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Information Provision Over the Phone Saves Lives: An RCT to Contain COVID-19 in Rural Bangladesh at the Pandemic's Onset[1]

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Abstract

Lack of information about COVID-19 and its spread may have contributed to excess mortality at the pandemic's onset. In April and May 2020, we implemented a randomized controlled trial with more than 3,000 households in 150 Bangladeshi villages. Our one-to-one information campaign via phone stressed the importance of social distancing and hygiene measures, and illustrated the consequences of an exponential spread of COVID-19. We find that information provision improves knowledge about COVID-19 and induces significant behavioral changes. Information provision also yields considerably better health outcomes, most importantly by reducing the number of reported deaths by about 50% in treated villages.

JEL-codes: C93, D01, D91, I12

Keywords: Field experiment, COVID-19, Information intervention, Death rates

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1. Introduction

At the beginning of a pandemic like COVID-19, death tolls are typically high because the pathways of a disease are still unfamiliar to most people, resulting in a failure to engage quickly in preventative health behaviors. Since pharmaceutical interventions like the development of vaccines take considerable time, in particular until they can be provided on a global scale, non-pharmaceutical interventions are crucial in containing a pandemic at its onset. This raises the question which non-pharmaceutical interventions are feasible and politically acceptable, and in particular which ones are effective in helping people protect themselves against the disease (Haushofer and Metcalf, 2020).

Here we present a randomized controlled trial with more than 3,000 households in 150 villages in rural Bangladesh in order to study the behavioral and health effects of an information campaign about COVID-19 at the pandemic's onset in April 2020. In general, information provision to the general public plays a key role in the non-pharmaceutical combat against a pandemic. In the COVID-19-crisis, large mass media campaigns (on TV, radio or social media platforms) have been launched worldwide with the intention to prevent the further spread of the disease (e.g., Bursztyn et al., 2020; Debnath and Bardhan, 2020). On an aggregate level, the effects of such mass media campaigns on the number of infections and deaths have been shown to depend critically on the level of trust in national governments (Fetzer et al., 2022) and on the actual content and reliability of the information (Bursztyn et al., 2020). Less is known, however, whom these mass media campaigns actually reach, and if they are equally successful in addressing the most vulnerable populations, including the poor and illiterate. Moreover, the channels through which information provision affects aggregate health conditions and death rates are unknown to date, because such mass media campaigns make it difficult to identify changes in knowledge and behavior on an individual or household level.

Of course, it is generally well understood that human behavior is strongly influenced by the information available to people (Haaland et al., 2021) and that, for instance, health-related behavior is often responsive to information, even if it takes time (such as when fighting HIV through mass media information campaigns; Dupas, 2011; Banerjee et al., 2019). Yet, in a pandemic the speed of reaction to new information matters to break mounting waves of new infections. Therefore, it is important to study whether and how information can induce quick behavioral changes to protect one's health. Recent work has shown that correcting misperceptions about exponential growth increased support for social distancing measures

(Lammers et al., 2020).[2] COVID-19-related text messages (with a link to a video about the disease) to 25 million Indians led to increased awareness, less travel, and more reporting of symptoms to local health workers within a span of 5 days after receiving the messages (Banerjee et al., 2020). Personal phone calls to households in rural areas of Bangladesh and India improved the willingness to comply with public health guidelines a month after the phone calls (with the caveat of lacking a proper control condition; Siddique et al., 2022). However, whether or not these self-reported changes in behavior actually improved health and prevented casualties remains unclear from these earlier studies.

In this paper, we examine how information provision about COVID-19 at the onset of the pandemic can have a significant effect on (i) individuals' knowledge and attitudes about the pandemic, (ii) individuals' behavior to contain its spread, and (iii) the health conditions and death rates of treated households and villages. While knowledge and health conditions are self-reported, we enrich self-reported information about behavior with what a household's neighbors report, exploiting our unique design introduced below. Moreover, we also have information on whether any household member deceased since the beginning of the pandemic.

Our information campaign was run over telephone and thus in a personal, one-to-one setting which makes it different from typical mass media communication. The campaign focused on prevention measures, symptoms of COVID-19, and in addition on the consequences of an exponential spread of the disease within a village, using easy language, concrete measures and concentrating on the most important information only. Humans fare, on average, relatively poorly in their understanding of exponential growth processes, such as compound interest (Stango and Zinman, 2009), for which reason we put emphasis on this important aspect of the pandemic.

Contrary to the papers on interventions during the pandemic cited above that measured the effects of an intervention only once, we measured potential effects twice, namely 14 days as well as about 2.5 months after our campaign and we are able to link our interventions to individual- and household-level health outcomes. This double measurement at the level of a household allows for a better understanding of the pathways from information provision to health outcomes via potential changes in knowledge and behavior. Moreover, having two points of measurement reveals whether an information campaign can yield quick behavioral responses – which is crucial in a pandemic – and at the same time sustain positive medium-

[2] It is also the case that social preferences are indicative of preventative measures like social distancing or wearing face masks (see, e.g., Campos-Mercade et al., 2021b) and that the pandemic has influenced social preferences themselves (see, e.g., Cappelen et al., 2021).

term effects such that potential information provision effects do not wear out quickly. While it has been acknowledged that nudges may not form new and persistent habits (Brandon et al., 2017), the achieved response, even if only observed in the short-run, may be enough to break a wave of new COVID-19 infections and thus change a pandemic's dynamic and therefore save lives.

We do not only study the effects of information provision on knowledge, behavior, and health outcomes, because one half of the treated households were additionally offered an unconditional cash transfer worth about 2-3 days of labor. This unconditional transfer was motivated as an at least partial compensation for possibly forgone earnings as a consequence of adhering to social distancing rules and stay-at-home policies. This treatment allows us to study whether offering monetary incentives may amplify the effects of information provision. This is by no means clear since monetary incentives can have unintended side effects on human behavior, for example by crowding out intrinsic motivation for specific behavior (Gneezy and Rustichini, 2000a, 2000b; Bénabou and Tirole, 2003; Gneezy et al., 2011). The same problem may occur when using unconditional cash transfers as an incentive to improve cooperative behavior to contain the pandemic, a policy that has been adopted in many countries around the world with governments supporting poor households or small enterprises with unconditional transfers.[3]

The rest of the paper is organized as follows. Section 2 presents the experimental design and the field setting. Section 3 introduces the implementation and the methods for estimating treatment effects. Section 4 shows the results, and section 5 concludes.

2. Experimental Design and Field Setup

Our study comprised 3,081 households from 150 villages in four rural districts of Bangladesh (Chandpur, Gopalganj, Netrokona, and Sunamganj). Each of these households was assigned to one of the following three conditions (see below for details on the assignment procedure): Households in the control condition did not receive any intervention (with respect to information or monetary incentives), but they were surveyed at the same points in time as the treated households. Of the latter, all were exposed to an information provision intervention, and half of them, in addition, received monetary incentives. We will refer to the latter conditions as INFO-ONLY and INFO+MONEY.

[3] Early on, the International Monetary Fund has compiled a collection of policy responses around the world. See https://www.imf.org/en/Topics/imf-and-covid19/Policy-Responses-to-COVID-19 (page last updated on 2 July 2021; accessed 22 November 2022).

The information campaign in both INFO-ONLY and INFO+MONEY was run over phone.[4] The calls were conducted by a professional, local survey firm which has been working already previously with these households (in pre-COVID-19 times also in personal encounters). The campaign (see the appendix for details and scripts) stressed the importance of social distancing and hygiene measures and illustrated the consequences of an exponential spread of COVID-19 within a village. In an interactive dialogue, enumerators conveyed the three most important prevention measures, the three most common symptoms, as well as three measures to take in case the respondent or someone else in the household suffered from the mentioned symptoms. After the phone call, the survey firm sent respondents a summary of the call as a text message.

Importantly, the information that we disseminated in our intervention was based on the government's information campaign and did not contain any additional information, except for the illustration of exponential growth. This means that, notwithstanding the latter exception, untreated households had, in principle, access to the same information through mass media campaigns of the government. Of course, our intervention of calling households and going through this information together with them made the information much more salient. Compared to the governmental mass media campaigns on television and radio, our campaign compressed the information, focused on the most important aspects, and suggested concrete measures to follow in a one-to-one interaction. A week after the first phone call, households received a reminder call (see the appendix for the wording), since reminders have been shown to support behavioral change in various health-related contexts (e.g., Calzolari and Nardotto, 2017; Dai et al., 2021).

For the households in the INFO+MONEY condition, we complemented the information campaign by additionally giving them an unconditional cash transfer of 1,000 Bangladeshi Taka (11.8 USD in May 2020) via mobile cash, worth 2-3 days of agricultural wages in rural areas of Bangladesh. This was motivated as a support for households to adhere to physical distancing and prevention measures. In rural contexts like ours, adhering to social distancing measures often means forgoing income, since people cannot sell their goods on the street or jointly work on a field or construction site, etc., if staying at home and avoiding social contact.

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[4] Access to phones is almost universal in Bangladesh. At the beginning of the pandemic, while the country had 164.69 million population, it had more than 165 million mobile phone subscribers (Source: Bangladesh Telecommunication Regulatory Commission; see http://www.btrc.gov.bd/content/mobile-phone-subscribers-bangladesh-january-2020). In our sample, 99.77% households had their own phone or access to a phone via a neighbor who lives close by.

Note that households knew that the cash transfer was unconditional and that compliance with social distancing was not monitored by us.

The assignment to experimental conditions was done as follows. First, we allocated 60 villages to the control condition and 90 villages to one of the two treatment conditions, 45 to INFO-ONLY and 45 to INFO+MONEY. This assignment to conditions was done randomly, yet considering information from an initial village questionnaire (which was done with local elected leaders and key informants in the villages two weeks prior to our intervention, see the appendix for details) in order to ensure balance across all conditions. The three considered pieces of information elicited in the village questionnaire were: a) the number of returned migrants from cities and abroad (because they could carry COVID-19 with them), b) social events and restrictions on social gatherings (as this was likely to influence the spread of the virus), and c) COVID-19 incidences in the village (as an indicator for the overall situation in a village). We did the assignment using the re-randomization method by Schneider and Schlather (2017) as implemented in the R package 'minMSE' (Schneider and Baldini, 2019).

In all villages, we grouped geographically close households in pairs, using their geolocations and a nearest neighbor algorithm for matching (Lu et al., 2011; Beck et al., 2016). This meant that these pairs of households lived close to each other and were often direct neighbors. In the 90 villages with a treatment, we then randomly assigned one household in the pair to the treatment (either INFO-ONLY or INFO+MONEY) and the other household to a within-village control group. With this design, we created two types of control households: (i) those in villages with a treatment (either INFO-ONLY or INFO+MONEY), which allows studying spillovers from treated to untreated households in the treatment villages, and (ii) those in what we denote as CONTROL-villages where no household got any treatment.

Overall, we have data for 929 treated households (447 with INFO-ONLY in 45 villages, and 482 with INFO+MONEY in another 45 villages) and for 943 control households in the 90 treated villages. In addition, we have 1,209 households in the 60 CONTROL villages (where we also built pairs of households to be able to match neighbors' responses).

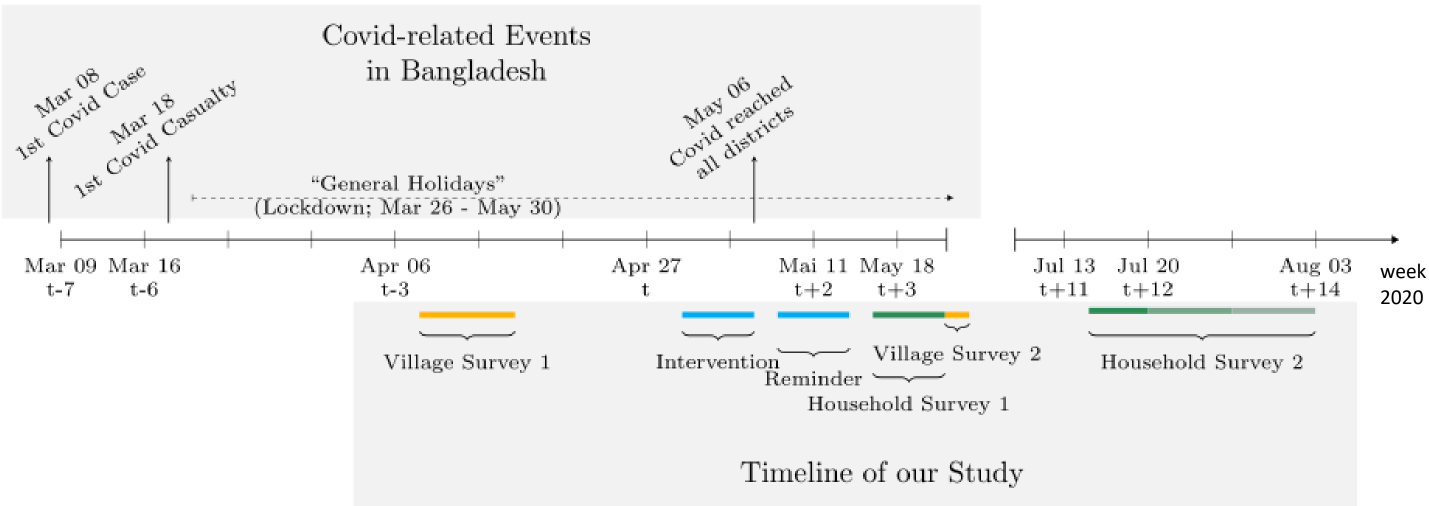
3. Implementation and methods

3.1 Procedures and measures

Our intervention was run between 30 April and 6 May 2020, less than two months after the first official COVID-19-cases had been confirmed in the country, and before the virus had reached the last of the 64 districts of Bangladesh. To evaluate the results of our randomized controlled trial, we conducted two separate household surveys, two weeks and about 2,5 months after the intervention (see Figure 1 for the timeline of our field study).[5]

The surveys focus on three preregistered outcome scales: Knowledge and attitudes, behavior, and health. The scales consider various aspects of the outcomes they aim to capture, and are coded such that higher values reflect more positive outcomes. The measures of the different aspects are standardized and we then take the average over all items to construct an overall outcome scale (see below for details). For ease of interpretation of the reported treatment effects, we normalize the resulting measures by, first, subtracting the mean of the respective measure for households in the CONTROL villages, and then dividing that by the standard deviation of the respective measure of this group.

Figure 1: Timeline of randomized controlled trial in the year 2020



Notes: The upper part of this figure displays the timeline of COVID-19 related events in Bangladesh, ranging from the first confirmed case on 8 March 2020 to the end of the general lockdown on 30 May. The lower part of the figure presents the timeline of our study. In yellow, we indicate periods during which we conducted village surveys with local elected leaders and key informants in the villages. The first village survey was used to ensure balance across our treatment conditions with respect to village characteristics. The intervention period – indicated in blue – was from 30 April to 6 May 2020. A week later, we made a reminder phone call (also shown in blue). In green, we show the timing of the household surveys. The first survey was run two weeks after the intervention. The second survey was stretched out between 14 July and 3 August 2020, with more households being surveyed earlier in this period (indicated by darker green). The average time between intervention and second household survey for the treated households was 77 days (minimum: 69 days, maximum: 89 days, median: 76 days).

[5] We had pre-registered to conduct the second household survey once the pandemic was over, or after three months. In light of Eid approaching (Eid is the biggest festival in the Muslim calendar) and an observable flattening of the curve of new incidents around the beginning of July 2020, we decided to slightly change the schedule such that all interviews could be completed before Eid.

To assess *knowledge and attitudes*, we proceed as follows. First, to measure *attitudes*, participants were asked to which degree they believed they could make a difference in fighting the pandemic, and whether they believed that everybody in society could make a difference. Second, to measure *knowledge*, we read a list of 12 possible measures to fight the pandemic, and asked for each possible measure whether it would be of any help or not (e.g., eating garlic, keeping a distance of at least three feet, wearing a mask or scarf that covers nose and mouth, or using one's elbow when sneezing and coughing; in total we listed five effective and seven ineffective measures). We then compute an overall measure of knowledge as the sum of correct answers to all 12 items. Finally, after standardization, we take the average of the three considered aspects (two for attitudes and one for knowledge).

To assess *behavior*, we asked for the extent of adherence to different preventative measures (such as, e.g., washing hands with soap) and of compliance to physical distancing measures, both for the past day and the last seven days. Because we have matched pairs of households that live close to each other (often direct neighbors), our design allows us to complement self-reported data on interaction with neighbors with responses from these neighbors to the corresponding questions (see the appendix for details how that information enters the scale). The situations referred to in the surveys include meeting one's neighbor at the well, on the way to school or to do grocery-shopping, etc. We use the weights from a principal component analysis (PCA) to account for potentially varying relevance of situations in which preventative measures are applied, physical distancing measures are practiced, or interaction with neighbors takes place. These aspects are then aggregated to the behavior scale as described above, analogously to the knowledge and attitudes scale.

Health was measured on the individual and on the household level. We asked a list of possible symptoms. We ask whether anybody in the household had suffered from these symptoms and thus concentrate on physical symptoms and disregard any mental health issues that the pandemic has also created (Giuntella et al., 2021; Fetzer et al., 2022). We count the number of symptoms, and use an indicator for high-risk individuals. Additionally, we inquired whether any household member had deceased since the onset of the pandemic.[6] These aspects

[6] Originally, we had planned to include data on diagnosis (or based on diagnosis, such as COVID-19-related deaths) and quarantine in this health scale as well. However, in praxis, testing was mostly done close to the capital region (but not in the rural areas of our field study), and the testing regime changed during our study (by shifting from free testing to costly testing in early May). Moreover, we believe that the results allow for a cleaner interpretation when excluding diagnosis and quarantine because they are arguably a function of individuals' own decisions, which might be affected by our treatment, for instance just by being more aware of possible symptoms; an effect that, e.g., Banerjee et al. (2020) report.

are then aggregated in analogy to the other two scales (see the appendix for details) into a health scale and a further variable that captures *casualties* only.

3.2 Methods

We estimate the effects of our treatment conditions on the outcome scales using the following linear model (OLS regression):

$$Y_i = \alpha + \beta_1 \cdot \{\text{INFO-ONLY}\}_i + \beta_2 \cdot \{\text{INFO+MONEY}\}_i + \gamma X_i + \varepsilon_i,$$

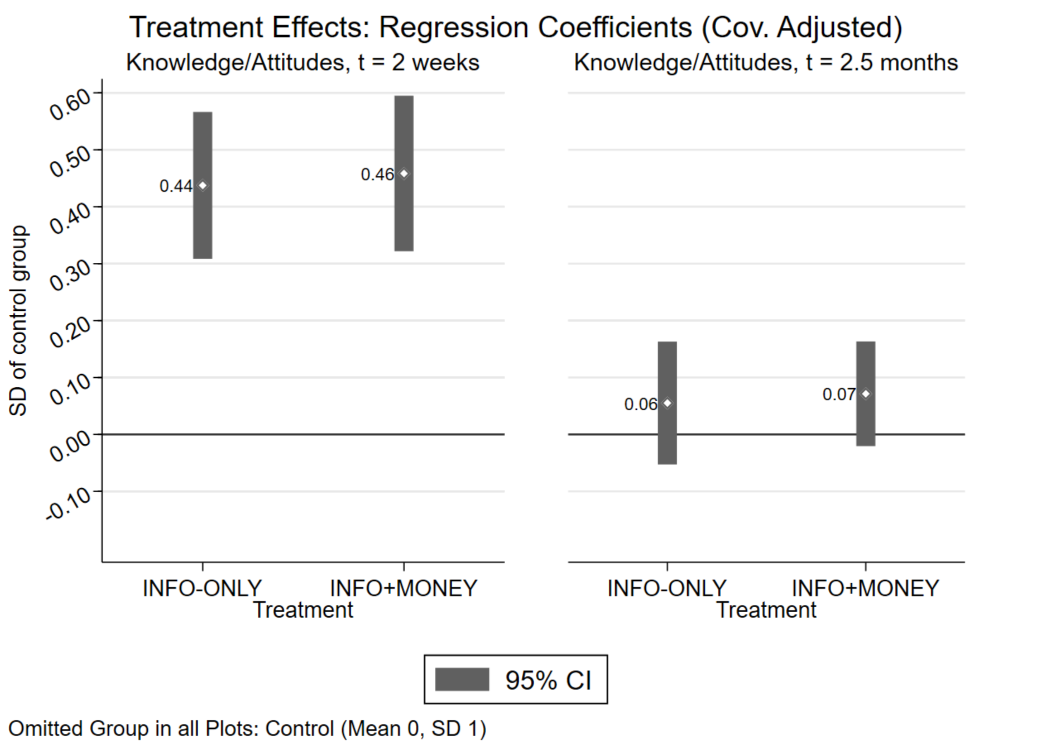
where Y_i is the outcome of interest for individual i either 14 days or about 2.5 months after the intervention, α is a constant term, X_i is a vector of village level information as used for assignment of villages to treatment groups, and ε_i is an error term. The coefficients of interest are β_1 and β_2 that relate to the two treatment dummies. The first measures how the INFO-ONLY condition affects the outcome of interest, and the second how INFO+MONEY changes the outcome. For the analysis of our main outcome scales, the omitted category are control households in pure CONTROL villages. Control households in treatment villages are excluded from these analyses. Regarding death rates, we pool all households in treatment villages to estimate the joint direct and spillover effects of the interventions in these analyses, the omitted category are households in pure CONTROL villages. We cluster standard errors at the village level. Our following results are robust to randomization inference that is independent of distributional or asymptotic assumptions (see Tables A7 to A10 in the appendix).

4. Results

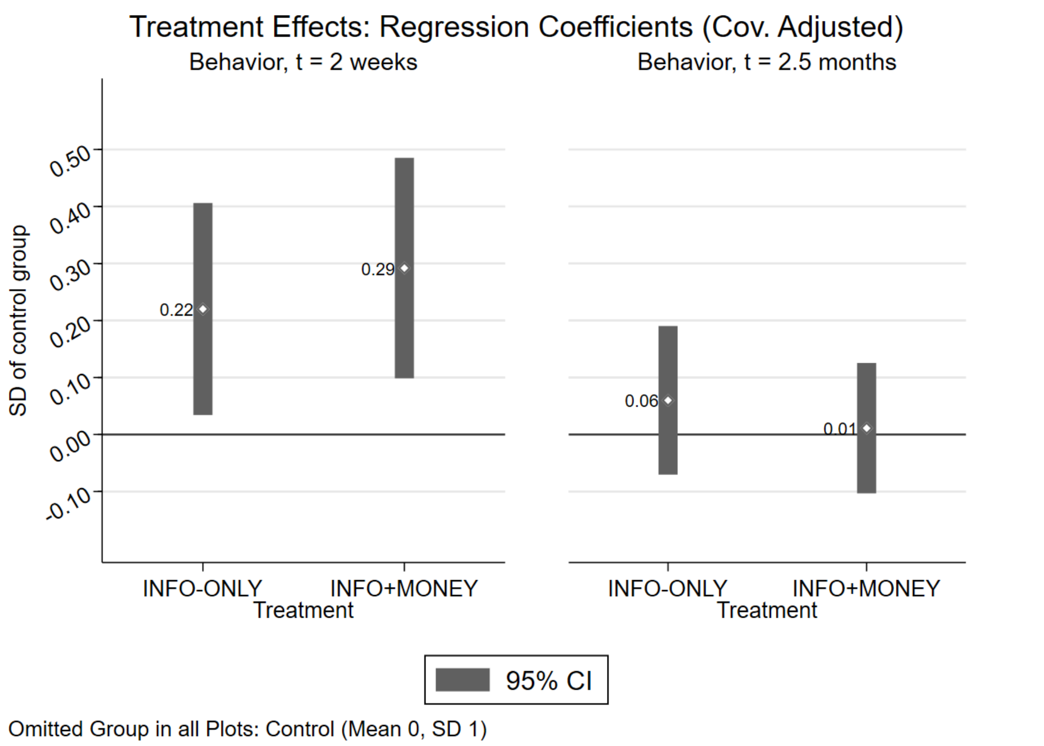
Figure 2 presents our first main result by showing the estimated coefficients for our two treatments when looking at our three pre-registered outcome scales. In this figure, households in the CONTROL-villages are the omitted reference group and scales are normalized such that they have a mean of zero and a standard deviation of one for the households in the CONTROL-villages. Positive coefficients for INFO-ONLY and for INFO+MONEY therefore indicate improvements in the respective scale, negative coefficients a deterioration compared to the control group.

Figure 2: Treatment effects on pre-registered outcome scales: Regression coefficients

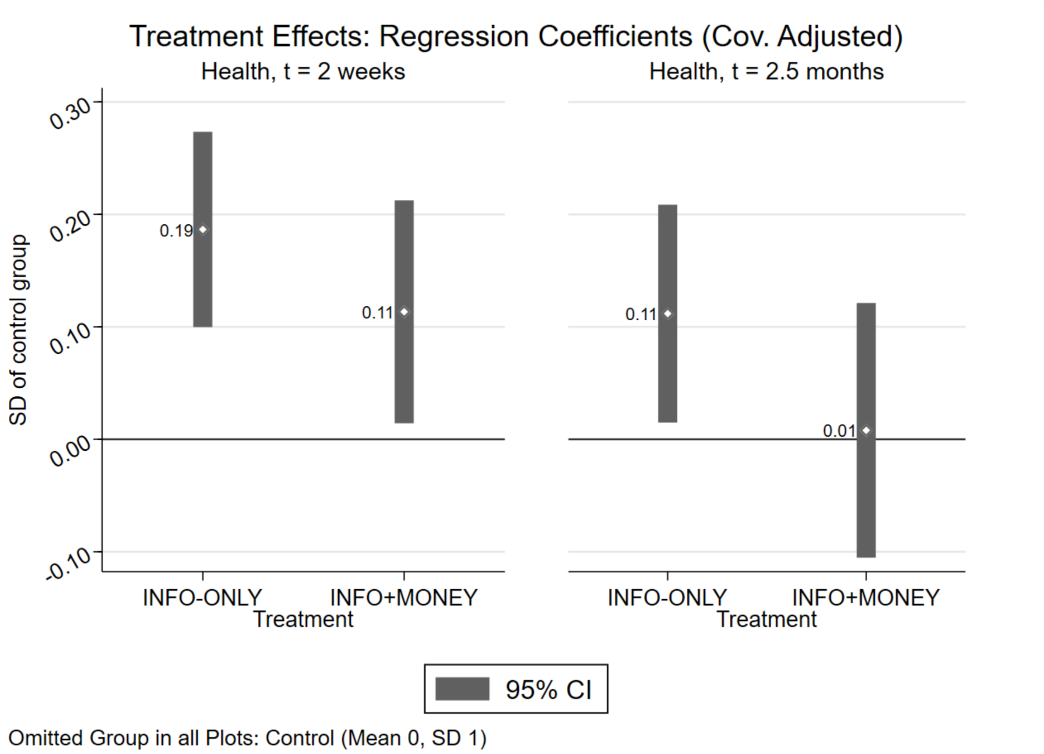
(a) Treatment Effects on Knowledge and Attitudes



(b) Treatment Effects on Behavior



(c) Treatment Effects on Health



Notes: The three panels show the effects of our two treatments (INFO-ONLY and INFO+MONEY) on the three pre-registered outcome scales. On the left hand-side of each panel, we show the effects at the time of the first household survey (2 weeks after the intervention) and on the right-hand side the effects at the time of the second household survey (about 2.5 months after the intervention). The bars show 95%-confidence intervals of the estimated coefficients for treatment dummies, the white diamonds with adjacent numbers the size of the treatment effect (see Table A1 in the appendix for regression details). The effects are expressed as a fraction of a standard deviation (SD) of the control group in the CONTROL-villages, i.e., for each scale, we have subtracted the mean of the households in the CONTROL-villages, and divided the result by their SD. The control group's scales thus always have a mean of zero and a standard deviation of one. Positive values in the graphs indicate better outcomes. The three panels show the effects for knowledge and attitudes in panel (a), for preventative behaviors in panel (b), and for health outcomes in panel (c).

Panel (a) of Figure 2 illustrates on the left-hand side the effects after two weeks, showing that both treatments improve knowledge about COVID-19 and attitudes (with respect to making a difference in fighting the pandemic) by almost half of a standard deviation ($p < 0.001$; OLS regression controlling for a set of 29 background variables; see Table A1 in the appendix). Handing out unconditional cash transfers on top of informing households does not make a significant difference. Looking at the right-hand side of panel (a) we note that after about 2.5 months the difference between CONTROL-villages and treated households has shrunk drastically and is no longer significant (although treated households still have higher values on average). This intertemporal waning of the treatment effect is due to treated households slightly deteriorating on the scale, but also to households in CONTROL-villages improving their outcomes over time. The latter does not seem surprising, given that the pandemic dominated the public discourse and was still covered extensively in mass media, probably supporting improvements in public knowledge about the disease. Yet, even after about 2.5 months, treated households still have significantly higher scores on this scale[7] than households in CONTROL-villages after two weeks (pooled effect (covariate adjusted) of +.25 SD in terms of the control group; $p < 0.001$), which implies some persistence of knowledge gains through the information campaign.

Panel (b) of Figure 2 shows treatment effects on preventative behavior, yielding a similar pattern as in panel (a). Both in INFO-ONLY and in INFO+MONEY we note an improvement of this scale by about a quarter of a standard deviation ($p < 0.022$ and $p < 0.004$, respectively; see Table A1 in the appendix) at our first measurement after two weeks. Again, there is no significant difference between the two treatments. Over the course of 2.5 months (see right hand side of panel (b) of Figure 2), the differences to the CONTROL-villages vanish almost completely, and they are no longer statistically significant. This is mainly driven by treated households reducing preventative behavior, rather than households in CONTROL-villages catching up in prevention over the course of about 2.5 months.

With respect to health, our intervention was successful both after two weeks and after about 2.5 months, as panel (c) of Figure 2 shows. Two weeks after the intervention, the health score improves by 19 percent, respectively 11 percent, of a standard deviation in our two treatment conditions ($p < 0.001$ and $p < 0.026$, respectively; see Table A1 in the appendix). After about 2.5 months, there is still an improvement of 11 percent of a standard deviation in INFO-ONLY, while INFO+MONEY has practically no further improvement over the CONTROL-villages ($p < 0.025$ and $p > 0.88$, respectively; see Table A1).

The latter may seem surprising at first sight. Yet, previous work (Gneezy and Rustichini 2000a, 2000b; Gneezy et al., 2011) suggests that monetary incentives may crowd out intrinsic motivation for cooperative behavior, which may also apply in our case. Giving money may also decrease subjects' motivation to pay attention to information. In fact, we find that the treatment effect on knowledge alone (i.e., without including attitudes) two weeks after the intervention is only about two thirds as big for the INFO+MONEY treatment as for the INFO-ONLY treatment (INFO-ONLY: 0.65 SD, INFO+MONEY: 0.43 SD; p(difference $\neq 0$) < 0.038; see Table A5 in the appendix). This may have an impact because knowledge after two weeks is significantly related to the health scale after about 2.5 months when looking at all

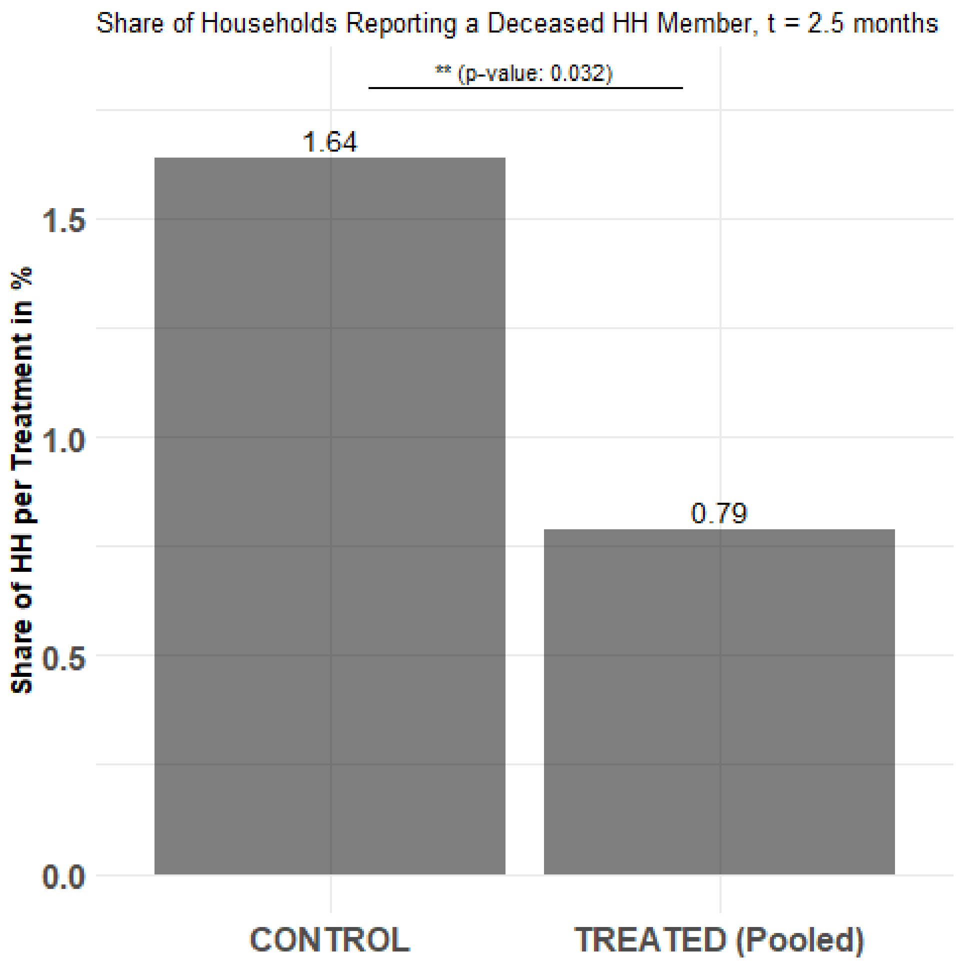
[7] For comparison reasons, we use a slightly modified scale here: Single aspects are not standardized before addition, as the variation (i.e., a variable's standard deviation) might differ in the first and second household survey.

households in our sample (OLS model, $p < 0.023$; see Table A6 in the appendix). Hence, the observed treatment difference in knowledge after two weeks could explain a part of the difference in the health score after 2.5 months. Despite this latter difference, overall we see from Figure 2 hardly any complementary effect of an unconditional cash transfer beyond the effect of the information provision through phone calls, implying that the information – that is given to all treated households – makes the difference.

Casualties within households are arguably not only the most dramatic health consequence of Covid-19, but probably also the most objectively measurable health outcome. This brings us to our second main result. In Figure 3, we compare all households (i.e. treated and untreated) in the treated villages to all households in the CONTROL-villages in order to provide the most comprehensive perspective on casualties. It displays the fraction of households reporting (in the second household survey) a deceased household member since the start of the pandemic: in CONTROL-villages, 1.64 percent of households report a deceased member. This is more than twice the fraction of households in treatment villages where 0.79 percent report a casualty. This difference is significant in a simple difference-in-mean comparison using OLS ($p < 0.033$, see Table A4 in the appendix), but also when applying more sophisticated approaches to model the death rate ($p < 0.03$; Poisson regression; see Table A4).

It is important to stress that the fraction of households reporting a casualty in the treated villages is almost identical for treated and untreated households in these villages (0.79 and 0.78 percent of households with a casualty among the treated and untreated households). This is an indication of positive spillovers among neighbors in treated villages with respect to casualties. In contrast, we do not find any evidence for positive spillover effects from treated to untreated households within treated villages regarding the scales on knowledge and attitudes and on preventative behaviors. Yet, there is a positive spillover effect on the health scale of untreated households in INFO-ONLY for the short time horizon ($p < 0.073$; see Table A2 in the appendix). Overall, these findings suggest that the reduction in casualties in untreated households in treated villages is likely to reflect fewer infections due to lower incidence levels in treated villages. This means that the changes in behavior and knowledge of treated households seem to protect the health of untreated households by containing the contagion of the disease.

Figure 3: Share of households reporting a deceased member since onset of pandemic, separately for treated villages (including treated and untreated households there) and CONTROL-villages.



Notes: The bars show the fraction of households (in percent) that reported a deceased household member since the beginning of the pandemic at the time of the second household survey. The left bar refers to all households in the CONTROL-villages that had not received information or money. The right bar refers to all households in the treated villages, thus including both treated households (either INFO-ONLY or INFO+MONEY) and untreated households in these villages. P-values result from a group comparison using OLS regression and a pooled treatment indicator variable (see Table A4 in the appendix for regression details). The fraction of households with a deceased member does not differ between treated households (0.79%) and untreated households (0.78%) in treated villages, which indicates positive spillovers from treated households on untreated households in these villages.

These results have an important implication: Providing a one-to-one information campaign to only some fraction of local households is able to reduce overall local casualties, amplifying the effects of an information campaign beyond directly treated households. Extrapolating the reduction of death rates among households in treated villages in comparison to CONTROL-villages also suggests that within the first year of the pandemic (from March 2020 to February 2021) about one half of casualties, i.e., about 5,500 lives, could have been saved in Bangladesh through a timely and reliable information campaign over the phone. The costs of our intervention amount to less than 1,500 US Dollar per death averted. Considering the World Health Organization's (2003) threshold for defining highly cost-efficient interventions as the average GDP per capita in a country (about 2,000 US Dollars in Bangladesh in 2020) per QALY (quality adjusted life year) gained, our intervention can be considered as highly cost-efficient even if all the deceased persons would have had a life expectancy (in good health) of only 9 more months.

5. Conclusion

We have shown that an information intervention at the pandemic's onset in rural Bangladesh has not only improved knowledge, preventative behavior, and self-reported health symptoms (i.e., the lack thereof), but has also reduced deaths by about 50% in comparison to control villages. The costs of our intervention can be considered low, qualifying for what the WHO denotes as a highly cost-efficient intervention. In fact, providing information via the phone to remote locations is not only cost-efficient, but also looks like a scalable solution to efficiently contain pandemics early on and thus save lives, even when most of the information is already being broadcast using different mass media channels. Our salient information over the phone changed knowledge, behavior, and health outcomes of treated households in comparison to households in control villages that had only access to mass media information. Importantly, the change in behavior of treated households also had noticeable positive spillovers on death rates of control households in treated villages. The latter benefit from the better prevention behavior of treated households and thus also face lower death rates, even though we have seen no statistically significant spillovers on the knowledge and behavior of control households in treated villages. It seems that informing many households in a village can be sufficient to protect also the uninformed households in these villages.

Adding an unconditional cash transfer (of about 2-3 days wages) has not produced further improvements. Thus, our randomized controlled trial confirms that monetary incentives need not shift human behavior in desired directions (Gneezy et al., 2011). They can even sometimes be detrimental for motivation to show a particular behavior (Gneezy and Rustichini, 2000a). Later in the pandemic, financial incentives have been found to be able to improve the take-up of vaccines (Campos-Mercade et al., 2021a; Schneider et al., 2022). Yet, at the pandemic's onset, when vaccines were not yet available, it seems that information about how to prevent the exposure to and the spread of the virus has been of major importance, independent of additional monetary incentives. This leads us to summarize our findings in a way that might also become relevant for further waves of the current or new waves of a next pandemic after COVID-19: Quick and reliable one-to-one information at the onset of a pandemic can save lives.

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