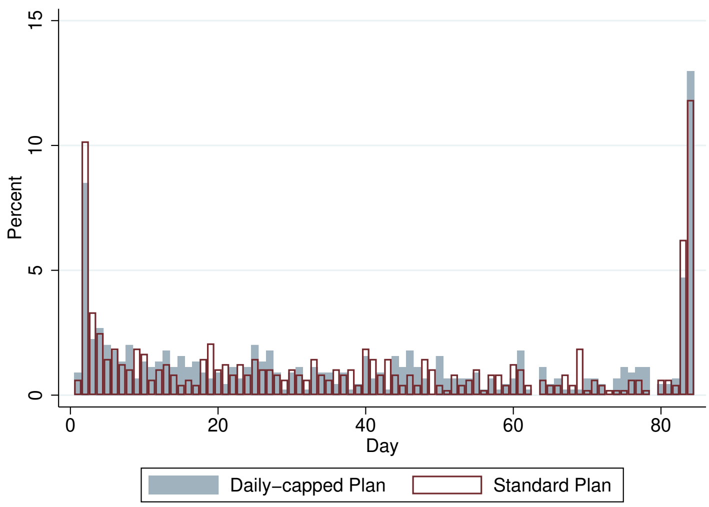
Figure 14: Mobile Data Replenishment by Day-of-Month

Notes: This panel presents the number of enrollments by the day of month on which a participant enrolled into the experiment. As participants are enrolled when they come to a telecom store to recharge or buy a new plan, this graph depicts the distribution of participants' data replenishment timing by day-of-month.

6 App Data and Other Robustness Tests



(a) Duration (in days) of the first contiguous spell of app data observations



(b) Total number of app data observations (in days)

Figure 15: Histograms of Days of App Data in our Sample

Notes: These figures are histograms of the daily number of observations collected by our usage-tracking app. Blue bars represent the daily-capped data plan arm and red bar frames indicate the standard plan arm. The top figure depicts the distribution of the number of days until the first time a no-usage day (i.e., a blackout) is observed for a respondent. On a no-usage day either the respondent did not use her phone (or a phone breakdown occured) or our usage-tracking app failed to capture data. The peak in the last day is due to respondents who have positive usage data for all 84 days of the experiment. The bottom figure shows the distribution of the number of app data observations per participant (measured in days). For each histogram, to compare the empirical distribution in the daily-capped plan arm and the standard plan arm we conducted Wilcoxon signed rank test. Comparing the two trial arms, we find no statistical evidence of a difference in the distribution of the number of days until the first time a no-usage day is observed (p=0.45), or in the distribution of the number of app data observations (p=0.67).

Table 15: Effects of Data Caps on No-usage Occurrence

	At least one no-usage day (1)	Days until the first no-usage day (2)	Days until the first no-usage day *survival model* (3)	Total number of app data days (4)
Daily-capped Plan	-0.11	0.038	-0.014	0.74
	(0.199)	(1.891)	(0.070)	(1.959)
Observations	929	929	929	929

Notes: This table presents robustness tests on app data no-usage day occurrences. In each regression specification, the dependent variable is regressed on the treatment indicator. Robust standard errors are in parentheses. Column 1 is a logit regression in which the dependent variable is an indicator for at least one no-usage day for a participant. Column 2 is an OLS specification in which the dependent variable is the duration (in days) of the first contiguous spell of app data – from day 1 in the experiment until the first no-usage day, for a participant. Column 3 is an exponential proportional hazard model specification for the duration the first spell of app data. Lastly, in Column 4 the dependent variable is the total number of days of app data for a participant.

Table 16: Effects of Data Caps on Smartphone Usage — Days Until First No-usage Day

	Total time use (mins) (1)	YouTube time use (mins) (2)	Social Media time use (mins) (3)	Social Media count (4)	App diversity (5)	WhatsApp time use (mins) (6)	Contacts (or Phone) time use (mins) (7)	Comm. apps time use (mins) (8)
Daily-capped Plan	4.854	-11.405**	-12.138**	-4.249**	-0.305	-0.339	9.896***	8.595*
	(12.587)	(5.789)	(5.249)	(1.970)	(0.385)	(1.988)	(2.746)	(4.471)
Weeks 3-4	-16.604**	-11.930***	-1.369	-0.168	-0.176	-3.467***	-1.817	-5.366***
	(6.450)	(3.702)	(2.078)	(0.503)	(0.157)	(1.162)	(1.116)	(1.986)
Daily-capped Plan	22.097***	10.605**	0.892	-0.009	0.086	6.189***	3.522**	11.431***
× Weeks 3-4	(8.106)	(4.246)	(2.631)	(0.698)	(0.217)	(1.684)	(1.733)	(2.999)
Observations	24,637	24,637	24,637	24,637	24,637	24,637	24,637	24,637

Notes: This table presents the smartphone usage regressions that use data from the usage tracking-app. The sample in this table includes all days in the first contiguous spell of app data for each user (i.e., all days until the first no-usage day). Therefore, the results in this table should be unaffected by any potential effects of no-usage occurrences (provided that Table 15 shows that the occurrence of no-usage days does not systematically differ across treatment and control arms). Each regression includes an indicator for the treatment arm, an indicator for the second half (Weeks 3-4) of a plan, their interaction, date fixed effects and a vector of control variables, as in Table 12. Standard errors are clustered at the individual level. Columns 3 and 4 are count measures. In all other columns dependent variables are duration, measured in minutes. The first day of the first 4-week plan is excluded (to eliminate bias due to pre-enrollment usage). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

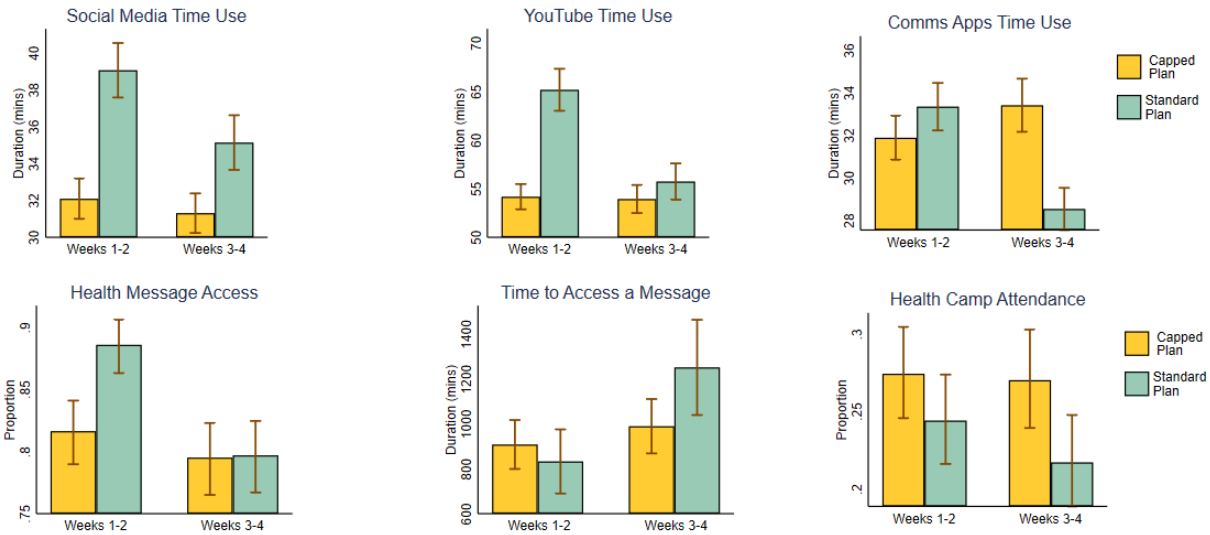


Figure 16: Adversarial Bounds for the Information Access and Smartphone Time Use Measures

There were 33 individuals who rejected to join our study due to not being interested in the next stage of the experiment. Of these, 8 stated that they did not have time, 20 stated that they had no particular reason, and 5 stated no reason. There is no reason to believe that people would hesitate to mention aversion to the capped plan as a reason for not joining. Nevertheless, we present worst-case scenario bounds for our main results using a standard econometrics approach (e.g., see Horowitz and Manski 2000), which assumes that these 33 individuals behaved in a highly adversarial fashion. Under this adversarial regime, we assume that had we not informed individuals about the plans we provide in the experiment, these 33 individuals would have enrolled into our study, and, furthermore, would have been allocated to the capped plan. As we have no data on this sample, we imputed all the missing information with worst-case values. We find that our results remain similar, despite using the highly adversarial data generating process describe above.

Intuitively, in what follows, we pretend that these 33 individuals followed an adversarial data generating process, where they were in the capped plan and:

- never accessed a WhatsApp health camp message
- never attended a health camp
- exhibited a preference towards the standard plan, even when it was more expensive
- used their smartphones exclusively for YouTube and social media until they reached the daily 0.5GB cap, every day. We impute 80 minutes on YouTube, 45 minutes on social media[a], 0 minutes on internet consuming communication apps, and the median duration of capped plan users for the phone app, which consumers no data. (Note, for comparison, that in the standard plan the average YouTube and social media usage is 56 minutes and 33.5 minutes, respectively.)

Since we pretend that these individuals did not access our WhatsApp health camp invitation messages, there would be no 'time-to-access a message' measure for this sample, and the results for this measure would remain identical to those reported in Table 2 Column 2 of the paper (because these observations would be excluded from the analysis). Nonetheless, to obtain a conservative lower bound for the time-to-access measure, regardless of these participants not having read the messages, we impute their time to access a message as the 90^{th} percentile of the time-to-access measure in the capped plan arm.

[a]Note that the spending 80 minutes on YouTube and 45 minutes of social media would, in fact, require more than 0.5 GB a day. 80 minutes of YouTube streaming with standard definition (480p) video quality consumes about 1GB, and 45 minutes of social media consumes about 0.2GB of data. Available at https://www.vodafone.co.uk/mobile/data-calculator and https://toolstud.io/video/filesize.php

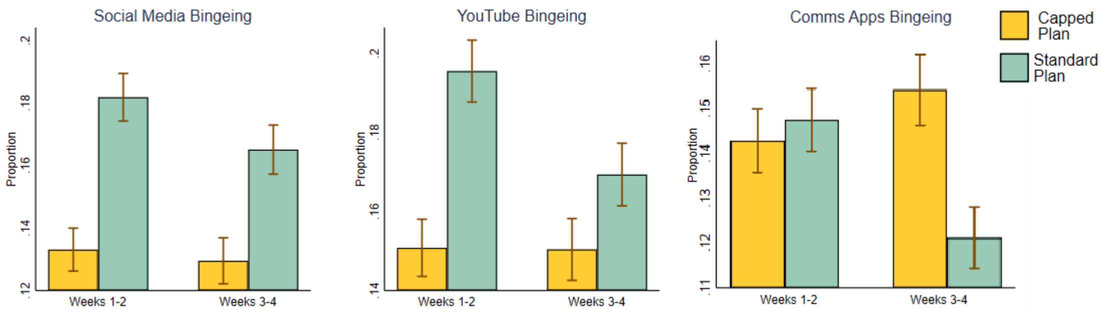


Figure 17: Binge Usage Based on a Bingeing Threshold of Twice the Average Usage

The Cambridge Dictionary defines bingeing as "to do something in a way that is extreme and not controlled".[a] Different people may differ on what constitutes bingeing, so it is hard to precisely characterize binge usage. Nevertheless, we drew a parallel to the Center for Disease Control (CDC) definition of alcohol binge usage. The CDC defines binge drinking as consuming 5 alcoholic drinks a day for men and 4 drinks for women.[b] An average U.S drinker consumes 2.2 standard drinks per day.[c] Taking the ratio of binge drinking to average drinking, one can define a bingeing threshold as consumption that is twice the average.

Using this definition of bingeing, we investigated the frequency of social media, YouTube and communications apps binge usage occurrences.[d] The results, presented above, are similar to what we find in our main analysis. We also tested a bingeing threshold of three times the average consumption. The results are qualitatively similar.

[a]Available at `https://dictionary.cambridge.org/dictionary/english/binge`

[b]Available at
`https://www.cdc.gov/alcohol/fact-sheets/binge-drinking.htm#:~:text=1\%2C2\%2C3,a\%20severe\%20alcohol\%20use\%20disorder`

[c]Available at https://apps.who.int/iris/bitstream/handle/10665/274603/9789241565639-eng.pdf The average consumption of 2.2 standard drinks is higher than what is defined as moderate drinking. The dietary guidelines developed by the Department of Agriculture define 2 drinks per day for men and 1 drink per day for women as a moderate amount of alcohol intake. Available at `https://health.gov/dietaryguidelines/2015/guidelines/appendix-9/`. A potentially better definition of a bingeing cut-off might be the ratio of binge drinking to moderate drinking. We chose to use average consumption, instead, because in our sample, it is simple to calculate the average smartphone time use, whereas there is no standard definition of moderate smartphone usage.

[d]Note that we developed a binge usage cut-off for each of these measures (social media usage, YouTube usage and comms app usage). Therefore, one should not make comparisons of Y-axis values across these measures.

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