# Personalized Information Provision and the Take-Up of Government Benefits*

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**Abstract**

Gaps between benefit receipt and entitlements limit the efficiency and equity of social programs. We shed light on the role of informational constraints in driving this gap during a crisis in a setting with high leakage. We develop a theoretical model of bargaining under asymmetric information between an intended beneficiary and an intermediary responsible for disbursing welfare benefits to show that information on benefit entitlements and complaint procedures can improve benefit receipts; but, under some conditions, also induce a change in the intermediary's strategic behaviour leading to *lower* benefits. We implemented a cluster Randomized Controlled Trial in a large Indian city to study whether and how information provision affected cash and in-kind government benefits during a crisis. Initially, households overestimated their entitlements, but received less than their dues. Providing personalized information on government benefits corrected beliefs and increased benefits received. The evidence is consistent with an "empowerment" mechanism: i.e. beneficiaries used program contact details to bargain more effectively with intermediaries to obtain their entitlements.

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# 1 Introduction

Gaps between benefit receipt and entitlements limit the efficiency and equity of social programs globally. Leakage is a major factor driving these gaps in developing countries (Olken, 2006; Niehaus and Sukhtankar, 2013). The scope for leakage is exacerbated during crises and emergencies, when the urgency of getting aid to beneficiaries leads to the suspension of standard scrutiny and monitoring checks.[1] Identifying effective ways of closing these gaps during crises and emergencies is crucial, as timely access to benefits can limit economic damages (Clarke and Dercon, 2016).

In this paper, we shed light on the role of informational constraints in driving this gap during a crisis in a setting with high leakage. We draw on a cluster randomized controlled trial (cRCT) that provided personalized information on eligibility and entitlements for cash and in-kind government benefits to families living in urban Indian slums in the midst of the COVID pandemic. With this study, we investigate two key questions: First, do informational constraints exacerbate the receipt-entitlement gap? Second, can providing personalized information on entitlements reduce this gap?

To address these questions, we first develop a theoretical model of bargaining under asymmetric information between an intended beneficiary and an intermediary responsible for disbursing welfare benefits. While the intermediary has full knowledge of benefit entitlements, the beneficiary may not. If benefits received fall below the beneficiary's expected entitlement, she can complain to the welfare programme at some effort cost. A complaint is successful with some positive probability, resulting in the beneficiary receiving her full entitlement and the intermediary being penalised. We show that alleviating informational constraints has the potential to reduce the receipt-entitlement gap through two key channels. First, providing accurate information on exact entitlements can correct misperceptions and knowledge gaps among beneficiaries. This is particularly important in settings with high corruption and low institutional capacity, where information from a trusted source could lend veracity to benefit amounts and encourage negotiations. Second, by providing information on how to make complaints, it can empower beneficiaries to demand their full entitlements from program intermediaries.

[1] This was very salient during the COVID pandemic when Governments sought to procure health equipment and rolled out support policies at pace. OECD (2022) documents several resulting incidents of leakage in developed and developing countries.

However, we show theoretically that information on entitlements, and reducing the cost of making complaints could, under some conditions, induce a change in the intermediary's strategic behaviour such that the beneficiary receives *fewer* benefits following the intervention.

Moreover, information alone may be insufficient if other barriers exist. Additionally, households may be exposed to a large volume of conflicting information during crises, diluting or nullifying the effectiveness of additional information. The extent to which informational constraints are the underlying cause of the receipt-entitlement gap is therefore an empirical question.

To answer our research questions empirically, we leverage the exogenous variation from the randomization along with primary data to identify the impacts of the informational intervention on closing the gap between actual benefit receipt and entitlements. We also study the impacts on beneficiaries' beliefs about their eligibility and entitlement amounts, and their actual receipts from the programs. Thereafter, we disentangle the drivers of the receipt-entitlement gap by analyzing heterogeneous intervention impacts based on pre-program beliefs and benefit receipt. We also exploit program-specific differences in design and implementation across three important government benefit programs to shed light on the causal mechanisms at play. Finally, we document intervention impacts on measures of household welfare and well-being including expenditures and mental health.

The experiment provided personalized information, via mobile phones, on eligibility and entitlements for three key Indian government aid programs responding to COVID-19 in the area. The programs provided food grains (PMGKY), liquefied petroleum gas (LPG) refills (PMUY), and cash transfers to women (Jan Dhan).[2] The information was tailored to each household based on pre-intervention survey data. The programs were part of a support package announced by the Government of India shortly after it announced one of the strictest lockdowns globally on 24 March 2020 to combat the COVID-19 pandemic. The lockdown had devastating economic impacts, leading to a 30% drop in employment and loss of 120 million jobs.

[2]Pradhan Mantri Garib Kalyan Anna Yojana (PMGKY) provided free food grains, Pradhan Mantri Ujjwala Yojana (PMUY) gave direct benefit transfers for the purchase of LPG refills, and Pradhan Mantri Jan Dhan Yojana (Jan Dhan) gave direct cash transfers to women.

Our study focuses on residents of 60 slums in and around the city of Kanpur, Uttar Pradesh. This population – which has grown rapidly, but remains relatively understudied – was especially badly hit by the strict lockdown. We randomly allocated 40 of the 60 slums to two treatment groups in a randomized saturation design. In 20 slums, a female adult received the tailored information, while in the other 20 treated slums, a male adult received it. This design allows us not only test whether the identity of the recipient matters but to also quantify the size of social multipliers of spillovers from sharing of information, or learning from peers. These could be exploited by policymakers to deliver lower-cost policies.[3] We base our main analysis on intention to treat (ITT) effects, and results are robust to correcting inference for multiple hypothesis testing.

The simultaneous announcement of a large number of support measures, which were rolled out very rapidly, could have led to misperceptions about the available support and entitlements. Indeed, despite widespread availability of information regarding the additional entitlements through news media, our data indicates that slum residents held significant misperceptions about their specific entitlements. In particular, they *overestimated* their entitlements from each of the three programs. At the same time, however, households received *less* than their entitlements from PMGKY and Jan Dhan. PMUY-eligible households were receiving their entitlement, likely due to the program paying the full market price to LPG suppliers. Our main analysis thus studies intervention impacts on beliefs, receipt and the gap between receipt and entitlement for PMGKY and Jan Dhan.

We find that around 1-2 months after the intervention, treated households updated their beliefs about their entitlements from the two programs. While households in the control slums continued to over-estimate their entitlements, treated households in treatment slums downgraded theirs to the actual entitlements. Moreover, the intervention increased the support treated households in treated slums received from both programs by an average of over 5kg more in food grains (29% increase over the average received by control households) from PMGKY and around INR 125 more (48% increase on the average received by control households) from Jan Dhan. This increase

[3] 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' households.

in receipts reduced the gap between receipts and entitlements for these households.

Next, we investigated the mechanisms through which the gap between benefit entitlements and receipt was closed. Impacts could have been driven by two key mechanisms – the relaxation of an information constraint, and/or through an empowerment effect.[4] We disentangle the information effect and empowerment effects (i.e. the treated households made more effort to obtain their due after being made aware about it) using two strategies. First, we compare the intervention impacts on the gap between receipts and entitlement across subgroups of households – by whether they overestimated their entitlements at baseline, and whether they were receiving their entitlements at baseline. We show that the intervention impacts are concentrated among the sub-groups who were not receiving their full entitlements at baseline, regardless of their initial beliefs. We argue that this pattern of results is consistent with the empowerment effect rather than an information effect.

Second, we exploit the differences in the design of the PMGKY and Jan Dhan compared to PMUY. The latter required minimal engagement with public officials, the cash transfer covered the full market price of the cooking gas, and vendors provided doorstep delivery of the gas. Thus, households had to make minimal effort to receive their entitlements, and vendors had no incentive to withhold the benefits. Thus, any impacts on the PMUY program would be due to an information effect. We find no treatment impacts on access to benefits from the PMUY program. This provides further validation for the empowerment channel as the key driver of our results.

Consistent with the empowerment channel, we also find evidence that our intervention influenced their *beliefs* about these constraints. In a setting with ambient corruption in public offices, policy ineffectiveness, or past experiences with politicians not upholding promises, citizens may well believe they will not have access to the benefits they are entitled to, and might not have made the effort to obtain the entitlements. Indeed, at baseline, 45% of households believed that ration shops (which distribute food aid) are either highly or very highly corrupt; and that over 80% of government officials were either highly or very highly corrupt. Our intervention made households more confident about contacting government officials, less likely to pay bribes and more likely to report

[4]We argue that a third explanation, that the intervention affected preferences or reduced stigma, is unlikely in our context since study households were already beneficiaries of these programs prior to their expansion during the COVID pandemic.

perceiving lower levels of corruption in ration shops.

The intervention impacts on entitlement beliefs and increased benefit receipts are accompanied by reduced household food insecurity and food expenditures. This suggests that households substituted away from purchasing food from the market in response to the higher receipt of food aid. We also document modest improvements in respondent mental health (measured using the GHQ scale) and in their financial and life satisfaction. Providing information that altered beliefs and increased benefit receipts can thus also alleviate poor mental health at times of severe economic distress, adding to the literature that shows that providing financial resources (Ridley et al., 2020; Haushofer and Shapiro, 2016) or psycho-social support interventions (Vlassopoulos et al., 2021; Baranov et al., 2020; Angelucci and Bennett, 2021) can improve mental health in low-income settings.

We find negligible differences in intervention impacts by the gender of the information recipient, suggesting relatively friction-less information sharing about these programs within households.[5] We also find negligible spillover effects for untreated households within treatment slums, suggesting that – in contrast to intra-household information-sharing – information diffusion across households was low, at least in the 1-2 months between the intervention and endline survey. The personalized nature of the information and prevalence of COVID restrictions – which discouraged in-person interactions – may have slowed down diffusion.

Our findings hold important implications for the design of effective public policies, especially in developing country settings where corruption may be high and trust in the government's ability to deliver promised benefits may be low. Provision of information tailored to the household's needs, that is delivered by a credible, third-party institution, can not only improve take-up of entitlements but also improve perceptions of the programs.

Our paper contributes to two key strands of literature. First, it contributes to a growing literature seeking to identify constraints limiting the take-up of government benefits and social programs. The literature finds mixed effects of the provision of information on the

[5] A notable exception is mental health: the (female) survey respondents reported, on average, a larger improvement in mental health if a male household member received the information compared to households where the respondent received the information herself.

take-up of government benefits. Studies such as Abramovsky et al. (2016), Berg et al. (2021) and Castell et al. (2022) find that providing information, including personalized information, does not increase government benefit applications or receipt among very poor households in Colombia, poor households in India and the unemployed in France respectively. Chetty and Saez (2013) found that providing personalized information about tax incentives to recipients of the 'Earned Income Tax Credit' had no systematic effect on average earnings. By contrast, other studies such as Banerjee et al. (2018)) and Carneiro et al. (2019) show that sending personalised information to recipients of a subsidized food program in Indonesia, and social worker visits to households in extreme poverty in Chile, increased benefit receipt and applications for government programs. We contribute to this literature by focusing on the urban poor and an emergency setting, and showing that providing personalized information about entitlements can correct misperceptions, increase access to government benefits and improve welfare. We find that the mechanism behind our results is the empowerment effect, rather than the information effect.

Second, several studies investigate the role of information as empowerment, primarily among rural populations, finding ambiguous results. Björkman and Svensson (2009), Björkman Nyqvist et al. (2017), Banerjee et al. (2018) and Dupas and Jain (2023) find that information empowers rural villagers in Uganda, Indonesia and India to demand better health care (Uganda and India) and food subsidies (Indonesia). By contrast, Raffler et al. (2021), Fabbri et al. (2019) find no evidence of information increasing accountability. We contribute to this literature by focusing on the urban poor in an emergency setting.

Our model is closely related to the one in Banerjee et al. (2018) in terms of the focus on information provision in the context of public program. Similar to their setup, we assume there is bargaining between an intended beneficiary who is uncertain about her entitlements, and an intermediary who makes take-it-or-leave-it offers. There are, however, some important differences in terms of approach and insights. In our setup, information changes *both* the beneficiary's beliefs about entitlements and the cost of making a complaint. We term the former a pure 'information effect' while the latter is a direct 'empowerment' effect. Banerjee et al. (2018) consider only the information effect – i.e. how altered beliefs change the bargaining process. By contrast, we are able to observe both the information effect and a direct empowerment effect at play, individually and jointly.

Like Banerjee et al. (2018), we find that the information effect can be ambiguous depending on underlying parameter values. However, the mechanisms responsible for this ambiguity in the two models are fundamentally dissimilar. In Banerjee et al. (2018), the ambiguity arises from the inter-temporal trade-offs facing the intermediary as increasing the offer in the present period reduces the future value of holding office. In our model, there are no such dynamic concerns for the intermediary but ambiguity arises when there is a change in the equilibrium type. For instance, information provision can lead to a shift from a pooling equilibrium where the intermediary withholds all benefits and the beneficiary *always* complains to a separating equilibrium where the beneficiary receives partial benefits and sometimes complains – this situation may well imply a decrease in benefits.

Our intervention was likely more effective at reducing the gap between entitlements and receipt compared to other interventions because it targeted an under-served population - urban slum dwellers - during an emergency situation. The challenging context of perceived government corruption and ineffectiveness in our setting may have also amplified impacts. Focusing specifically on assisting a vulnerable group to access benefits during a crisis, when needs were high yet trust in government low, could explain why our intervention had greater impacts on closing the receipt gap than others.

The rest of the paper is organized as follows. In the next section we describe the background and study context and provide details on the benefit programs. Section 4 describes our intervention and the experiment design. Thereafter, we describe the data and our empirical methodology in Section 5. Section 6 reports the results, studies mechanisms and displays robustness checks. Section 8 concludes.

# 2 Context and Background

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. 88% of households in our sample reported having lost all or part of the income of their main earner. 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 (Jan Dhan). 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 INR 1.70 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 5 kilograms (kg) of wheat and rice (per person, per ration card) along with 1 kg of 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 in our intervention was the Pradhan Mantri Jan Dhan Yojana (Jan Dhan), which provided women already holding Jan Dhan Yojana accounts with INR 500 per month for three months, from April to June 2020 through transfers into their Jan Dhan accounts. Like the PMGKY, the announcement of this scheme was also made on 26 March 2020. In addition, the scheme allowed for poor widows, poor senior citizens and poor disabled people to receive INR 1000 for three months into their Jan Dhan accounts.

The last 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). Starting in April 2020, PMUY beneficiaries received transfers in their registered bank accounts that could be used to purchase three LPG cylinder refills.

Although beneficiaries were expected to pay the LPG delivery agents the market price for the refills, the credit received under the scheme meant that they were effectively free. The relief scheme was initially for a 3-month period, with a maximum of one refill per month. The scheme was later extended with the option of three additional refills till March 2021.

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 expect a significant amount of confusion and uncertainty regarding what individuals were eligible for. Some schemes were extended and amended as the COVID crisis continued, potentially adding to this confusion and uncertainty.

Relatedly, preexisting issues with some schemes may have meant that the Covid relief did not reach some intended beneficiaries unless they took specific steps to ensure it. For example, even before the pandemic Jan Dhan account holders faced challenges accessing their accounts, and were often unaware of transfers made into them (Field et al., 2022). Account dormancy due to infrequent use was a significant issue (Patel et al., 2020). Many Jan Dhan account holders may not have even known that they had these accounts (Pande et al., 2020; Somanchi, 2020) and, therefore, would not have known about their eligibility for Jan Dhan Covid relief or how to access it. A rapid survey conducted among Jan Dhan account holders in 13 Indian states in May 2020 found that about 21% of women were yet to receive their first Covid relief transfer and 13% had no knowledge about the transfers.

A large proportion of eligible households reported receiving food grains through the Pradhan Manti Garib Kalyan Yojana (PMGKY) scheme during the early months of the pandemic: 81.3% of eligible households in urban India, and 91.8% of eligible households in the state of Uttar Pradesh according to a survey conducted between May and August 2020.[6] However, the survey figures do not reveal whether ration card holders were receiving the quantities of rice and wheat they were eligible for. Earlier research has documented sizeable leakages in the public distribution system (PDS). Gulati and Saini (2015) estimate that, in 2011-12, 46.7% of food grains released for distribution via

[6]The figures are from the Supplementary Survey to the 21st wave of the CPHS (Consumer Pyramid Household Survey) as reported in Bhattacharya and Sinha Roy (2021)

the PDS across India failed to reach intended beneficiaries. Drèze and Khera (2015) estimate a lower, but nevertheless sizeable, leakage of 32% for the same period. (For Uttar Pradesh, the state in which our experiment was conducted, Drèze and Khera (2015) obtain a substantially higher estimate of 57.6%).

Drèze and Khera (2015) hypothesize that frequent adjustments in what was due to APL (Above Poverty Line) ration card holders meant they were often unaware of their food grain entitlements, the bulk of which ended up in the black market through corrupt dealers. It is plausible that lack of awareness among the intended beneficiaries also contributed to leakage in food grains intended for distribution under PMGKY during the early months of the pandemic. Our survey data shows that, at baseline, households were receiving, on average, about 8.5kg (per month) less in food grains than their entitlements (see Table D4 in the Appendix).

Compared to the PMGKY and Jan Dhan schemes, the available evidence suggests that a high proportion of PMUY beneficiaries were able to access the Covid relief designed for them. A survey conducted in September 2020 found that 97% of beneficiaries had taken delivery of at least one LPG cylinder (Giri and Aadil, 2021). A potentially important factor in the scheme is that agents who delivered the fuel to the doorstep would receive the full market price from the beneficiary, thus reducing the risk of diversion of the fuel to the black market.[7] A pre-Covid study had found a majority of rural beneficiaries could not get LPG cylinders delivered to their doors (Giri and Aadil, 2018) but this is less likely to have been a problem in urban areas (Ravindra et al., 2021). By September 2020, 58% of urban beneficiaries had taken three refills compared to 40% in rural areas (Giri and Aadil, 2021).

[7] This approach echos that of the Indian Government's DBTL (Direct Benefit Transfer for LPG) reform. The latter scheme, in operation during 2013-14, replaced the sale of subsidised cooking fuel to households below an income threshold with direct transfers of the subsidy to the beneficiary's bank account. Barnwal (2023) provides evidence that the DBTL reform reduced leakage of subsidised fuel to the black market, thus improving the likelihood that the subsidies reached the intended beneficiaries.

# 3 A Model of Benefit Disbursements under Asymmetric Information

In this section, we develop a model to investigate, theoretically, the potential effects of providing information about benefit entitlements to the intended beneficiaries when disbursements are made by an intermediary, and there is asymmetric information between the beneficiary and the intermediary. The model allows a conceptual distinction between what we call the 'information' and 'empowerment' effects of information provision and indicates how these effects can be tested empirically using our information intervention and experimental design.

## 3.1 Setup

We model the behaviour of a number of beneficiaries, indexed by $i = 1, 2, .., N$ and that of a single intermediary. We denote the actual benefit received by beneficiary $i$ by $b_i$ and the entitlement by $\bar{b}_i \in \{b_l, b_h\}$ where $0 \leq b_l < b_h$ . For ease of notation, we denote the entitlement-benefit gap by $\Delta_i = \left(\bar{b}_i - b_i\right)$ . Throughout the exposition below, we drop the subscript/superscript $i$ from the notation above when there is no scope of ambiguity.

The intermediary has full information about $\bar{b}_i$ but the beneficiary may not. We describe the beneficiary's beliefs about her entitlement below. The intermediary chooses $b_o\left(\bar{b}_i\right) \in \left[0, \bar{b}\right]$, the benefit offer to the beneficiary. After observing $b_o$, the beneficiary chooses $a \in \{0, 1\}$ indicating whether s/he complains to a programme official about not receiving the payment they believe they are entitled to. This has an effort cost $C$ for the beneficiary. [8]

In case of a complaint by the beneficiary, the beneficiary receives the full benefit s/he is entitled to with probability $\pi > 0$. If the intermediary was initially offering benefits below the entitlement, the latter receives a penalty with a utility cost $\tilde{P}$. For ease of notation, we define $P = \pi\tilde{P}$.

[8] Implicitly, we are assuming that the intermediary can make a take-it-or-leave-it offer to the beneficiary. If the beneficiary accepts, s/he receives that offer. If s/he refuses, she incurs the cost of complaint $C$. Then, with probability $\pi$, she receives her entitlement; otherwise, she receives the intermediary's offer.

The payoff to beneficiary $i$ from a benefit offer $b_o$ and choosing action $a$ is given by

$$\begin{aligned} U_b(b_o, a) &= a\pi\bar{b}_i + (1 - a\pi)\, b_o - aC \\ &= b_o - aC + a\pi\Delta_i \end{aligned}$$

The payoff to the intermediary from a benefit offer $b_o$ to beneficiary $i$ is given by

$$\begin{aligned} U_I(b_o, a) &= a\pi 0 + (1 - a\pi)\,\Delta_i - a\pi\tilde{P} \\ &= \Delta_i - a\pi\left(\Delta_i + \tilde{P}\right) \end{aligned}$$

As the payoffs above are linear in benefit receipts, the beneficiaries and intermediaries are, by assumption, risk neutral. We consider the implications of relaxing this assumption in the next section. The prior belief of beneficiary $i$ about the entitlement is given by $\Pr\left(\bar{b}_i = b_h\right) = \theta_i \in (0, 1)$. We use $\mathbf{E}_0^i(.)$ to denote expected values based on this belief. We assume that the beneficiary's beliefs and the values of the parameters $C$, $\tilde{P}$ and $\pi$ are common knowledge.

We consider Perfect Bayesian Equilibria of this game. Specifically, the equilibrium will satisfy the following conditions: (i) When the intermediary makes an offer, the beneficiary updates beliefs about her entitlement, consistent with Bayes' Rule, and the intermediary's strategy; (ii) The beneficiary's decision whether to accept or reject the offer will be optimal given this posterior belief; (iii) The intermediary's strategy will be optimal given the beneficiary's strategy.

## 3.2 Equilibrium Analysis

We consider pure strategy equilibria of the game.[9] If the intermediary pursues a pure strategy, there are two types of equilibria possible: separating or pooling.

In the case of a **pooling equilibrium**, the intermediary will make the same offer to all beneficiaries (with the same expected entitlement) regardless of the actual entitlement. Then, the offer is uninformative to the beneficiary in terms of actual entitlement. Therefore, the beneficiary's posterior beliefs about her/his entitlement is the same as

[9] Although mixed strategy equilibria may exist for certain parametric configurations, we focus on pure strategy equilibria for the sake of tractability.

the prior beliefs. The beneficiary accepts the offer if and only if it yields a (weakly) higher expected payoff than putting in a complaint:

$$b_o \geq b_o - C + \pi \mathbf{E}_0 \Delta \tag{1}$$

$$\implies \mathbf{E}_0 \Delta \leq \frac{1}{\pi} C$$

$$\implies \theta_0 \left(b_h - b_o\right) + \left(1 - \theta_0\right)\left(b_l - b_o\right) \leq \frac{1}{\pi} C$$
$$\implies \mathbf{E}_0 \left(\bar{b}\right) - b_o \leq \frac{1}{\pi} C$$
$$\implies b_o \geq \mathbf{E}_0 \left(\bar{b}\right) - \frac{1}{\pi} C \tag{2}$$

Therefore, the smallest benefit offer that the intermediary can make and avoid a complaint is given by the expression on the right-hand side of (2).

If the benefit offer falls below this threshold, the beneficiary will always complain. Then, with probability $\pi$, the complaint is successful and the intermediary receives a payoff of zero. But with probability $(1 - \pi)$, the complaint is unsuccesful and the intermediary receives a payff of $\left(\bar{b} - b_o\right)$. Thus, the intermediary's expected payoff is decreasing in $b_o$. Therefore, if the offer is sufficiently low to result in a complaint, the offer that maximises the intermediary's payoff is $b_o = 0$.

Therefore, the two options available to the intermediary are as follows:

| | $b_o$ | Intermediary's Payoff |
|---|---|---|
| Option 1 | $\mathbf{E}_0 \left(\bar{b}\right) - \frac{1}{\pi} C$ | $\bar{b} - \mathbf{E}_0 \left(\bar{b}\right) + \frac{1}{\pi} C$ |
| Option 2 | 0 | $(1 - \pi)\, \bar{b} - P$ |

Option 1 yields a higher payoff for the intermediary if and only if (ignoring 'knife-edge' cases)

$$\bar{b} - \mathbf{E}_0 \left(\bar{b}\right) + \frac{1}{\pi} C > (1 - \pi)\, \bar{b} - P$$

$$\implies \mathbf{E}_0 \left(\bar{b}\right) - \pi \bar{b} < \frac{1}{\pi} C + P \tag{3}$$

Therefore, if the condition in (3) holds for $\bar{b} \in \{b_l, b_h\}$ we obtain a pooling equilibrium in which the intermediary always offers $b_o = \mathbf{E}_0\left(\bar{b}\right) - \frac{1}{\pi}C$ and the beneficiary accepts the offer without making a complaint. Note that, since $b_h > b_l$, if (3) holds for $\bar{b} = b_l$, it also holds for $\bar{b} = b_h$. So the condition for this pooling equilibrium can be written as

$$\mathbf{E}_0\left(\bar{b}\right) - \pi b_l < \frac{1}{\pi}C + P \tag{4}$$

On the other hand, if the condition is violated for both $\bar{b} = b_l$ and $\bar{b} = b_h$ we obtain a pooling equilibrium in which the intermediary always offers $b_o = 0$ and the beneficiary makes a complaint. If the condition in (3) is violated for $\bar{b} = b_h$, it is also violated for $\bar{b} = b_l$. So the condition for this pooling equilibrium can be written as (again ignoring 'knife-edge' cases)

$$\mathbf{E}_0\left(\bar{b}\right) - \pi b_h > \frac{1}{\pi}C + P \tag{5}$$

If the condition in (3) is satisfied for $\bar{b} = b_h$ but violated for $\bar{b} = b_l$, then we cannot have a pooling equilibrium. We can summarise these results as follows.

**Proposition 1.** *(i) If the condition in (4) holds, there is a pooling equilibrium in which the intermediary always offers* $b_o = \mathbf{E}_0\left(\bar{b}\right) - \frac{1}{\pi}C$*, and the beneficiary accepts the offer without making a complaint.*

*(ii) If the condition in (5) holds, there is a pooling equilibrium in which the intermediary always offers* $b_o = 0$*, and the beneficiary complains.*

Next, we consider **separating equilibria**. We can establish that there cannot be separating equilibria in which (i) the beneficiary always accepts the offer without making complaint; or (ii) the beneficiary always makes a complaint; or (iii) the intermediary's offer is accepted when the entitlement is $b_h$ but rejected when the entitlement is $b_l$. We provide proofs by contradiction to rule out these potential cases in the proof of Proposition 2 in the Theoretical Appendix.

The only remaining candidate for a separating equilibrium is one in which the intermediary's offer is rejected when the entitlement is $b_h$ but accepted when the entitlement is $b_l$. If such an equilibrium exists, we must have $b_o\left(b_h\right) = 0$ and $b_o\left(b_l\right) = b_l - \frac{1}{\pi}C$. As per our reasoning above, the following conditions are necessary for this strategy to occur in equilibrium (again ignoring 'knife-edge' cases):

$$b_l\left(1-\pi\right) < \frac{1}{\pi}C + P \tag{6}$$

$$b_h\left(1-\pi\right) > \frac{1}{\pi}C + P \tag{7}$$

If the conditions in (6) and (7) hold, then the proposed strategy is optimal for the intermediary. We can summarise these results as follows.

**Proposition 2.** *The only possible separating equilibrium takes the following form: the intermediary adopts the strategy $b_o\left(b_h\right) = 0$ and $b_o\left(b_l\right) = b_l - \frac{1}{\pi}C$; the beneficiary correctly infers the entitlement from the offer and accepts it when $b_o = b_l - \frac{1}{\pi}C$ but rejects it when $b_o = 0$. Conditions (6) and (7) are necessary and sufficient for these strategies and beliefs to constitute an equilibrium.*

Figure 1 shows the different types of equilibria, as described in Propositions 1 and 2, for different values of $C$, $\pi$ and $P$. Note that, as drawn, the regions for pooling and separating equilibria do not overlap (and some areas where there are no possible equilibria). However, there are potential parametric configurations for which these regions do overlap (not shown in the figure).

## 3.3 Effects of an Information Intervention

We consider next how an information intervention can change the equilibrium. As per the discussion above, there are three possible types of pure strategy equilibria: (i) a pooling equilibrium in which the intermediary offers a positive amount of benefits and the beneficiary does not complain; (ii) a pooling equilibrium in which the intermediary offers zero benefits and the beneficiary complains; (iii) a separating equilibrium in which the offer varies according to the entitlement and the beneficiary complains if and only if the entitlement is high.

In our context, we can rule out the third category of equilibria above because it would mean that the beneficiary has correct beliefs about entitlement. However, we show in Section 6 that very few beneficiaries have accurate information about their entitlements at baseline. Therefore, we consider below how the intervention affects behaviour only

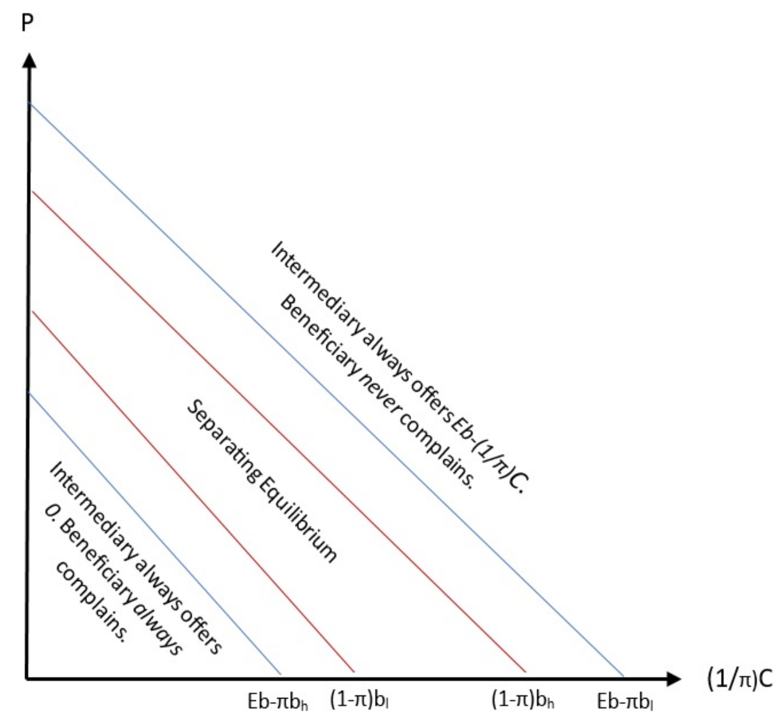



Figure 1: Pure Strategy Equilibria in Bargaining Game

for the first two equilibrium categories.

The information intervention could alter the cost of making a complaint to programme officials (for example by making programme contact information available to the beneficiary) and/or shift beliefs about the entitlement. We assume, for now, that these changes are common knowledge between the beneficiary and the intermediary. We denote by $\mathbf{E}_1^i\left(\bar{b}_i\right)$ beneficiary $i$'s post-intervention expectation about the entitlement and by $C_1$ the post-intervention cost of making a complaint. In this setup, we can think of the change in $C$ as the empowerment effect and the change in $E(\bar{b})$ as the information effect.

Suppose that the initial equilibrium is in the first category described above, i.e. the intermediary offers partial benefits and the beneficiary does not complain. If $C_1 < C_0$, then the right-hand side of the condition (4) becomes smaller. If the condition continues to hold post-intervention, we obtain a pooling equilibrium in which the intermediary offers $\mathbf{E}_j\left(\bar{b}\right) - \frac{1}{\pi}C_j$, where $j \in \{0, 1\}$ and the beneficiary accepts the offer without making a complaint.

If a beneficiary's beliefs were initially correct, then the information intervention will not affect this person's beliefs. Hence, $\mathbf{E}_0^i\left(\bar{b}\right) = \mathbf{E}_1^i\left(\bar{b}\right) = \bar{b}$. This implies that the intervention will simply increase the offer the intermediary makes to this beneficiary owing to a decline in $C$. This is a *pure* empowerment effect.

If a beneficiary initially overestimated the benefits, then the information intervention will affect this person's beliefs: $\mathbf{E}_0^i\left(\bar{b}\right) > \mathbf{E}_1^i\left(\bar{b}\right) = \bar{b}$. Thus, the overall effect on the intermediary's offer is ambiguous. It depends upon the relative movements in beliefs and costs.

If a beneficiary initially underestimated the benefits, then $\mathbf{E}_0^i\left(\bar{b}\right) < \mathbf{E}_1^i\left(\bar{b}\right) = \bar{b}$. As the change in beliefs and the cost work in the same direction, the intervention will cause the intermediary to increase the offer to the beneficiary. We can summarise these results as follows.

**Proposition 3.** *Suppose the beneficiary was initially receiving less than her full entitlement, and that the condition in (4) continues to hold post-intervention. Then, the net effect of this intervention on the beneficiary's receipt is as follows:*

1. *If a beneficiary's beliefs were initially <u>correct</u>, then the information intervention will result in <u>increased</u> receipts.*

2. *If a beneficiary initially <u>underestimated</u> her entitlement, then the information intervention will result in <u>increased</u> receipts.*

3. *If a beneficiary initially <u>overestimated</u> her entitlement, then the information intervention has an <u>ambiguous</u> effect on receipts.*

Consider next the other category of pooling equilibrium, i.e. the beneficiary is offered zero transfers and always *elects* to complain. It is indeed plausible that some of the beneficiaries in our sample were in this particular pooling equilibrium, because of high expected beliefs about their entitlement. In Section 6, we provide evidence of a large variation in beliefs about benefit entitlements among the study households.

If a beneficiary's beliefs were initially correct then the information intervention will not affect this person's beliefs. Hence, $\mathbf{E}_0^i\left(\bar{b}\right) = \mathbf{E}_1^i\left(\bar{b}\right) = \bar{b}$. This implies that the intervention will simply have no effect as the condition in (5) continues to hold in the post-intervention period. Hence, the same pooling equilibrium continues to apply here. There will be no change in the beneficiary's behaviour: she will continue to complain and receive her full entitlement with probability $\pi$.

If a beneficiary initially overestimated the benefits, then the information intervention will decrease her expected benefits: $\mathbf{E}_0^i\left(\bar{b}\right) > \mathbf{E}_1^i\left(\bar{b}\right) = \bar{b}$. Whether the condition in (5) continues to hold depends on the relative movements in beliefs and costs. If we assume that the pooling equilibrium condition continues to hold, then we obtain the same behaviour and outcome as in the case of initially accurate beliefs.

If a beneficiary initially underestimated the benefits, then the information intervention will increase her expected benefits: $\mathbf{E}_0^i\left(\bar{b}\right) < \mathbf{E}_1^i\left(\bar{b}\right) = \bar{b}$. The condition in (5) continues to hold in the post-intervention period and the beneficiary's behaviour and receipt of benefit remains unchanged. We summarise the results above as follows.

**Proposition 4.** *Suppose the beneficiary was initially receiving her full entitlement, and that the condition in (5) continues to hold post-intervention. Then, the net effect of this intervention on the beneficiary's receipt is the same irrespective of the beneficiary's accuracy of initial belief. The information intervention will result in no change in receipts* *for this beneficiary.*

Table 1 describes the predictions of the effects of intervention from the propositions above by beneficiary type, distinguishing between two distinct potential mechanisms — the 'information' effect and the 'empowerment' effect.

Table 1: Model Predictions

| | Receipt | Belief | Treatment Effect | Change in Benefits |
|---|---|---|---|---|
| 1 | Full | Accurate | Neither | No Change |
| 2 | Full | Overestimate | Pure Information | No Change |
| 3 | Full | Underestimate | Pure Information | No Change |
| 4 | Partial | Accurate | Pure Empowerment | Increase |
| 5 | Partial | Overestimate | Info+Power Combo | Ambiguous |
| 6 | Partial | Underestimate | Info+Power Combo | Increase |

The predictions in rows (4) – (6) are based on Proposition 3 while the predictions in rows (1) – (3) are based on Proposition 4. Comparing (4) with (1), we obtain the prediction that the empowerment mechanism, on its own, should increase benefits received by the beneficiary. On the other hand, comparing (2) with (1), we obtain the prediction that the information mechanism on its own should not change benefits.

However, it is worth noting that the predictions above relies on the assumption that any changes in beliefs and costs due to the intervention are sufficiently small that the equilibrium type is unchanged. If we allow for the possibility that changes to $C$ and $E(\bar{b})$ are sufficiently large to change the equilibrium type then the intervention can, in theory, lead to a *decrease* in benefits.

For example, suppose the intermediary was initially withholding all benefits – as in the second type of pooling equilibrium described above – but the beneficiary secured her full entitlement after making a complaint. If the beneficiary initially overestimated her entitlement, then correcting her beliefs could produce a separating equilibrium in which all beneficiaries with low entitlement receive partial benefits and cease to complain. In other words, those who had previously obtained their full entitlement would now receive less. Thus, a pure information effect could lead to a decrease in benefits.

Alternatively, suppose that the beneficiary had accurate beliefs about her entitlement but received partial benefits – as in the first type of pooling equilibrium described above. If the intervention lowers the cost of making complaints sufficiently, this could lead to a separating equilibrium in which the intermediary withholds all benefits from beneficiaries with high entitlement. All of these beneficiaries would complain but only a fraction would be successful in securing their full entitlement. As a result, average receipts could go down. Thus, a pure empowerment effect could lead to a decrease in average benefits.

The examples above highlight that, in the case of an intervention that substantially affect beliefs and costs, the pure information effect, and the pure empowerment effect, on benefits can both be negative. We describe the range of intervention effects due to a change in equilibrium in the Theoretical Appendix.

# 4 Intervention and Design of Experiment

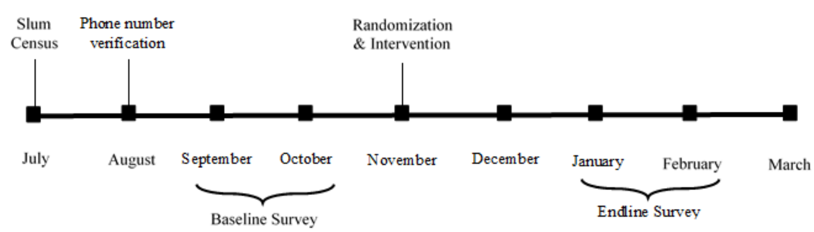



Figure 2: Project Timeline, 2020-21

Our experiment took place with 1,006 households living in 60 different slums in the city of Kanpur, Uttar Pradesh, as shown in Appendix B. The treatments were randomly allocated at slum-level in a random saturation design (as in Baird et al., 2018) to capture the nature and size of 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 C) and follow-up voice calls. The content of the voice calls was tailored to each household, based on information we gathered in a baseline survey. For instance, when contacting households who were eligible for PMGKY (Jan Dhan) but were not receiving their full entitlement at baseline, enumerators would follow the text related to 'category 1A' (category 1B) calls (as in Appendix C). During the voice calls, households who were not receiving their entitlements from a program were also provided with the contact details for that program. If they believed that they were being purposefully denied benefits, they were given the phone number where they could lodge official complaints.

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

# 5 Data and Empirical Methodology

## 5.1 Study sample and sample balance

Our study focuses on "urban poor" households residing in urban slums, a group that has grown significantly over the past few years, but has been overlooked by government policies leading to a shift of the locus of poverty from rural to urban areas. Slum dwellers face significant challenges including high crime, increased exposure to disease and violence and a lack of access to basic facilities.

Our study site is Kanpur, the largest city in the state of Uttar Pradesh, India and home to one of the largest slum populations in India (Sawhney (2016)). 9.5% of the population of Uttar Pradesh, and over 13% of children in the state live in slums (of the Registrar General & Census Commissioner (2013)), making this a particularly apt setting for our study. Our sample comprises of 60 slums selected using stratified random sampling

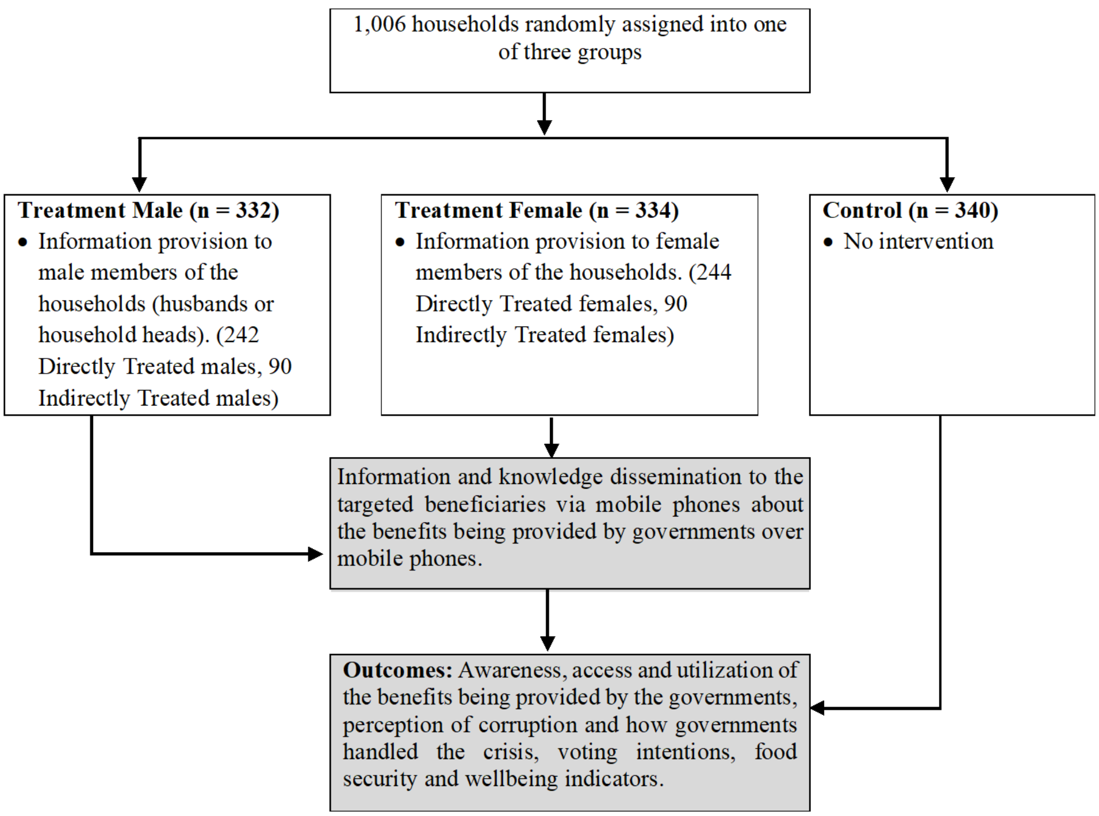



Figure 3: Experimental Design

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 city.

Our baseline survey was fielded, via phone surveys conducted between September and October 2020, to 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, also via phone, 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 respondent and her household, earnings, household income and expenditure, and on the outcomes detailed in the next sub-section.

Appendix Table D1 displays the baseline descriptive statistics and sample balance for individual and household characteristics for our sample. Survey respondents in the control group were 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.

Around 84% of control group households were Hindu, 45% from scheduled castes/tribes and 33% from other backward classes. Respondents lived in households with just over 5 household members, with average monthly income of INR 9,500. The occupation of the main earner was day labor (48% of sample), with a further 30% of sample households earning their main income from a business. Fewer than 15% of control group households had a private sector job, with around 1% engaged in government jobs. Over 90% of control group households were eligible to receive aid from PMGKY, just over 23% for PMUY and just over 53% were eligible for aid from Jan Dhan. Reassuringly, we see that the sample is balanced across the rich set of variables considered.

In Appendix Table D2, we compare the characteristics of our sample households with those of households in the bottom 60% of the wealth distribution of urban households in India as a whole and in Uttar Pradesh in the fifth round of the nationally representative

National Family Health Survey (NFHS-5) which was collected at a similar time as our experiment.[10] The table shows that on average, our experimental sample is similar in age (slightly older) than the average Indian (Uttar Pradesh) household in the bottom 60% of the wealth distribution. A smaller proportion of households in our sample has fewer than 10 years of schooling, while a larger proportion has more than 12 years of schooling relative to households in the sub-samples for India and Uttar Pradesh. Similar proportions of households in the three groups have between 10 and 12 years of schooling. Our sample includes similar proportions of households from advantaged (disadvantaged) castes as that for India and Uttar Pradesh. However, it includes a higher proportion of Hindu households than in the India or Uttar Pradesh sub-samples.

## 5.2 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 Appendix Tables D3 and D4 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, and their unconditional entitlement; and whether they

[10] Households are classified according the value of an asset index provided by NFHS-5.

received any aid from the programs, and how much the household received from the program. The omnibus test of joint significance is rejected for the baseline knowledge outcomes, but not for the baseline aid receipt outcomes. Consequently, our main empirical specification controls for the baseline outcome in order to be able to estimate the causal effects of the interventions.

It is worthwhile to highlight an important asymmetry that exists across the three programs as it holds implications for identifying the relevant mechanisms at play. The baseline means for PMUY eligibility (0.234 in the control group) and receipts (0.243 in the control group) indicate that PMUY beneficiary households were, for the most part, receiving the Covid relief that they were eligible for prior to the intervention: one free LPG cylinder refill per month. The same is not true for the other schemes — PMGKY and Jan Dhan. This supports our earlier hypothesis (see Section 2) that, at least in urban areas, the scheme worked relatively well and was less prone to corruption and mismanagement than the two other schemes under consideration. For these reasons, we argue that the potential of the intervention to affect outcomes through the empowerment channel was largely absent in the case of PMUY benefits.

That said, our intervention could, nevertheless, have informed PMUY beneficiaries better about their total Covid-related entitlement under the scheme (maximum of 3 LPG refills); and, thus, the potential for the information provision effect is present here, in a way similar to that of the other schemes.

Given this significant asymmetry across schemes, we focus – in our main analysis – on PMGKY and Jan Dhan (both empowerment and information provision channels at play). We then compare the effects for these two schemes with that of PMUY to assess the extent to which the two potential channels may be responsible for the main findings. The details regarding the mechanisms and how we test for them is contained in Section 6.1.3.

## 5.3 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 $t$, $T^r_{jt}$, 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.

$$y_{ijt} = \alpha + \sum_r \beta_r \; T^r_{jt} * Q_{ij} + \sum_r \eta_r \; T^r_{jt} * Spill_{ij} + G'_{ijt}\gamma + \varepsilon_{ijt}$$

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 across 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)). In order to reduce the likelihood of false rejections due to multiple hypothesis testing, we report Westfall-Young stepdown adjusted p-values for our main results. These adjusted p-values fix the family-wise error rate within all related tests of a specific hypothesis.[11]

[11] In practice we take a family of tests to be all those tests within the same panel of a table.

# 6 Results

We first present the impacts of the treatments on directly treated households before discussing findings for the untreated 'spillover' households in the treated slums.

## 6.1 Impacts on Directly Treated Households

### 6.1.1 Knowledge

Table 2 presents results on the impacts of the information intervention on knowledge of entitlements from the two programs. We focus on three key outcomes: (1) whether the household was aware that the program was offering any covid relief (extensive margin); (2) the household's entitlement from the program (intensive margin); and (3) the gap between the household's beliefs of its entitlement and its actual entitlement.

Columns 1 and 2 of the Table show that the interventions had almost no impacts on the extensive margin. 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 interventions on the extensive margin.

The results in columns 3 and 4 show that the interventions led to a reduction in beliefs about entitlements from the two programs. Among the control group, households expected to receive on average 26.8kg of grains from PMGKY and INR 1752 from Jan Dhan. By contrast, treated households expected to receive close to 3.5kg less grains and INR 1388 less of cash from the two programs. Interestingly, the effects are very similar regardless of the gender of the recipient of the information and are robust to our adjustments for multiple hypothesis testing (Westfall-Young stepdown adjusted p-values are reported in brackets under the standard errors).

The reduction in beliefs about entitlements corrects households' overestimation of program entitlements. The overestimation, and its correction by our intervention, are both evident in Figure B1, which plots the baseline and endline distributions of respondent knowledge of program entitlements (in per-person terms), by treatment, for the two programs.[12] At baseline, significant proportions of respondents over-estimated –

[12]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.

consistently across the treatment arms – the amount they were eligible for from each program. At endline, while the distribution of the gap remains the same for control households, we observe a shift towards zero among the treated households, indicating that the treatment induced households to correct their beliefs. Regression estimates presented in columns 5 and 6 in Table 2 show that the overestimation was almost completely eliminated by our interventions, with no difference detected by the gender of the recipient.

Table 2: Intervention Impact on Knowledge Outcomes

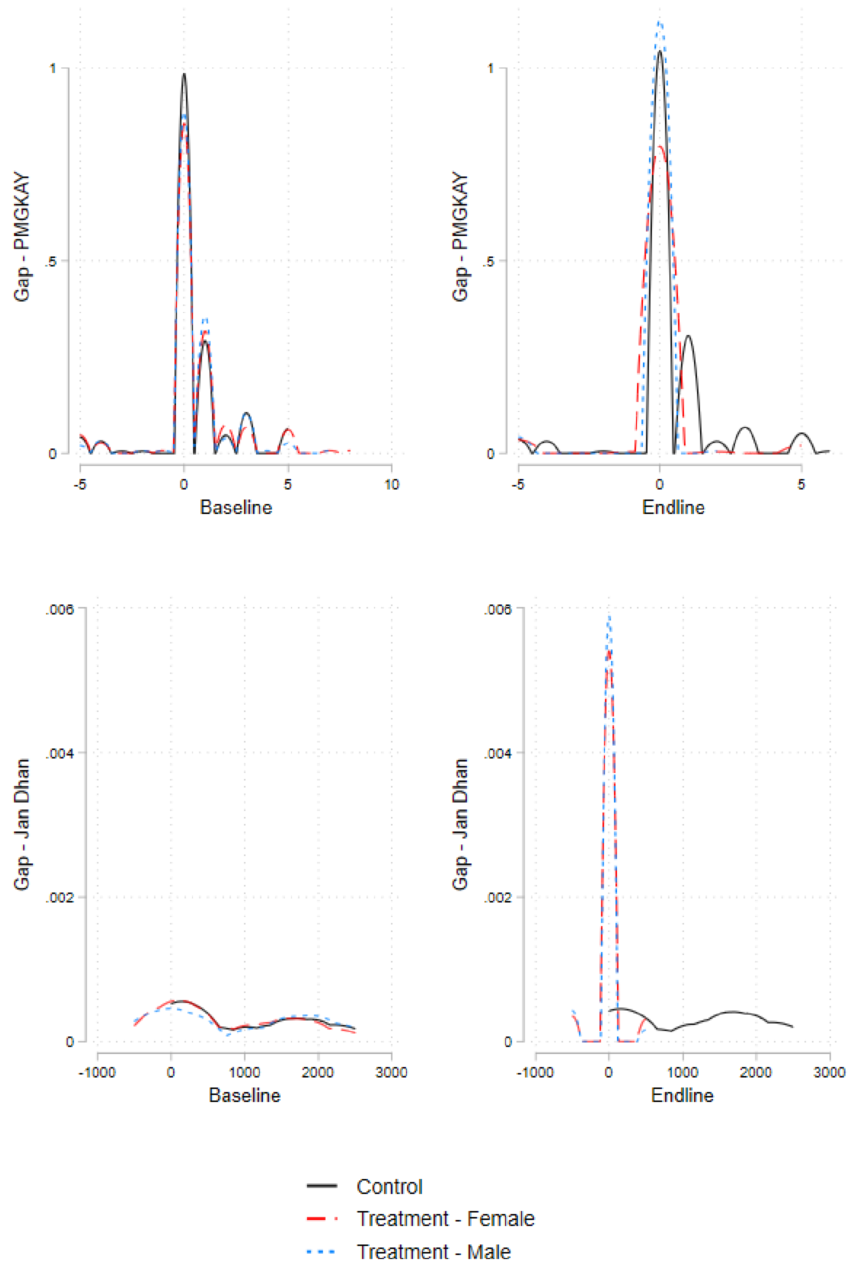
| | Mentioned | | HH Entitlement Belief | | Belief-Entitlement | |
|---|---|---|---|---|---|---|
| | PMGKY | Jan Dhan | PMGKY | Jan Dhan | PMGKY | Jan Dhan |
| *A: Male vs Female* | | | | | | |
| Treat Female | 0.002 | 0.001 | -3.531*** | -1402.872*** | -2.345*** | -1151.948*** |
| | (0.006) | (0.014) | (0.885) | (135.266) | (0.608) | (117.989) |
| | [ 0.973] | [ 0.973] | [ 0.009] | [ 0.000] | [ 0.010] | [ 0.000] |
| Treat Male | -0.022** | -0.013 | -3.411*** | -1372.505*** | -3.085*** | -1170.955*** |
| | (0.008) | (0.012) | (0.844) | (124.591) | (0.493) | (117.081) |
| | [ 0.073] | [ 0.636] | [ 0.008] | [ 0.000] | [ 0.002] | [ 0.000] |
| *B: Pooled Treatment* | | | | | | |
| Treatment | -0.010* | -0.006 | -3.466*** | -1388.342*** | -2.716*** | -1161.763*** |
| | (0.006) | (0.011) | (0.739) | (121.469) | (0.464) | (111.651) |
| | [ 0.148] | [ 0.585] | [ 0.001] | [ 0.000] | [ 0.000] | [ 0.000] |
| Baseline Outcomes | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Covariates | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Female = Male (pval) | 0.027 | 0.327 | 0.893 | 0.741 | 0.231 | 0.791 |
| Control Mean (EL) | 0.916 | 0.495 | 26.814 | 1752.252 | 2.105 | 1145.646 |
| Observations | 993 | 993 | 987 | 989 | 987 | 993 |

*Note*: Standard errors (in parentheses) are clustered at the slum level. Covariates include eligibility for the programs, variables for which there was an imbalance at baseline (whether the main source of earnings for the household was from day labor, a business, a government job, or private sector job). Stars indicate statistical significance: **p <0.10, ** p <0.05, *** p <0.01*. Westfall-Young stepdown adjusted p-values - which control the family-wise error rate for all tests in a panel - are shown in brackets.

### 6.1.2 Access

Next, we study the intervention impacts on access to benefits on the extensive margin – whether or not the household received any assistance from the program – and the intensive margin – the amount of aid actually received, and how this differs from what the household should receive. Thereafter, we study impacts on gaps between (i) how much aid the household received from the program and its entitlement; and (ii) the amount of aid received from the program and the household's beliefs.

Figure 4: Gap in Perceptions (in per eligible person terms)




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

Table 3: Intervention Impact on Program Receipt

| | Any | | HH Amount | |
|---|---|---|---|---|
| | PMGKY | Jan Dhan | PMGKY | Jan Dhan |
| *A: Male vs Female* | | | | |
| Treat Female | 0.013 | 0.002 | 5.593*** | 120.219*** |
| | (0.008) | (0.014) | (0.492) | (20.425) |
| | [ 0.411] | [ 0.925] | [ 0.000] | [ 0.000] |
| Treat Male | -0.005 | -0.011 | 4.933*** | 131.097*** |
| | (0.011) | (0.012) | (0.481) | (24.037) |
| | [ 0.902] | [ 0.754] | [ 0.000] | [ 0.000] |
| *B: Pooled Treatment* | | | | |
| Treatment | 0.004 | -0.005 | 5.260*** | 125.730*** |
| | (0.008) | (0.011) | (0.375) | (17.393) |
| | [ 0.819] | [ 0.819] | [ 0.000] | [ 0.000] |
| Baseline Outcomes | ✓ | ✓ | ✓ | ✓ |
| Covariates | ✓ | ✓ | ✓ | ✓ |
| Female = Male (pval) | 0.123 | 0.401 | 0.296 | 0.697 |
| Control Mean (EL) | 0.904 | 0.492 | 18.384 | 259.763 |
| Observations | 993 | 993 | 983 | 993 |

*Note*: Standard errors (in parenthesis) are clustered at the slum level. Covariates include eligibility for the programs and variables for which there was an imbalance at baseline (i.e. whether the main source of earnings of the household head was from day labor, a business, a government job, or private sector job). Stars indicate statistical significance: **p <0.10, ** p <0.05, *** p <0.01*. Westfall-Young stepdown adjusted p-values - which control the family-wise error rate for all tests in a panel - are shown in brackets.

Columns 1 and 2 of Table 3 show that the interventions had no impacts on the extensive margin. The coefficients are all small in magnitude and statistically insignificant, indicating that the intervention did not change a household's likelihood of receiving benefits.

Turning to the intensive margin, columns 3 and 4 of the Table show that in the month after our intervention, treated households received, on average, around 5kg more in grains per month and INR 125 in cash as a result of our interventions. These impacts are economically significant, accounting for an increase of 27% - 30% in grains, and 46% - 50% increase in cash transfer relative to that received by the average control group household at endline. Interestingly, we find no differences in impacts by the gender of the recipient.

This increase in the amount of aid received from both programs significantly reduces the gap between what households receive and how much they should receive as we show in Table 4. Columns 1 and 2 show that at endline, control group households were receiving, on average, around 8.42kg (per month) less in food grains and INR 240.24

(per month) less in cash transfers than their entitlements. Our simple information interventions closed this gap by close to 6kg grains and INR 127.76 in cash for treated households. There are no statistically significant differences in impacts for the two treatment groups, and these impacts are robust to multiple hypothesis testing.

Table 4: Intervention Impact on Program Receipt Gaps

| | Gap Rec - Entitled | | Gap Rec - Belief | |
|---|---|---|---|---|
| | PMGKY | Jan Dhan | PMGKY | Jan Dhan |
| *A: Male vs Female* | | | | |
| Treat Female | 6.221*** | 148.810*** | 8.384*** | 1630.934*** |
| | (0.529) | (23.436) | (0.697) | (146.509) |
| | [ 0.000] | [ 0.000] | [ 0.000] | [ 0.000] |
| Treat Male | 5.724*** | 106.884*** | 8.375*** | 1510.896*** |
| | (0.535) | (23.516) | (0.676) | (138.568) |
| | [ 0.000] | [ 0.031] | [ 0.000] | [ 0.000] |
| *B: Pooled Treatment* | | | | |
| Treatment | 5.970*** | 127.758*** | 8.376*** | 1571.842*** |
| | (0.408) | (18.767) | (0.676) | (139.325) |
| | [ 0.000] | [ 0.001] | [ 0.000] | [ 0.000] |
| Baseline Outcomes | ✓ | ✓ | ✓ | ✓ |
| Covariates | ✓ | ✓ | ✓ | ✓ |
| Female = Male (pval) | 0.471 | 0.156 | 0.970 | 0.046 |
| Control Mean (EL) | -8.417 | -240.237 | -8.429 | -1492.489 |
| Observations | 983 | 993 | 990 | 993 |

*Note*: Standard errors (in parenthesis) are clustered at the slum level. Covariates include eligibility for the programs and variables for which there was an imbalance at baseline (i.e. whether the main source of earnings of the household head was from day labor, a business, a government job, or private sector job). Stars indicate statistical significance: **p <0.10, ** p <0.05, *** p <0.01*. Westfall-Young stepdown adjusted p-values - which control the family-wise error rate for all tests in a panel - are shown in brackets.

Combined with the correction of beliefs documented in Section 6.1.1, Columns 3 and 4 of the table also show that the increase in aid receipts due to the intervention also closed the gap between what households *actually* receive and what they *believed* they should receive from the programs. Across all three programs, at endline, control households 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.

Thus, our simple intervention was successful not only in correcting beliefs, but also in increasing the aid received by households, thereby significantly reducing (though not eliminating) the gap between aid receipt and aid entitlements.

### 6.1.3 Mechanisms: What drove the closure of the receipt-entitlement gap?

An immediate question that arises is how did the intervention close the receipt-entitlement gap, given that households were, on average, overestimating their entitlements at baseline. There are three possible channels highlighted in the literature.

First, the intervention could have altered preferences or stigma related to receiving government benefits that were stopping households from claiming their entitlements. We believe this is unlikely for two reasons. First, the economic shock from the COVID crisis was widespread and large, reducing the force of such preferences. Second, the study sample were already receiving assistance from these programs prior to the COVID crisis. Moreover, our intervention did not have any impacts on the extensive margin for access, contrary to what we would have expected if the receipt-entitlement gap was driven by this channel.[13]

Second, the personalized information and contact details of officials could have weakened informational constraints. Our RCT sample was already aware of the existence of the programs, but they were *overestimating* their entitlements. The overestimation rules out the gap being driven by beneficiaries not claiming entitlements because they believed that the benefits were too low.On the other hand, as the theoretical model in Section 3.3 shows, in the case of beneficiaries who were initially overestimating their entitlements, correcting their misperception could potentially change the strategic behaviour of the intermediary and lead to a *decrease* in benefits received.

Finally, our intervention could have had an "empowerment effect". The theoretical model shows how lowering the cost of making complaints could enable the beneficiaries to negotiate higher benefits from the intermediary. Prior to the intervention, households may have believed that they were not receiving the benefits due to them because of leakage or policy ineffectiveness, but did not know how reach the right officials to make a complaint. Following our information intervention, beneficiaries could have used the contact details of program officials and the official complaints line to bargain more effectively with intermediaries to obtain their entitlements. Separately, our intervention (which entailed receiving information from researchers at a reputed Indian university)

[13] This result also rules out explanations that the gap was driven by the perception that the (hassle) costs of claiming were very high, or that our intervention provided a reminder to eligible households to go and claim their entitlements.

could have increased the beneficiaries' confidence in their beliefs about their entitlement. Even if the expected entitlement was unaffected, the change in beliefs would improve the bargaining position of risk-averse beneficiaries.[14]

We can disentangle the latter two explanations – the information effect and the empowerment effect – by exploiting differences in the heterogeneous impact of the intervention. In particular, we look at how the intervention's impact on the receipt-entitlement gap varies with respondents' beliefs of their entitlements at baseline and their receipt of benefits at baseline. Guided by the theoretical predictions in Table 1, we divide our sample into four subgroups: (i) the respondent was not overestimating at baseline but the household was not receiving its full entitlement; (ii) the respondent was overestimating at baseline, but the household was receiving its full entitlement; (iii) the respondent was not overestimating at baseline, and the household was receiving its full entitlement; and (iv) the respondent was overestimating at baseline, and the household was not receiving its full entitlement.

As shown in Table 1, the information effect should drive impacts only for the households who had incorrect beliefs at baseline (i.e., those in groups (ii) and (iv); while the empowerment effect would generate impacts for those who were not receiving their full entitlement, regardless of their beliefs (i.e., those in groups (i) and (iv)). Impacts on group (i) households would be solely due to the empowerment effect, while those on group (iv) households would be due to a combination of the information and empowerment effects. To confirm formally that the impacts are driven by the empowerment effect, we select group (i) as the reference category and test for effects in the other groups relative to the treatment effect on group (i). Analyzing effects for group (iii) will also allow us to rule out concerns that the respondents may have used the information to bully or coerce intermediaries to give them more than their entitlements.

We estimate these heterogeneous treatment effects by interacting the dummy for receiving any treatment with indicators for whether, at baseline, the full entitlement was received and whether the respondent overestimated the entitlement. Since we did not detect any significant differences in impacts between the male and female treatment arms, we simplify this analysis by pooling together the two treatment groups, with the

[14] Note that, in our theoretical model, agents are assumed to be risk neutral. But it is straightforward to extend the model to show that, in the case of risk-averse beneficiaries, reducing the variance in the potential entitlement can improve receipts in equilibrium

pooled treatment denoted by T.

Table 5 displays these heterogeneous treatment effects and shows that our results can be explained by the empowerment effect. We find that the coefficient on the dummy T, which captures the impacts for group (i), is positive and statistically significantly different from 0. The impacts for this group are driven solely by the empowerment effect. We also find that the intervention did not have any significant impacts on reducing the gap for households that were already receiving their full entitlement: the coefficients associated with the interaction terms between T and indicators for being in groups (ii) and (iii) are negative and similar in magnitude to the coefficient on T, leading to a (close to) zero net effect. That this effect does not vary with baseline beliefs provides evidence suggesting that the impacts are not driven by the information effect.

Further evidence comes from testing whether the coefficient on the interaction term between T and the sub-group that had incorrect beliefs while not receiving the full entitlement (group iv) is statistically significant. This coefficient would capture the additional impacts due to the elimination of the overestimation. We find that this interaction term is close to 0 and not statistically significant for both programs. This provides further evidence that the impacts are not due to the information effect.

Further support for the empowerment mechanism comes from examining intervention impacts on the PMUY program. As discussed earlier, the program was less prone to leakage and mismanagement, with most eligible households receiving their entitled aid at baseline. Thus, the potential of the intervention to affect outcomes through the empowerment channel was largely absent for PMUY. However, it could have informed PMUY beneficiaries about their total entitlement. Thus the information channel would have been at play. By studying average intervention impacts on PMUY entitlement beliefs and aid receipt, we can provide further validation for the mechanism. In Table 6 we first find that beliefs about entitlements were revised downwards by the intervention. However, there was no effect on the amounts received. Put together, these results provide further evidence that intervention impacts were driven primarily by the empowerment channel rather than the information channel.

We also find evidence – in line with the empowerment channel – that the intervention made households more likely to act and expend effort to receive their entitlement, as

Table 5: Mechanisms: Information vs Empowerment effect

| | PMGKY | Jan Dhan |
|---|---|---|
| T | 8.172*** | 269.695*** |
| | (0.656) | (54.963) |
| T x full entitlement=1 & overestimating=1 | -7.981*** | -298.605*** |
| | (1.222) | (79.410) |
| T x full entitlement=1 & overestimating=0 | -7.376*** | -234.882*** |
| | (0.794) | (58.421) |
| T x full entitlement=0 & overestimating=1 | -0.246 | 67.453 |
| | (0.971) | (87.105) |
| Baseline Outcomes | Yes | Yes |
| Covariates | Yes | Yes |
| Control Mean (overestimators) | -8.268 | -554.446 |
| Control Mean (others) | -9.546 | -388.889 |
| T = 0 if full entitlement=0 and overestimating=1? | 0.000 | 0.000 |
| T = 0 if full entitlement=1 and overestimating=0? | 0.131 | 0.030 |
| T = 0 if full entitlement=1 and overestimating=1? | 0.875 | 0.714 |
| Observations | 983 | 993 |

*Note*: This table regresses the gap between what a household receives and what it is entitled to receive against treatment status interacted with dummy variables indicating i) whether a household was receiving it's full entitlement of the program at baseline, and ii) whether the respondent overestimated the entitlement at baseline. The reference category is group (i) (i.e. the respondent was not overestimating at baseline but the household was not receiving its full entitlement). The coefficients on the interaction terms indicate the additional effect of receiving the treatment for the other household groups (ii through iv). At the bottom of the table we report the p-value from joint t-tests for whether the sum of coefficient on the treatment indicator and the interaction term for each of the groups (ii) to (iv) is statistically significantly different from 0. Standard errors (in parenthesis) are clustered at the slum level. Stars indicate statistical significance: **p <0.10, ** p <0.05, *** p <0.01.*

shown in Table 7. Columns 1 and 2 of this table show that the intervention increased the reported willingness of treated households that were not receiving their full benefits at baseline to contact the local elected officials (though only for those with correct beliefs at baseline) and public distribution system shop (which distributes the food aid).[15] Treated households were also more likely to report having contacted local officials (column 3), which is consistent with the fact that the intervention may have increased the

[15]Since these outcomes are not program-specific, we divide the sample into four sub-groups according to their over-estimation of entitlements from *any* program and receipt of full entitlements from both programs. In particular, we divided the sample into the following: those who did not over-estimate entitlements on either program but did not receive full entitlement from both programs, those who received the full entitlement from both programs and over-estimated entitlements from either program, those who received the full entitlement from both programs and did not over-estimate entitlements from any and those who did not receive the full entitlement from either program and overestimated entitlements from either program.

Table 6: Mechanism Validation: PMUY

| | HH Entitlement Beliefs | | HH Entitlement Receipts | |
|---|---|---|---|---|
| | Belief Amount | Belief - Entitled Gap | Received Amount | Rec - Entitled Gap |
| *A: Male vs Female* | | | | |
| Treat Female | -0.090* | -0.066 | 0.016 | 0.040* |
| | (0.053) | (0.051) | (0.024) | (0.022) |
| Treat Male | -0.139*** | -0.118*** | -0.003 | 0.018 |
| | (0.029) | (0.029) | (0.012) | (0.011) |
| *B: Pooled Treatment* | | | | |
| Treatment | -0.115*** | -0.092** | 0.006 | 0.029** |
| | (0.037) | (0.036) | (0.015) | (0.014) |
| Baseline Outcomes | X | X | ✓ | ✓ |
| Covariates | ✓ | ✓ | ✓ | ✓ |
| Female = Male (p-val) | 0.278 | 0.221 | 0.438 | 0.306 |
| Control Mean (EL) | 0.363 | 0.129 | 0.228 | -0.006 |
| Observations | 993 | 993 | 990 | 990 |

*Note*: Standard errors (in parenthesis) are clustered at the slum level. Covariates include eligibility for the programs and variables for which there was an imbalance at baseline (i.e. whether the main source of earnings of the household head was from day labor, a business, a government job, or private sector job). Stars indicate statistical significance: **p <0.10, ** p <0.05, *** p <0.01.*

respondents' confidence in the entitlements and thereby encouraged them to make an effort to obtain them.[16]

Importantly, the intervention also altered households' perceptions of corruption and their reported likelihood of paying a bribe, as shown in the last two columns of the table. Control group households believed that, on average, corruption in ration shops stood at 3.42 on a scale from 1 to 5, where 5 represented high corruption and 1 no corruption. We see a drop in the perception of corruption in ration shops among treated households in the sub-group that did not receive their full entitlements and were not overestimating their beliefs at baseline. Interestingly, we find no drop in perceived corruption for the other sub-groups, including the households that were not receiving

[16] Interestingly, and consistent with the empowerment effect, we find that the intervention increased the likelihood of men contacting officials at the expense of women doing so. Results in Appendix Table F10 show that providing information to the household increased the likelihood that males made contact with officials for both programs, while there was a decrease in the probability that females made contact. In a society with strong gender norms, men are going to be more likely to have the bargaining power to claim their entitlements in a ration shop or to contact banking officials in relation to bank account. This is consistent with the empowerment effect in a male dominated society context.

their full entitlements and had incorrect beliefs. Moreover, while at endline, 7.8% of control group households reported having paid a bribe to receive government benefits, treated households were 5.1 percentage points less likely to report having done so, with no statistically significant differences in impacts across the sub-groups (though the interaction terms are somewhat noisy). Put together, these findings indicate that the intervention influenced households' corruption perceptions and reduced their likelihood of paying a bribe to receive government benefits.

Table 7: Mechanisms: Contacts, corruption and bribes

| | Would contact local elected official | Would contact PDS shop | Contacted local officials | Perception of corruption ration shop | Paid bribe |
|---|---|---|---|---|---|
| Treatment | 0.110** | 0.181*** | 0.536*** | -0.419*** | -0.051** |
| | (0.052) | (0.052) | (0.039) | (0.090) | (0.019) |
| T x full entitlement=1 & overestimating any=1 | -0.007 | -0.101 | 0.081 | 0.325 | 0.072 |
| | (0.133) | (0.102) | (0.097) | (0.201) | (0.049) |
| T x full entitlement=1 & overestimating any=0 | 0.002 | -0.106 | 0.013 | 0.626*** | 0.044 |
| | (0.124) | (0.102) | (0.094) | (0.183) | (0.039) |
| T x full entitlement=0 & overestimating any=1 | -0.075 | -0.058 | 0.086* | 0.451*** | 0.013 |
| | (0.066) | (0.059) | (0.050) | (0.108) | (0.022) |
| Baseline Outcomes | No | No | Yes | Yes | Yes |
| Control Mean (overestimators) | 0.412 | 0.660 | 0.062 | 3.247 | 0.093 |
| Control Mean (others) | 0.444 | 0.644 | 0.083 | 3.522 | 0.068 |
| T = 0 if full entitlement=1 and overestimating=1? | 0.432 | 0.391 | 0.000 | 0.625 | 0.657 |
| T = 0 if full entitlement=1 and overestimating=0? | 0.309 | 0.356 | 0.000 | 0.252 | 0.811 |
| T = 0 if full entitlement=0 and overestimating=1? | 0.468 | 0.003 | 0.000 | 0.685 | 0.012 |
| Observations | 993 | 993 | 993 | 993 | 993 |

*Note*: This table regresses various outcomes against treatment status interacted with dummy variables indicating i) whether a household was receiving it's full entitlement of all programs at baseline, and ii) whether the respondent overestimated the entitlement for any program at baseline. The reference category is group (i) (i.e. the respondent was not overestimating at baseline but the household was not receiving its full entitlement). The coefficients on the interaction terms indicate the additional effect of receiving the treatment for the other household groups (ii through iv). At the bottom of the table we report the p-value from joint t-tests for whether the sum of coefficient on the treatment indicator and the interaction term for each of the groups (ii) to (iv) is statistically significantly different from 0. Standard errors (in parenthesis) are clustered at the slum level. Stars indicate statistical significance: **p <0.10, ** p <0.05, *** p <0.01.*

### 6.1.4 Food Insecurity and Expenditures

Having established that the intervention corrected households' beliefs and increased access to emergency aid, we now study the intervention impacts on measures of household welfare and wellbeing. We consider two sets of measures: food insecurity and expenditures, and mental health and life and financial satisfaction. In this subsection, we report impacts on food insecurity and food expenditures, while impacts on mental health and life and financial satisfaction are discussed in Subsection 6.1.5.

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. This reduction is in line with the 27% - 30% increase in receipts of food grains from the PMGKY program documented in Subsection 6.1.2. 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.

### 6.1.5 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. Originally developed to identify individuals who are likely to have a mental disorder within a clinical setting, the scale has been widely used to screen for psychological distress in nonclinical settings (Weich and Lewis 1998, Weich et al. 2001). Importantly, it has been validated and shown to have high reliability among adult Indian men and women (Jacob et al. 1997,Kashyap and Singh 2017).

Table 8: Intervention Impact on Food Security and Expenditures

| | Food Insecurity | Ln Food Expense | Ln Total Expense |
|---|---|---|---|
| *A: Male vs Female* | | | |
| Treat Female | -0.846*** | -0.273*** | -0.082*** |
| | (0.061) | (0.041) | (0.028) |
| | [ 0.000] | [ 0.000] | [ 0.012] |
| Treat Male | -0.905*** | -0.266*** | -0.100*** |
| | (0.061) | (0.044) | (0.029) |
| | [ 0.000] | [ 0.000] | [ 0.006] |
| *B: Pooled Treatment* | | | |
| Treatment | -0.876*** | -0.270*** | -0.091*** |
| | (0.052) | (0.039) | (0.026) |
| | [ 0.000] | [ 0.000] | [ 0.002] |
| Baseline Outcomes | ✓ | ✓ | ✓ |
| Covariates | ✓ | ✓ | ✓ |
| Female = Male (pval) | 0.359 | 0.838 | 0.474 |
| Control Mean (EL) | 0.404 | 8.108 | 8.732 |
| Observations | 993 | 993 | 989 |

*Note*: Standard errors (in parentheses) are clustered at the slum level. Covariates include eligibility for the programs and variables for which there was an imbalance at baseline (i.e. whether the main source of earnings of the household head was from day labor, a business, a government job, or private sector job). Stars indicate statistical significance: *$*p<0.10$, $**\ p<0.05$, $***\ p<0.01$*. Westfall-Young stepdown adjusted p-values - which control the family-wise error rate for all tests in a panel - are shown in brackets.

For each question, responses ranged from 0 to 3 with higher values indicating worse mental health. We use these responses to construct a uni-dimensional measure of psychological well-being or distress which takes the sum of the 12 responses, re-coded such that the sum ranges from 12 to 48 and higher values indicate better mental health.[17][18] 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. At endline, control households scored on average 24.81 points on the index, and the intervention increases this score by 2.9 - 3.5 points, indicating a small improvement in

[17] 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).

[18] Reassuringly, we obtain very similar effects when we use an index equal to the first principal component of the responses to the 12 mental health questions, with higher values associated with worse mental health.

mental health. Interestingly, the improvement is larger, and statistically significantly so, among households where the man rather than 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 in both treatment arms. These increases are not significantly different between the two arms.

Table 9: Intervention impact on Mental Health and Life Satisfaction

| | Mental Health (Score) | Life Satisfaction | Financial Satisfaction |
|---|---|---|---|
| *A: Male vs Female* | | | |
| Treat Female | 2.880*** | 0.443*** | 0.837*** |
| | (0.332) | (0.116) | (0.109) |
| | [ 0.000] | [ 0.005] | [ 0.000] |
| Treat Male | 3.535*** | 0.359*** | 0.779*** |
| | (0.342) | (0.123) | (0.129) |
| | [ 0.000] | [ 0.011] | [ 0.000] |
| *B: Pooled Treatment* | | | |
| Treatment | 3.210*** | 0.400*** | 0.808*** |
| | (0.304) | (0.108) | (0.103) |
| | [ 0.000] | [ 0.006] | [ 0.000] |
| Baseline Outcomes | ✓ | ✓ | ✓ |
| Covariates | ✓ | ✓ | ✓ |
| Female = Male (pval) | 0.037 | 0.405 | 0.635 |
| Control Mean (EL) | 24.808 | 7.243 | 5.508 |
| Observations | 993 | 993 | 993 |

*Note*: Standard errors (in parentheses) are clustered at the slum level. Covariates include eligibility for the programs and variables for which there was an imbalance at baseline (i.e. whether the main source of earnings of the household head was from day labor, a business, a government job, or private sector job). Stars indicate statistical significance: **p <0.10, ** p <0.05, *** p <0.01*. Westfall-Young stepdown adjusted p-values - which control the family-wise error rate for all tests in a panel - are shown in brackets.

Thus, our intervention not only corrected beliefs and improved access to emergency aid, it also improved household wellbeing by reducing food insecurity and non-durable expenditures, including food expenditures, and improving mental health and life and financial satisfaction. Next, we investigate the mechanisms through which our intervention closed the gap between benefit receipt and entitlements.

## 6.2 Spillovers: Impacts on untreated households in treated slums

In this section, we present the findings on spillovers of the treatments from treated to untreated households within the treated slums. Characterizing the nature and size of spillovers is important for the design of effective policies: positive spillovers can generate social multiplier effects, meaning 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. It is also essential to know the nature and size of spillovers for designing randomized experiments providing information cost-effectively. The existence of cross-household within-slum spillovers would violate the Stable Unit Treatment Value Assumption (SUTVA) in a research design with household-level randomization, thereby motivating designs with slum-level randomization. However, these require larger sample-sizes of slums (and households) to achieve sufficient power, increasing the costs of the experiment significantly.

Tables E5 to E9 in Appendix E displays the findings for the untreated households in treated slums. Our results show that, aside from some spillovers in knowledge of entitlements in the male treatment arm and in the gap between entitlements and beliefs in the female treatment arm, the treatments did not generate significant spillover effects on most outcomes of untreated households within the treated slums.[19] This suggests that spillovers arising from information sharing may be small in the short-run (1-2 months) in urban settings where networks are sparser (Banerjee et al. (2021)).[20] The personalized nature of the information may also have limited these spillovers.

[19] 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.

[20] The information intervention increased the likelihood that treated households talked to other households about government support during the covid crisis (99% of treated households in treatment slums reported talking to those outside their household, compared to 84% of control households), and the number of people they talked to (2.76 vs 3.1). However, this information did not spillover to the untreated households in our sample, likely due to sparser urban networks.

## 6.3 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 (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 Guirkinger 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 6.1.1 and 6.1.2, 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 6.1.4. 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), shown in Section 6.1.5. 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.

### 6.4 Experimenter Demand Effects

A concern is that our findings may be a driven by experimenter demand effects. We consider this unlikely for several reasons. First, we detect impacts on downstream outcomes such as wellbeing and household expenditures which are in line with the increase in access to benefits that we find. If respondents were reporting better knowledge and higher access to please the research team, we wouldn't expect this to be accompanied by changes in measures of wellbeing such as food security, household expenditures, and mental health. Importantly, our surveys collected these measures *prior* to asking respondents to provide information on the emergency support they had received, thereby minimising priming effects from asking about aid received. Second, were our findings driven by treated households reporting higher access to government benefits, we should also expect to find intervention impacts on benefits received among households that were receiving their full entitlement at baseline. However, we do not find such effects. Similarly, we do not find any impacts of the intervention on aid received from PMUY, which was functioning well and delivering the entitled aid at baseline.

## 7 Program Costs

We calculate the costs of the intervention, which includes (i) the costs of the enumerators who conducted the baseline phone survey which collected the information used to personalize the intervention messages and delivered the personalized information, (ii) the costs of sending the SMS messages and making the calls to deliver the personalized information, (iii) costs of programming the baseline survey questionnaire and related software and database management, and (iv) overall survey and implementation supervision costs. Our calculations indicate that the cost of providing personalized information on up to 3 government benefit programs was approximately 7.68 USD (576

INR) per directly treated household.

This cost could be further reduced by (i) only collecting the information required to personalize the information with a significantly shorter baseline survey,[21] or (ii) dropping the baseline survey if information on eligibility and entitlements is already available (e.g., if the government is the agency sending the information) or collecting the required information as part of the call to deliver the personalized information. With a shorter baseline survey, we estimate a per-household intervention cost of approximately 4.17 USD (313 INR). Dropping the baseline survey and allowing for a longer intervention call reduces costs further to an estimated USD 2 - 3 per household.

We can obtain a lower bound estimate on the benefits of the intervention from the reduction in out-of-pocket food expenditures and the increased cash transfer received from the Jan Dhan program.[22] Our information reduced out-of-pocket food expenditures by INR 786 (for households with average ln food expenditures in the control group), and increased cash transfer receipt by INR 125, yielding total additional aid of INR 912 per month. A back-of-the-envelope calculation indicates a lower bound economic return of 58.3% per month. Thus, this simple intervention yielded a high return.

# 8 Conclusion

We find that our tailored information intervention had large effects on the directly treated slum households in our study. First, it affected participants' knowledge about benefits from two programs, correcting their beliefs and reducing the gap between perceived and actual entitlements. Second, the intervention increased the quantities of program benefits received from the food aid and cash transfer programs, reducing the gap between household receipts and household entitlements. This, in turn, lowered food insecurity and food expenditures and improved respondents' mental wellbeing and

[21] Our baseline survey included several questions that were relevant for research purposes, but would not be required for intervention design purposes.

[22] We take the reduction in food expenditures as the value of the additional food aid received by treated households, ignoring any income and substitution effects from the reduction in the effective price paid for food with the additional receipt of free food grains. This approach also ignores the benefits from improved mental health and reduced bribe payments, but allows us to estimate a lower-bound economic return over the intervention costs.

life satisfaction. Thus, the intervention improved household welfare in a crisis setting with significant (perceived) corruption.

Importantly, we show that the intervention achieved these impacts by empowering beneficiaries. The intervention reduces the gap between benefit receipt and entitlement among those who were not receiving their full entitlement at baseline, with no additional impacts among those who, in addition, had incorrect beliefs about their entitlements. We also show that there were no intervention impacts on benefits receipt from the PMUY program, which was better functioning and significantly less vulnerable to leakage than the other two programs. Consequently, the information improves respondents' perceptions of program fairness, perceptions of corruption and reduces the likelihood of paying bribes to obtain government benefits.

In contrast with a growing literature documenting that intervention impacts depend on the gender of the recipient of information, we find almost no differences in impacts between households where a woman was given the information relative to households where a man was informed. An exception is in mental wellbeing, where female survey respondents in households where men were informed experienced a larger improvement in their mental wellbeing relative to those in households where women were informed. This suggests the presence of a cost to acting on the information for female recipients which affects their mental health. Our findings also indicate no significant information spillovers on the untreated slum households in treated slums, suggesting that information spillovers are small in the short-run in urban settings where networks may be sparser.

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 had very significant positive benefits on directly treated households, suggesting that similarly designed interventions could provide a low cost but highly effective way of increasing access to government benefits among the urban poor in times of crises.

This intervention holds great promise in other low-and-middle-income countries, given widespread adoption of mobile phones. The COVID crisis spurred the rapid development and expansion of social protection programs in LMICs (Gentilini et al., 2022), which are much-needed amidst a rising incidence of emergencies such as natural disasters and pandemics (Bank, 2013). Ensuring that these programs reach the intended beneficiaries in settings like ours – with high leakage and low government capacity – is critical for their efficient functioning. Our study shows that providing personalized information via mobile phones can be effective in a sample where a significant minority – 45% – had not completed secondary school. Our intervention can be easily adapted and scaled to be implemented in these settings, with lower costs than effective interventions involving in-person visits (as in Berg et al., 2019) and significantly higher effectiveness than cheaper and less-intensive interventions involving the use of SMS messages (e.g. Blanco and Vargas, 2014) or chat bots (Ajzenman et al., 2023).

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# APPENDIX

## A Theoretical Proofs and Additional Results

### A.1 Theoretical Proofs

**Proof of Proposition 2:**

*Proof.* First, we establish that there cannot be a separating equilibrium in which the beneficiary always accepts the offer without making complaint. We provide a proof by contradiction. Suppose there is such an equilibrium, i.e. the intermediary's offer varies according to whether the entitlement is $b_h$ or $b_l$ and, in both instances, the beneficiary accepts the offer. In a separating equilibrium, the beneficiary can correctly infers the entitlement from the offer. Then, among strategies that do not trigger a complaint, it is optimal for the intermediary to offer $b_o = \overline{b} - \frac{1}{\pi}C$ (this is the smallest offer that makes the beneficiary indifferent between accepting the offer and making a complaint).[2] However, given the beneficiary's beliefs, the intermediary can choose $b_o = b_l - \frac{1}{\pi}C$ instead of $b_o = b_h - \frac{1}{\pi}C$ when $\overline{b} = b_h$ and, thus, improve her/his payoff. Therefore, the proposed strategies and beliefs do not constitute an equilibrium.

Similarly, we establish that there cannot be a separating equilibrium in which the beneficiary always makes a complaint. Again, we provide a proof by contradiction. Suppose

[2] The expected payoff to the beneficiary from a complaint equals $\overline{b} - C + \pi\left(\overline{b} - b_o\right)$. And the expected payoff from not making a complaint equals $\overline{b} - \frac{1}{\pi}C$. It is straightforward to verify that the two choices yield exactly the payoff. To see this, note that $b_o - C + \pi\left(\overline{b} - b_o\right)$

$$
\begin{aligned}
&= b_o(1-\pi) - C + \pi\overline{b} \\
&= \left(\overline{b} - \frac{1}{\pi}C\right)(1-\pi) - C + \pi\overline{b} \\
&= \overline{b}(1-\pi) + \pi\overline{b} - \left(\frac{1-\pi}{\pi}\right)C - C \\
&= \overline{b} - \left(1 + \frac{1-\pi}{\pi}\right)C \\
&= \overline{b} - \left(\frac{\pi + 1 - \pi}{\pi}\right)C \\
&= \overline{b} - \frac{1}{\pi}C
\end{aligned}
$$

there is such an equilibrium, i.e. the intermediary's offer varies according to whether the entitlement is $b_h$ or $b_l$ and, in both instances, the beneficiary rejects the offer. Among offers that are rejected, the intermediary's optimal strategy is to offer $b_o = 0$ regardless of the entitlement, i.e. the offer is the same regardless of the entitlement. Therefore, the proposed equilibrium is not a separating equilibrium.

Next we consider possible separating equilibria in which the intermediary's offer is accepted when the entitlement is $b_h$ but rejected when the entitlement is $b_l$. We prove by contradiction that there cannot be such a separating equilibrium. If there is then, by our reasoning above, it must be that $b_o(b_l) = 0$ and $b_o(b_h) = b_h - \frac{1}{\pi}C$. If this strategy is optimal for the intermediary, it must be that it yields a higher payoff than offering $b_l - \frac{1}{\pi}C$ when $\overline{b} = b_l$. Choosing $b_o(b_l) = 0$ leads to an expected payoff of $b_l(1-\pi) - P$ for the intermediary while choosing $b_o(b_l) = b_l - \frac{1}{\pi}C$ leads to an expected payoff of $\frac{1}{\pi}C$. Therefore, the proposed separating equilibrium requires (ignoring 'knife-edge' cases)

$$b_l(1-\pi) - P > \frac{1}{\pi}C$$

$$\implies b_h(1-\pi) - P > \frac{1}{\pi}C \tag{8}$$

The expression on the left-hand side of (8) corresponds to the intermediary's expected payoff from offering 0 when $\overline{b} = b_h$ while the expression on the right-hand side corresponds to the expected payoff from offering $b_h - \frac{1}{\pi}C$ when $\overline{b} = b_h$. As the expression on the left-hand side is greater, an offer of $b_h - \frac{1}{\pi}C$ when $\overline{b} = b_h$ cannot be optimal. This contradicts the conditions required for an equilibrium.

The only remaining candidate for a separating equilibrium is one in which the intermediary's offer is rejected when the entitlement is $b_h$ but accepted when the entitlement is $b_l$. If such an equilibrium exists, we must have $b_o(b_h) = 0$ and $b_o(b_l) = b_l - \frac{1}{\pi}C$. As per our reasoning above, the following conditions are necessary for this strategy to occur in equilibrium (again ignoring 'knife-edge' cases):

$$b_l(1-\pi) < \frac{1}{\pi}C + P \tag{9}$$

$$b_h(1-\pi) > \frac{1}{\pi}C + P \tag{10}$$

If the conditions in (9) and (10) hold, then the proposed strategy is optimal for the intermediary; the beneficiary correctly infers the entitlement from the offer and accepts it when $b_o = b_l - \frac{1}{\pi}C$ but rejects it when $b_o = 0$. $\square$

## A.2 Changes in equilibria type owing to the intervention

Consider a beneficiary who receives less than her full entitlement in the pre-intervention period. What if we were to drop the assumption that the condition in (4) holds in both periods (i.e., before and after the intervention)? Specifically, suppose it does for the pre-intervention period and it does *not* for the post-intervention period. From the figure, it implies a move from the pooling equilibrium described above to the separating equilibrium outlined in Proposition 2. Moreover, there could even be a shift to the other pooling equilibrium as per Proposition 1 if the drop in $C$ is sufficiently substantial. What would this imply for actual receipts?

A move to the separating equilibrium would mean that the intermediary offers 0 when the entitlement is high, and $(b_l - \frac{C_1}{\pi})$ when the entitlement is low. In the former case, the beneficiary complains and receives her full entitlement with probability $\pi$; in the latter case, she accepts the intermediary's offer. By comparison, the pre-intervention receipt equals $(\mathbf{E}_0\bar{b} - \frac{C_0}{\pi})$. Thus, receipts can increase or decrease, in the cases of both high and low entitlement, following an intervention, depending on where $\mathbf{E}_0\bar{b}$ stands in relation to $b_l$ and $b_h$, and where $C_1$ stands in relation to $C_0$.

A move to the *other* pooling equilibrium would imply a receipt of 0 and hence a complaint with certainty and receipt of the full entitlement with probability $\pi$. How does this compare with the pre-intervention receipt of $(\mathbf{E}_0\bar{b} - \frac{C_0}{\pi})$? Once again, there is no unambiguous answer; it depends upon where $\bar{b}$ stands in relation to $(\mathbf{E}_0\bar{b} - \frac{C_0}{\pi})$. The following proposition summarises these arguments.

**Proposition 5.** *Suppose the beneficiary was initially receiving less than her full entitlement, and that the shift in beliefs or costs are sufficiently large that the condition in (4) does not hold post-intervention. Then, the net effect of this intervention on the beneficiary's receipt is ambiguous.*

Now we return to the case where the beneficiary was initially receiving her full entitlement. As discussed earlier, if a beneficiary initially overestimated the benefits, then the information intervention will drive this person's beliefs downwards. This, in turn, implies that the intervention's effect can lead to a violation of the condition in (5). If the condition is indeed violated, then different possibilities exist. Specifically, the changes in $C$ and $\mathbf{E}^i\left(\bar{b}\right)$ may be such that the conditions in (6) and (7) are met. This implies that the intervention delivers the separating equilibrium described in Proposition 2. Alternatively, the changes in $C$ and $\mathbf{E}^i\left(\bar{b}\right)$ may be such that the condition in (4) is now met post-intervention, If so, then we are in the other pooling equilibrium where the transfer to the beneficiary is positive and there is no complaint.

The effect on the actual receipt is ambiguous. In the pre-intervention period, the beneficiary received the full amount – which is either $b_l$ or $b_h$ – after complaining. In the post-intervention period, if the beneficiary is in the "no complaints" pooling equilibrium, she is offered $\bar{b}-\frac{C_1}{\pi}$ and ceases to complain. Thus, benefits go down. If the beneficiary is in the separating equilibrium and the entitlement is high, the intermediary offers 0 and the beneficiary continues to complain, as in the initial equilibrium; if the entitlement is low, the beneficiary is offered $b_l-\frac{C_1}{\pi}$ and ceases to complaint. Once again, benefits go down. The following proposition summarises this result.

**Proposition 6.** *Suppose the beneficiary was initially receiving her full entitlement while overestimating her entitlement, and that the condition in (5) does not hold post-intervention. Then, the intervention may have no effect on benefits or cause the beneficiary's receipt to go down.*

# B Experiment Study Area

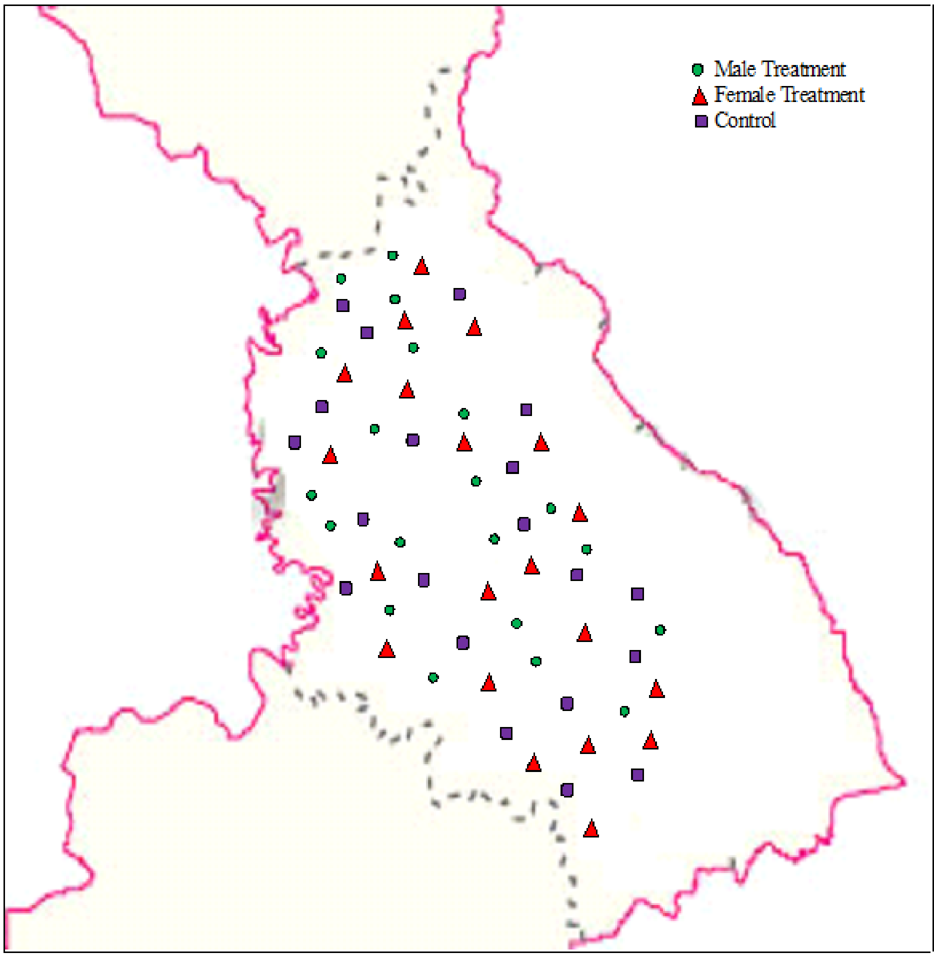
# C Text of Intervention

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*

Figure B1: Map of Study Area




Notes: Figure shows map of study slums in Kanpur by treatment status.

*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 programs 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."*

# D Sample Balance Tables

Table D1: Balance tests: Individual and household characteristics

| Variable | (1) Control Mean/SE | (2) Treatment Female Mean/SE | (3) Treatment Male Mean/SE | (1)-(2) | Normalized difference (1)-(3) | (2)-(3) |
|---|---|---|---|---|---|---|
| Respondent age | 30.270<br>(0.319) | 30.122<br>(0.358) | 30.713<br>(0.483) | 0.021 | -0.060 | -0.079 |
| Respondent is married | 0.760<br>(0.022) | 0.769<br>(0.028) | 0.776<br>(0.023) | -0.022 | -0.039 | -0.018 |
| Respondent education: none | 0.141<br>(0.021) | 0.149<br>(0.022) | 0.127<br>(0.021) | -0.022 | 0.042 | 0.064 |
| Respondent education: 1-9 years | 0.309<br>(0.032) | 0.289<br>(0.030) | 0.350<br>(0.031) | 0.045 | -0.087 | -0.132 |
| Respondent education: >10 years | 0.550<br>(0.031) | 0.562<br>(0.032) | 0.523<br>(0.036) | -0.026 | 0.054 | 0.080 |
| Household religion: Hindu | 0.835<br>(0.060) | 0.784<br>(0.077) | 0.834<br>(0.052) | 0.129 | 0.003 | -0.126 |
| Household religion: Muslim | 0.165<br>(0.060) | 0.216<br>(0.077) | 0.166<br>(0.052) | -0.129 | -0.003 | 0.126 |
| General caste | 0.225<br>(0.029) | 0.234<br>(0.050) | 0.236<br>(0.033) | -0.021 | -0.025 | -0.004 |
| Other Backward Classes | 0.327<br>(0.047) | 0.353<br>(0.054) | 0.335<br>(0.038) | -0.053 | -0.017 | 0.036 |
| Scheduled Caste/Scheduled Tribe | 0.447<br>(0.052) | 0.413<br>(0.059) | 0.429<br>(0.051) | 0.069 | 0.037 | -0.032 |
| Household size | 5.294<br>(0.094) | 5.325<br>(0.085) | 5.275<br>(0.087) | -0.020 | 0.012 | 0.032 |
| Household income (INR) | 9526.447<br>(304.318) | 10093.040<br>(450.712) | 10090.030<br>(527.507) | -0.093 | -0.088 | 0.000 |
| HH expenditure last month (INR) | 5407.508<br>(180.704) | 5152.584<br>(206.976) | 5376.737<br>(224.710) | 0.099 | 0.011 | -0.081 |
| Main household earnings from agriculture | 0.066<br>(0.011) | 0.070<br>(0.020) | 0.057<br>(0.012) | -0.015 | 0.036 | 0.051 |
| Main household earnings from day labor | 0.489<br>(0.024) | 0.471<br>(0.036) | 0.502<br>(0.030) | 0.037 | -0.024 | -0.061 |
| Main household earnings from business | 0.291<br>(0.028) | 0.240<br>(0.026) | 0.215<br>(0.021) | 0.116 | 0.177 | 0.061 |
| Main household earnings from government job | 0.009<br>(0.005) | 0.015<br>(0.006) | 0.009<br>(0.005) | -0.057 | -0.001 | 0.056 |
| Main household earnings from private job | 0.144<br>(0.020) | 0.204<br>(0.019) | 0.218<br>(0.019) | -0.157 | -0.191 | -0.034 |
| Household eligible for PMGKY | 0.907<br>(0.017) | 0.918<br>(0.016) | 0.924<br>(0.015) | -0.039 | -0.063 | -0.024 |
| Household eligible for PMUY | 0.234<br>(0.024) | 0.231<br>(0.022) | 0.251<br>(0.018) | 0.008 | -0.039 | -0.046 |
| Household eligible for Jan Dhan | 0.532<br>(0.028) | 0.587<br>(0.026) | 0.562<br>(0.017) | -0.111 | -0.061 | 0.050 |
| N | 333 | 329 | 331 | | | |
| Clusters | 20 | 20 | 20 | | | |
| F-test of joint significance (F-stat) | | | | 1.153 | 1.082 | 0.487 |
| F-test, number of observations | | | | 662 | 664 | 660 |

*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 D2: Sample comparison

| | India (Urban) Mean | Uttar Pradesh (Urban) Mean | Experimental Mean | Sample Std. Dev. |
|---|---|---|---|---|
| Age | 30.652 | 29.188 | 30.369 | 7.371 |
| Household Size | 5.162 | 5.989 | 5.298 | 1.582 |
| Education $< 10$ years | 0.561 | 0.605 | 0.455 | 0.498 |
| Education $10-12$ years | 0.286 | 0.243 | 0.208 | 0.406 |
| Education $> 12$ years | 0.153 | 0.162 | 0.336 | 0.473 |
| Advantaged Caste | 0.258 | 0.219 | 0.232 | 0.422 |
| Disadvantaged Caste | 0.742 | 0.781 | 0.768 | 0.422 |
| Hindu | 0.704 | 0.678 | 0.818 | 0.386 |
| Muslim | 0.189 | 0.320 | 0.182 | 0.386 |

*Note*: The table compares the characteristics of the experimental sample (mean and standard deviation in columns 3 and 4) with the average characteristics of the bottom 60% (in terms of wealth) of the urban population for India as a whole (column 1) and Uttar Pradesh (column 2) from the NFHS-5 survey collected from 2019-21.

Table D3: Balance tests: Baseline knowledge

| Variable | (1) Control Mean/SE | (2) Treatment Female Mean/SE | (3) Treatment Male Mean/SE | Normalized difference (1)-(2) | (1)-(3) | (2)-(3) |
|---|---|---|---|---|---|---|
| Mentioned PMGKY | 0.916 | 0.921 | 0.927 | -0.018 | -0.043 | -0.025 |
| | (0.017) | (0.014) | (0.014) | | | |
| Mentioned PMUY | 0.246 | 0.225 | 0.245 | 0.050 | 0.004 | -0.047 |
| | (0.023) | (0.021) | (0.018) | | | |
| Mentioned Jandhan | 0.502 | 0.581 | 0.559 | -0.158 | -0.115 | 0.044 |
| | (0.025) | (0.027) | (0.018) | | | |
| Entitlement belief wheat PMGKY (per person) | 5.167 | 5.256 | 5.249 | -0.045 | -0.043 | 0.004 |
| | (0.129) | (0.099) | (0.096) | | | |
| Entitlement belief PMUY (per person) | 0.287 | 0.253 | 0.302 | 0.065 | -0.026 | -0.089 |
| | (0.027) | (0.024) | (0.028) | | | |
| Entitlement belief Jandhan (per person) | 716.667 | 742.073 | 806.402 | -0.027 | -0.091 | -0.068 |
| | (68.375) | (55.728) | (49.775) | | | |
| N | 333 | 329 | 331 | | | |
| Clusters | 20 | 20 | 20 | | | |
| F-test of joint significance (F-stat) | | | | 1.929 | 0.732 | 1.630 |
| F-test, number of observations | | | | 662 | 664 | 660 |

*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 D4: Balance tests: Baseline aid receipt

| Variable | (1) Control Mean/SE | (2) Treatment Female Mean/SE | (3) Treatment Male Mean/SE | Normalized difference (1)-(2) | (1)-(3) | (2)-(3) |
|---|---|---|---|---|---|---|
| Household received any aid: PMGKY | 0.910 | 0.915 | 0.915 | -0.018 | -0.019 | -0.002 |
| | (0.017) | (0.016) | (0.015) | | | |
| Household received any aid: PMUY | 0.243 | 0.225 | 0.239 | 0.043 | 0.011 | -0.033 |
| | (0.023) | (0.021) | (0.018) | | | |
| Household received any aid: Jan Dhan | 0.495 | 0.581 | 0.559 | -0.170 | -0.127 | 0.044 |
| | (0.025) | (0.027) | (0.018) | | | |
| Amount household received: PMGKY | 17.299 | 16.537 | 17.728 | 0.082 | -0.047 | -0.131 |
| | (0.615) | (0.613) | (0.584) | | | |
| Amount household received: PMUY | 0.236 | 0.232 | 0.251 | 0.009 | -0.033 | -0.039 |
| | (0.023) | (0.027) | (0.024) | | | |
| Amount household received: Jan Dhan | 270.288 | 299.495 | 320.369 | -0.100 | -0.141 | -0.060 |
| | (16.427) | (15.751) | (19.878) | | | |
| Gap: Receipt - Entitlement, PMGKY | -8.698 | -9.058 | -7.936 | 0.040 | -0.085 | -0.129 |
| | (0.480) | (0.378) | (0.469) | | | |
| Gap: Receipt - Entitlement, PMUY | 0.000 | 0.009 | 0.006 | -0.047 | -0.039 | 0.014 |
| | (0.006) | (0.019) | (0.007) | | | |
| Gap: Receipt - Entitlement, Jan Dhan | -234.216 | -281.052 | -292.924 | 0.112 | 0.120 | 0.023 |
| | (21.073) | (20.676) | (20.866) | | | |
| N | 333 | 329 | 331 | | | |
| Clusters | 20 | 20 | 20 | | | |
| F-test of joint significance (F-stat) | | | | 1.908* | 3.339*** | 4.275*** |
| F-test, number of observations | | | | 662 | 664 | 660 |

*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.

# E Tables: Impacts on untreated households in treated slums

The following tables replicate the main results of the paper found in Tables 2 to 9, while also displaying spillover effects (i.e. the effect on households located in treated slums who did not personally receive the information treatment).

Table E5: Intervention Impact on Knowledge Outcomes (full results)

| | Mentioned | | HH Entitlement Belief | | Belief-Entitlement | |
|---|---|---|---|---|---|---|
| | PMGKY | Jan Dhan | PMGKY | Jan Dhan | PMGKY | Jan Dhan |
| *Panel A: Male vs Female* | | | | | | |
| Treat Female | 0.002 | 0.001 | -3.531*** | -1402.872*** | -2.345*** | -1151.948*** |
| | (0.006) | (0.014) | (0.885) | (135.266) | (0.608) | (117.989) |
| Treat Male | -0.022** | -0.013 | -3.411*** | -1372.505*** | -3.085*** | -1170.955*** |
| | (0.008) | (0.012) | (0.844) | (124.591) | (0.493) | (117.081) |
| Treat Female (Spillover) | -0.010 | 0.012 | 1.131 | -117.633 | -0.077 | -83.201 |
| | (0.010) | (0.017) | (1.102) | (228.756) | (0.772) | (202.446) |
| Treat Male (Spillover) | -0.014 | 0.005 | -1.715 | 593.984* | -1.028* | 162.490 |
| | (0.012) | (0.013) | (1.205) | (313.562) | (0.598) | (164.189) |
| p-val (F = M) | 0.027 | 0.327 | 0.893 | 0.741 | 0.231 | 0.791 |
| p-val Spillover (F = M) | 0.830 | 0.716 | 0.045 | 0.046 | 0.281 | 0.253 |
| *Panel B: Pooled Treatment* | | | | | | |
| Treat | -0.010* | -0.006 | -3.466*** | -1388.342*** | -2.716*** | -1161.763*** |
| | (0.006) | (0.011) | (0.739) | (121.469) | (0.464) | (111.651) |
| Treat (Spillover) | -0.012 | 0.009 | -0.275 | 234.109 | -0.548 | 38.261 |
| | (0.008) | (0.012) | (0.941) | (217.949) | (0.541) | (151.671) |
| Baseline Outcomes | Yes | Yes | Yes | Yes | Yes | Yes |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes |
| Control Mean (EL) | 0.916 | 0.495 | 26.814 | 1752.252 | 2.105 | 1145.646 |
| Observations | 993 | 993 | 987 | 989 | 987 | 993 |

*Note*: Standard errors (in parentheses) are clustered at the slum level. Covariates include eligibility for the programs, variables for which there was an imbalance at baseline (whether the main source of earnings for the household was from day labor, a business, a government job, or private sector job). Stars indicate statistical significance: **p <0.10, ** p <0.05, *** p <0.01.* Westfall-Young stepdown adjusted p-values - which control the family-wise error rate for all tests in a panel - are shown in brackets.

Table E6: Intervention Impact on Program Receipt (full results)

| | Any | | HH amount | |
|---|---|---|---|---|
| | PMGKY | Jan Dhan | PMGKY | Jan Dhan |
| *Panel A: Male vs Female* | | | | |
| Treat Female | 0.013 | 0.002 | 5.593*** | 120.219*** |
| | (0.008) | (0.014) | (0.492) | (20.425) |
| Treat Male | -0.005 | -0.011 | 4.933*** | 131.097*** |
| | (0.011) | (0.012) | (0.481) | (24.037) |
| Treat Female (Spillover) | -0.004 | 0.016 | -0.472 | 19.871 |
| | (0.010) | (0.017) | (0.329) | (17.605) |
| Treat Male (Spillover) | -0.011 | 0.005 | -0.666 | -32.997 |
| | (0.013) | (0.014) | (0.451) | (20.956) |
| p-value (F = M) | 0.123 | 0.401 | 0.296 | 0.697 |
| p-value Spillover (F = M) | 0.665 | 0.593 | 0.691 | 0.027 |
| *Panel B: Pooled Treatment* | | | | |
| Treat | 0.004 | -0.005 | 5.260*** | 125.730*** |
| | (0.008) | (0.011) | (0.375) | (17.393) |
| Treat (Spillover) | -0.008 | 0.010 | -0.570* | -6.211 |
| | (0.009) | (0.012) | (0.312) | (16.115) |
| Baseline Outcomes | Yes | Yes | Yes | Yes |
| Covariates | Yes | Yes | Yes | Yes |
| Control Mean (EL) | 0.904 | 0.492 | 18.384 | 259.763 |
| Observations | 993 | 993 | 983 | 993 |

*Note*: Standard errors (in parenthesis) are clustered at the slum level. Covariates include eligibility for the programs and variables for which there was an imbalance at baseline (i.e. whether the main source of earnings of the household head was from day labor, a business, a government job, or private sector job). Stars indicate statistical significance: **p <0.10, ** p <0.05, *** p <0.01*. Westfall-Young stepdown adjusted p-values - which control the family-wise error rate for all tests in a panel - are shown in brackets.

Table E7: Intervention Impact on Program Receipt Gaps (full results)

| | Gap Receipt-Entitled | | Gap Receipt-Belief | |
|---|---|---|---|---|
| | PMGKY | Jan Dhan | PMGKY | Jan Dhan |
| *Panel A: Male vs Female* | | | | |
| Treat Female | 6.221*** | 148.810*** | 8.384*** | 1630.934*** |
| | (0.529) | (23.436) | (0.697) | (146.509) |
| Treat Male | 5.724*** | 106.884*** | 8.375*** | 1510.896*** |
| | (0.535) | (23.516) | (0.676) | (138.568) |
| Treat Female (Spillover) | -0.207 | 13.264 | -2.902** | 237.710 |
| | (0.375) | (18.811) | (1.290) | (275.199) |
| Treat Male (Spillover) | 0.123 | -26.687 | 0.663 | -720.784* |
| | (0.390) | (22.557) | (1.259) | (393.764) |
| p-value (F = M) | 0.471 | 0.156 | 0.970 | 0.046 |
| p-value Spillover (F = M) | 0.487 | 0.093 | 0.026 | 0.033 |
| *Panel B: Pooled Treatment* | | | | |
| Treat | 5.970*** | 127.758*** | 8.376*** | 1571.842*** |
| | (0.408) | (18.767) | (0.676) | (139.325) |
| Treat (Spillover) | -0.045 | -6.487 | -1.130 | -235.959 |
| | (0.301) | (17.492) | (1.032) | (268.744) |
| Baseline Outcomes | Yes | Yes | Yes | Yes |
| Covariates | Yes | Yes | Yes | Yes |
| Control Mean (EL) | -8.417 | -240.237 | -8.429 | -1492.489 |
| Observations | 983 | 993 | 990 | 993 |

*Note*: Standard errors (in parenthesis) are clustered at the slum level. Covariates include eligibility for the programs and variables for which there was an imbalance at baseline (i.e. whether the main source of earnings of the household head was from day labor, a business, a government job, or private sector job). Stars indicate statistical significance: **p <0.10, ** p <0.05, *** p <0.01.* Westfall-Young stepdown adjusted p-values - which control the family-wise error rate for all tests in a panel - are shown in brackets.

Table E8: Intervention Impact on Food Security and Expenditures (full results)

| | Food insecurity | ln food expense | ln total expense |
|---|---|---|---|
| *Panel A: Male vs Female* | | | |
| Treatment Female | -0.846*** | -0.273*** | -0.082*** |
| | (0.061) | (0.041) | (0.028) |
| Treatment Male | -0.905*** | -0.266*** | -0.100*** |
| | (0.061) | (0.044) | (0.029) |
| Treatment Female - Spillover | -0.017 | -0.075 | -0.055 |
| | (0.088) | (0.048) | (0.038) |
| Treatment Male - Spillover | 0.024 | -0.019 | -0.027 |
| | (0.127) | (0.067) | (0.044) |
| p-value (Female = Male) | 0.359 | 0.838 | 0.474 |
| p-value Spillovers (Female=Male) | 0.778 | 0.403 | 0.571 |
| *Panel B: Pooled Treatment* | | | |
| Treatment | -0.876*** | -0.270*** | -0.091*** |
| | (0.052) | (0.039) | (0.026) |
| Treatment - Spillover | 0.003 | -0.047 | -0.041 |
| | (0.082) | (0.048) | (0.033) |
| Baseline Outcomes | Yes | Yes | Yes |
| Covariates | Yes | Yes | Yes |
| Control Mean (EL) | 0.404 | 8.108 | 8.732 |
| Observations | 993 | 993 | 989 |

*Note*: Standard errors (in parentheses) are clustered at the slum level. Covariates include eligibility for the programs and variables for which there was an imbalance at baseline (i.e. whether the main source of earnings of the household head was from day labor, a business, a government job, or private sector job). Stars indicate statistical significance: **p <0.10, ** p <0.05, *** p <0.01*. Westfall-Young stepdown adjusted p-values - which control the family-wise error rate for all tests in a panel - are shown in brackets.

Table E9: Intervention impact on Mental Health and Life Satisfaction (full results)

| | Mental health (Score) | Life satisfaction | Financial satisfaction |
|---|---|---|---|
| *Panel A: Male vs Female* | | | |
| Treatment Female | 2.880*** | 0.443*** | 0.837*** |
| | (0.332) | (0.116) | (0.109) |
| Treatment Male | 3.535*** | 0.359*** | 0.779*** |
| | (0.342) | (0.123) | (0.129) |
| Treatment Female - Spillover | -0.107 | -0.095 | -0.107 |
| | (0.483) | (0.141) | (0.152) |
| Treatment Male - Spillover | -0.157 | -0.056 | 0.085 |
| | (0.380) | (0.161) | (0.195) |
| p-value (Female = Male) | 0.037 | 0.405 | 0.635 |
| p-value Spillovers (Female=Male) | 0.920 | 0.801 | 0.379 |
| *Panel B: Pooled Treatment* | | | |
| Treatment | 3.210*** | 0.400*** | 0.808*** |
| | (0.304) | (0.108) | (0.103) |
| Treatment - Spillover | -0.131 | -0.076 | -0.012 |
| | (0.355) | (0.130) | (0.137) |
| Baseline Outcomes | Yes | Yes | Yes |
| Covariates | Yes | Yes | Yes |
| Control Mean (EL) | 24.808 | 7.243 | 5.508 |
| Observations | 993 | 993 | 993 |

*Note*: Standard errors (in parentheses) are clustered at the slum level. Covariates include eligibility for the programs and variables for which there was an imbalance at baseline (i.e. whether the main source of earnings of the household head was from day labor, a business, a government job, or private sector job). Stars indicate statistical significance: **p <0.10, ** p <0.05, *** p <0.01*. Westfall-Young stepdown adjusted p-values - which control the family-wise error rate for all tests in a panel - are shown in brackets.

# F Additional results

## F.1 Impacts on who contacted officials

In our intervention we provided information to a male or a female in the household, in what follows we will test whether this affects who in the household contacts the relevant officials for both programs. Results in Table F10, show that it did not matter who the information was provided to, but that providing information to the household increased the likelihood that males made contact in reference to both programs, while there was a decrease in the probability that females made contact. In a society with strong gender norms, men are going to be more likely to have the bargaining power to be able to claim their entitlements in a ration shop or to contact banking officials in relation to bank account. This is consistent with the empowerment effect in a male dominated society context.

Table F10: Mechanisms: Male and Female contacts

| | PGMKY fem contact | PGMKY male contact | Jan Dhan fem contact | Jan Dhan male contact |
|---|---|---|---|---|
| *Panel A: Male vs Female* | | | | |
| Treat Female | -0.128*** | 0.063* | -0.082*** | 0.020 |
| | (0.020) | (0.035) | (0.017) | (0.028) |
| Treat Male | -0.147*** | 0.103*** | -0.077*** | 0.056* |
| | (0.019) | (0.036) | (0.016) | (0.029) |
| Treat Female - Spillover | 0.013 | -0.017 | -0.005 | 0.047 |
| | (0.044) | (0.054) | (0.035) | (0.044) |
| Treat Male -Spillover | 0.034 | -0.049 | -0.030 | 0.090** |
| | (0.041) | (0.048) | (0.033) | (0.043) |
| p-value (Female = Male) | 0.126 | 0.244 | 0.641 | 0.209 |
| p-value Spillover (Female = Male) | 0.708 | 0.604 | 0.554 | 0.429 |
| *Panel B: Pooled Treatment* | | | | |
| Treat | -0.138*** | 0.084*** | -0.079*** | 0.038 |
| | (0.019) | (0.031) | (0.016) | (0.025) |
| Treat - Spillover | 0.023 | -0.033 | -0.017 | 0.069** |
| | (0.033) | (0.040) | (0.026) | (0.034) |
| Covariates | Yes | Yes | Yes | Yes |
| Control Mean (EL) | 0.160 | 0.699 | 0.084 | 0.348 |
| Observations | 987 | 987 | 990 | 990 |