Adoption and Impact of Mobile Health Services: Experimental Evidence from Bangladesh

Md Ferdous Zaman Sardar*

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Abstract

In this paper, I provide experimental evidence that the adoption of a beneficial healthcare technology can be increased by nudging people to try it once. Though mobile health services (MHS) are freely available from trained health professionals in Bangladesh, very few rural households use this service. I conduct a cluster randomized controlled trial where households randomly receive information about the MHS, are encouraged to save phone numbers of MHS providers, and are encouraged to try the service once. I find that all treatments improve awareness, but only experimentation leads to higher adoption of MHS both in the extensive and intensive margins. Using random assignment into treatments that lead to higher adoption of MHS as an instrument, this paper shows that the adoption of MHS decreases households' health expenditure, mostly due to the reduction in medicine expenditure. This happens because households, who adopted MHS, are also less likely to visit informal providers who often overprescribe medicine.

JEL Codes: I11, I15, I18, O12, O33, C93

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* Department of Economics, University of Washington. Email: fzsardar@uw.edu.
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1 Introduction

Due to a lack of access to qualified health professionals, most rural households in developing countries seek medical advice from untrained informal providers (Banerjee et al., 2004; Das et al., 2008; WHO and World Bank, 2017) . Theoretical and empirical literature in this area suggests overprovision of treatment by the informal providers (Emons, 1997; Gautham et al., 2014; Das et al., 2016). This is not only costly for individuals but also contributing to drug resistance which is a growing concern for global public health. With the growth of mobile phone subscriptions, healthcare services through mobile phones have the potential to transform health service delivery in rural areas. Though free consultation from qualified public healthcare providers is freely available over the phone, the awareness and adoption of these mobile health services (MHS) are surprisingly low among rural households in Bangladesh.

This paper studies why rural households keep seeking health advice from untrained informal providers when mobile health services (MHS) are freely available from qualified public healthcare providers and how they can be nudged into adopting the MHS. By implementing a randomized controlled trial, I show that awareness is not adequate for these households to adopt this beneficial technology due to credence and experience characteristics of healthcare services, but they can be nudged to adopt it by simply being encouraged to try the service once.[1] Using random assignment into treatments that lead to higher adoption of the MHS as an instrument, this paper further provides the causal effect of the adoption of the mobile health services. I find that the adoption of MHS decreases households' health expenditure, mostly by lowering their medicine consumption. This happens because the households who adopted MHS are also less likely to visit informal providers who usually prescribe overtreatment of medicine.

Healthcare has both credence and experience characteristics. Because of the credence characteristics, the private informal providers recommend too much treatment to maximize profit as profit is more at treatment than at diagnosis (Emons, 1997). The informal providers with low demand also set a low price for diagnosis and spend more time with the customer with its idle capacity (Emons, 1997). A customer can only assess the quality of experience characteristics (e.g.,

[1] Experience characteristics of a good are those which a buyer can observe only after consumption. For example, the politeness of the healthcare provider. Credence characteristics are those which the consumer cannot ascertain even after the consumption. For example, medical treatment.

interactions with the providers, short waiting time, etc.) and, therefore, feel satisfied with their providers (Banerjee et al., 2004). Loyal customers of a service with experience characteristics where they have strong interpersonal connections with the providers are less likely to try alternatives (Oliver, 1999; Hausman, 2004).[2]

Experimentation allows a potential user of a profitable technology to learn the benefits in the short run and adopt the technology in the long run (Dupas, 2014) . In the setting of this study, when a rural household tries the MHS once, they can learn about value (i.e., how conveniently one can talk to a qualified health professional). This learning might lead to the adoption of this technology. Social learning can also change the adoption of new health-related technology (Oster & Thornton, 2012; Miller & Mobarak, 2015).

To test whether information, experimentation, and social learning can change the adoption of mobile health services (MHS) I conduct a cluster randomized controlled trial among 2900 rural households from 580 *para*s (neighborhoods) in Bangladesh. The *para*s were randomly assigned into a control group or one of the following three treatments – (1) Treatment 1 *para*s: treated households received information about the available free public health services through a mobile phone verbally by the enumerators and a flyer containing the same information, (2) Treatment 2 *para*s: in addition to the treatment 1, treated households in these *para*s were encouraged by the enumerators to save the phone numbers of mobile health service (MHS) providers, and (3) Treatment 3 *para*s: in addition to the treatment 2, treated households in these *para*s were encouraged to experiment with MHS. In each of the treated *para*s, one household was randomly selected to not receive any treatment to measure the effect of social learning.

I find that around two months after the intervention the treated households are 33 percentage points more likely to be aware of one of the free public health services through a mobile phone than the households in the control *para*s. Saving the phone numbers of MHS providers does not have any additional effect on awareness. Experimentation increases awareness by additional 6 percentage points. There is also evidence of within-*para* spillover as the control households in the treated *para*s are 12 percentage points more likely to be aware of these services than the households in control *para*s.

[2] Because of their strong local roots, informal providers are socially connected with most of their patients (Gautham et al., 2014).

I find that experimentation plays a significant role in the adoption of this service. Only providing information does not have any effect on adoption. Encouraging participants to save the phone numbers of MHS providers has no additional effect on adoption. However, encouraging to experiment with MHS has a large effect on adoption. In-person encouragement made 63% respondents to attempt to try the service during the intervention. Among the participants who successfully talked to a MHS provider during the intervention, 31% used the service in the following two months compared to only 10% households among the households who received information used the service during the same period. Households which were given information and encouraged to both save the numbers and try MHS at the baseline are 25 percentage points more likely to receive the service than the households in the control *para*s. I do not observe any within-*para* spillover effect on adoption.

As experimentation leads to higher adoption of MHS, it also lowers the health expenditure by 23%, mostly the expenditure for medicine. These decreases in health expenditures are due to 21% fewer visits to informal providers who usually recommend overprovision of medicine. Using random assignment into treatments that lead to higher adoption of mobile health services (MHS) as an instrument, I show that the adoption of MHS significantly decreases households' health expenditure and visit to informal providers for consultation. As a result, households' satisfaction with the existing public healthcare system for the rural populations improves.

In the literature of technology adoption, this paper makes two contributions. First, it shows that information does not work for a reason not previously studied. Information usually was not sufficient because of the cost of the technology adoption (Abdul Latif Jameel Poverty Action Lab (J-PAL), 2018). Various non-price impediments can also cause the lower adoption of profitable free technology.[3] In this paper, I show that information alone cannot persuade satisfied customers of a credence good to switch to a better alternative. For the same reason, social learning also does not have any effect on adoption.

The second contribution of this paper to the literature of technology adoption is that adoption of a profitable technology with both credence and experience characteristics can be significantly improved by nudging people to try it once. If the customers can be nudged to experiment with the

[3] For example, intra-household externality (Mobarak et al., 2012), convenience and various socio-cultural factors (Thurber et al., 2013).

technology once they can learn about the experience characteristics and if they perceive it profitable, they adopt it. This is consistent with the literature that shows experimentation increases adoption by improving learning (Foster & Rosenzweig, 1995; Dupas, 2014; Bryan et al., 2014).

Another major contribution of this paper is estimating the causal effect of access to mobile health services on households' health decisions and health expenditure. To the best of my knowledge, there currently exists no experimental evidence on the impact of health services through mobile phones on households. There exist some empirical works on the use of SMS feature of mobile technology for healthcare delivery in a developing country setting (Mohammed et al., 2016; Liu et al., 2015; Shet et al., 2014; Jamison et al., 2013; Banarjee et al., 2020). This is the first paper to provide rigorous evidence that the adoption of mobile health services can lower people's health expenditure and medicine consumption. Finally, the paper also provides empirical evidence on how access to formal healthcare service affects the demand for informal healthcare providers in developing countries.

This study also has significant policy implications. With the continued growth in coverage of cellular phone networks and the rise in mobile phone subscriptions, most governments, donors and developing agencies around the world saw the potential to transform health service delivery. Therefore, as a complementary strategy to achieve the health-related Millennium Development Goals (MDGs), many countries "*began to invest considerable resources in mHealth, even in the absence of high-quality evidence*" as noted by WHO mHealth Technical and Evidence Review Group (mTERG). The findings of this study can guide decision-makers in low- and middle-income countries (LMICs) to improve adoption of mobile health services among rural households.

The finding that improving access to formal healthcare can decrease medicine consumption also has significant policy implications not only for rural households but also for global public health. Overuse of medicine is the main driver of the development of antimicrobial resistance (AMR). Declaring AMR as one of the top 10 global public health threats facing humanity, WHO appealed for urgent multisectoral action to contain AMR. Both improving access to quality healthcare and addressing the growing AMR are among the Sustainable Development Goals (SDGs) set by the United Nations.

2 Context

2.1. Healthcare in rural Bangladesh

Rural households in Bangladesh receive healthcare services from the public sector, private sector, and NGO sector.[4] Rural public healthcare facilities comprise *Upazila* Health Complex (UHC) at the sub-district level, Union Subcenter and Union Health and Family Welfare Center at the union level and Community Clinics at the village level. Though the number of rural healthcare facilities has increased in recent years, most of those lack healthcare workers, necessary equipment, and supplies. Since most of the public healthcare professionals also work at private clinics, they often remain absent in public healthcare centers, even more so in smaller facilities (Chaudhury et al., 2006). When present, the doctors in public facilities often provide poor service, and sometimes charge unofficial fees (Kremer and Glennerster, 2011).

Therefore, most rural households turn to private providers for healthcare (BBS, 2019). Most of the formal providers in private sector practice in urban areas requiring households to travel for service. These qualified private providers also charge significantly higher fee for consultation. Around 23% of rural patients seek treatment from these providers. Since distance (37%) and cost (23%) are the top reasons for preferring healthcare providers, according to HIES 2016 (BBS, 2019) most rural households (58%) choose local informal providers for healthcare services.[5] These informal providers are the local medicine/drug sellers and self-declared "doctors" without any formal training. Almost all medicine/drug sellers provide free consultation and almost always available with the lowest waiting time. The rural non-qualified doctors charge a very small fee for consultation and mostly practice in a rural dispensary/pharmacy many of which they themselves own. Most of these rural providers do not have any diagnostic facilities. So, these informal providers have strong incentives to overprescribe medicine and under-prescribe tests (Emons, 1997). Inappropriate treatments such as over prescribing, prescribing multiple, unnecessary and expensive drugs, and overuse of antibiotics and injections are commonly observed in many studies (Guyon et al., 1994; Ronsmans et al., 1996; Ahmed & Hossain, 2007). For example, fever (54%) is by far the number one illness among the rural households (BBS, 2019). In a study in northern part of Bangladesh, Ahmed & Hossain (2007) found that for treatment of fever 77% of the rural doctors and 79% of drug store salespeople prescribe antibiotics for 5 days and 4.5 days, respectively. In comparison only 11.2% community health workers (who received basic preventive

[4] Less than 1% received treatment from providers of NGO sector as reported in HIES 2016 by BBS (2019).
[5] These figures are consistent with what Das et al. (2016) found in India where 83% go to private providers and 60% get their treatment from informal providers.

and curative health care training from formal institutions) prescribe antibiotics for 3.7 days. Therefore, cost of medicines (57%) is the largest item in total health expenditure of rural households (BBS, 2019).[6] This inappropriate and overprovision of medicine by informal providers is not only creating a health expenditure burden on the rural households, but also, in the long run, contributing to drug resistance which is a growing global threat to public health.

2.2. Health services through mobile phones in Bangladesh

With the continued growth in coverage of cellular phone networks and the rise in mobile phone subscriptions, governments around the world saw the potential to transform the health service delivery. As a complementary strategy to achieve the health-related Millennium Development Goals (MDGs), many countries "*began to invest considerable resources in mHealth, even in the absence of high-quality evidence*" (WHO, 2021). A 2009 global survey by WHO among member countries found health call centers as the top mHealth initiative (WHO, 2011). Like many other countries, Bangladesh government also inaugurated health services through mobile phones for the public.

2.2.1. From local public hospitals

In 2009, free medical advice over the phone from the doctors at the local public hospitals (*Upazila* Health Complex, District Hospital) became available for the public in Bangladesh. The Directorate General of Health Services (DGHS) provided mobile phones to all district and *upazila* (sub-district) hospitals and instructed to make sure that a doctor in those facilities receives call 24/7.

2.2.2. From national health call center (*Shasthya Batayon*)

The Ministry of Health and Family Welfare established a centralized national health call center (*Shastho Batayon*) in 2016. Under the supervision of the MIS department of DGHS, Synesis IT Limited, a private firm, operates the call center with qualified doctors and trained health information service providers. Anyone in Bangladesh can call at the five-digit number 16263 anytime to get the following services:

- Doctors' Advice and Treatment
- Health Information

[6] The share of expenditure on drugs is only around 10% in the USA as reported in National Health Expenditure Accounts (NHEA) 2019.

- Ambulance information
- Filing complaints about any public or private health services
- Reporting accident

2.2.3. Awareness and adoption of the MHS in Bangladesh

The phone numbers for health services from the local hospitals were publicized in the local community using various local media, mosques, temples, etc. and displayed in local hospitals, clinics, markets etc. (WHO, 2011). The DGHS shared the phone numbers of local MHS providers in its website. The DGHS also publicized the health call center nationally by advertising in print and electronic media. However, in my data, I find very few (less than 5%) rural households are aware of a service where one can get health advice from a qualified doctor over the phone. When requested to name one such service, not even 1% mentioned one of the public mobile health services.

Administrative data from the DGHS suggests that most *upazila* hospitals usually receive around 2-7 calls per day whereas around 300,000 people live in a subdistrict. Use of centralized health call center is relatively higher, but only among urban households. In my data, only 0.62% households talked to a doctor over the phone in the month prior to intervention.

2.3. Usage and effectiveness of mobile health services

mHealth (short for mobile health) is a medical and public health practice which can be supported by mobile devices. There are a wide range of mHealth services, but many of those require advanced functionality and might not be suitable for a large portion of the rural population in the developing countries (Nahar et al., 2017). Mobile health services which use and capitalize a mobile phone's core utility of voice and short messaging service (SMS) might be more suitable in rural context. Therefore, health call centers were the most popular mHealth initiatives among WHO member countries (WHO, 2011).[7] However, there are limited rigorous evidence on adoption and impact of this service. A 2009 global survey by WHO among member countries reported the lack of empirical evidence on mHealth adoption and effectiveness as one of the key barriers to mHealth implementation. The WHO mHealth Technical and Evidence Review Group (mTERG)

[7] Health call center is a mobile health service that delivers health care advice triage services by trained health professionals over the phone.

acknowledge that "*there is considerable demand from ministries of health, donors, and decision-makers for evidence-based guidance*" on adoption of mHealth solutions (WHO, 2021).

There exists some empirical works on the use of SMS feature of mobile technology for healthcare delivery in a developing country setting. To improve medication adherence, Mohammed et al. (2016) sent daily SMS reminders to random participants in Karachi, Pakistan and found no effect. Liu et al., (2015) and Shet et al. (2014) also had a similar finding where text messaging reminders did not improve medication adherence among TB patients in China and had no effect on time to virological failure or ART adherence in India, respectively. In Uganda, Jamison et al. (2013) provided reliable information about sexual health via SMS to improve sexual health knowledge and safer sexual behavior and found no effect. A large-scale messaging campaign in West Bengal, India improved COVID-19 preventive behaviors (Banarjee et al., 2020).

There is a growing literature that studies the adoption of products that use mobile technology in other areas such as agriculture, banking, etc. in developing countries.[8]

3 Conceptual Framework

3.1. Healthcare as a credence good

The information asymmetry between the providers and patients makes healthcare a credence good where the patients cannot be certain of the quality of the treatment even *ex post* (Darby and Karni, 1973). This information advantage creates an incentive for the opportunistic provider to recommend treatment which is profitable to the provider. If profit is more at treatment than at diagnosis, the profit maximizing provider will recommend too much treatment (Emons, 1997). This is applicable for the rural informal providers who earn profit mostly from treatment than diagnosis, and therefore, mostly provides overtreatment (see Kremer and Glennerster (2011) for a review).

One might think that because of their strong local roots (Gautham et al., 2014), the informal providers should provide the appropriate treatment to maintain their reputation. A field study by

[8] Cole & Fernando (2021) study demand for advisory service for cotton farmers in India, Muralidharan et al. (2021) evaluate phone-based monitoring of a last mile service delivery program, Darko-Osei et al., (2021) study adoption of mobile banking in rural Ghana.

Schneider (2012) shows that because of credence characteristics, the providers do not treat customers differently due to reputational concern.

However, when the customers signal knowledge about the service, they are less likely to get overprovision of treatment. Currie et al. (2011) show that the signal of knowledge about inappropriate antibiotic use lowers the probability of receiving an antibiotic prescription from 64% to 39%, and thereby, reduces drug expenditures as well.

If demand is more than capacity, a provider can be honest if and only if their profit is equal from diagnosis and treatment (Emons, 1997). This is the case for the MHS provider as they get paid by the government and their profit from both diagnosis and treatment is zero. So, they have very little incentive to recommend overtreatment.

When the demand is less than the capacity, the healthcare provider will charge only marginal cost and provide unnecessary treatments using idle capacity (Emons, 1997). Therefore, the informal providers charge very minimal fee for consultation (in my data) and spend more time patients (Das, Holla, et al., 2016).

3.2. Perceived quality of healthcare and patients' satisfaction

Quality of healthcare depends on both technical (clinical) and functional (non-clinical) elements. The technical aspect focuses on the skills, accuracy of procedures and medical diagnosis and the functional aspect refers to the way health services are delivered. *Ex post* though the patients cannot assess the clinical quality, they can evaluate the non-clinical aspects of the service delivery like waiting time, provider-patient interactions, etc.

The functional aspects of healthcare delivery are better at the informal providers. The wait time is significantly less at informal providers compared to formal providers (BBS, 2019). These informal providers also spend much more time with the patients (Das, Chowdhury, et al., 2016) which is consistent with the theoretical prediction of Emons (1997).

When patients find expected social elements (responding to emotions, fostering relationships, etc.) in their interactions with the providers, they evaluate the overall service more favorably (Bogart et al. 2004; Glanz, 2008). In my data, I observe that the rural households who usually go to their local informal providers are satisfied with the service which is consistent with what Banerjee et al. (2004) found in rural Rajasthan, India. More interpersonal interactions with the providers not only make the patients satisfied, but also make them stay with the current providers (Hausman, 2004; Russell-Bennett 2010).

3.3. Adoption of a new technology

In the setting of this study, very few rural households are aware of the available public health services through mobile phone. When households lack information about a health product, they might respond to information (see Dupas (2011)for a review). However, sometimes information alone is not sufficient in changing adoption of health products (Kremer & Miguel, 2007; Meredith et al., 2013). One possible reason for this can be the cost (J-PAL, 2018). However, this should not be a constraint here as the MHS is free.

Low adoption of a new technology can also arise due to lack of awareness of returns or how to use the technology (see Foster & Rosenzweig (2010) for a review). Experimentation allows a potential user of a profitable technology to experience the benefits and learn the value. Dupas (2014) provides evidence that learning from experimentation plays an important role in adoption of an experienced good, an improved antimalarial bed net, in Kenya. Social learning can also contribute to adoption of new health-related technology (Oster & Thornton, 2012; Miller & Mushfiq Mobarak, 2015).

4 Research design

In the setting of this study, the participants are not aware of a free profitable technology and therefore, may respond to information. However, due to their loyalty to the current providers, many may not even try this technology (Oliver, 1999). Experimenting this service once may allow participants to learn about the value of this service and continue using it in the future. Therefore, I design an experiment with multiple arms of information and encouragement to experiment.

4.1. Treatment arms

This study uses one control and three treatment arms:

- Control: receive no intervention
- Treatment 1 (T1): receive information about these services and phone numbers verbally and with a flyer
- Treatment 2 (T2): T1 + enumerators encourage the participants to save the phone numbers

- Treatment 3 (T3): T2 + enumerators encourage the participants to make a call at one of the MHS providers

4.2. Randomization

First, the random treatment is assigned at the cluster (*para*) level to maintain Stable Unit Treatment Value Assumption (SUTVA). The second level of randomization is done at the household level to measure within-*para* spillover. In each of the treated *para*s/clusters, randomly selected four out of five households received treatment, leaving one household as a within-treatment control to identify within-para spillover effects.

The randomization was done in Stata and stratified by administrative unit (*upazila)* as treatment implementation may vary by region (Bruhn & McKenzie, 2009).

4.3. Sample selection

This study takes place in rural setting in Bangladesh. 600 *para*s (neighborhoods) from 30 upazilas (sub-districts) covering all 8 divisions of Bangladesh are selected as the study areas. The number of *para*s is restricted to 20 in each *upazila* to ensure the intervention does not create an extra burden for the local mobile health service providers. To limit contamination, the *para*s are selected dispersedly – limiting only two *para*s from each village and only two villages from each union. In each *para*, five households that satisfy the following inclusion criteria and agrees to participate are surveyed by the enumerators:

- At least one member of the household has a mobile phone, and
- The household has at least one child under 5 years of age.[9]

5 Baseline data collection and characteristics

5.1. Data collection

The baseline survey was administered in late July and early August in 2021. To collect the household and neighborhood (*para*) data, 30 enumerators were recruited through a local survey firm, Apprentice Consulting. These trained interviewers visited households in their respective *upazila*s and collected data on their phone using KoBo Toolbox survey software. All household related information is collected from a single member. Where available the head of the households

[9] According to BBS 2011 census data, 43.3% households in Bangladesh have at least one child below the age of 5.

were interviewed, but in absence of the head, any other adult member was interviewed. To collect the neighborhood level data, the enumerators interviewed a local expert, someone who is educated, lives there for a long time and is well aware of the *para* and the households. The backchecks consisting of around 10% of the survey were performed over the phone, with a focus on non-changing information.[10] All enumerators were made aware of the backchecks *ex ante*. Finally, 2900 households from 580 *para*s in 29 *upazila*s were surveyed at the baseline.

5.2. Baseline characteristics and balance

Table 3 presents baseline characteristics of some of the key variables and tests for balance. Most of the characteristics are not significantly different across treatments at the 10% level. The only variables which are not balanced are the following: whether the respondent was the head of the household, the education level of the respondent and satisfaction with rural public healthcare. As mentioned in my pre-analysis plan, I will control for these variables in my analysis. I also perform a joint orthogonality test for each treatment separately, i.e., whether all the characteristics are jointly zero. At 10% level of significance, I cannot reject overall balance.

In 43% of the households, the head of the household was interviewed. Around 60% of the respondents were female. Average household size was 5.35 which similar to 2011 census data (avg. household size with kids under the age of 5 is 5.24). 62% of the households have more than one phone. In 47% households, no member uses smartphones.

Informal providers are the main source of health-related information in rural households in Bangladesh (Figure 1). When the households have a health-related query, 90% of them ask either local medicine sellers or rural doctors. Around 40% of the households go to formal providers. Only 1.55% search the information from the internet and only 2.2% call to a MHS provider or their hospital.

When these households need to see a healthcare provider for treatments, 57% of them usually go to informal providers and 43% go to formal providers. This is consistent with the Household Income and Expenditure Survey 2016 by Bangladesh Bureau of Statistics (BBS) which reports

[10] After finding inconsistency between enumerator reported data and backcheck data and data from further verification by the supervisor in one of the *upazila*s, all households from that *upzila* were excluded from the sample. Since I identified this very early, I added another *upzila* in my sample immediately to maintain the power of the study.

that 58% of rural households visit informal providers for treatment.[11] This is also similar to what Das et al. (2016) finds in rural India where 60% of the households go to informal providers for primary care. As shown in Table 1, mean fee, travel time and travel cost are significantly lower to see informal providers than to see formal providers. Households are highly satisfied with their current providers, and it does not vary by whether they see a formal or an informal provider. Households being satisfied with their current providers, even the informal ones, is consistent with the findings of (Banerjee et al., 2004).

64% households reported that some members of their households were sick in the previous month. Among these households 98% took some actions, e.g., sought advice from health service providers, purchased medicine, etc. Among these households, 87% received medical advice from various informal providers in the previous month while 45% received services from formal providers. 36% received service from both formal and informal providers. 51% received service from only informal providers and only 9% received health advice only from formal providers.

On an average, rural households spent 1,893 Bangladeshi taka which is 12% of their monthly income in the previous month. This is higher than what is found in the HIES 2016 report. According to the HIES report, average monthly health expenditure is 1,192 Bangladeshi taka which is 9% of their monthly income.[12] This could be due to the survey being done at a time when the COVID-19 infection was at the peak in Bangladesh. The median health expenditure was only 500 taka which is around 4% of median household income. Consultation fees (visit) and transport cost were 10.4% and 8.2% of total health expenditure, respectively which is close to what HIES 2016 reported (11.7% and 9.7%, respectively).

The majority of the households (62%) are "somewhat satisfied" with the existing public healthcare system for the rural population. Only 10 are very satisfied and only 11% are dissatisfied. On a scale of one to seven (7 being the highest satisfaction), the average level of satisfaction is 5.3.

12% of the respondent reported that they already received the COVID-19 vaccine and 67% of the remaining were expecting/planning to get vaccinated in the future. More than two thirds of the

[11] HIES 2016 reports that 25.06% of the rural households receive treatment from non-qualified doctors and 32.79% from drug sellers (BBS, 2019).
[12] Health expenditure is calculated by adding avg. visit fee, avg. medicine cost, avg. test cost and average transport cost.

respondents reported they do not know the process to get the COVID-19 vaccine.13 The willingness to get vaccinated is much higher among the ones who know the process compared to those who do not know (91% vs. 59%).

Only 4.62% of the households are aware of existence of health services through mobile phone. Only 0.62% of the households used MHS for treatment in the previous month. 54% of the respondent said they would have talked to a healthcare provider over the phone right at that moment if there were such opportunity indicating high demand of MHS service. The demand is relative higher among the households who usually go to informal providers for treatment than those who usually go to formal providers (57.3% vs. 49.6%). On an average, households are willing to pay 61 Bangladesh taka to receive this service once (median 20 Bangladeshi taka). Around 28% households are willing to pay more than one US dollar to receive health service through mobile phone once while 30% do not want to spend a single penny. The willingness to pay for MHS is higher among the households who usually go to an informal provider for treatment than those who usually go to a formal provider (64 BDT vs. 56 BDT). Male respondents are willing to pay 28% more than female respondent.

6 Intervention

6.1. Treatment assignment

The households of this study received their randomly assigned treatment immediately after the baseline interviews. The enumerators were given a list of unique household numbers and were trained on how to assign those unique numbers to the households they would interview. The list also contained information about the set of survey questionnaires assigned for each household. Based on the entered set, the survey software guided the enumerators to carry out the intervention.

Treatment 1 households only received information both verbally and with a flyer/leaflet. The flyer contained the information about the free health services they could get over the phone from the national health call center, their *upazila* hospital complex, and their district hospital. For the households assigned for the treatment 2, the enumerators first handed over the flyer/leaflet, informed them verbally about the free public mobile health services and then encouraged the

[13] Anyone interested to get COVID-19 vaccine first required to be eligible based on some criteria set by the government. If eligible, they need to sign up for vaccination using an app or online. Once a time slot is assigned, the person receives an SMS notification to arrive at a certain vaccination center.

respondent to save the phone numbers (which are mentioned in the flyer) on their mobile phone. The enumerators offered help to save the numbers for those who needed. Finally, for the households assigned for the treatment 3, after handing over the flyer/leaflet and informing about the MHS the enumerators encouraged the respondent to save the phone numbers (which are mentioned in the flyer) on their mobile phone and further encouraged to make a call at one of the phone numbers of the mobile health service providers. The respondents were free to ask their own queries. In case, the respondents did not have their own queries, the enumerators gave them some examples (e.g., their child having a fever, dysentery, etc.) of what they could ask[14]. In case the respondent's mobile phone was not present during the intervention, the enumerators offered their phone to make the call. The respondents were free to talk in front of the enumerators or in private. In some case, the enumerator helped to connect the call and/or the respondent talked using the loudspeaker. The enumerators kept detailed record of how the call experience was gone.

20 households received a different treatment by the enumerators than what was originally randomly assigned to them. All analysis is done excluding those 20 households as recommended by McKenzie (2018) in his World Bank blogs.

6.2. Compliance

I expected some non-compliance in two of the three treatments. For the information treatment, all assigned households received information as expected. However, there were concern of non-compliance in saving phone number and experimentation. Respondents assigned to save phone numbers of MHS providers may not save phone numbers for two reasons – (i) the phone not being present during the survey or (ii) they are free to choose not to save. The compliance was very high in this treatment. 81.55% of the participants assigned to save phone numbers saved at least one of the three phone numbers shared with them. The compliance does not vary statistically by whether the households are from treatment 2 and treatment 3. Among the participants who complied, 51% saved only one phone number while 23% saved all three phone numbers. Households showed preference for the phone numbers of the local providers. 81.64% of complied households saved the phone number of their *Upazila* (sub-district) health complex. The phone numbers of the District

[14] To address the concern that calling at those numbers for unnecessary medical advice like this might create additional burden for the health service providers, the study is designed in a way that would minimize it. In a upazila, maximum 8 households could receive the treatment 3 in a single day. Moreover, not all households made call, or made call at the same number. Also, more than half of the respondents had their own valid queries.

Hospital and *Shastho Batayon* (central call center) were saved by 52% and 39%, respectively, of the respondents who complied to this treatment. The main reasons for not saving the phone number were “will do later” (51%) and “phone not present” (38%).

Relatively more non-compliance was expected in experimentation treatment. In addition to the two reasons mentioned above for the saving phone number treatment, supply side issue could be another factor for non-compliance. Even if the assigned respondents make calls to MHS providers, some of those may not be answered due to poor service quality in public healthcare like absence of healthcare providers (Chaudhury et al., 2006), low effort (Das et al., 2016).

63% of the participants assigned into treatment 3 called at one of the MHS providers. Among those who did not try, 54% reported the main reason as “I will call later when I need it.” Among other reasons were phone not being present (19%) and “I don’t need it” (12%). Among those who earlier during the interview reported that given an opportunity they would talk to a healthcare professional over the phone right at that moment, 76% attempted a call compared to only 47% among those who were not interested to talk to a MHS provider.

Again, the participants showed preference for local providers with 64% choosing the numbers of their *Upazila* (sub-district) health complex for their first call. The rest of the calls were almost equally targeted to the phone numbers of the District Hospital and *Shastho Batayon* (central call center).

Finally, 47% (324 out of 694) of those who were assigned into this treatment (which is 74% of those who tried) were able to successfully talk to a MHS provider. 34 out of these 324 respondents talked after trying more than once. The top 3 reasons for the call not being successful were – (i) calls were not answered (42%), (ii) the numbers were busy (32%) and (iii) the numbers could not be reached (16%). 77% of the respondents were satisfied or very satisfied with the service they received.

7 Endline data collection and attrition

7.1. Data collection

Around 2 months after the intervention trained enumerators conducted in-person interview in late September and early October in 2021 to collect the endline data. In addition to the 3000 households from the baseline, 1500 new households were surveyed by interviewing 5 households from another *para* of each of the 300 villages. The same inclusion criteria have been applied for these new households.

Similar to the baseline, the backchecks consisting of around 7% of the survey were performed over the phone, with a focus on non-changing information.[15] All enumerators were made aware of the backchecks *ex ante*.

7.2. Attrition

To minimize attrition, the same enumerators were hired if available. The enumerators were given detailed contact information (physical address, phone numbers (two if available), name of the household head and the baseline respondent, occupation of the household head, etc.) of the baseline households. In the end, the attrition rate was very low (2.59%), and it does not vary by treatment status (as shown in Appendix Table A1).

8 Effects on Awareness and Adoption

8.1. Empirical specification

First, I estimate intent-to-treat (ITT) effects using a regression specification of the following form:

$$y_{ipuE} = \alpha + \beta_1 I_{ipu} + \beta_2 S_{ipu} + \beta_3 E_{ipu} + \gamma y_{ipuB} + \lambda X_{ipuB} + \delta_u + \varepsilon_{ipu} \qquad (1)$$

where, y_{ipuE} is the outcome of interest for household i in *para* p in *upazila* (subdistrict) u measured at the endline. I, S, and E are indicator variables each of which equals 1 if households received information, were encouraged to save phone numbers of MHS providers, and were encouraged to try the service once, respectively. Following McKenzie (2012), I include the outcome variable measured at the baseline y_{ipuB}, if available. Following Glennerster and Takavarasha (2013), I include X_{ipuB} which is a set of covariates which were not balanced at the baseline and might be correlated with the outcome variables. As randomization was stratified by *upazila* (subdistrict), I also include *upazila* fixed effect δ_u as recommended by Bruhn & McKenzie (2009).[16] To account for possible correlation in outcomes within *para*s, the error term is clustered

[15] After finding inconsistency between enumerator reported data and backcheck data and data from further verification by the supervisor in one of the *upazila*s, all households from that *upzila* were excluded from the sample.
[16] I did not mention including this fixed effect in my pre-analysis plan. I will check whether the effect changes due the inclusion of this strata fixed effect. If I find any significant changes, I will report both results in appendix.

at the *para* level which was the unit of the first level of randomization. For each of my results, I check robustness to alternative specification.

β_1, β_2 and β_3 identifies ITT effects of receiving information, being encouraged to save phone numbers of MHS providers, and being encouraged to try the service once, respectively. The ITT effect for treatment 2 households who received both information and were encouraged to save the phone numbers of the MHS providers is $\beta_1 + \beta_2$. Similarly, the ITT effect for treatment 3 households is $\beta_1 + \beta_2 + \beta_3$.

Since there was non-compliance during the intervention, I also estimate the local average treatment effects (LATE) which is the effect for those who were induced to save the phone numbers or experiment with MHS by the intervention. I estimate LATE of saving phone numbers by instrumenting whether the household saved one of the phone numbers during the intervention with their random assignment into treatment 2.[17] Similarly, I instrument whether a household talked to a MHS provider during the intervention with the random assignment of a household into treatment 3.

The experimental design of this study allows me to estimate the spillover effect of the intervention by comparing control households in the treated paras to the households in the control paras where none got treated. To estimate the spillover effect, I use a modified version of the estimating equation (1) by changing how the indicator variables I, S, and E are defined. Now each of these variables equals 1 if the household is in a *para* where the treated households received information, were encouraged to save phone numbers of MHS providers, and were encouraged to try the service once, respectively. β_1, β_2 and β_3 identifies within-*para* spillover effects. Since the randomization is done at the *para* level, there is a possibility that households in control *para*s can access information from the households in the treated *para*s of the same village. This suggests that the within-*para* spillover effect and the ITT estimates of the intervention are underestimating the effect.

8.2. Effect on awareness of mobile health services

[17] One needs to be careful with the interpretation of the IV estimates here. The intervention might induce households to save phone numbers on both the extensive margin and the intensive margin. On the intensive margin, the households may save more than phone numbers of more than one MHS providers and more than one member of a household may save the numbers.

I find that around two months after the intervention the treated households are significantly more aware of the free public health services through mobile phone than the households in the control *para*s. The Panel A of the Table 4 presents the intent-to-treat (ITT) estimates on different measures of awareness. Column 1 shows that the households who received information are 33 percentage points more likely to be aware of existence of health services through mobile phone than the households in the control *para*s. Encouraging to save or to make a call during the intervention does not have any significant additional effect on this measure of awareness.

The ITT effect is larger when the outcome variable for awareness is defined in a stricter way, e.g., whether they can name one of the providers of health service through mobile phone (column 2) or have a phone number of one of the providers (column 4). The households who received information are 38 percentage points more likely can name one of the providers of health services through mobile phones and 37 percentage points more likely to have a phone number of one of the providers than the households in the control *para*s. Encouraging to save phone numbers does not have any additional effect on these outcomes as well. Those who were encouraged to experiment with the service during the intervention are 7 percentage points more likely to have one of the phone numbers than the households who received information and were encouraged to save phone numbers. These results are robust to various specifications as shown in Appendix Table A2.

Table 5 reports ITT estimates of within *para* spillover. Control households in the treated *para*s are 9 percentage points more likely to be aware of the existence of any providers of health services through mobile phone and 12 percentage points more likely to have a phone number of one of the providers than the households in the control *para*s where no one was treated. The spillover effects do not vary by whether the control households are in a *para* where treated households received just information or were encouraged to save phone numbers or were encouraged to talk to a provider over the phone. Note that this is within *para* spillover. There could also be within village spillover, i.e., households in control *para*s can learn from households in their neighboring treated *para*s. If spillover happens across *para*s, then the reported ITT estimates are biased downward (Duflo et al., 2006) [within village spillover can be measured by comparing households in *para*s where no *para*s were treated in a village with control *para*s where the other *para* was treated. may not have enough power to detect the effect].

Though households in control group became aware of the existence of health services through mobile phone due to survey effect or/and spillover effect, their level/nature of awareness is different than awareness of the treated households. Around 96% of the households in the treatment 3 group (i.e., encouraged to experiment) who claim to know about the existence of health services through mobile phone at the endline can actually name one of the providers and 89% have a phone number of at least one of the three providers. In comparison, 80% of the households from control *para*s who claim to know about the existence of health services through mobile phone can name one of the three providers and only 55% have a phone number of one of the providers. This indicates information loss in the networks (Banarjee et al., 2021).

Panel B of Table 4 reports the IV estimates on awareness. These are the local average treatment effects (LATE) on awareness for those who were induced to save the phone numbers or experiment with MHS by the intervention. Saving the phone numbers still do not have any significant effect on any of the awareness measures. The IV estimates for the one who tried MHS during the intervention is larger than the ITT estimates reported in Panel A.

Households in all treatment groups systematically showed preference to have phone numbers of their local providers than distant providers – around 34% of the households have the phone numbers of their *upazila* health complex, 27% have phone numbers of their district hospital and 23% have phone numbers of the centralized health call center (Table 7). This is consistent with household's preference to save phone numbers of local providers over the distant providers during the intervention. [1. Add some literature for preference for local providers. 2. Interesting that people have a 11-digit local number instead of a 5-digit centralized and more publicized number]

Awareness significantly improved among the households in the control group too. Only 4.67% of the baseline participants were aware of the existence of any providers of health services through mobile phone. At the endline, 43% of the control households know about the existence of health services through mobile phone and 26% have phone numbers of at least one of the public providers. This significant increase in awareness among the control households could be due to within-village spillover (Duflo et al., 2006) or survey effect (Zwane et al., 2011).

I find the evidence of survey effects on awareness. As reported in Table 6, households in control *para*s are 19 percentage points more likely to know about the existence of any providers of health services through mobile phone and 17 percentage points more likely to have a phone number of one of the providers than the households which were not surveyed at the baseline. Note that these pure control (endline/new control) households are selected from new *para*s in existing villages[18]. Most of the non-changing household characteristics of these households are the same as the baseline households and the estimates shown in the Table 6 adjust for the variables which are not balanced. Only 24% of the pure/endline/new control households are aware of the existence of any providers of health services through mobile phone and 9% have a phone number of one of the providers. These point estimates are also subject to within village spillover effect as these households are selected from the same villages as the households surveyed at the baseline.

8.3. Effects on adoption of mobile health services

As outlined in the pre-analysis plan, the adoption of the free public health services through mobile phone is one of the main outcome variables of this study. Since many calls might not be successful due to various supply side reasons, I considered both attempts and successful calls as outcome variables. Among those who attempted, 87% successfully talked to a MHS provider. About 72.6% calls were successful which is around the same as the call success rate during the intervention (which was 73.4%).[19] As shown in Table 11, call success rate slightly varied by the MHS providers – from 70% success rate at the health call center to 75% call success rate at the *upazila* health complexes.

The intent-to-treat (ITT) estimates on various adoption outcomes are presented in Panel A of Table 8. I find that just information, though improved awareness, have no significant effect on adoption of this service (neither on attempts nor on successful call). This result is consistent with a number of studies that information alone is not sufficient in changing adoption of health products (Kremer & Miguel, 2007; Meredith et al., 2013). One possible reason for this can be the cost of the technology adoption (Abdul Latif Jameel Poverty Action Lab (J-PAL), 2018). However, this should not be a constraint here as the MHS is free.

Various non-price impediments can also cause the lower adoption of a profitable free technology. In a study in rural Bangladesh, Mobarak et al., (2012) finds only 69% households accepted a free improved cookstove due to the presence of intra-household externality.

[18] Households were surveyed in 580 paras from 290 villages at the baseline. Households of pure control (endline/new control) group are selected from a different *para* of each of the 290 baseline villages.
[19] The fact that 82% respondents successfully talked while 72.6 calls were successful indicates that some respondents tried more than once to talk to a MHS provider.

Convenience and various socio-cultural factors can also play a role in households' decision to adopt a technology (Thurber et al., 2013). In this setting, information has not been effective because households, who usually get their primary healthcare from the local informal providers, are satisfied with their service. Because of inertial repurchasing, these loyal patients are less likely to try alternatives (Oliver, 1999).

Encouraging people to save phone numbers also have had no additional effect on adoption. However, information and encouragement to save phone numbers (i.e., Treatment 2) jointly have significant effect, i.e., households who received information and were encouraged to save phone numbers are 5 percentage points more likely to adopt this service than the households in the control *para*s.

I find encouraging people to experiment with the service has a very large effect on its adoption – both in extensive margin and intensive margin. Encouraging to experiment with the service improved the likelihood to attempt to receive the service by 15 percentage points (column 1). The households who received information and were encouraged to save phone numbers and make a call at one of the MHS providers are 20 percentage points more likely to attempt to receive the service and 17 percentage points more likely to receive the service than the households in the control *para*s (column 3). They attempted 0.37 more calls (column 2) and received 0.25 more services (column 4) than the control households. The effects are even larger conditional on someone in the households were sick after the intervention. These results are robust to various specifications as shown in Appendix Table A2.

The IV estimates of saving phone numbers and experimentation on adoption are shown in Panel B. These are the average treatment effects on adoption for those who were induced to save phone numbers or experiment with the MHS by their random assignment into their respective treatment group. Saving number still does not have any effect on adoption and experimenting with the service has very large effect on adoption – those who experimented are 36 percentage points more likely to receive service and receive 0.53 more services than the households in the control *para*s.

Experimentation played two key roles which led to higher adoption: (1) it removed any uncertainty people had about this service, and (2) it allowed them to learn about the benefits about the experience characteristics of the service.

Though there are significant spillover effects on awareness, I do not find any spillover effect on adoption (Table 9). This spillover effect on adoption also does not vary by whether the within-treatment control household is in treatment 1 or treatment 2 or treatment 3 *para*s. As none of the treated households in treatment 1 *para*s and treatment 2 *para*s experimented during the intervention and very few treated households in these *para*s adopted the service, the control households in those *para*s might learn only about the existence of the service. But in treatment 3 *para*s, the control households should learn not only about the existence of the MHS service but also the experience of this service by the treated households. Unlike Oster & Thornton, (2012) and (Miller & Mushfiq Mobarak, 2015), I do not observe social learning to play a role in adoption of this health products. However, it is possible that the around two months duration between the intervention and the endline is not adequate for social learning or spillover to impact adoption. One could argue that with sufficient time social learning or spillover can have effects on adoption.

I also do not find any significant survey effect on adoption. There is very small survey effect on attempt to receive the health service through mobile phone but no effect on actual adoption (Table 10). This result is consistent with the effect of information treatment that just being aware of the service does not lead to adoption.

Households in all treatment groups attempted and received most services by calling at phone numbers of the upazila health complex showing preference for local providers. Higher call success rate at the upazila could also be the reason behind it. However, they called at the centralized health call center more than their district health complex. This could be due to the fact that the providers at upazila health complex and district hospital are substitutes – provide similar services, but the health call center provides some additional services.

8.4. Heterogeneity of effects

This section will be added in the next version.

9 Effects of Mobile Health Services

Now I study the effects of using health services through mobile phones on visits to healthcare providers, health expenditure and some other health-related outcomes. Due to lack of access to formal healthcare providers, most rural households used to receive health services from informal providers. Now as some households are using health services through mobile phones from the formal providers, this might change the way they used to seek consultation from various providers. This might also affect various health expenditures. In this section, I document the effect of MHS on rural households.

9.1. Empirical specification

First, I estimate intent-to-treat (ITT) effects using the estimating equation (1). Next, I show the local average treatment effects (LATE) which is the effect for those who were induced to use mobile health services by the intervention. This is a well-defined and policy relevant parameter of this study. I calculate the effect by estimating

$$y_{ipuE} = \alpha + \beta MHS_{ipu} + \gamma y_{ipuB} + \lambda X_{ipuB} + \delta_u + \varepsilon_{ipu} \qquad (2)$$

where MHS_{ipu} is an indicator of whether the households received mobile health services (MHS) in previous two months since the intervention. The other variables are defined the same way as in estimating equation (1). The endogenous choice of receiving MHS is instrumented with whether the households were randomly assigned into the nudge treatments (i.e., treatment 2 or treatment 3):

$$MHS_{ipu} = \alpha + \eta Z_{ipu} + \gamma y_{ipuB} + \lambda X_{ipuB} + \delta_u + \varepsilon_{ipu} \qquad (2)$$

where the set of instruments Z_{ipu} includes indicators for the random assignment into treatment 2 or treatment 3. I defined the instrument this way because both treatment 2 and treatment 3 led to higher adoption of MHS (Table 8). I verify the strength of the instruments by reporting first-stage F statistic for each IV results.

9.2. Effect of mobile health services on visits to healthcare providers

Panel A of Table 12 reports ITT estimates of intervention on visit to various providers of health services. Giving people information or encouraging them to save numbers of MHS providers do not change the way households receive services from various informal and formal health service providers. Encouraging people to experiment with the MHS service lower their visit to informal providers both in extensive margin and intensive margin. Households which were encouraged to experiment during the intervention are 5 percentage points less likely to receive health services from rural doctors and receive 0.24 less services from rural doctors than the households in the control *para*s. Though not significant, the coefficients for their visit to their local medicine sellers are also negative. The reason for smaller effect on their visit to medicine sellers can be explained by their lower fees (typically zero) compared to the fees for the rural doctors. Encouraging people to experiment with MHS does not change their visit to various formal providers. This could be due to the fact that formal providers could be both substitute and compliment to MHS providers. While some households might need less visit to formal providers due to being able to receive the service over the phone, some other households might be recommended by the MHS providers to see an expert, mostly at a formal service provider.

The local average treatment effect (LATE), i.e., the average treatment effects for those who were induced to save phone numbers or experiment with the MHS by their random assignment into their respective treatments are presented in Panel B of Table 12. The IV estimates also show that saving phone numbers does not change their visit to informal or formal providers. As expected, due to non-compliance in experimentation treatment, the IV estimates are much larger.

Finally, the average treatment effect of MHS for the households who were induced to received MHS by the intervention are presented in Table 13. As reported in Panel A, the households who received MHS since intervention, visited informal providers 2.66 times less than the households who did not adopt MHS. Panel B shows that one service from MHS providers decreases 0.9 visit to local medicine sellers and 0.87 visit to rural doctors. The adoption of MHS does not change the visit to formal providers. Note that these are the LATE results, i.e., average causal effect of MHS for the households who were induced to received MHS by the intervention.

9.3. Effect of mobile health services on health expenditure

Using self-reported health expenditure data, I look into how the intervention and adoption of MHS impact various health expenditure. Panel A of Table 14 reports the ITT estimates of the intervention. Information does not have any effect on total or any category of health expenditure. Encouraging to save phone numbers of MHS has negative coefficients but not statistically significant. The total health expenditure of the households who were encouraged to experiment with MHS decreased by 553 Bangladeshi Taka (a 23.5% decrease) compared to the total health expenditure of the control households in two months since intervention. Most of this reduction in total health expenditure is due to decrease in medicine expenditure which is the largest category of health expenditure of rural households in Bangladesh as shown in Table 2. Encouraging households to experiment with MHS reduce their medicine expenditure by 336 Bangladeshi Taka (a 23.8% decrease). As shown in panel B, the average treatment effect is larger for the compliers, i.e., the households who actually tried MHS during the intervention.

The effect of adoption of MHS is shown in Table 15. The households who were induced by the intervention to adopt the health service through mobile phone had significantly lower medicine, fees and total cost. The coefficients for test and transportation expenditure are also negative, though not significant. These coefficients are admittedly very large. These gets smaller when winsorized at 95% or 90% level but remain still significant.

The main reason for this effect is that the households which adopted MHS received less services from the informal providers. Informal providers often tend to prescribe overtreatment (Banerjee et al., 2004; Gautham et al., 2014; Das et al., 2016; see Kremer and Glennerster (2011) for a review). A survey in eight African countries and found that “25 to 96 percent of outpatient visits resulted in an injection, and in five of the surveyed countries, 70 to 99 percent of the injections were judged unnecessary” (see Kremer and Glennerster (2011) for a review).

Adoption of MHS might also improve the inefficiency caused by asymmetric information. Incentives of patients and providers are often misdirected due to asymmetric information (Kremer and Glennerster, 2011). Advised by the MHS providers, the households will have better information about the need of treatment which can make it difficult for the informal providers to prescribe unnecessary treatment. Signaling knowledge about health problems reduce drug prescription (Currie et al., 2011; Currie et al., 2014).

9.4. Effect on satisfaction with public health services

As presented in Table 18, being informed about the access to MHS does not change the level of satisfaction with public healthcare in rural areas. Encouraging households to save phone numbers improve their satisfaction. The households which were encouraged to try MHS during the intervention are significantly more satisfied with the available public health services for rural people than the control households. The effect size is even larger for those who were induced to adopt MHS by the intervention.

9.5. Heterogeneity of effects

This section will be added in the next version.

10 Conclusion

Though free consultation is available from qualified public healthcare providers via phone, most rural households in developing countries get healthcare service from untrained informal providers who usually recommend overtreatment of medicine (Banerjee et al., 2004; Das et al., 2008; Gautham et al., 2014; Das et al., 2016). This paper explains why households continue to get healthcare advice from these informal providers.

Because of low visit fees, short waiting time, personal interactions, the patients are satisfied with their current providers. And loyal customers of an experience good are very unlikely to try an alternative. However, if they can be nudged to experience the service once, they can learn about the benefits of the beneficial technology and adopt it.

This paper is also the first to estimate the causal effect of access to mobile health services on households' health decisions and health expenditure. The adoption of mobile health services lowers the visit to informal providers and the health expenditure.

One of the limitations of this study is that all the outcome data is self-reported. It is possible that self-reported data is systemically over- or under-reported. I compared some of the baseline characteristics with publicly available data and mostly found those consistent (see the baseline characteristics section). Even if some of the outcome variables at the endline were under- or over-reported, this should not be correlated with the treatment status of the households.

Another limitation is the time horizon of this study. Some might argue that two months are not adequate for this study as households only use MHS if there is sickness. However, this should not be a concern as sickness is not correlated with treatment status. It is possible that the effect size might vary with the time horizon.

One final limitation is that the study includes only those households which have at least one child under the age of five. The 2011 census data shows that more than 40% of rural households have at least one child under the age of five. While these households with kids may need the service more frequently, the service could also be very useful for the remaining households which includes a large group of aging populations.

This study also has significant policy implications. The findings of this study can guide decision-makers in in low- and middle-income countries (LMICs) to improve adoption of mobile health services among rural households. Decreasing medicine consumption by increasing rural households' access to formal healthcare via mobile phone can contribute to global collaboration to attain antimicrobial resistance (AMR).

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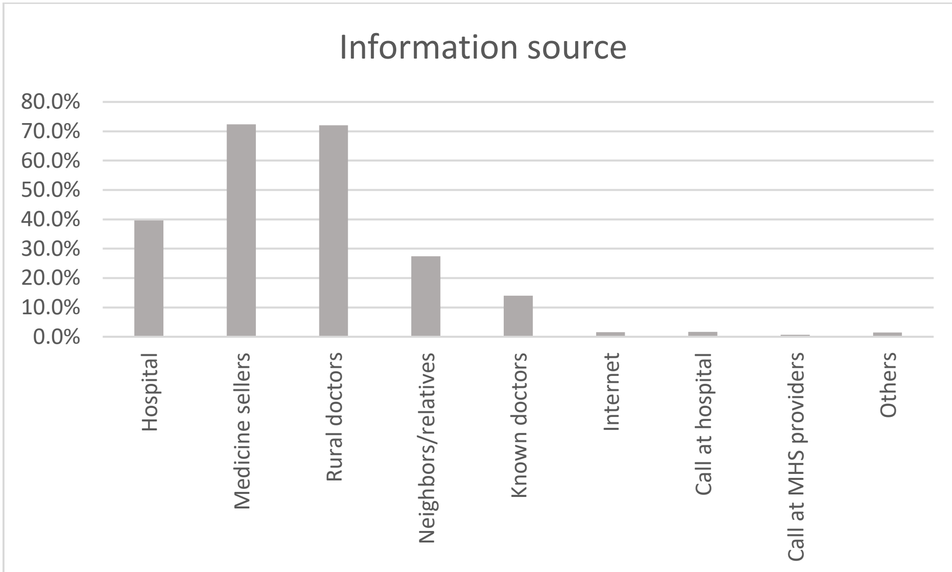
Figures and Tables

Figure 1: Source of health information



Note: The bar chart shows where households seek information from when they have a health-related query

Table 1: Descriptive statistics of typical visit to health providers

	(1) Informal providers	(2) Formal providers
N	1662	1238
Percentage	57.3	42.7
Mean fee (Taka)	40.6	195.1
	(69.5)	(227.5)
Mean travel time (minute)	16.9	41.7
	(14.6)	(35.0)
Mean travel cost (Taka)	15.3	69.3
	(30.3)	(102.9)
Mean satisfaction	6.02	6.02
	(0.66)	(0.69)

Note: The table presents descriptives of the households' usual visit to health providers. Standard deviations are in parenthesis. Satisfaction is measured using a likert scale question where 7 is the very satisfied and 1 is very dissatisfied.

Table 2: Item-wise breakdown health expenditure

	(1) All	(2) Control (Baseline)	(3) Pure control (Endline)	(4) HIES 2016
Fees	7.5%	7.1%	7.4%	11.7%
Medicine	62.1%	59.9%	62.8%	57.3%
Test	15.1%	14.3%	14.4%	21.2%
Travel	7.5%	6.5%	7.3%	9.7%
Other	4.2%	4.6%	4.3%	-

Note: The table reports item wise percentage of total health expenditure measured at the endline. The last column is based on HIES 2016 by BBS.

Table 3: Descriptive statistics and balance of baseline characteristics

	N	Control	Difference from control mean				Joint *p*-value
			T1	T2	T3	Within treatment control	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No. of members	2,799	5.29	0	-0.04	0.06	0.16	0.50
		(1.83)	(0.11)	(0.11)	(0.11)	(0.12)	
Respondent HH head	2,806	0.43	-0.03	-0.01	0.05*	-0.01	0.03
		(0.50)	(0.03)	(0.03)	(0.03)	(0.03)	
Education of respondent	2,786	6.86	0.68***	0.24	0.16	0.34	0.07
		(4.23)	(0.25)	(0.25)	(0.24)	(0.26)	
Education of HH head	2,802	5.95	0.67**	0.26	0.49*	0.41	0.12
		(4.44)	(0.27)	(0.27)	(0.25)	(0.28)	
Main source: employment	2,806	0.36	-0.02	-0.03	0	-0.03	0.56
		(0.48)	(0.03)	(0.03)	(0.03)	(0.03)	
Any migrant in HH	2,806	0.21	0	0.01	-0.02	0	0.77
		(0.41)	(0.02)	(0.02)	(0.02)	(0.03)	
Monthly expense	2,456	11796	-426.5	-372.1	20.1	7.3	0.58
		(5,910)	(375.4)	(373.5)	(357.4)	(397.6)	
Monthly income	2,464	15657	117.8	156.4	961.2	616.7	0.49
		(9,584)	(652.5)	(648.2)	(621.3)	(690.1)	
Health expenditure	2,709	1893	321.7	156.1	49.2	106.7	0.63
		(3,523)	(218.4)	(218.0)	(209.4)	(233.2)	
Advice from rural doctor	2,806	0.40	0.03	0.04	0.02	0.02	0.75
		(0.49)	(0.03)	(0.03)	(0.03)	(0.03)	
Advice from medicine sellers	2,806	0.41	0.02	0.01	0	-0.02	0.81
		(0.49)	(0.03)	(0.03)	(0.03)	(0.03)	
Satisfaction with rural public healthcare	2,241	2.77	0.03	-0.25***	-0.17*	-0.09	0.01
		(1.49)	(0.09)	(0.09)	(0.09)	(0.10)	
Aware MHS	2,804	0.04	0.02*	0.01	0.01	0.01	0.52
		(0.19)	(0.01)	(0.01)	(0.01)	(0.01)	
Received COVID-19 vaccine	2,806	0.12	0	-0.02	0	-0.01	0.82
		(0.33)	(0.02)	(0.02)	(0.02)	(0.02)	
No. of households	2,806	566	560	561	676	443	
Joint *p*-vale		0.23	0.12	0.40	0.12	0.99	

Notes: The table presents summary statistics of key baseline variables. Column (2) reports the mean and the standard deviation of the households in the baseline control *para*s. Column (3)-(6) show differences of various treatments from the control mean and their standard errors. Column (7) shows the *p*-value of F-test of the joint significance of the treatment indicators. The last row shows the *p*-value of F-test of the joint significance of the variables. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effect on awareness of mobile health services (MHS)

	(1) MHS exists	(2) Can name one of the three	(3) Aware of one of the three	(4) Have one of the three numbers
	Panel A: OLS (ITT estimates)			
Information	0.33***	0.38***	0.32***	0.37***
	(0.04)	(0.04)	(0.04)	(0.04)
Saving number	-0.03	-0.04	-0.02	0.01
	(0.03)	(0.03)	(0.03)	(0.03)
Experimentation	0.04	0.06**	0.06**	0.07**
	(0.03)	(0.03)	(0.03)	(0.03)
Observations	2,338	2,338	2,338	2,338
R-squared	0.22	0.26	0.23	0.29
Control mean	0.43	0.34	0.42	0.26
Treatment 2	0.30***	0.33***	0.31***	0.39***
	(0.04)	(0.04)	(0.04)	(0.04)
Treatment 3	0.34***	0.40***	0.37***	0.46***
	(0.03)	(0.04)	(0.03)	(0.04)
	Panel B: IV (ATET estimates)			
Information	0.33***	0.38***	0.32***	0.37***
	(0.04)	(0.04)	(0.04)	(0.04)
Saving number	-0.04	-0.05	-0.02	0.02
	(0.04)	(0.04)	(0.04)	(0.04)
Experimentation	0.08	0.13**	0.14**	0.15**
	(0.05)	(0.06)	(0.05)	(0.06)
R-squared	0.22	0.26	0.22	0.28
Treatment 2	0.37***	0.41***	0.37***	0.47***
	(0.04)	(0.05)	(0.04)	(0.05)
Treatment 3	0.73***	0.86***	0.80***	1.00***
	(0.08)	(0.09)	(0.08)	(0.09)

Note: Panel A presents ITT estimates. The outcome variables are binary indicator equals to 1 if the respondent (1) is aware of existence of any MHS providers, (2) can name of the three MHS providers, (3) knows any of the three MHS providers when asked specifically, and (4) has phone numbers of one of the MHS providers. Each specification adjusts for awareness of MHS at the baseline, and the variables which were not balanced at the baseline. *Upazila* (sub-district) fixed effects are also included in each specification. Panel B presents IV estimates where saving phone number is instrumented by random assignment to a treatment in which enumerators encourage the participants to save the MHS numbers in their phone and experimentation is instrumented by random assignment to a treatment in which enumerators encourage the participants to try calling at one of the MHS numbers. Standard errors are clustered by *para* (neighborhood) level. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Spillover effect on awareness of mobile health services (MHS)

VARIABLES	(1) MHS exists	(2) Can name one of the three	(3) Aware of one of the three	(4) Have one of the three numbers
T1 control	0.10**	0.11**	0.13***	0.13***
	(0.04)	(0.04)	(0.04)	(0.04)
T2 control	0.06	0.08*	0.08*	0.11**
	(0.05)	(0.04)	(0.04)	(0.04)
T3 control	0.10**	0.13***	0.11***	0.12***
	(0.04)	(0.04)	(0.04)	(0.04)
Observations	999	999	999	999
R-squared	0.26	0.28	0.27	0.26
Control mean	0.44	0.35	0.44	0.27
All within treatment control	0.09***	0.11***	0.11***	0.12***
	(0.03)	(0.03)	(0.03)	(0.03)
p-vale T1=T2	0.48	0.61	0.27	0.66
p-vale T1=T3	0.97	0.71	0.66	0.81
p-vale T2=T3	0.45	0.36	0.49	0.83

Note: This table presents OLS estimates of spillover effect. The outcome variables are binary indicator equals to 1 if the respondent (1) is aware of existence of any MHS providers, (2) can name of the three MHS providers, (3) knows any of the three MHS providers when asked specifically, and (4) has phone numbers of one of the MHS providers. Each specification adjusts for awareness of MHS at the baseline, and the variables which were not balanced at the baseline. *Upazila* (sub-district) fixed effects are also included in each specification. The bottom rows show the p-values testing the equality of the coefficients of various treatment arms. Standard errors are clustered by *para* (neighborhood) level. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Survey effect on awareness of MHS

	(1) MHS exists	(2) Can name one of the three	(3) Aware of one of the three	(4) Have one of the three numbers
Control	0.19***	0.19***	0.21***	0.17***
	(0.03)	(0.03)	(0.03)	(0.03)
Treatment 1	0.53***	0.57***	0.53***	0.55***
	(0.03)	(0.03)	(0.03)	(0.03)
Treatment 2	0.49***	0.52***	0.51***	0.56***
	(0.03)	(0.03)	(0.03)	(0.03)
Treatment 3	0.53***	0.58***	0.57***	0.63***
	(0.02)	(0.02)	(0.02)	(0.02)
Within treatment control	0.28***	0.30***	0.32***	0.30***
	(0.03)	(0.03)	(0.03)	(0.03)
Observations	4,234	4,234	4,234	4,234
R-squared	0.30	0.35	0.34	0.37
Endline/pure control mean	0.24	0.16	0.22	0.09

Note: This table presents OLS estimates of survey effect. The outcome variables are binary indicator equals to 1 if the respondent (1) is aware of existence of any MHS providers, (2) can name of the three MHS providers, (3) knows any of the three MHS providers when asked specifically, and (4) has phone numbers of one of the MHS providers. Each specification adjusts for non-changing variables which were not balanced. *Upazila* (sub-district) fixed effects are also included in each specification. The bottom row reports mean awareness of the households which were only surveyed at the endline. Standard errors are clustered by *para* (neighborhood) level. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Awareness of specific MHS providers by treatment groups

	None	*Upazila*	District	Central call center	All three
Endline/pure control	90.7%	7.4%	4.1%	3.7%	1.1%
Control	73.3%	24.7%	16.3%	13.1%	8.5%
Treatment 1	35.4%	56.8%	47.0%	37.1%	25.2%
Treatment 2	34.9%	57.6%	46.0%	40.5%	27.5%
Treatment 3	27.8%	61.7%	54.0%	44.2%	30.5%
T1 Control	59.1%	38.0%	28.5%	21.9%	15.3%
T2 Control	62.9%	32.1%	20.0%	20.7%	14.3%
T3 Control	60.2%	35.5%	28.3%	24.1%	16.3%
Total	60.6%	34.4%	27.1%	22.6%	14.9%

Note: This table presents the percentage of households under various treatments having phone numbers of specific MHS providers.

Table 8: Effects on adoption of mobile health services (MHS)

	(1)	(2)	(3)	(4)
	Attempted		Service received	
	Yes/No	No. of times	Yes/No	No. of times
	Panel A: OLS			
Information	0.03	0.08	0.03	0.03
	(0.02)	(0.06)	(0.02)	(0.04)
Save numbers	0.02	0.01	0.02	0.02
	(0.02)	(0.06)	(0.02)	(0.05)
Experimentation	0.15***	0.28***	0.12***	0.20***
	(0.03)	(0.08)	(0.03)	(0.06)
Observations	2,339	2,339	2,339	2,339
R-squared	0.22	0.17	0.21	0.15
Control mean	0.07	0.16	0.06	0.13
Treatment 2	0.05**	0.09	0.05**	0.05
	(0.02)	(0.06)	(0.02)	(0.05)
Treatment 3	0.20***	0.37***	0.17***	0.25***
	(0.03)	(0.07)	(0.03)	(0.06)
	Panel B: IV			
Information	0.03	0.08	0.03	0.02
	(0.02)	(0.06)	(0.02)	(0.04)
Save numbers	0.03	0.01	0.02	0.03
	(0.03)	(0.08)	(0.02)	(0.06)
Experimentation	0.32***	0.61***	0.26***	0.43***
	(0.06)	(0.16)	(0.05)	(0.13)
R-squared	0.21	0.17	0.22	0.16
Treatment 2	0.07**	0.11	0.06**	0.06
	(0.03)	(0.07)	(0.03)	(0.06)
Treatment 3	0.43***	0.80***	0.36***	0.53***
	(0.06)	(0.15)	(0.05)	(0.12)

Note: Panel A presents ITT estimates. Each specification adjusts for the variables which were not balanced at the baseline. *Upazila* (sub-district) fixed effects are also included in each specification. Panel B presents IV estimates where saving phone number is instrumented by random assignment to treatment 2 in which enumerators encourage the participants to save the MHS numbers in their phone and experimentation is instrumented by random assignment to treatment 3 in which enumerators encourage the participants to try calling at one of the MHS numbers. Standard errors are clustered by *para* (neighborhood) level. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Spillover effect on adoption of mobile health services (MHS)

	(1)	(2)	(3)	(4)
	Attempted		Service received	
	Yes/No	No. of times	Yes/No	No. of times
T1 control	-0.01	-0.01	-0.01	-0.00
	(0.02)	(0.06)	(0.02)	(0.05)
T2 control	-0.01	-0.04	0.00	-0.02
	(0.02)	(0.05)	(0.02)	(0.05)
T3 control	0.01	0.02	0.01	0.00
	(0.02)	(0.06)	(0.02)	(0.04)
Observations	1,000	1,000	1,000	1,000
R-squared	0.29	0.29	0.26	0.20
Control mean	0.0707	0.164	0.0583	0.125
All within treatment control	-0.00	-0.01	0.00	-0.01
	(0.02)	(0.04)	(0.02)	(0.04)
p-vale T1=T2	0.888	0.616	0.666	0.751
p-vale T1=T3	0.396	0.627	0.497	0.885
p-vale T2=T3	0.489	0.293	0.831	0.593

Note: This table presents OLS estimates of spillover effect. Each column adjusts for the variables which were not balanced at the baseline. *Upazila* (sub-district) fixed effects are also included in each specification. The bottom rows show the p-values testing the equality of the coefficients of various treatment arms. Standard errors are clustered by *para* (neighborhood) level. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Survey effect on adoption of mobile health services (MHS)

	(1)	(2)	(3)	(4)
	Attempted		Service received	
	Yes/No	No. of times	Yes/No	No. of times
Baseline control	0.03*	0.05	0.02	0.03
	(0.02)	(0.04)	(0.02)	(0.04)
Treatment 1	0.07***	0.15***	0.06***	0.07**
	(0.02)	(0.05)	(0.02)	(0.03)
Treatment 2	0.08***	0.14***	0.07***	0.09**
	(0.02)	(0.05)	(0.02)	(0.04)
Treatment 3	0.23***	0.41***	0.19***	0.28***
	(0.03)	(0.07)	(0.02)	(0.05)
Within treatment control	0.03**	0.05	0.02*	0.03
	(0.01)	(0.04)	(0.01)	(0.03)
Observations	4,234	4,234	4,234	4,234
R-squared	0.23	0.20	0.22	0.17
Endline control mean	0.04	0.11	0.04	0.09

Note: This table presents OLS estimates of survey effect. Each specification adjusts for non-changing variables which were not balanced. *Upazila* (sub-district) fixed effects are also included in each specification. The bottom row reports mean awareness of the households which were only surveyed at the endline. Standard errors are clustered by *para* (neighborhood) level. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Call success rate by providers of MHS

	Attempted	Answered	Success rate
Upazila health complex	515 (52.6%)	384 (53.9%)	74.6%
District hospital	166 (16.9%)	118 (16.6%)	71.1%
Central call center	299 (30.5%)	210 (29.5%)	70.2%
Total	980	712	72.7%

Note: This table reports the number of calls attempted and answered by different providers. The percentage of total is shown in the parenthesis.

Table 12: Effect of intervention on visit to various health service providers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Medicine sellers		Rural doctors		Informal providers		Formal providers	
	Yes/No	No. of times	Yes/No	No. of times	Yes/No	No. of times	Yes/No	No. of times
	Panel A: OLS							
Information	0.02	0.15	-0.03	-0.02	0.01	0.10	0.01	0.02
	(0.03)	(0.12)	(0.03)	(0.11)	(0.03)	(0.20)	(0.03)	(0.05)
Save numbers	-0.00	-0.12	0.03	-0.06	0.01	-0.16	-0.02	-0.02
	(0.03)	(0.12)	(0.03)	(0.11)	(0.03)	(0.20)	(0.03)	(0.05)
Experimentation	-0.04	-0.20*	-0.05*	-0.15	-0.05*	-0.35**	0.03	-0.01
	(0.03)	(0.10)	(0.03)	(0.09)	(0.03)	(0.17)	(0.02)	(0.05)
Observations	2,284	2,284	2,284	2,284	2,299	2,299	2,275	2,275
R-squared	0.28	0.30	0.22	0.22	0.20	0.27	0.11	0.09
Control mean	0.37	1.03	0.36	0.93	0.52	1.99	0.22	0.41
Treatment 2	0.02	0.04	0.00	-0.09	0.03	-0.06	-0.02	0.00
	(0.03)	(0.11)	(0.03)	(0.11)	(0.03)	(0.20)	(0.03)	(0.05)
Treatment 3	-0.02	-0.16	-0.05*	-0.24**	-0.02	-0.41**	0.01	-0.01
	(0.03)	(0.11)	(0.03)	(0.10)	(0.03)	(0.17)	(0.02)	(0.05)
	Panel B: IV							
Information	0.02	0.15	-0.03	-0.02	0.01	0.10	0.01	0.02
	(0.03)	(0.12)	(0.03)	(0.11)	(0.03)	(0.20)	(0.03)	(0.05)
Save numbers	-0.00	-0.14	0.04	-0.08	0.02	-0.20	-0.03	-0.02
	(0.03)	(0.15)	(0.04)	(0.13)	(0.04)	(0.24)	(0.03)	(0.07)
Experimentation	-0.09	-0.44**	-0.10*	-0.33*	-0.11*	-0.76**	0.05	-0.02
	(0.06)	(0.22)	(0.06)	(0.19)	(0.06)	(0.37)	(0.05)	(0.11)
R-squared	0.27	0.30	0.22	0.22	0.20	0.26	0.11	0.09
Treatment 2	0.02	0.05	0.00	-0.11	0.04	-0.08	-0.02	0.00
	(0.03)	(0.14)	(0.03)	(0.13)	(0.04)	(0.24)	(0.03)	(0.06)
Treatment 3	-0.05	-0.35	-0.10*	-0.52**	-0.05	-0.89**	0.02	-0.01
	(0.06)	(0.23)	(0.06)	(0.21)	(0.06)	(0.38)	(0.05)	(0.11)

Note: Panel A presents ITT estimates. Outcomes are winsorized at the 99% level. Each specification adjusts for the outcome variable at baseline, and the variables which were not balanced at the baseline. *Upazila* (sub-district) fixed effects are also included in each specification. Panel B presents IV estimates where saving phone number is instrumented by random assignment to treatment 2 in which enumerators encourage the participants to save the MHS numbers in their phone, and experimentation is instrumented by random assignment to treatment 3 in which enumerators encourage the participants to make a call at one of the MHS numbers. Formal providers do not include MHS providers. Standard errors are clustered by *para* (neighborhood) level. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 13: Effect of receiving mobile health services (MHS) on visit to various health service providers

	(1) Medicine sellers	(2) Rural doctors	(3) Informal providers	(4) Formal providers
		Panel A		
Received MHS	-1.37**	-1.33**	-2.66**	-0.02
	(0.69)	(0.61)	(1.15)	(0.33)
Observations	2,722	2,721	2,738	2,710
R-squared	0.26	0.17	0.22	0.10
First stage F-stat	19.63	19.96	19.89	19.80
		Panel B		
No. of times received MHS	-0.90*	-0.87**	-1.76**	-0.01
	(0.49)	(0.43)	(0.82)	(0.22)
Observations	2,722	2,721	2,738	2,710
R-squared	0.19	0.09	0.11	0.10
First stage F-stat	14.18	14.50	14.33	14.17

Note: The table presents the IV estimates of receiving MHS on visit to various health service providers. In panel A, binary indicator of whether one received MHS is instrumented by random assignment into treatment 2 or 3. In panel B, the number of times one received MHS is instrumented by random assignment into treatment 2 or 3. The outcome variables are the number of times on received health service from those providers. Outcomes are winsorized at the 99% level. Each specification adjusts for the outcome variable at baseline, and the variables which were not balanced at the baseline. *Upazila* (sub-district) fixed effects are also included in each specification. Standard errors are clustered by *para* (neighborhood) level. The bottom row of each panel shows the first stage F-stat. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 14: Effect of intervention on health expenditure

	(1) Total	(2) Medicine	(3) Fees	(4) Test	(5) Transport
			Panel A: OLS		
Information	18.03	41.07	27.84	-8.11	17.94
	(337.46)	(164.50)	(23.98)	(79.27)	(25.77)
Save numbers	-463.20	-256.73	-61.00**	-41.12	-28.03
	(313.23)	(167.35)	(23.76)	(73.23)	(27.73)
Experimentation	-108.22	-120.51	5.11	28.78	5.06
	(269.02)	(147.34)	(20.95)	(71.94)	(24.99)
Observations	2,305	2,304	2,300	2,294	2,300
R-squared	0.10	0.09	0.09	0.07	0.11
Control mean	2354	1411	166.3	336.4	152.6
Treatment 2	-445.17	-215.65	-33.16	-49.23	-10.10
	(322.61)	(170.30)	(22.27)	(79.47)	(25.41)
Treatment 3	-553.40*	-336.16**	-28.05	-20.45	-5.03
	(293.99)	(146.64)	(21.84)	(77.99)	(23.76)
			Panel B: IV		
Information	15.09	40.18	27.48	-8.39	17.76
	(333.35)	(162.95)	(23.67)	(78.42)	(25.47)
Save numbers	-561.05	-312.10	-73.90***	-49.72	-34.01
	(375.56)	(200.98)	(28.56)	(87.97)	(33.34)
Experimentation	-261.25	-273.81	7.45	59.58	9.39
	(564.15)	(308.96)	(44.48)	(151.66)	(52.68)
R-squared	0.10	0.09	0.08	0.07	0.11
Treatment 2	-542.68	-263.67	-40.44	-60.08	-12.33
	(390.17)	(206.37)	(27.01)	(96.27)	(30.77)
Treatment 3	-1,199.12*	-727.14**	-61.06	-44.93	-11.05
	(633.54)	(317.17)	(47.10)	(167.60)	(51.14)

Note: Panel A presents ITT estimates. Outcomes are measured in Bangladeshi Taka and winsorized at the 99% level. Each specification adjusts for the outcome variable at baseline (when available), and the variables which were not balanced at the baseline. *Upazila* (sub-district) fixed effects are also included in each specification. Panel B presents IV estimates where saving phone number is instrumented by random assignment to a treatment 2 in which enumerators encourage the participants to save the MHS numbers in their phone, and experimentation is instrumented by random assignment to a treatment 3 in which enumerators encourage the participants to make a call at one of the MHS numbers. Standard errors are clustered by *para* (neighborhood) level. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 15: Effect of mobile health services (MHS) on health expenditure

	(1) Total	(2) Medicine	(3) Fees	(4) Test	(5) Transport
Received MPHS	-3,817.71*	-2,586.49**	-335.60**	-199.81	-178.45
	(1,980.12)	(1,098.97)	(153.08)	(452.21)	(168.40)
Observations	2,745	2,742	2,738	2,731	2,738
R-squared	0.05	0.01	0.01	0.07	0.10
First stage F-stat	19.93	20.48	19.90	20.60	20.05
No. of times received MPHS	-2,536.51*	-1,730.82**	-224.46**	-133.45	-119.18
	(1,442.18)	(841.52)	(112.86)	(304.26)	(117.51)
Observations	2,745	2,742	2,738	2,731	2,738
R-squared	-0.08	-0.20	-0.11	0.06	0.04
First stage F-stat	14.35	14.63	14.19	14.68	14.26

Note: The table presents the IV estimates of receiving MHS on various health expenditure. In panel A, binary indicator of whether one received MHS is instrumented by random assignment into treatment 2 or 3. In panel B, the number of times one received MHS is instrumented by random assignment into treatment 2 or 3. Outcomes are measured in Bangladeshi Taka and winsorized at the 99% level. Each specification adjusts for the outcome variable at baseline, and the variables which were not balanced at the baseline. *Upazila* (sub-district) fixed effects are also included in each specification. Standard errors are clustered by *para* (neighborhood) level. The bottom row of each panel shows the first stage F-stat. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 16: Effect of intervention on COVID-19 vaccination

	(1)	(2)	(3)	(4)
	Respondent		Household	
VARIABLES	Vaccinated	Vaccinated or signed up	No. vaccinated	No. vaccinated or signed up
	Panel A: OLS			
Information	0.07**	0.01	0.11	-0.05
	(0.03)	(0.04)	(0.08)	(0.13)
Save numbers	-0.06**	-0.04	-0.10	-0.03
	(0.03)	(0.03)	(0.08)	(0.12)
Experimentation	0.08***	0.07**	0.16**	0.22**
	(0.03)	(0.03)	(0.07)	(0.11)
Observations	2,336	2,339	2,329	2,317
R-squared	0.12	0.13	0.17	0.15
Control mean	0.19	0.45	0.72	1.77
Treatment 2	0.01	-0.03	0.01	-0.08
	(0.02)	(0.03)	(0.07)	(0.12)
Treatment 3	0.09***	0.04	0.17**	0.14
	(0.02)	(0.03)	(0.07)	(0.12)
	Panel B: IV			
Information	0.07**	0.01	0.11	-0.05
	(0.03)	(0.04)	(0.08)	(0.13)
Save numbers	-0.07**	-0.05	-0.12	-0.04
	(0.03)	(0.04)	(0.10)	(0.14)
Experimentation	0.16***	0.15**	0.33**	0.48**
	(0.05)	(0.06)	(0.15)	(0.23)
Observations	2,336	2,339	2,329	2,317
R-squared	0.11	0.12	0.16	0.14
Treatment 2	0.01	-0.04	0.02	-0.10
	(0.03)	(0.04)	(0.09)	(0.15)
Treatment 3	0.19***	0.08	0.37**	0.31
	(0.05)	(0.07)	(0.15)	(0.25)

Note: Panel A presents ITT estimates. Each specification adjusts for the outcome variable at baseline (when available), and the variables which were not balanced at the baseline. *Upazila* (sub-district) fixed effects are also included in each specification. Panel B presents IV estimates where saving phone number is instrumented by random assignment to a treatment 2 in which enumerators encourage the participants to save the MHS numbers in their phone, and experimentation is instrumented by random assignment to a treatment 3 in which enumerators encourage the participants to make a call at one of the MHS numbers. Standard errors are clustered by *para* (neighborhood) level. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 17: Effect of receiving MHS on COVID-19 vaccination

	(1)	(2)	(3)	(4)
	Respondent		Household	
	Vaccinated	Vaccinated or signed up	No. vaccinated	No. vaccinated or signed up
Received MHS	0.22	0.05	0.34	0.84
	(0.17)	(0.20)	(0.47)	(0.72)
Observations	2,777	2,782	2,769	2,757
R-squared	0.11	0.13	0.17	0.14
First stage F-stat	20.03	20.06	20.04	19.93

Note: The table presents the IV estimates of receiving MHS on COVID-19 vaccination. A binary indicator of whether one received MHS is instrumented by random assignment into treatment 2 or 3. Each specification adjusts for the outcome variable at baseline, and the variables which were not balanced at the baseline. *Upazila* (sub-district) fixed effects are also included in each specification. Standard errors are clustered by *para* (neighborhood) level. The bottom row shows the first stage F-stat. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 18: Effect on satisfaction with exiting rural public healthcare

	(1) OLS	(2) IV	(3) IV
Information	-0.04	-0.04	
	(0.05)	(0.05)	
Save numbers	0.11**	0.13**	
	(0.05)	(0.06)	
Experimentation	0.05	0.11	
	(0.04)	(0.08)	
Received MHS			1.05***
			(0.31)
Observations	2,337	2,337	2,780
R-squared	0.21	0.20	0.04
Control mean	2.93	2.93	
Treatment 2	0.07	0.09	
	(0.04)	(0.05)	
Treatment 3	0.12***	0.26***	
	(0.04)	(0.10)	
First stage F-stat		46.78	20.61

Note: The table presents effect of intervention on satisfaction with public health services for rural people. Column 1 reports ITT estimates. Each specification adjusts for the outcome variable at baseline, and the variables which were not balanced at the baseline. *Upazila* (sub-district) fixed effects are also included in each specification. Column 2 reports IV estimates where saving phone number is instrumented by random assignment to a treatment 2 in which enumerators encourage the participants to save the MHS numbers in their phone, and experimentation is instrumented by random assignment to a treatment 3 in which enumerators encourage the participants to make a call at one of the MHS numbers. Column 3 presents the IV estimates where whether the household received MHS is instrumented by random assignment into treatment 2 or 3. Standard errors are clustered by *para* (neighborhood) level. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.

12 Appendix

Table A1: Attrition

	(1) Attrition
Treatment 1	0.002
	(0.010)
Treatment 2	-0.005
	(0.009)
Treatment 3	0.002
	(0.009)
T1 Control	0.018
	(0.018)
T2 Control	0.004
	(0.015)
T3 Control	0.011
	(0.015)
Control mean	0.024***
	(0.007)
Observations	2,880
R-squared	0.001
p-vale T1=T2	0.485
p-vale T1=T3	0.988
p-vale T2=T3	0.433

Note: Linear regression of treatment indicators on attrition=1 if one participant of the household was not surveyed at the endline.

Table A2: ITT estimates of awareness and adoption under various specifications

	(1)	(2)	(3)	(4)
	Panel A: Has one of three MHS numbers			
Flyer	0.380***	0.377***	0.377***	0.370***
	(0.044)	(0.043)	(0.038)	(0.038)
Save	0.004	0.006	0.008	0.015
	(0.041)	(0.041)	(0.032)	(0.032)
Talk	0.071*	0.071**	0.070**	0.071**
	(0.036)	(0.036)	(0.028)	(0.028)
Observations	2,363	2,362	2,362	2,338
R-squared	0.129	0.132	0.276	0.286
	Panel B: Number of times service received from MHS			
Flyer	0.048	0.044	0.040	0.021
	(0.052)	(0.052)	(0.043)	(0.042)
Save	0.012	0.015	0.015	0.028
	(0.059)	(0.059)	(0.049)	(0.049)
Talk	0.193***	0.193***	0.194***	0.196***
	(0.071)	(0.071)	(0.063)	(0.062)
Observations	2,363	2,363	2,363	2,339
R-squared	0.016	0.016	0.134	0.152
Baseline outcome	No	Yes	Yes	Yes
Subdistrict fixed effect	No	No	Yes	Yes
Control imbalanced variables	No	No	No	Yes

Note: The table present ITT estimates on awareness and adoption under different specifications. The outcome variable in Panel A is a binary indicator equals to 1 if the household has phone numbers of one of the MHS providers. The outcome variable in Panel B is the number of times the household received MHS after intervention. Column 1 reports pure ITT estimate. Next columns present results under different specification as mentioned in the bottom rows. Standard errors are clustered by *para* (neighborhood) level. Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1.