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University of Kent

School of Economics Discussion Papers

# **Personalized Information Provision and the Take-Up of Emergency Government Benefits: Experimental Evidence from India**

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May 2022

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# Personalized Information Provision and the Take-Up of Emergency Government Benefits: Experimental Evidence from India\*

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May 2022

## Abstract

The COVID-19 pandemic has adversely affected the livelihoods of hundreds of millions of households, resulting in widespread poverty and food insecurity. To mitigate these effects, many governments have introduced additional benefits as part of their existing welfare schemes. However, there is often a gap between the introduction of these programs and access to the benefits. To shed light on the source of these gaps, we conduct a field experiment with just over 1,000 slum-dwelling households in Uttar Pradesh, India during the COVID-19 pandemic. The intervention randomly exposed individuals to personalised information about government benefits via cell phones. We find that the simple and low-cost provision of personalised information i) increased the accuracy and precision of participants' knowledge about their entitled benefits, ii) increased access to and utilization of benefits, and iii) improved wellbeing (as measured through consumption, food insecurity and mental health). We do not find significant differences in effects based on whether males or females are targeted. Our findings show that there are large gaps in knowledge of and access to government benefits (despite widespread publicity about the programs) which can be reduced via a simple and low-cost information intervention.

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# 1 Introduction

Emergency government benefits are often used to mitigate the effects of severe shocks on individuals and households due, for example, to natural disasters, conflicts and epidemics. A recent example is, of course, the COVID-19 pandemic. As lockdowns and social distancing measures adversely affected incomes and livelihoods, causing widespread poverty and food insecurity, governments around the world introduced a variety of emergency relief packages, often by extending existing welfare schemes.<sup>1</sup> However, there is often a gap between the introduction of these measures and access to the benefits they entail, potentially due to ineffective communication, lack of awareness or understanding among the intended beneficiaries, complexity of the schemes or corruption. It is thus important to understand both the nature of these gaps and the things that governments can do to ensure that programmes effectively reach their intended beneficiaries (particularly the poorest individuals).

In this paper, we develop and evaluate a simple, low-cost information intervention to address the following question: does personalised information on access to emergency government benefits improve knowledge, uptake and wellbeing? To answer this question, we conduct a cluster randomised controlled trial (cRCT) with about 1,000 slum-dwelling households in Uttar Pradesh, India during the COVID-19 pandemic. The intervention randomly exposed individuals to personalised information about government benefits via cell phones.

Our study focuses on three large programs - the Pradhan Mantri Garib Kalyan Anna Yojana (PMGKY hereon), Pradhan Mantri Ujjwala Yojana (PMUY hereon) and Pradhan Mantri Jan Dhan Yojana (Jan Dhan hereon) which provided aid in the form of food grains (PMGKY), free liquefied petroleum gas (LPG) refills (PMUY) and direct cash transfers to women (Jan Dhan). Aid entitlements delivered through these programs - the latter two of which existed prior to COVID - provided additional aid on top of existing entitlements. The additional entitlements formed part of a larger package of relief measures, which were announced at a similar time, and which had differing eligibility requirements. In this fast-moving and chaotic roll out, it is likely that eligible recipients might have faced confusion and uncertainty about what aid they should receive.

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<sup>1</sup>According to a recent estimate, a total of 3,333 social protection measures were introduced in 222 countries and territories between March 2020 and May 2021 in response to the pandemic ([Gentilini et al. \(2021\)](#)).

Indeed, our baseline survey conducted in September - October 2020 documents, on the one hand, relatively accurate knowledge regarding which programs households are eligible for, but also widespread inaccuracies in knowledge of *how much* they should receive. In particular, we document widespread *over-estimation* of entitlements across all three programs. Moreover, households' reports of how much aid they received indicated that they were receiving less aid than their actual (and perceived) entitlement.

To counter these misconceptions, we provided personalised information on households' aid entitlements from each of these programs through text messages, which were followed up by a phone call. To test the effectiveness of this low-cost intervention, we designed a cluster randomised controlled trial using a randomised saturation design to measure information spillovers. 75% of randomly selected study households living in 40 randomly selected slums out of a set of 60 slums received the messages and calls, with study households in the further 20 slums forming part of a pure control group. The remaining 25% of study households in the 40 treated slums are 'spillover controls'. Within the 40 treatment slums, we further randomised the gender of the recipient of the information: in 20 slums, a female adult received the information, while in the other 20 slums, a male adult received it.

Comparing the average outcomes of the treated households in treated slums with those of control households identifies the impacts of the information, while a comparison of average outcomes of the spillover controls with those of control households identifies within-slum information spillovers. We can further identify the impacts of providing information to a male or female household member by comparing the average outcomes of treated households in the slums where a male adult received the information with those where a female adult received it.

We find that around two months after the intervention, treated households updated their beliefs regarding how much they should receive from each program. While households in the control slums continued to over-estimate their entitlements, treated households in treatment slums downgraded theirs to the actual entitlements. In the case of Jan Dhan, they also downgraded their beliefs on the number of household members eligible to receive aid from the program. Thus, this simple intervention improved the accuracy of households' beliefs.

Next, we turn to study the impact on actual access to benefits. We find that treated households in treated slums receive around 6kg more - on average - in food grains from PMGKY and just over INR 100 more, on average, from Jan Dhan. The increased aid also closes the gap between the amount households are entitled to and what they receive, with control households receiving less than their entitlements on average.

Finally, we consider whether the increased aid receipts due to the intervention impacted households' food security, mental health and life satisfaction. First, we find that the intervention reduced food insecurity. Interestingly, we also document a simultaneous reduction in expenditures – for food and total expenditures – among the treated households, which suggests that they substituted away from purchasing food from the market in response to the higher actual receipt of food aid.

Turning to our measures of mental health and life satisfaction, we document significant improvements in the mental health of respondents from treated households, and in their financial and life satisfaction. We hypothesize that the more accurate knowledge of benefit entitlements improved households' actual benefit receipts, thereby improving their food security and financial situation and thus generating better mental health.

Across the different outcomes, we document few differences between the effects on households where the male adult received the information relative to households where the female adult received the information, suggesting relatively frictionless information sharing within the household on this issue. A notable exception is in mental health, where the female survey respondents living in households where a male adult received the information experienced a higher improvement in mental health relative to households where a female adult received the information.

The analysis of spillover effects in informational interventions is important to assess whether their existence would improve cost-effectiveness. Our results, however, suggest that there are few or limited within-slum information spillovers in this context. We document some small within-slum spillovers of the male treatment on respondents' knowledge of program entitlements, but not on other outcomes. The female treatment, by contrast, did not generate any within-slum information spillovers on any of the outcomes. Further analysis shows that treated households were more likely to chat to people outside their

household about government support for covid. However, the spillover controls do not increase their chats about this topic, suggesting that network connections in the slums are very sparse leading to little information flow from the treated households to spillover control households in the treated slums.

Our paper contributes to a growing literature on the effects of providing information on government benefit entitlements. One of the first papers in this literature, [Chetty and Saez \(2013\)](#), report on a randomised intervention in which tax preparers gave personalised information to recipients of 'Earned Income Tax Credit', the largest cash transfer programme for low-income families in the United States. The study finds that providing information about tax incentives had no systematic effect on average earnings. However, the earning decisions of the intended beneficiaries responded to tax-related advice. [Abramovsky et al. \(2016\)](#) investigate a large scale pilot of a major welfare intervention in Colombia that combined home visits by trained social workers to households in extreme poverty to improve knowledge of, and access to, existing social welfare programmes, together with an expansion in the supply of existing programmes. The authors find no consistent effects of the intervention on knowledge and uptake of social programmes. [Carneiro et al. \(2019\)](#) investigate a similar programme in Chile using a regression discontinuity design. They find that the programme led to increased uptake of employment programmes and a family allowance for poor children but, otherwise, had no impact on the beneficiaries in the short or medium term.

[Berg et al. \(2021\)](#) evaluates a programme in India that recruited, trained and incentivised agents to provide information to households eligible for welfare schemes, and assisted them with applications. The programme increased awareness about welfare schemes in the eligible population but had no effect on applications or approval. Nevertheless, the intervention led to increased satisfaction with government services. Unlike the previous studies, we find large effects not only on awareness about government welfare schemes but also uptake and welfare although the intervention deployed only a modest amount of resources per household (one SMS message and a phone call providing personally tailored information on eligibility). This may be due to the fact that, unlike the previous studies, our intervention focused on emergency government benefits introduced in the midst of a severe economic shock.

We also contribute to the recent literature on the efficacy of information interventions in changing the behaviour of individuals and households during the COVID-19 pandemic. However, the existing literature is focused, for the most part, on providing health-related information (see, for example, [Banerjee et al. \(2020\)](#); [Abaluck et al. \(2021\)](#); [Islam et al. \(2021\)](#)) while we study an intervention which is specifically about providing information on emergency government benefits introduced during the pandemic.

The rest of the paper is organized as follows. In the next section we describe the background and context in which our study takes place, including the state of the pandemic at the time of the study, along with the particular programs that the government of India instituted to provide relief. In Section 3 we describe the intervention we conduct and the design of our experiment in detail. In the subsequent section, we describe the data and our empirical methodology. In Section 5, we report the results of our analysis, while Section 6 concludes.

## 2 Background/Context

On March 24, 2020, the government of India imposed one of the most severe lockdowns in the world, throwing the economy into disarray. In a matter of weeks, employment had fallen by 30%, resulting in the loss of approximately 120 million jobs ([Vyas, 2020](#)), including millions of migrant workers stranded between their places of work and their homes. In response to this economic calamity and the widespread suffering documented in the country's media, the government started to develop and announce a series of emergency measures designed to offer economic relief to the poor.

Among the many measures that the government introduced or amended over the course of the pandemic response were the following: the Pradhan Mantri Garib Kalyan Anna Yojana (PMGKY), the Pradhan Mantri Ujjwala Yojana (PMUY), and the Jan Dhan Yojana (JDY). Our information intervention was designed to provide information on these three programs in particular. Below we provide a brief description of the contents of these programs.

The Pradhan Mantri Garib Kalyan Anna Yojana was announced on 26 March 2020, just days after the nationwide Covid-19 lockdown was implemented, under the Rs 1.70



lakh crore (i.e. Rs. one trillion) Pradhan Mantri Garib Kalyan Yojana welfare package for COVID-19. Under this scheme, all ration cardholders – who were already beneficiaries of the public distribution system – were eligible to get five kilograms (kg) of wheat and rice (per person, per ration card) along with 1 kg pulses (per household) free of cost – on top of any previous entitlements. The Pradhan Mantri Garib Kalyan Yojana package included other elements (e.g. direct cash payments to farmers and government provision of Employee Provident Fund (EPF) contributions for certain businesses), on which our intervention did not provide information.

The next scheme included in our intervention was the Pradhan Mantri Ujjwala Yojana (PMUY), which was originally launched in 2016 to provide free liquified petroleum gas (LPG) connections to women in poor households (i.e. households with a below poverty line (BPL) card). The PMUY was extended starting from July 2020, allowing PMUY recipients to obtain free of cost refills for a period of three months. This was later extended to March 2021.

The last scheme in our intervention was the Jan Dhan Yojana (JDY), which provided women already holding Jan Dhan Yojana accounts with Rs. 500 per month for three months, from April to June 2020 through transfers into their JDY accounts. Like the PMGKY, the announcement of this scheme was also made on March 26th. In addition, the scheme allowed for poor widows, poor senior citizens and poor disabled people to receive Rs 1000 for three months into their JDY accounts.

Due to the large number of relief measures adopted (far in excess of the three numerated above) and the various eligibility requirements and varying amount of relief allowed under each scheme, it is reasonable to imagine a significant amount of confusion and uncertainty regarding what individuals believed they were eligible for. Schemes were also extended and amended as the COVID crisis continued, adding to this confusion and uncertainty. The goal of our intervention was to provide targeted information for these three schemes in order to eliminate some of this uncertainty. The details of our intervention and the design of our experiment are elucidated in the following section.

### 3 Intervention and Design of Experiment

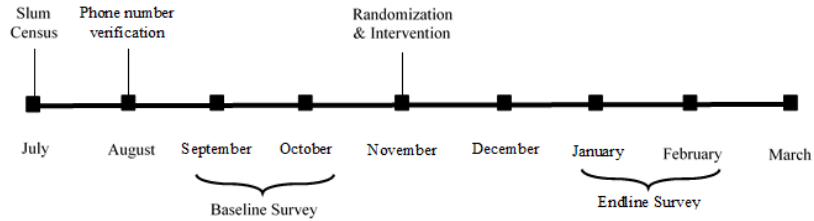


Figure 1: Project Timeline, 2020-21

Our experiment took place with 1,006 households living in 60 different slums in the city of Kanpur, Uttar Pradesh. The treatments were randomly allocated at slum-level in a random saturation design (as in [Baird et al. \(2018\)](#)) to capture information spillovers among households living in the same slum.

The slums were randomly allocated to one of two treatment groups or a control group in equal proportion for a sample size of 20 slums per arm. Within the treated slums, 75% of households were randomly selected to receive the intervention (so were directly treated), with the remaining 25% serving as ‘spillover controls’, households in the same slum that did not receive any messages from the research team, but could have received information from the treated households in the same slum. They were thus indirectly treated. This last group allows us to capture within-slum information spillovers.

Our experiment included two treatment arms. In both treatment arms, we provided information on government benefits the household is entitled to, including the exact entitlement as determined from data provided at baseline, via SMS messages (reproduced in [Appendix 7](#)) and follow-up voice calls. However, we varied the identity of the individual to whom the information was given. In the first treatment arm, Treatment Male (*Treat M*), the information was provided to an adult male household member, typically the head of household. In the second treatment arm, Treatment Female (*Treat F*), the information was given to an adult female. The voice calls were conducted by trained female enumerators in both treatment arms.

Figure 2 summarizes the experimental design, including the sample sizes in each arm.

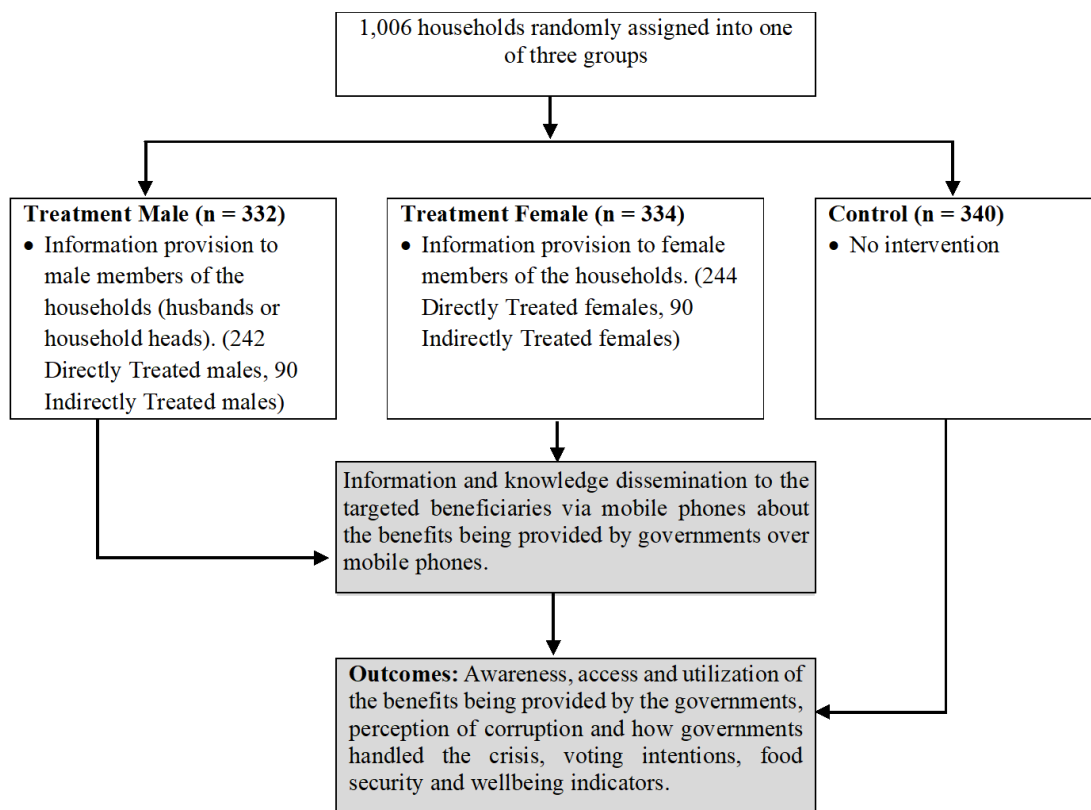


Figure 2: Experimental Design

## 4 Data and Empirical Methodology

### 4.1 Study sample and sample balance

Our sample focuses on the "urban poor", in particular, poor households residing in urban slums, who have typically been treated as a homogeneous group. Government policies have focused on the poor residing in the rural regions, overlooking the plight of the "urban poor". As a result the locus of poverty has gradually shifted from the rural areas to the urban slums. The current situation of urban slum dwellers in India is of great concern to development planners and policymakers, as it is often associated with crime, disease, violence and lack basic facilities.

In terms of numbers, only 5 states in India: Maharashtra, Andhra Pradesh, Uttar Pradesh, West Bengal, Tamil Nadu account for about two-thirds (61.9 per cent) of the total slum population in the country (of the Registrar General Census Commissioner (2013)). Uttar Pradesh has 9.5% of its population and more than 13% of its children living in slums, and also has the second largest number of Scheduled Caste population living in these slums (around 1.21 million). We have chosen Kanpur as our study site as it is the largest city in Uttar Pradesh and is home to one of the largest slum populations in the country (Sawhney (2016)).

Slum size in Kanpur varies significantly – there are slums with population of more than 20,000 households and there are also very small slums with less than 100 households. The 60 Kanpur slums included in the study were selected using stratified random sampling from all the slums in the city, with strata defined in terms of slum size, to ensure that the 60 slums covered in the study constitute a representative sample of all slums in the district.

For the baseline survey, conducted between September and October 2020, 1,200 households were randomly selected from an initial list of 1,800 from whom phone numbers had been collected in 2019. 1,006 households were successfully interviewed by phone in the baseline survey after removing invalid and "no response" phone numbers. The endline survey was conducted between January and February 2021, when 993 households were successfully interviewed; thus, the attrition rate between the baseline and endline surveys was very low (1.3%).

The main respondent to the baseline and endline surveys was a female adult, typically of reproductive age. Both surveys collected information on basic demographic characteristics of the respondents and the other household members, information on their earnings, and on the outcomes detailed in the next sub-section.

Tables 11 and 12 show the baseline descriptive statistics and sample balance for individual and household characteristics, respectively. The survey respondent in the control group was around 30 years old on average. Over 75% were married, with around 14% with no education, 32% having completed fewer than 10 years of schooling and 54% having completed at least 10 years of schooling. 75% were housewives and just over 10% were unemployed.

Around 84% of control group households were Hindu, 45% from scheduled castes/tribes and 33% from other backward castes. Respondents lived in households with just over 5 household members, with average monthly income of Rs. 9,500. The main occupation was day labor (48% of sample), with a further 30% of the sample earning their main income from business activities. Fewer than 15% of control group households had a private sector job. Even if a small number of differences across treatments appear statistically significant, the omnibus test of joint significance is always rejected.

Table 13 shows balance in slum characteristics, where only the proportion of households receiving the Jandhan program appear to be significantly different. As before, we can reject joint significance.

## 4.2 Outcome Variables

We investigate intervention impacts on two sets of primary outcomes and two sets of secondary outcomes. The two sets of primary outcomes include measures related to knowledge and awareness of the three programs, and receipt of benefits. On the former, we analyse impacts on knowledge on the existence of the program, and on program entitlements. We also study impacts on the gap between respondents' beliefs about what they should receive from a program and what they should actually receive (as per the program eligibility criteria).

On the latter - receipt of benefits - we analyse intervention impacts on whether households receive anything from the program, and how much they receive. We further study impacts on the gap between what a household receives and what it is eligible to receive; and on the gap between what it receives and what it believes it should receive.

As secondary outcomes, we consider intervention impacts on measures of food security – measured through an index of food insecurity and household food and total expenditures – and on self-reported mental health, life satisfaction and satisfaction with their financial situation.

In Table 14 we check for sample balance in the baseline measures of some of the primary outcomes, such as whether the respondents mention the different programs at the baseline, whether they report being entitled to receive the programs and also unconditional entitlement. In this case the omnibus test of joint significance is not rejected. Consequently, our main empirical specification controls for the baseline outcome in order to be able to estimate the causal effects of the interventions.

In a companion paper, we investigate the effects of the intervention on a set of political outcomes, including perceptions about how different levels of government have handled the COVID-19 crisis, perceptions of corruption and the respondents' voting intentions in future elections.

### 4.3 Econometric Specification

Our experimental design allows us to identify, not only the effects of the different treatments on the outcomes of interest, but the possible spillover effects on households located within a treated cluster. In order to estimate the causal effect of treatments and spillovers on the outcomes of interest, our analysis employs the regression equation below, where  $y_{ijt}$  is the outcome of individual  $i$  in slum  $j$  in period  $t$ ,  $T_{jt}^r$ , where  $r = m, f$  is equal to 1 if slum  $j$  is allocated to the treatment group  $r$  and 0 otherwise,  $Q_{ij}$  is a dummy variable which indicates whether a household is directly treated and  $Spill_{ij}$  is a dummy variable which indicates whether the household is a 'spillover control' household within a treated cluster.

$$y_{ijt} = \alpha + \sum_r \beta_r T_{jt}^r * Q_{ij} + \sum_r \eta_r T_{jt}^r * Spill_{ij} + G'_{ijt} \gamma + \varepsilon_{ijt}$$

The coefficient  $\beta_r$  will give us the effect of the male and female treatments on the directly treated households, while  $\eta_r$  measures the effect of the treatment on the spillover households. The latter will only be significant if there is information transmission within households residing in the same slum.

We will also estimate whether the differences between the coefficients  $\beta_m$  and  $\beta_f$  are statistically significant, in order to understand whether results are different for households where the male or the female are treated. These differences will be zero if there is effective within-household informational transmission.

We also add  $G_{ij}$ , which is a vector of control variables that vary both at the individual and slum level. These are variables that determined eligibility for the program, and variables for which there was an imbalance at the baseline, such as the main activity of the household head. In particular, we control for whether the main source of earnings of the household is day labor or a business, in a government or private sector job, and whether households are eligible for the three programs.

For some outcomes, and depending on availability,  $G_{ij}$  includes the baseline value of the variable in order to improve power (McKenzie (2012)). Since treatment varies at the slum level, standard errors are clustered at the slum level (Abadie et al. (2017)).

## 5 Results

### 5.1 Knowledge

We start by presenting results on the impacts of the information intervention on knowledge of any entitlement from the program, and the size of the entitlement. These are shown in Table 1. Aside from a small reduction in the proportion of households mentioning the PMGKY program in the Male treatment arm, we find no significant effects of the intervention on the extensive margin. The coefficients for the spillover households are also small and statistically insignificant, suggesting a lack of spillovers to these households. Thus, the intervention did not affect knowledge on the extensive margin.

Table 1: Intervention Impact on Knowledge of Program Existence

	(1) Mentioned PMGKY	(2) Mentioned PMUY	(3) Mentioned Jan Dhan
Treatment Female	0.002 (0.006)	-0.001 (0.008)	0.001 (0.014)
Treatment Male	-0.022** (0.008)	-0.005 (0.008)	-0.013 (0.012)
Treatment Female - Spillover	-0.010 (0.010)	-0.001 (0.003)	0.012 (0.017)
Treatment Male - Spillover	-0.014 (0.012)	0.002 (0.003)	0.005 (0.013)
Baseline Outcomes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes
Control Mean (EL)	0.916	0.246	0.495
p-value (Female = Male)	0.027	0.679	0.327
Observations	993	993	993

Note: Standard errors (in parenthesis) are clustered at the slum level. Stars indicate statistical significance: \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Next, we study whether the interventions impacted households' knowledge of entitlements. The three programs differed in whether the entitlement was at the individual or household level. For PMGKY, entitlements were based on the number of individuals listed on eligible households' ration cards. By contrast, PMUY entitlement was at the household level only, and Jan Dhan entitlement at the individual level.



We start by plotting the raw baseline and endline distributions of responses on how much each household is entitled to on (depending on the scheme) a per-person or per-household basis. In Figure 3, we plot these distributions separately for each treatment group for the households which reported knowledge of the existence of the program.<sup>2</sup>

At baseline, significant proportions of respondents over-estimated – consistently across the treatment arms – the amount they were eligible for from each program. However, after the intervention, we observe a shift towards zero in the two treated arms, while the distribution in the control areas closely resembles that at baseline. The latter suggests little updating of misperceptions over this time period in the absence of our interventions. The updating is most strongly observed in the case of the Jan Dhan program, which directly deposited funds into the bank accounts of eligible household members. At baseline, most households significantly over-estimated the amount each recipient in their household should receive. By endline, however, there is a stark correction in this misperception in both treated groups, while it persists in the control group.

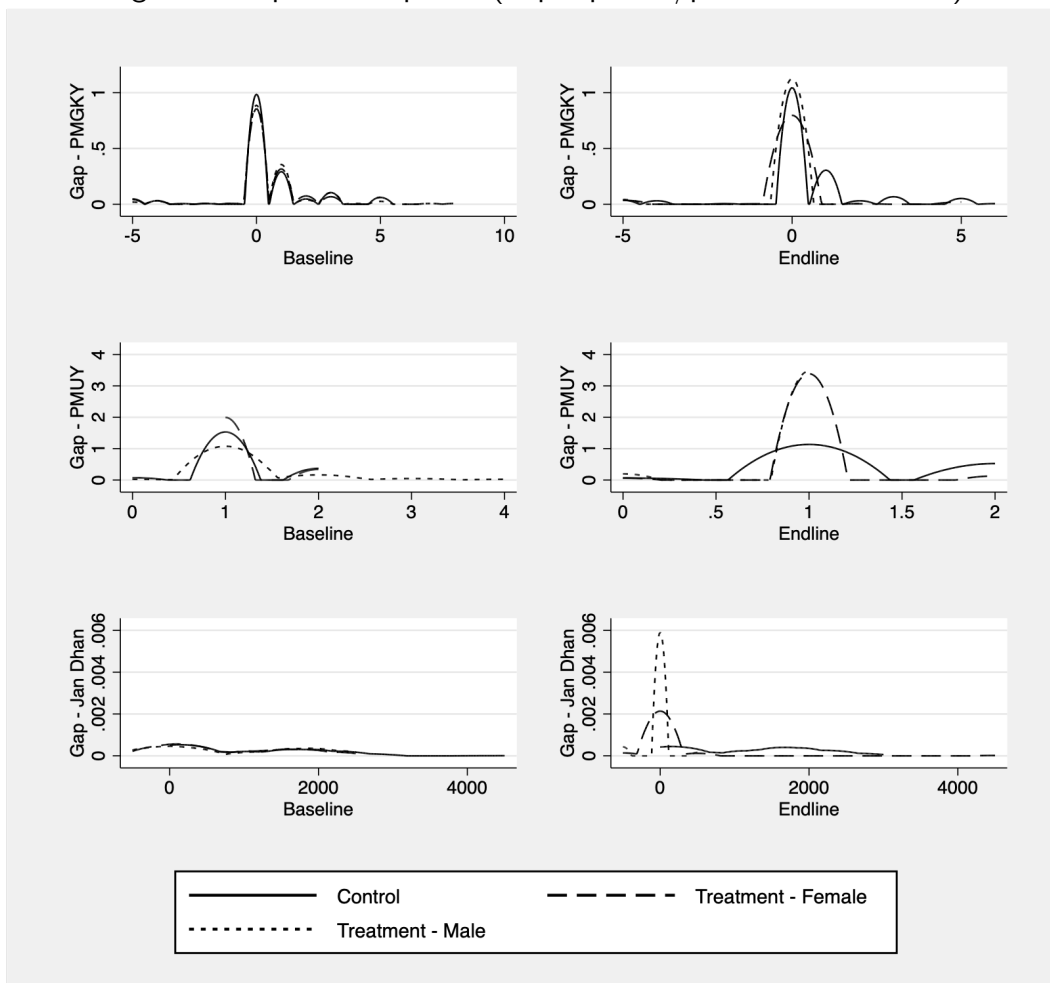
We study the impacts on knowledge of entitlements, and the gap in perceptions relative to actual eligibility more formally by estimating the main econometric specification on perceived per-person (or per-household for PMUY) entitlements for each program, the number of household members the respondent believed should be eligible for the program and the gap between perceived household-level entitlement and actual entitlement. We do so for all households in our sample, regardless of their awareness of the program. These are presented in Tables 2 and 3.

Columns (1) to (4) of Table 2 show that both treatments revised downwards respondents' knowledge of how much each eligible person (or household in the case of PMUY) was entitled to receive from the three programs. The coefficients are large in magnitude and statistically significant at the 1% level of significance. In control households, respondents continue to over-estimate how much they should receive from these programs. However, this over-estimation is reversed in the treated households. Interestingly, we find that this downward revision is slightly higher among respondent households where the male member was provided the information, though it is only (weakly) statistically significantly different for the case of wheat entitlements under PMGKY. The results also indicate little evidence

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<sup>2</sup>For legibility, the Figure only includes directly treated households in treatment group and the control households. Patterns for the spillover control households closely match those for the control households.

Figure 3: Gap in Perceptions (in per-person/per-household terms)



Notes: Figure plots distributions of gap between what respondents believe they should receive from program and actual entitlement for each of the three programs. Figure plotted for respondents who were aware of the program.

Table 2: Intervention impact on knowledge of entitlements

	Entitlement per person/per HH				Number entitled in HH		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	PMGKY wheat	PMUY	Jan Dhan	PMGKY pulses	PMGKY wheat	PMUY	Jan Dhan
Treatment Female	-0.508*** (0.078)	-0.068*** (0.019)	-528.542*** (53.004)	-0.228*** (0.058)	-0.138 (0.156)	-0.022 (0.024)	-0.313*** (0.062)
Treatment Male	-0.620*** (0.072)	-0.092*** (0.017)	-542.782*** (47.982)	-0.304*** (0.032)	-0.107 (0.140)	-0.044*** (0.013)	-0.226*** (0.059)
Treatment Female - Spillover	-0.026 (0.112)	-0.034 (0.020)	-42.006 (75.524)	0.041 (0.061)	0.112 (0.176)	-0.022 (0.015)	0.033 (0.091)
Treatment Male - Spillover	-0.289** (0.124)	-0.017 (0.022)	90.352 (70.595)	0.073 (0.076)	-0.145 (0.191)	0.007 (0.021)	0.272** (0.123)
Baseline Outcomes	Yes	Yes	Yes	No	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean (EL)	5.090	0.321	782.282	1.192	4.610	0.267	1.027
p-value (Female = Male)	0.092	0.112	0.697	0.138	0.856	0.342	0.105
Observations	987	990	989	993	993	993	993

Note: Standard errors (in parenthesis) are clustered at the slum level. Stars indicate statistical significance: \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 3: Intervention Impact on Gap in Beliefs Re. Entitlement

	(1)	(2)	(3)
	PMGKY gap	PMUY gap	Jan Dhan gap
Treatment Female	-0.409*** (0.106)	-0.065*** (0.019)	-529.596*** (49.695)
Treatment Male	-0.484*** (0.072)	-0.095*** (0.019)	-544.901*** (45.374)
Treatment Female - Spillover	-0.016 (0.108)	-0.027 (0.021)	-53.015 (74.564)
Treatment Male - Spillover	-0.220** (0.091)	-0.018 (0.024)	88.693 (71.878)
Baseline Outcomes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes
Control Mean (EL)	0.344	1.268	1081.818
p-value (Female = Male)	0.484	0.052	0.633
Observations	987	990	993

Note: Standard errors (in parenthesis) are clustered at the slum level. Stars indicate statistical significance: \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

of spillovers, aside from the case of wheat entitlements in the Male Treatment arm.

Columns (5) to (7) show some downward revision (on average) for households' perceptions of number of household members entitled to receive aid from the programs. Our interventions had no impacts on knowledge for PMGKY (see column 5). However, we observe statistically significant downward revisions for the PMUY and Jan Dhan programs. As with the per person entitlements, we find no evidence of statistically significant differences between the two treatment arms, or of spillovers (other than for the Jan Dhan program in the Male Treatment arm).

The downward revisions in knowledge of entitlements result in a reduction in the gap between households' perceptions of how much they should receive from each program and how much they are actually entitled to receive. While households in control areas overestimate entitlements, those in the treatment areas revise their beliefs towards the actual entitlement for all three programs. Interestingly, these effects are similar for both treatments, and there is little evidence of spillovers to the spillover control households aside for the case of the Male Treatment arm for PMGKY.

## 5.2 Access

Next, we study the intervention impacts on access to benefits from the three programs. We start by analysing impacts on the extensive margin – whether or not the household received any assistance from the program – before considering the intensive margin – the amount of aid actually received, and how this differs from what the household should receive.

Table 4 shows that the interventions had no impact on access to the programs on the extensive margin in the month after receiving the information. The coefficients are all small and statistically insignificant both for programs with high take-up (e.g. PMGKY) and for those with lower receipt (e.g. PMUY). We also fail to find any evidence of spillovers within the slums.

We then turn to study impacts on the intensive margin, in terms of amounts received per eligible person (or household in the case of PMUY) and the number of household members (if applicable) who receive support from the program in the month after the household

Table 4: Intervention Impact on Program Receipt (any, unconditional)

	(1) PMGKY any received	(2) PMUY any received	(3) Jan Dhan any received
Treatment Female	0.013 (0.008)	0.002 (0.009)	0.002 (0.014)
Treatment Male	-0.005 (0.011)	0.006 (0.008)	-0.011 (0.012)
Treatment Female - Spillover	-0.004 (0.010)	0.004 (0.004)	0.016 (0.017)
Treatment Male - Spillover	-0.011 (0.013)	0.007 (0.005)	0.005 (0.014)
Baseline Outcomes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes
Control Mean (EL)	0.904	0.237	0.492
p-value (Female = Male)	0.123	0.754	0.401
Observations	993	993	993

Note: Standard errors (in parenthesis) are clustered at the slum level. Stars indicate statistical significance: \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

received the information.

Table 5: Intervention Impact on Program Receipt (amt per person and number per household, unconditional)

	(1) PMGKY amt received	(2) PMUY amt received	(3) Jan Dhan amt received	(4) PMGKY no. received	(5) PMUY no. received	(6) Jan Dhan no. received
Treat Female	0.465*** (0.060)	0.024 (0.024)	12.087 (9.525)	0.748*** (0.096)	0.001 (0.013)	0.236*** (0.040)
Treat Male	0.291*** (0.057)	0.003 (0.012)	4.875 (7.925)	0.677*** (0.103)	-0.006 (0.011)	0.240*** (0.045)
Treat F. Spill	-0.122 (0.081)	0.010 (0.009)	20.131 (12.403)	-0.034 (0.070)	0.005 (0.009)	0.054 (0.034)
Treat M. Spill	-0.196 (0.121)	0.011 (0.012)	-8.841 (16.016)	-0.113 (0.092)	0.011 (0.011)	-0.044 (0.038)
Baseline Outcomes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean (EL)	4.165	0.228	238.742	3.796	0.234	0.526
p-value (F = M)	0.014	0.383	0.351	0.575	0.548	0.953
Observations	983	990	993	993	993	993

Note: Standard errors (in parenthesis) are clustered at the slum level. Stars indicate statistical significance: \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . A small number of large outliers in the dependent variable were dropped from Col. 1.

Table 5 shows that the interventions increased the amount of wheat per-eligible per-

son that households received from the PMGKY program. At endline, control households reported receiving on average 4.165 kg of wheat per person, but this increased among the treated households. Interestingly, we find that the increase is larger among households in the Female Treatment arm compared to those in the Male Treatment arm ( $p=0.014$ ). For pulses, we find a very small, weakly significant negative impact of the Male Treatment arm on the amount of pulses received and small negative spillover effects. For the other two programs – PMUY and Jan Dhan – we find no impacts of the program on the amounts received per-person. This is not very surprising for Jan Dhan since the funds are paid out directly into recipients' bank accounts by the Government, with no involvement from any intermediary.

Columns (5) - (7) of the Table display intervention impacts on the number of eligible household members (or the household itself) who received aid from each program. We find that the interventions increased the number of household members receiving aid from PMGKY and Jan Dhan programs, with no statistically significant increases between the treatment arms and no evidence of spillovers within the slum. Combined with the increase in quantity of wheat received per-person from the PMGKY, we see that the interventions increased the amount of aid directly treated households received from PMGKY by around 6kg (4kg) of wheat on average in the Female (Male) Treatment arms respectively. The coefficients also indicate that the directly treated households received on average Rs. 60 more (relative to control households) from the Jan Dhan program (regardless of whether they were aware of the program).

The increased aid helped to close the gap between what households *actually* receive from the PMGKY and Jan Dhan programs and their entitlement, as we show in Table 6. In control slums, at endline, households report receiving around 8.5kg less of grains, and INR 240 less than their entitlement from PMGKY and Jan Dhan respectively. This gap is closed for the treated households, as shown by the positive and statistically significant coefficients for the treatment dummies. The coefficients in column 2 suggest no similar impact for the PMUY program.

Combined with the correction of beliefs documented in Section 5.1, the increased aid also closed the gap between what households *actually* receive and what they *believed* they should receive as shown in Table 7. Across all three programs, at endline, control house-

Table 6: Intervention impact on difference between HH receipt and eligibility (household level, unconditional)

	(1) PMGKY	(2) PMUY	(3) Jan Dhan
Treatment Female	6.221*** (0.529)	0.018 (0.024)	148.810*** (23.436)
Treatment Male	5.724*** (0.535)	-0.006 (0.014)	106.884*** (23.516)
Treatment Female - Spillover	-0.207 (0.375)	0.015 (0.010)	13.264 (18.811)
Treatment Male - Spillover	0.123 (0.390)	0.007 (0.015)	-26.687 (22.557)
Baseline Outcomes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes
Control Mean (EL)	-8.417	0.219	-240.237
p-value (Female = Male)	0.471	0.342	0.156
Observations	983	990	993

Note: Standard errors (in parenthesis) are clustered at the slum level. Stars indicate statistical significance: \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 7: Intervention impact on difference between HH receipt and its beliefs (household level, unconditional)

	(1) PMGKY	(2) PMUY	(3) Jan Dhan
Treatment Female	19.490** (8.385)	0.101* (0.052)	1630.934*** (146.509)
Treatment Male	9.549*** (1.083)	0.132*** (0.028)	1510.896*** (138.568)
Treatment Female - Spillover	8.445 (10.651)	0.068 (0.046)	237.710 (275.199)
Treatment Male - Spillover	0.901 (1.508)	-0.002 (0.064)	-720.784* (393.764)
Observations	993	993	993
Baseline Outcomes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes
Control Mean (EL)	-8.429	-0.135	-1492.489
p-value (Female = Male)	0.216	0.486	0.046

Note: Standard errors (in parenthesis) are clustered at the slum level. Stars indicate statistical significance: \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

holds receive a lot less than what they believe they are entitled to. However, this is reversed for the directly treated households in both treatment arms.

### **5.3 Food Security and Expenditures**

We study whether the increased access to emergency aid affected households' food insecurity and expenditures. We measure food insecurity using a score which combines responses to 12 survey questions using principal components analysis (similar to [Filmer and Pritchett \(2001\)](#)). The index is constructed so that a higher value indicates higher food insecurity. It is standardized to have a 0 mean and standard deviation of 1. We complement this measure with information on food expenditures and total household expenditures.

Table 8 presents the results. Column 1 shows that the interventions reduced food insecurity by 0.8 - 0.9 standard deviations. This is a large effect, with no difference detected between the treatment groups. The lowered food insecurity is accompanied by a reduction in food expenditures of around 27% in both intervention groups. The reduced food expenditures lead to a reduction in overall household expenditures by 8-10 percentage points. Put together, these results suggest that the increased aid, and in particular, food aid from PMGKY lowered food and overall household expenditures and reduced food insecurity among the treated households.

However, these impacts do not spill over to the spillover control households within the treated slums for whom we find very small and statistically insignificant coefficients across the three measures.

### **5.4 Mental Health and Life Satisfaction**

Finally, we study how the interventions affected respondents' mental health and life satisfaction. Our measures of mental health are based on the responses to 12 questions on mental health from the General Health Questionnaire (GHQ-12) in the baseline and endline surveys. For each question, responses ranged from 0 to 3 with higher values equivalent to worse health outcomes. We use these responses to construct two uni-dimensional measures of psychological well-being or distress: (i) an index equal to the first principal component



Table 8: Intervention Food Security and Expenses)

	(1) food insecurity	(2) In food expense	(3) In total expense
Treatment Female	-0.846*** (0.061)	-0.273*** (0.041)	-0.082*** (0.028)
Treatment Male	-0.905*** (0.061)	-0.266*** (0.044)	-0.100*** (0.029)
Treatment Female - Spillover	-0.017 (0.088)	-0.075 (0.048)	-0.055 (0.038)
Treatment Male - Spillover	0.024 (0.127)	-0.019 (0.067)	-0.027 (0.044)
Baseline Outcomes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes
Control Mean (EL)	0.404	8.108	8.732
p-value (Female = Male)	0.359	0.838	0.474
Observations	993	993	989

Note: Standard errors (in parenthesis) are clustered at the slum level. Stars indicate statistical significance: \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

of the responses to the 12 mental health question, with higher values associated with worse mental health; (ii) a Likert scale equal to the sum of the 12 responses, recoded such that the sum ranges from 12 to 48 and higher values indicate better mental health.<sup>3</sup>

Life satisfaction and financial satisfaction were collected on a scale of 0-10 with higher values capturing higher reported life/financial satisfaction. Table 9 displays the impacts on these outcomes.

Column 1 shows that both interventions improved mental health of respondents. Interestingly, the improvement is larger, and statistically significantly so, among households where the man was given the information relative to households where the woman was given the information. Respondents also report higher life satisfaction (column 2) in both treatment arms, with increases of between 0.36 and 0.44 points from a control mean of 7.24 (on a 0-10 scale). Reported financial satisfaction – which is lower than life satisfaction in control areas with a mean at endline of 5.5 on the 0-10 scale – also increases significantly

<sup>3</sup>In constructing these indices, we follow the original intended use of GHQ-12 as a uni-dimensional measure of psychological distress (Goldberg, 1988). Although subsequent studies have found more support for a two-factor or three factor structure, recent research suggests that a uni-dimensional measure leads to minimal bias in most applications (Hystad and Johnsen, 2020).

in both treatment arms. These increases are not significantly different between the two arms.

Finally, we find no evidence of spillovers within slums of the treatments with very small and statistically insignificant coefficients on the spillover terms.

Table 9: Intervention impact on Mental Health and Life Satisfaction

	(1) mental health (PCA)	(2) mental health (Score)	(3) life satisfaction	(4) financial satisfaction
Treatment Female	-0.792*** (0.089)	2.880*** (0.332)	0.443*** (0.116)	0.837*** (0.109)
Treatment Male	-0.963*** (0.089)	3.535*** (0.342)	0.359*** (0.123)	0.779*** (0.129)
Treatment Female - Spillover	0.046 (0.115)	-0.107 (0.483)	-0.095 (0.141)	-0.107 (0.152)
Treatment Male - Spillover	-0.068 (0.106)	-0.157 (0.380)	-0.056 (0.161)	0.085 (0.195)
Baseline Outcomes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Control Mean (EL)	0.431	24.808	7.243	5.508
p-value (Female = Male)	0.030	0.037	0.405	0.635
Observations	993	993	993	993

*Note:* Standard errors (in parenthesis) are clustered at the slum level. Stars indicate statistical significance: \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 6 Discussion

### 6.1 Spillovers

We discuss further the findings on spillovers of the treatments from treated to spillover control households within the treated slums. Characterizing the nature and size of spillovers is important for the design of effective policies: positive spillovers of information onto the knowledge and actions of untreated households would mean that policies could be effective even when targeting only a proportion of eligible recipients, thereby potentially lowering their costs. Negative spillovers, by contrast, would undermine the effectiveness of policies

targeting a proportion of the population only.

Our results show that, aside from some spillovers in knowledge of entitlements in the male treatment arm, the treatments did not generate significant spillover effects on most outcomes of untreated households within the treated slums. Ex-ante the power calculations indicated that the design would detect effect sizes of 0.31 - 0.36 standard deviations with 80% power, assuming within-slum intra-cluster correlation coefficients of 0.05 and 0.1 respectively. In a large majority of cases, the coefficients on the treatment effects for the spillover controls are small and close to zero, suggesting that the statistical insignificance is not due to low power.

A question then is what drives these findings. Treated households were more likely to chat to individuals outside their household, and to chat with more of them about government support to cope with the coronavirus crisis in both treatment arms (Table 10). However, there is no increase, relative to control slums, in the likelihood that spillover control households in the treated slums chat with individuals outside their households, or in the number of contacts they chat with, about government COVID support. This suggests that, combined with the fact that our sample consisted of a small proportion of slum households, network connections in the slums are very sparse such that there was a low probability that information flowed from treated households to spillover control households within the same slum.<sup>4</sup> Moreover, the relatively tight – one month – timeframe between information provision and the endline survey may not have been sufficiently long for the information to diffuse more broadly.

## 6.2 Female versus Male Treatment

The intervention design allows us to address the question whether it matters who in the household receives the information about emergency government benefits. From a policy perspective, the answer to the question is important as the effect of the intervention on household behaviour and the wellbeing of individual members may depend on the identity of the recipient, or because communicating the information to certain household members

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<sup>4</sup>A number of other studies document sparser networks in urban rather than rural areas of developing countries. In the Indian context, [Banerjee et al. \(2021\)](#) show that networks among slum households in Hyderabad have a lower density, clustering coefficient and closeness centrality than those in rural villages in Karnataka.

Table 10: Intervention impact on chats about government covid support outside the household

	(1) Any chat	(2) Number chatted with
Treatment Female	0.158*** (0.025)	0.443*** (0.101)
Treatment Male	0.150*** (0.026)	0.397*** (0.095)
Treatment Female - Spillover	-0.031 (0.038)	-0.082 (0.147)
Treatment Male - Spillover	0.029 (0.043)	0.059 (0.152)
Covariates		
Control Mean (EL)	0.841	2.760
p-value (Female = Male)	0.219	0.504
p-value Spillovers (Female=Male)	0.197	0.421
Observations	992	993

Note: Standard errors (in parenthesis) are clustered at the slum level. Stars indicate statistical significance: \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

(e.g. a household head of working age) may be more difficult than reaching others.

In theory, if a household behaves according to the Collective Model (i.e. household decisions are Pareto Efficient) then members always have the incentive to share information that can increase the household surplus provided that information sharing is costless. However, if the household is characterised by non-cooperative decision-making, then household members may have an incentive to withhold information if doing so improves their strategic position within the household (Baland and Ziparo, 2018). Separately, information may not be fully communicated within the household if the information recipient has insufficient human capital to process the information and communicate it effectively. For households in developing countries, there is substantial evidence of non-cooperative behaviour (see Guirking and Platteau (2020) and Quisumbing and Doss (2021) for reviews) and lack of information-sharing between household members (see Baland and Ziparo (2018) for a review), suggesting that the identity of the information recipient may be important in our study context.

As discussed in Sections 5.1 and 5.2, we find no systematic differences between the male

and female treatment arms in the effects of the intervention on knowledge of and access to the three programs. Similarly, providing information to a female or male household member had similar effects on food security and food expenditures as noted in Section 5.3 . A notable exception to this pattern is that the primary female respondent reports significantly better mental health in the arm in which the information was provided to the spouse or household head compared to the arm in which she herself was the information recipient (a difference of about 0.17 standard deviations in the mental health index at endline). This differential effect suggests there may be some cost to acting on the information that affects the mental health of the information recipient. As we did not collect information on the mental health of the male information recipient, we are unable to check whether or not his mental health is similarly affected when he is the information recipient.

It is also worth noting that, as we do not have information on consumption at the individual level, we are unable to test whether providing information to a male or female household member affects the intra-household allocation of resources.

## **7 Conclusion**

We find that our tailored information intervention had large effects on the directly treated households in our study. First, it affected participants' knowledge about program benefits - although only on the intensive margin. In particular, it lowered perceptions of the amounts that households believed they were entitled to receive, reducing the gap between perceived and actual entitlements.

Second, the intervention had effects on participants' access and receipt of program benefits, by increasing the quantities received and reducing the gap between household receipts and household entitlements (both believed and actual entitlements). Perhaps most significantly, the intervention also lowered reported food insecurity and household expenses, while improving self-reported mental health and life satisfaction.

These findings of a downward revision in beliefs and an increase in benefit receipts suggest that receiving this information changes the willingness of households to undertake effort to obtain the benefits for which they are eligible. This might be because house-

holds initially believe that they won't receive the (misperceived) high benefit entitlements due to corruption, policy ineffectiveness, or past experiences with politicians not upholding promises. Thus, they might not have made the effort to obtain the entitlements. Alternatively – or in addition – our intervention (which entailed receiving information from researchers at a reputed Indian university) may have increased respondents' confidence in the legitimacy of the entitlements, encouraging them to make the necessary effort to obtain them.

We find mostly null effects for differences of information provision between genders (with some exceptions), which could suggest the existence of relatively frictionless information sharing within the household in this setting. We also find mostly null effects for information spillovers across households in the community, suggesting that the information was not widely shared outside the household (perhaps because much of the information we provided was tailored to the household's particular circumstances).

In summary, this light-touch intervention seems to have had very significant positive benefits on the directly treated households, suggesting that similarly designed interventions could provide a very low cost but highly effective way of increasing access to emergency government benefits among the poorest citizens.

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## Appendix

### Text of Intervention

The respondent (male or female according to treatment assignment) was sent the following 3 SMS messages:

**SMS 1:** *Due to Covid-19: PMGKY members with ration cards entitled to 5kg rice/wheat per person and 1kg pulses per family free per month April-November 2020.*

**SMS 2:** *PMUY members entitled to three large/14.2 kg LPG refills, delivery up-to March 2021.*

**SMS 3:** *PMJDY members entitled to Rs 500 per month April-June 2020. Widows, senior citizens and disabled to receive Rs 1000.*

Follow-up calls followed the following script, according to information provided in the baseline survey.

**CATEGORY 1:** If the pre-intervention survey identified government programs for which the household is eligible (based on reported eligibility) but was yet to receive any support, the enumerator gave the following message:

*"A colleague of mine interviewed you X days ago to ask about government support for Covid-19 that you have received in the past. Based on the information you provided, we have identified that you are eligible for the following relief packages but have yet to receive the full benefits. I will explain in this call who to contact for these relief packages."*

**CATEGORY 1A:** If the baseline data indicated that the household had not received any/all benefits that it is entitled to under PMGKY:

*"You told my colleague that you are a beneficiary under PMGKY (Pradhan Mantri Garib Kalyan Yojana) and that you (your household) has a ration card. If this is correct, then you were entitled to 5kg of rice or wheat for each household member who is listed on a ration card per month during the months April, to November 2020. In addition, your family was entitled to receive 1kg of pulses per month during these months."*

**CATEGORY 1B:** If the baseline data indicated that there was a widow, senior citizen

(60 years and above) or disabled person living in the household:

*"You told my colleague that there is at least one widow, senior citizen or disabled person living in the household. If this is correct, then each household member fitting this description is entitled to received Rs 1000 under the PMGKY relief package for Covid-19."*

*"The government's contact information for PMGKY is as follows: Website: fcs.up.gov.in Contact Number: 1967 and 14445 Toll free number: 18001800150"*

**CATEGORY 1C:** If the baseline data indicated that the household had not received any/all benefits that it is entitled to under PMUY:

*"You told my colleague that your household is a beneficiary under PMUY (Pradhan Mantri Ujjwala Yojana). If this is correct, then your household was entitled to receive three monthly LPG refills . If you already have the advances transferred to your bank account, then you can take delivery of the refills till March 2021."*

*"The government's contact information for PMUY is as follows: Ujjwala Yojana Website: mylpg.in Inane Gas: 18002333555 Bharat Gas: 1800224344 HP Gas: 0522 2308863 and 0522 2309197"*

**CATEGORY 1D:** If the baseline data indicated that the household had not received any/all benefits that it is entitled to under PMJDY:

*"You told my colleague that you or another household member is has a Jan Dhan account. If this is correct, then each Jan Dhan account holder should have received Rs 500 per month directly in the account between April-June 2020."*

*"The government's contact information for PMJDY is as follows: Website: www.jansunwai.up.nic.in E Mail: jansunwai-up@gov.in Toll Free Number: 18001027788 (State); 1800110001 (National)"*

Individuals were also advised not to directly confront a wrongdoer who was purposefully denying benefits, and were provided with a phone number where official complaints could be lodged.

**CATEGORY 2:** If they believed they were purposefully denied benefits:

*"If you believe you were purposefully denied benefits that you are entitled to, please do not enter into a confrontation with the wrongdoer that may cause harm to you or your household. But you can complain about black marketing at this number: 8931094988."*

**CATEGORY 3:** If the pre-intervention survey did not identify any government pro-

grams for which the household was eligible but not receiving any support, the enumerator gives the following message: (Receiving everything).

*“A colleague of mine interviewed you X days ago to ask about government support for Covid-19 that you have received in the past. You told my colleague that you received X1 under PGMKY, X2 under PMUY and X3 under PMJDY. If this is correct, I can confirm that you are currently receiving all the government relief for Covid-19 for which you are eligible under these schemes.”*

## **Sample Balance Tables**

Table 11: Balance tests: Individual characteristics

Variable	(1)	(2)	(3)	Normalized difference		
	Control Mean/SE	Treatment Female Mean/SE	Treatment Male Mean/SE	(1)-(2)	(1)-(3)	(2)-(3)
Age	30.197 (0.297)	30.231 (0.371)	30.729 (0.479)	-0.005	-0.072	-0.066
Disabled	0.021 (0.008)	0.054 (0.011)	0.054 (0.014)	-0.176	-0.178	-0.001
Married	0.759 (0.021)	0.769 (0.028)	0.777 (0.023)	-0.025	-0.043	-0.018
No education	0.138 (0.021)	0.150 (0.022)	0.127 (0.021)	-0.033	0.035	0.067
Education 1-9	0.318 (0.035)	0.287 (0.029)	0.352 (0.031)	0.066	-0.074	-0.139
Education above 10	0.544 (0.033)	0.563 (0.033)	0.521 (0.036)	-0.038	0.046	0.084
Housewife	0.747 (0.019)	0.754 (0.030)	0.774 (0.025)	-0.017	-0.063	-0.046
Unemployed	0.103 (0.015)	0.099 (0.017)	0.105 (0.019)	0.014	-0.008	-0.022
Student	0.132 (0.014)	0.138 (0.021)	0.108 (0.016)	-0.016	0.073	0.089
N	340	334	332			
Clusters	20	20	20			
F-test of joint significance (F-stat)				1.567	1.472	0.933
F-test, number of observations				674	672	666

*Notes:* The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. Standard errors are clustered at variable slumid. All missing values in balance variables are treated as zero.\*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

Table 12: Balance tests: Household characteristics

Variable	(1) Control Mean/SE	(2) Treatment Female Mean/SE	(3) Treatment Male Mean/SE	(1)-(2)	Normalized difference (1)-(3)
Hindu	0.838 (0.059)	0.781 (0.079)	0.834 (0.052)	0.145	0.011
Muslim	0.162 (0.059)	0.219 (0.079)	0.166 (0.052)	-0.145	-0.011
General	0.221 (0.030)	0.240 (0.050)	0.235 (0.033)	-0.045	-0.034
OBC	0.329 (0.046)	0.353 (0.054)	0.334 (0.038)	-0.050	-0.010
SC/ST	0.450 (0.051)	0.407 (0.059)	0.431 (0.050)	0.086	0.039
Hh size	5.297 (0.092)	5.341 (0.093)	5.303 (0.088)	-0.029	-0.004
Hh income	9528.844 (298.261)	10076.677 (446.732)	10083.735 (525.941)	-0.090	-0.087
Hh exp.last month	5410.882 (177.321)	5171.257 (203.557)	5375.602 (223.695)	0.094	0.013
Main earnings from agriculture	0.068 (0.010)	0.075 (0.021)	0.060 (0.013)	-0.028	0.030
Main earnings from day labor	0.479 (0.024)	0.464 (0.035)	0.500 (0.030)	0.031	-0.041
Main earnings from business	0.300 (0.028)	0.243 (0.025)	0.214 (0.020)	0.129	0.197
Government job	0.009 (0.005)	0.015 (0.006)	0.009 (0.005)	-0.057	-0.002
Private job	0.144 (0.019)	0.204 (0.017)	0.217 (0.019)	-0.157	-0.189
Eligible PMGKY	0.538 (0.027)	0.584 (0.026)	0.563 (0.018)	-0.092	-0.050
Eligible PMUY	0.235 (0.023)	0.234 (0.022)	0.250 (0.018)	0.004	-0.034
Eligible Jandhan	0.906 (0.017)	0.913 (0.016)	0.925 (0.015)	-0.025	-0.067
N	340	334	332		
Clusters	35 20	20	20		
F-test of joint significance (F-stat)				1.002	1.377
F-test, number of observations				674	672

Notes: The values displayed for t-tests are the differences in the means across the means

Table 13: Balance tests: Village characteristics

Variable	(1)	(2)	(3)	Normalized difference		
	Control Mean/SE	Treatment Female Mean/SE	Treatment Male Mean/SE	(1)-(2)	(1)-(3)	(2)-(3)
Population	1674.200 (230.307)	1787.500 (305.810)	1787.550 (319.609)	-0.095	-0.092	-0.003
Hindu prop	0.600 (0.112)	0.650 (0.109)	0.600 (0.112)	-0.102	0.000	0.102
Receive PMGKY prop	0.909 (0.017)	0.913 (0.016)	0.917 (0.015)	-0.066	-0.120	0.054
Receive PMUY prop	0.245 (0.022)	0.232 (0.019)	0.236 (0.018)	0.140	0.097	0.043
Receive Jandhan prop	0.508 (0.024)	0.582 (0.026)	0.560 (0.019)	-0.632	-0.526	-0.106
Know about PMGKY prop	0.913 (0.017)	0.920 (0.014)	0.929 (0.014)	-0.093	-0.229	0.136
Know about PMUY prop	0.248 (0.022)	0.232 (0.019)	0.242 (0.018)	0.168	0.059	0.109
Know about Jandhan prop	0.513 (0.024)	0.582 (0.026)	0.560 (0.019)	-0.584	-0.468	-0.116
N	20	20	20			
F-test of joint significance (F-stat)				0.807	0.670	0.137
F-test, number of observations				40	40	40

*Notes:* The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. \*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

Table 14: Balance tests: Baseline characteristics

Variable	(1) Control Mean/SE	(2) Treatment Female Mean/SE	(3) Treatment Male Mean/SE	(1)-(2)	Normalized difference (1)-(3)
Mention PMGKY	0.915 (0.016)	0.919 (0.014)	0.928 (0.014)	-0.016	-0.048
Mention PMUY	0.247 (0.022)	0.228 (0.021)	0.244 (0.018)	0.046	0.007
Mention Jandhan	0.509 (0.024)	0.578 (0.026)	0.560 (0.019)	-0.138	-0.103
Reported entitlement wheat PMGKY	5.659 (0.084)	5.703 (0.059)	5.660 (0.061)	-0.035	-0.001
Reported entitlement PMUY	1.183 (0.010)	1.160 (0.009)	1.235 (0.013)	0.120	-0.219
Reported entitlement Jandhan	1414.162 (52.275)	1278.125 (47.241)	1450.820 (46.301)	0.205	-0.054
Unconditional entitlement wheat PMGKY	5.172 (0.127)	5.240 (0.097)	5.248 (0.096)	-0.034	-0.039
Unconditional entitlement PMUY	0.287 (0.027)	0.261 (0.023)	0.301 (0.028)	0.048	-0.025
Unconditional entitlement Jandhan	719.559 (66.793)	736.937 (52.290)	806.991 (49.828)	-0.018	-0.089
N	340	334	332		
Clusters	20	20	20		
F-test of joint significance (F-stat)				19.469***	3.639***
F-test, number of observations				674	672

Notes: The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. Standard errors are clustered at variable slumid. All missing values in balance variables are treated as zero.\*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.