Can information campaigns reduce last mile payment delays in public works programme?

Evidence from a field experiment in India

Upasak Das,[1] Amartya Paul,[2] and Mohit Sharma[3]

January 2021

United Nations University World Institute for Development Economics Research

wider.unu.edu

Abstract: Does information dissemination among beneficiaries of welfare programmes mitigate their implementation failures? We present experimental evidence in the context of a rural public works programme in India, where we assess the impact of an intervention that involves dissemination of publicly available micro-level data on last mile delays in payment and programme uptake, along with a set of intermediate outcomes. The findings point to a substantial reduction in last mile payment delays along with improvements in awareness of basic provisions of the programme and process mechanisms while indicating a limited effect on uptake. However, we find a considerable increase in uptake in the subsequent period, which is potentially indicative of an 'encouragement' effect through the reduction in last mile delays. A comparatively higher impact on payment delay was found for deprived communities. The findings lay a platform for an innovative information campaign that can be used by government and civil society organizations as part of transparency measures to improve efficiency.

Note: On the 1st of March 2021, the title of the working paper in this document was slightly amended due to an error that occurred at the time of publication.

---

[1] Global Development Institute, University of Manchester, and Centre for Social Norms and Behavioral Dynamics, University of Pennsylvania, corresponding author: upasak.das@manchester.ac.uk; [2] Centre for Development Studies, Trivandrum, Kerala, India; [3] Madras School of Economics, Chennai, India, and Collaborative Research and Dissemination (CORD), New Delhi, India

1   Introduction

The success of welfare interventions, including public works programmes, largely depends on how they are implemented at the local level. Multiple market failures leading to implementation shortfalls, transaction costs and elite capture are often cited as the reasons for their failure to produce the desired impact (Bardhan and Mookherjee 2000; Narayanan et al. 2017; Pritchett 2009; Skoufias 2005). A key reason for the prevalence of such failures is arguably the dearth of correct information among beneficiaries, which makes it difficult for them to hold the functionaries accountable (Drèze and Sen 2013). It is often argued that information plays an important role in better public service delivery, which otherwise suffers because of rent-seeking behaviour by the implementing authorities. This is largely due to multiple information asymmetries, which are often utilized by them for their own benefit, resulting in hefty welfare losses for the intended beneficiaries (Banerjee et al. 2018). Accordingly, the literature has emphasized the pivotal role of information in the efficient functioning of the markets and proper provisioning of public goods and services (Dal Bó and Finan 2020; Jensen 2007; Protik et al. 2018; Stigler 1961).

However, it is not exactly clear if providing information to the citizen acts as a magic bullet. It is often the case that citizens are not able to make use of the information to demand their entitlements. Further, even if the information is provided, the implementing authorities may not care about their demands without the right incentive mechanism or sanctions. Hence, gauging whether dissemination of information improves service delivery depends on the context, along with the way in which the information is disseminated. Previous studies have found mixed evidence on this subject. For example, Banerjee et al. (2018) found that dissemination of information increased receipts of benefits in a subsidized rice programme in Indonesia. However, Ravallion et al. (2013) found no such effects on similar outcomes related to a rural public works programme in India, apart from enhancing awareness.

This paper experimentally evaluates an intervention based on accessing information from a public website and disseminating the same to the beneficiaries of the Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS), which is a public works programme implemented in India since 2005. More specifically, the intervention harnesses public micro-level administrative records on the programme that are available online and disseminates personalized information to beneficiaries or groups of beneficiaries. The main component of the intervention is as follows: once the workers' wages have been credited to their bank or post office accounts and this information has been entered on the system, the names of the relevant workers are listed and the lists are posted at main points in the village. In addition, messages on various provisions of the programme are sent out through local meetings and mobile phone calls. This intervention was rolled out randomly in parts of the southern state of Telangana in India. We make use of this randomized design and examine the impact of the intervention in terms of two main outcomes related to the programme, namely, preventing delayed payments and increasing uptake in terms of days worked, in addition to associated intermediate outcomes. The design of the intervention and survey also allowed us to look at spillover effects from the intervention.

The findings reveal a substantial reduction in last mile payment delays owing to the wage credit list posting, but a limited impact in reducing payment delays that occur at higher levels. Interestingly, the gains are found to revert to the pre-intervention level within three months of the conclusion of the intervention. The average effect on uptake of the programme during the intervention is found to be insignificant. Nevertheless, we find a significant gain in average uptake in the period after the intervention, potentially because of the reduction in last mile payment delays, i.e. through a plausible 'encouragement effect'. In terms of intermediate outcomes, we observe a significantly

1

positive impact on awareness and process mechanism improvement. Further, we find modest spillover effects on these intermediate outcomes, while no impact is observed on uptake. Notably, a higher reduction in last mile delays for deprived communities is observed in comparison with others.

The paper contributes to five strands of the literature. First, it provides evidence that technology-based interventions can be effective in improving the efficacy of safety net programs. These work directly through the dissemination of information to beneficiaries as well as indirectly by encouraging them to hold the implementing authorities to account (Björkman and Svensson 2009; Nagavarapu and Sekhri 2016). With respect to this, and to the importance of improving last mile service delivery, our paper complements that by Muralidharan et al. (forthcoming), who find significant gains from the reduction of payment delays under a cash transfer programme implemented in Telangana in 2018.

Second, we contribute to the existing literature on the effectiveness of different types of information campaign (Alik-Lagrange and Ravallion 2019; Banerjee et al. 2018; Das 2016; Kaufmann et al. 2018). We find a limited effect of direct generalized awareness campaigns while revealing evidence of effectiveness in more personalized campaigns.

Third, the design of the survey and randomization allow us to gauge the impact of the intervention not only on the treated villages but also on the adjoining non-treated villages, thereby making it possible to measure the impact of spillovers of the treatment. Hence, the paper contributes to the set of literature that examines spillover effects of welfare interventions (Alik-Lagrange and Ravallion 2019; Chong et al. 2013; Miguel and Kremer 2004).

Fourth, it presents evidence of a potential 'encouragement effect' similar to the discouraged worker effect that has been pointed out in the literature (Benati 2001; Clark and Summers 1981; Narayanan et al. 2017). This emanates from the fact that we observe an increase in uptake in terms of days of work in the period following the intervention, which is potentially due to the reduction in last mile payment delays during the intervention period.

Finally, the study also contributes to the growing research on MGNREGS and related welfare programmes and shows ways in which their implementation and service delivery could be improved. On this note, the significance of the study lies in finding ways to increase accountability among local-level implementers. Thus, the intervention can be a useful alternative to the actions of civil society organizations (CSO) and other programme-implementing authorities in providing public service delivery.

The structure of the paper is as follows. Section 2 briefly describes the MGNREGS programme. Section 3 gives a description of the intervention design and the mechanisms through which such interventions can lead to the desired outcomes. Section 4 presents the study design along with a discussion of the data, the variables, and the process of randomization. Section 5 discusses the estimation strategy and Section 6 presents the main findings from the regressions and the analysis. Section 7 examines the intervention in terms of its cost-effectiveness and Section 8 concludes with a discussion of the potential takeaways and policy recommendations.

2   MGNREGS

MGNREGS was introduced on 23 August 2005 and initially implemented in 200 rural districts of India. Since 2008, it has been extended to all rural parts of the country. Under this programme, any adult from a rural household who is willing to do unskilled manual labour at the statutory minimum wage is entitled to be employed for at least 100 days a year on public works. Those willing to do this work must register for the programme, and after verification of their place of residence and age, the household is issued a job card, which is mandatory under the programme. An application must then be made, indicating the dates and duration of the work to be undertaken. If no work is provided within 15 days of the application, an unemployment allowance is paid; and if wages are not paid within 15 days of completion of the work, compensation for the delay must also be paid. Normally, the democratically elected village head and their office are responsible for implementing the programme at the Gram Panchayat (GP) level.[1] However, in the state of Telangana, responsibility lies with an employee of the state government called the Field Assistant (FA).

A number of studies have examined the welfare impacts of MGNREGS on indicators related to poverty, women's empowerment, nutrition, education, and reduction in distress-driven migration, among others (Afridi et al. 2017; Das 2015; Dasgupta et al. 2017; Deininger and Liu, 2013; Imbert and Papp 2015; Khera and Nayak 2009; Nair et al. 2013). Other studies have documented administrative problems—including high unmet demand and delayed payments—that have undermined the potential benefits of the programme (Dutta et al. 2012; Liu and Barrett 2013; Narayanan et al. 2017; Narayanan et al. 2019). Because dearth of information is a major reason for such failures, our intervention intends to enhance awareness, giving information about process failures and disseminating personalized information on wage credits that can enable the beneficiaries to hold the local authorities accountable. A detailed explanation of the intervention and the mechanisms through which the desired outcomes might be achieved is presented in the next section.

3   Intervention description and mechanisms

3.1   Intervention description

The intervention, developed by the LibTech team, which consists of researchers, social activists and engineers interested in improving public service delivery in India, was rolled out in randomly selected GPs of the Damaragidda and Maddur blocks in the Mahbubnagar district of Telangana under the name Upadhi Hami Phone Radio.[2] The intervention was carried out for 13 months, from November 2017 to November 2018. The different ingredients of the intervention are as follows. First, information about various rights and entitlements guaranteed under MGNREGS was disseminated through periodic voice broadcasts over mobile phones. These broadcasts included information on general processes that could help workers to access their entitlements.

---

[1] A GP is the basic unit of the three-tier structure of local self-government in the rural parts of India. A GP consists of a number of villages.

[2] Currently these blocks come under the Narayanpet district. More information on LibTech can be found on the website http://libtech.in/ (accessed 10 July 2020).

Local-level meetings were also arranged with the intervention team to discuss these provisions in detail.

Personalized wage credit information was posted at central points in the villages (GP headquarters or marketplace) and this information publicized via voice broadcasts over mobile phones. The objective of this important part of the intervention was to reduce delays in the disbursement of MGNREGS wages after they had been credited to workers' accounts. This last mile delay is due to the fact that workers are often not aware that their wages have been credited to their accounts and may therefore make multiple visits to banks or post offices to check whether their accounts have been credited, forgoing the wages they could have earned had they not made these visits. Moreover, officials often use this fact to their own advantage. For example, the Branch Post Master (BPM) may take the opportunity to collect the wages from the main post office and, instead of disbursing them to the beneficiaries, retain them for an extended period to meet personal needs. In this situation, timely dissemination of information as to when the wage is credited can enhance transparency and hence accountability among the BPMs, who will be obliged to pay the wages as soon as they are sent to the main post office. It can also enable workers to avoid making multiple trips to post offices or banks.

Notably, wage lists were also posted in localities where there are deprived communities— Scheduled Castes and Scheduled Tribes (SC/ST). This was to ensure inclusiveness, since SC/ST households, being socially ostracized, have historically been found to lag behind non-SC/ST households in terms of various indicators of welfare (Sundaram and Tendulkar 2003). We thus attempt to avoid the possibility of these communities not receiving the information because of their lower access to core junctions.

3.2 Mechanisms

As discussed, the intervention consists of four components: (a) radio broadcasts through phone calls; (b) phone messages; (c) local meetings; and (d) posting of personalized wage credit lists. The first three components are generalized items that convey the basic provisions of the programme and means of grievance redressal. In terms of their possible impact on the outcomes, these three components can increase awareness of entitlements from the programme through individual- and community-level interactions. These act as a catalyst to improve process mechanisms through the channel of higher accountability and learning. For example, a more aware individual may raise more grievances against the way MGNREGS is implemented in the village. This in turn may encourage beneficiaries to apply for more work and also insist on payments being made on time. Both these outcomes—increased uptake and reduced payment delays—can lead to improvements in welfare outcomes.

The fourth component of the intervention, wage credit list posting, is more personalized in nature and can also have a bearing on uptake and payment delays through a number of direct and indirect channels. The direct channel through which it can lessen delayed payments is the reduction of information asymmetry among beneficiaries with regard to wage credit information. An indirect channel of collective bargaining power can also be hypothesized because the list-posting exercise can enable beneficiaries to collectively demand faster payments. In fact, a reduction in payment delays can 'encourage' workers to demand more work under the programme and possibly in the subsequent period. Figure 1 shows how these mechanisms interact.

4

Figure 1: Mechanisms from intervention to outcomes
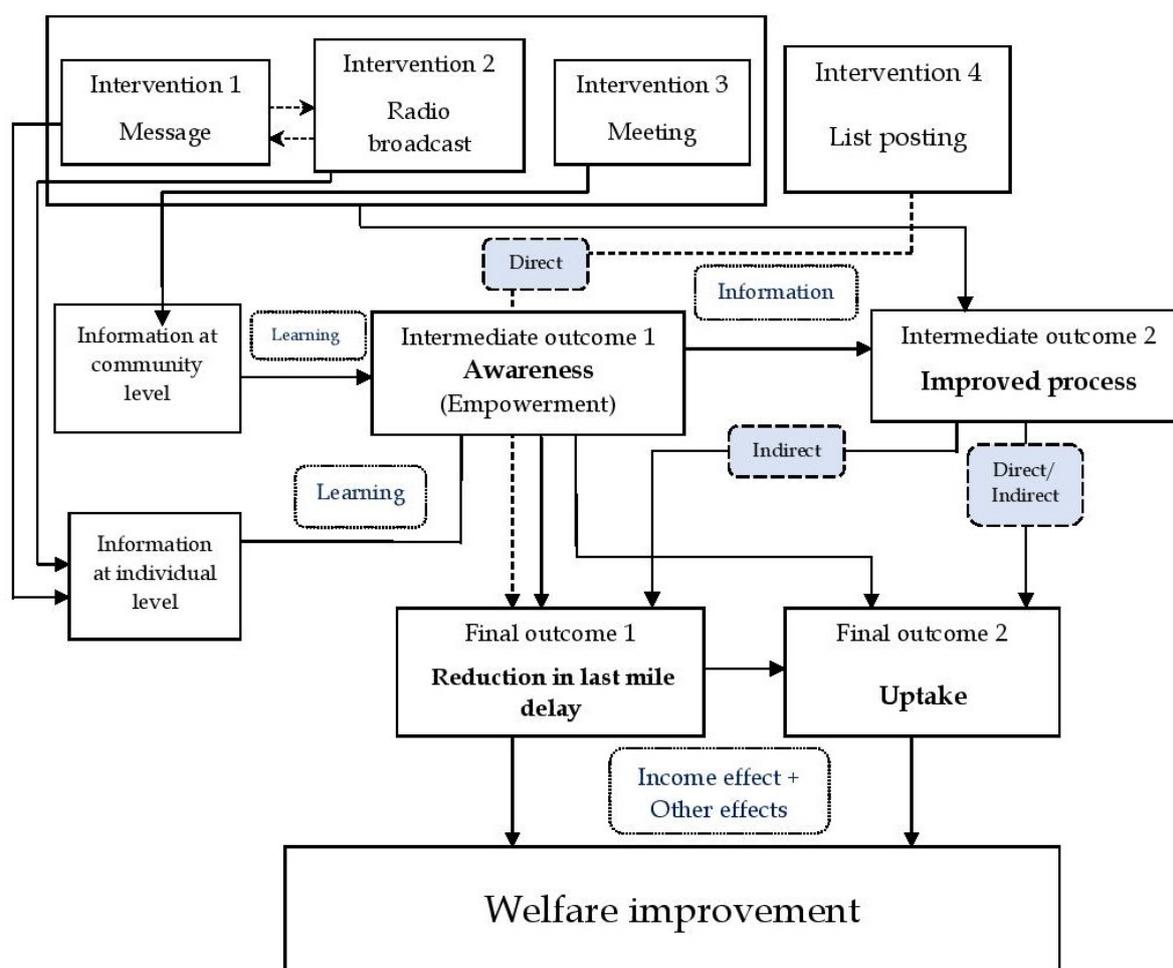
Source: authors' illustration.

4 Study design, data, variables, and randomization process

4.1 Study design

The intervention was rolled out randomly at the GP level in the Damaragidda and Maddur blocks of the Mahbubnagar district, where the randomization was stratified across the blocks. Accordingly, we intervened in 12 randomly selected GPs out of the 22 GPs of the Damaragidda block and 14 GPs out of 27 in the Maddur block. Please note that we omitted Mogala Madaka GP of the Damaragidda block from evaluation because, as it is adopted by the local Member of Parliament, the implementation and payment procedures might be entirely different from those of other GPs. Hence, the 26 selected GPs form our intervention group and the remaining 23 GPs in these two blocks constitute the control group. We further consider two other blocks within the Mahbubnagar district, Hanwada and Koilkonda, which have broadly similar geographic and demographic and population characteristics.[3] Since there was no intervention at all in these two blocks, the GPs within them constitute another set of controls, which we refer to as the 'additional

---

[3] The basic characteristics of these four blocks, taken from the Census 2011 conducted by the Government of India, are presented in Table B1 in Appendix B.

control' group. The block map of the Mahbubnagar district with these four blocks highlighted is shown in Figure 2. The GPs in which the intervention took place (treated GPs) and the two sets of control GPs are shown in Figure 3.

Figure 2: Geographical location of the selected blocks
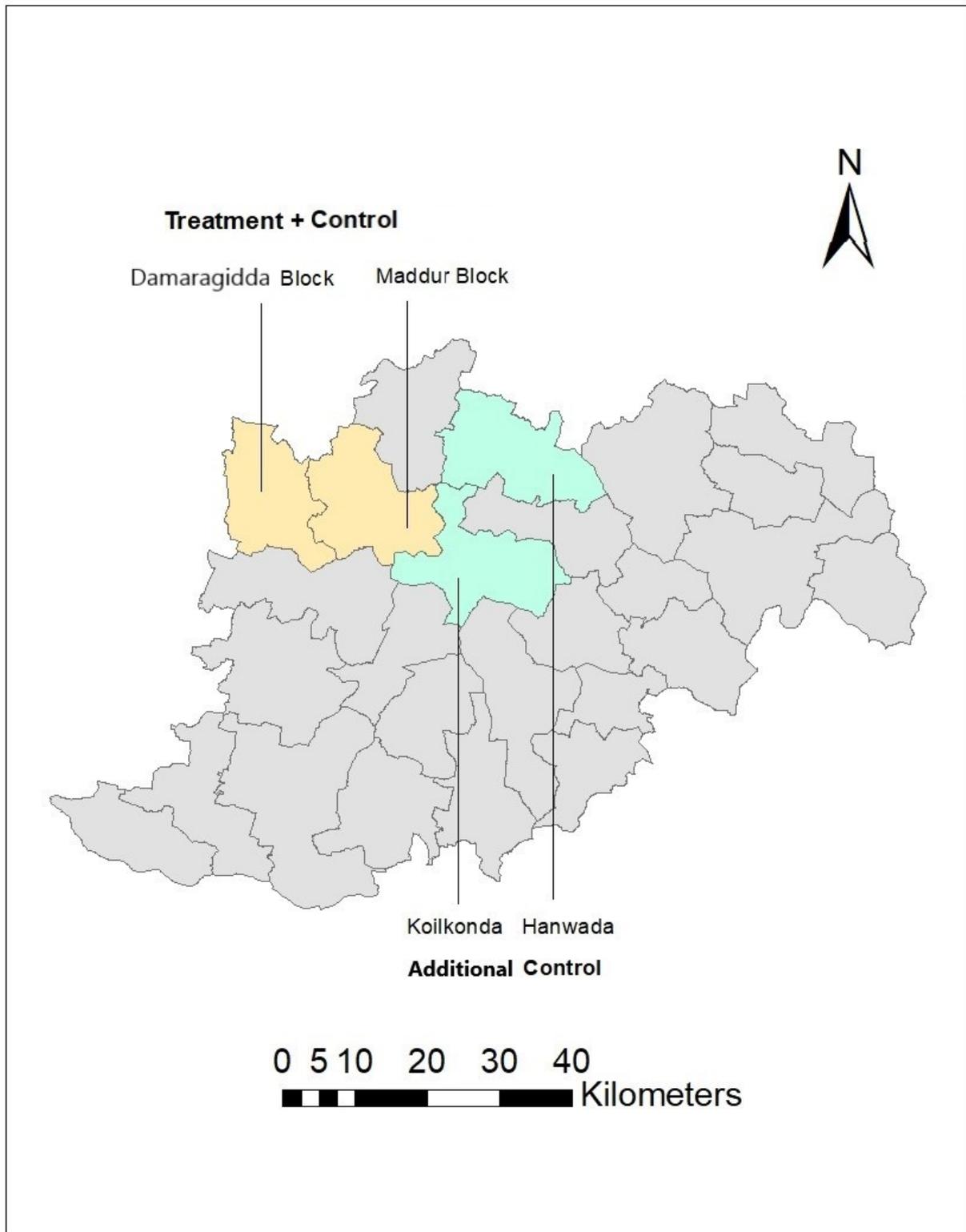
Note: not to scale.

Source: authors' illustration based on Census (2011).

Figure 3: Geographical location of the GPs receiving the intervention
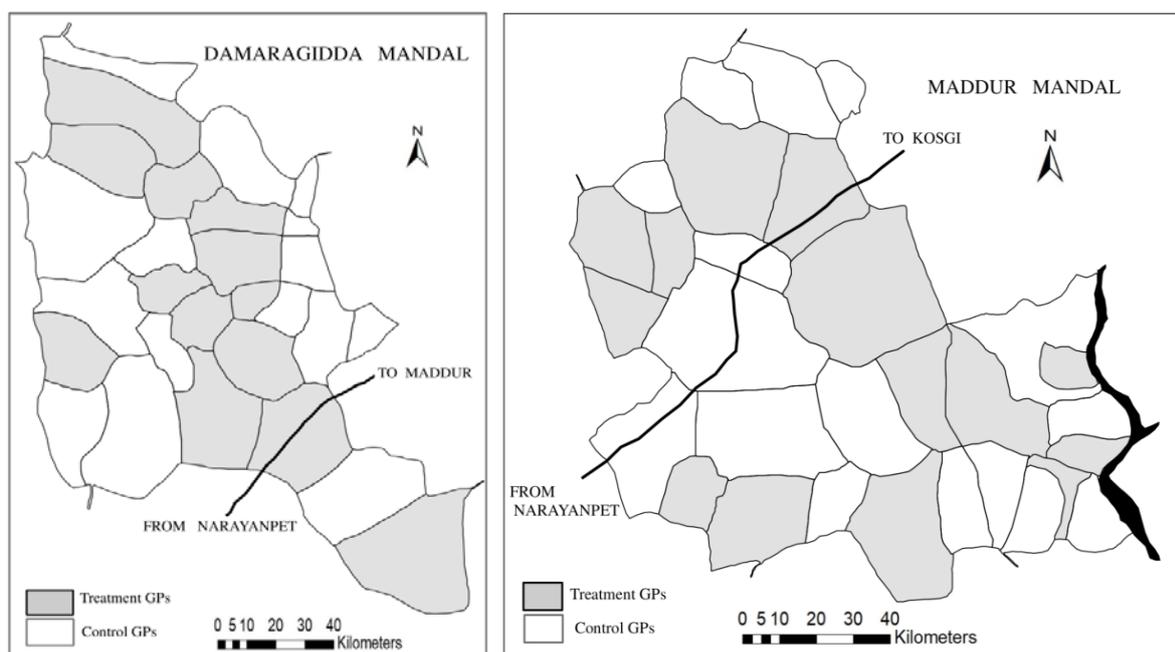
Note: not to scale.

Source: authors' illustration based on Census (2011).

It may be noted that all the treated GPs in the two intervention blocks adjoin at least one control GP, so that there is a possibility of spillover of the intervention from the beneficiaries into these GPs. For example, the information disseminated as a part of the intervention may be shared with villagers in an adjoining control GP. Hence gains from some of the interventions in the treatment GPs may flow to the adjoining control GPs within the same block. However, the chance of spillovers to GPs in the Hanwada and Koilkonda blocks is negligible because of greater distance from the treated GPs and lesser interaction between individuals of different blocks. Therefore, we assume that spillover can flow across GPs within the same block but not across blocks. In any case, spillover would only be possible from the generalized messages disseminated during local meetings or voice broadcast over phones and not from the personalized wage credit lists.

4.2 Data

We use data primarily from the administrative website of the programme in Telangana for the period January 2017 to December 2018.[4] Specifically, data on payment delays and days of work relating to all the job cards from the treatment and control GPs (28,984 job cards in total) are used.

We also conducted two waves of a household survey among job card holders in the 96 GPs within the four blocks to gauge the impact of the intervention on intermediate outcomes. The baseline survey was conducted in September and October 2017, before the start of the intervention. The endline survey was conducted from December 2018 to February 2019 after 13 months of exposure to the intervention. The same households and respondents that were surveyed in the baseline survey were also surveyed in the endline survey, where possible.

---

[4] www.nrega.telangana.gov.in/Nregs/ (accessed 8 June 2020).

For the baseline survey, among the job card holders, approximately 15 households (from power calculations) from each GP were randomly chosen from the list of households that had worked at least once in 2016/17 under MGNREGS. The total number of households surveyed was 1,444 in the baseline survey and 1,352 in the endline survey. Some households were left out in the second wave since the respondents were not found even after three visits. To ensure that the sample of non-resurveyed households was random, we compared their characteristics with those that were resurveyed. The results, which are shown in Table D1 in Appendix D, indicate no major difference in the characteristics and hence the sample of households we were able to resurvey can be treated as a random sample. It may be argued that our sample size is too low to yield unbiased estimates, but it must reiterated that we use the *full* population of job card holders to determine the effect of the intervention on last mile payment delays and uptake. We use the survey data largely to estimate the impact on intermediate outcomes and examine the heterogeneous effects. It should also be noted that our sample size is adequately powered (power = 0.8).

The survey questionnaire covered a wide range of demographic, socio-economic, and household information and a detailed set of information on MGNREGS, including questions on whether the respondent's household had a job card, whether they had worked under the programme, and whether they reported any grievances Apart from general questions on the programme, specific questions were asked to get a clear picture of the awareness among beneficiaries of the scheme and their entitlements (including delay compensation, unemployment allowance, minimum days of work, and wage rates), of process-related information, and of local-level meetings. In addition, we collected information about the FAs and the salient characteristics of the GPs. During the second wave, we also gathered further information from the surveyed households belonging to the treated GPs, including qualitative/subjective questions on their perception of the intervention and its effects on MGNREGS participation and delays in payment.

The tablet-based survey was carried out using Google Forms in the first wave, but in the second wave we used KoBoToolbox, an android-based Open Data Kit (ODK) interface application developed by the Harvard Humanitarian Initiative.[5] The survey team consisted of enumerators who had completed at least higher secondary education and were conversant in Telugu as well as the local dialects.

Additionally, a midline qualitative survey was conducted in two treated GPs from each of the Maddur and Damaragidda blocks to take stock of the intervention from the point of view of both the beneficiaries and the implementing authorities,[6] and to understand the process of change and the impact of the intervention.

4.3   Variables

As indicated, the two main outcome variables are last mile payment delays and uptake of the programme in terms of days of work. The uptake in terms of number of days per month/year for every job card is obtained directly from the online administrative data portal. The last mile payment delay is calculated as the difference in days between the wage credit and debit dates in the post office account (for a detailed explanation see Section 6.1).[7] The underlying assumption is that as soon as beneficiaries know that their wages have been credited to their account, they withdraw the money. This assumption is based on the fact that the beneficiaries are poor and have high marginal

---

[5] More information can be obtained from https://www.kobotoolbox.org/ (accessed 30 June 2019).

[6] The two intervention functionaries at block level were interviewed, along with their supervisors.

[7] This information is given only for job cards attached to postal accounts.

utility of money, disproportionately high disutility of delay, and low propensity to save. Nevertheless, even if we consider some bias in our measurement of last mile delay, the causal estimates would remain unbiased due to random assignment of the intervention across the GPs.[8]

The intermediate outcomes include six indicators of awareness level, i.e. whether the respondent knows (i) about the work entitlement of 100 days every year to each household; (ii) about the work application process within MGNREGS; (iii) that an unemployment allowance is made in the event of not receiving work; (iv) that payment has to be made within 15 days of their completion of work; (v) the correct wage rate (INR197 (~US$2.8) at baseline and INR205 (~US$3) at endline); and (vi) about payment delay compensation. The measurements for these outcomes are binary in nature.[9]

The other set of intermediate variables consists of process-related information about MGNREGS: (i) whether the respondent's job card was updated by the FA during the year before the survey; (ii) whether a receipt was received for a work application during the year before the survey; (iii) whether the respondent had to travel more than once to a bank/post office to withdraw their wages from their account the last time they worked; (iv) whether any respondents attended Gram Sabha (GS) meetings;[10] (v) whether any respondents attended social audit meetings;[11] and (vi) whether concerns about MGNREGS were raised at the GS meetings. All these six indicators are dichotomous in nature.

In the regressions to estimate the impact of the intervention on the intermediate variables, we include a set of control variables measured at baseline to increase the precision of the estimates. To capture the economic conditions of the households, variables included type of household (non-cemented or not), area of land cultivated by the household (in acres), number of livestock (including oxen, bullocks, and cows), main occupation of the household (casual labour or not), and whether the household has a toilet or not. In addition, whether the household members watch television and the number of adult members in the household are included in the regressions. Since caste is one of the major barriers to social inclusion (Deshpande 2011; Sundaram and Tendulkar 2003), we introduce whether the household belongs to the SC/ST community. For estimation of intermediate variables, which are at the respondent level, we control for gender, age, and education of the respondent along with possession of a mobile phone.

To ensure success of the randomization procedure for the sample of 1,352 respondents that were surveyed across both waves, we compare the baseline characteristics across the respondents from treated and control GPs. Table 1 gives the results of the difference-in-means test between the respondents from the two groups. We find that the mean levels of none of the 12 outcome variables are statistically significant at the 5 per cent level. We look at 17 control variables, which include the characteristics of the respondent and their household. Four variables—proportion of respondents who are illiterate, mean age, proportion of houses that are non-cemented, and

---

[8] A potential confounder for selection bias in this case can be rejected payments. However, because our intervention was based on a random selection of GPs, the bias, if any, should not be systematically different between the treatment and control groups, and hence should cancel out.

[9] Based on the distribution, we consider the range of INR180–200 (~US$2.5–2.9) as the correct wage at baseline and INR202–220 (~US$2.9–3) at endline.

[10] The Gram Sabha (GS) is a forum that is used by citizens to discuss local governance and make need-based plans for the village.

[11] Social audit is the process of formal review of an intervention (here MGNREGS), which involves, among other things, meetings with the potential beneficiaries to assess the quality of implementation. In this paper, we refer only to the participation of respondents in these meetings.

proportion of households whose main occupation is casual labour—are found to be significantly different in the two arms. While this imbalance is likely to bias the estimates, our regression strategy controls for these household and respondent characteristics and also the outcome variable measured at baseline, along with the block fixed effects. Hence we minimize the bias when we estimate the impact of the treatment. We also plot a set of Kernel density plots for the treated and control GPs for a number of GP-level indicators (Figure C1 in Appendix C). The plots clearly indicate a close match of these characteristics between the treated and control GPs. Results of a Kolmogorov-Smirnov balance test are given in Table C1 (Appendix C).

Table 1: Comparison of means for the treated and control GPs

	Observations (1)	Control (2)	Observations (3)	Treatment (4)	Difference (2) - (4)
Outcome variables					
Work entitlement	312	0.571	348	0.506	0.065
Work application	312	0.308	348	0.244	0.063
Unemployment allowance	312	0.045	348	0.078	-0.033
Payment duration	312	0.087	348	0.075	0.012
Wage rate	312	0.054	348	0.046	0.009
Job card updated by FA	235	0.328	263	0.312	0.016
Got receipt for work	312	0.147	348	0.158	-0.011
Travelled more than once to bank/post office	302	0.901	316	0.915	-0.014
Attended GS meetings	282	0.319	324	0.34	-0.02
Attended social audit meetings	282	0.319	324	0.34	-0.02
Number of days of work	312	40.042	348	40.816	-0.774
Last mile delay (in days)	3016	34.53	3524	33.00	1.53
Control variables					
Female respondent	312	0.449	348	0.474	-0.025
Age of the respondent	312	44.135	348	42.083	2.051**
Education of the respondent					
Illiterate	310	0.81	347	0.735	0.075**
Below secondary	310	0.103	347	0.147	-0.044
Secondary and above	310	0.087	347	0.118	-0.031
SC/ST	312	0.244	348	0.276	-0.032
Number of adults in hh	312	3.875	348	3.92	-0.045
Land cultivated in acres	312	3.128	348	3.205	-0.077
Oxen, bullocks, and cows	312	1.558	348	1.612	-0.054
Has a flush toilet	312	0.135	348	0.098	0.037
Highest education in hh					
Illiterate	312	0.301	348	0.276	0.025
Below secondary	312	0.202	348	0.187	0.015
Secondary and above	310	0.497	348	0.537	-0.041
Watches television	310	0.571	347	0.506	0.065
Owns a mobile phone	312	0.635	348	0.612	0.023

Note: the mean level of the baseline characteristics is presented. hh = household; FA = field assistant; GS = Gram Sabha. The last mile payment delay is calculated by taking the time difference in days between the wage credit and wage debit date. The average delay from January 2017 to October 2017 is given in the table. Mean difference test using ttest command in STATA 14 is applied for computation. ** $p < 0.05$

Source: authors' calculations.

5   Estimation strategy

We make use of the randomized experimental design that controls for potential selection or omitted variable bias and hence yields unbiased causal estimates. To gauge the impact of the intervention, we mainly rely on the monthly average difference in last mile delay and uptake between the job cards in treated GPs and control GPs. We compare this difference during the pre-intervention period with that during the intervention and the post-intervention periods. In essence this is similar to a DID comparison, which assumes that the indicators in the treated GPs would have shown similar values to those in the control GPs in the absence of the treatment and therefore that the observed difference between the two post-intervention can be causally linked to the intervention.

With regard to the impact on the intermediate variables, we use the Analysis of Covariance (ANCOVA) method to estimate the treatment effect. This controls for the baseline value of the outcome variables. The literature indicates that this increases statistical power, especially when autocorrelation of outcomes is low (Haushofer et al. 2020; Hidrobo et al. 2016; McKenzie 2012). Since the autocorrelation of the outcome variables is low and most of the variables of interest are binary in nature, we estimate the following probit model:

$$Prob(Y_{ijb1} = 1) = \Phi(\alpha + \beta.T_{jb} + \chi.Y_{ijb0} + \lambda.X_{ijb0} + \delta.B_b) \tag{1}$$

where Y_{ijb1} is the binary outcome variable of interest for individual i from GP j of block b, which is the cluster in our case at endline. Y_{ijb0} is the same variable at baseline. These binary outcomes include a set of awareness and process-related variables, as discussed. T_{jb} is the treatment dummy variable, which is equal to 1 if the GP j is in the treatment arm. X_{ijb0} is the vector of control variables that include baseline individual- and household-level characteristics of individual i. B_b is the vector of block level dummies. β is the estimate of the causal impact of the intervention.

To calculate the spillover effect, we categorize the control GPs into two groups: the control GPs in the treatment blocks of Damaragidda and Maddur, and the additional control GPs from the non-intervention blocks of Hanwada and Koilkonda. Accordingly, two dummy variables are generated for the control GPs: one for the normal control and the other for the additional control GPs. We specifically make this adjustment to estimate the spillover and pure treatment effects. If the additional control GPs are taken as the reference group, the marginal effect associated with the control GP dummy gives us the estimate of the spillover effect; and the association with the treatment dummy gives us the estimate of the treatment effect adjusted for spillovers. Formally, we estimate the following probit model:

$$Prob(Y_{ijb1} = 1) = \Phi(\alpha + \beta_T.T_{jb} + \beta_s.CC_{jb} + \chi.Y_{ijb0} + \lambda.X_{ijb0} + \delta.B_b) \tag{2}$$

where everything remains the same except CC_{jb}, which is the dummy for the normal control GPs. Here, as mentioned, additional control GPs are in the reference group. β_T and β_S are the estimators and measure the pure treatment effect and the spillover effect, respectively. Bootstrapped standard errors with 500 replications, clustered at the GP level, are used (Cameron et al. 2008).

6   Results

6.1   Impact on delay

The system of payment under MGNREGS in Telangana is as follows. When a job is completed, there is physical verification of the work by the office of the Block Development Officer (BDO). After this, a Fund Transfer Order (FTO) is generated at local level. The FTO is then approved by the central ministry, which sends its details to payment intermediaries, who are responsible for the electronic transfer of wages. The final payment status is shown on the public website and includes the date on which the wages were credited to the relevant bank or post office, provided that the payments were not rejected.[12] Hence, in terms of payment delays, it should be noted that these might result from late FTO generation or from wages being credited by the central ministry, which may happen for various reasons, including shortage of funds.[13]

In addition to these delays, a last mile delay after wages have been credited to the workers' accounts is prevalent. More often than not, beneficiaries are not informed when their wages are credited to their accounts; and post office officials, including managers, make use of this information asymmetry to delay payments for personal needs, as described. Figure 4 indicates the magnitude of the last mile delay in days (defined by the number of days between the wage credit and debit dates) during 2017, along with the FTO ('payorder') generation and total delay separately for the four blocks. FTO generation delay is defined as the number of days it took for the FTO to be generated after completion of the work, and total delay is the total time taken in days for the wages to be credited to the account after the completion of work. The average total delay across the four blocks is about 66 days and, even after the wage is credited, an average worker has to face an average last mile delay of more than 34 days. This is substantial, particularly for a subsistence worker who is dependent on MGNREGS and especially during the lean agricultural season. As argued by Basu et al. (2020), these payment delays are detrimental to the welfare of the poor through two potential channels: the imposition of an implicit consumption tax and a decline in the 'human and financial net worth' of the household. These issues have also been discussed elsewhere in the literature, which has documented the high prevalence of delayed payment under the programme (Narayanan et al. 2017).

Our intervention allows us to crawl the available public data and provide personalized information to beneficiaries once wages are credited to their bank or postal account through wage list posting. The purpose of this information is to reduce information asymmetry and enable the beneficiaries to demand the credited wages from the postal officials. A brief theoretical framework of the set-up is given in Appendix A.

We use information on the credit and debit dates for all active 7,733 job cards from the GPs that use postal accounts for the disbursement of MGNREGS wages to the beneficiaries.[14] We specifically use these data to calculate the monthly mean difference between the credit and debit dates (defined as the last mile delay) and then plot the monthly mean difference in last mile delay in the treated and control GPs from January 2017 to April 2019. Figure 5 presents this plot along

---

[12] Wages may be 'rejected' on account of technical errors such as incorrect entry of account numbers in the system.

[13] See Narayanan et al. (2019) for a more detailed description of the payment process.

[14] The RN6 table from the data portal gives the credit and debit dates.

with that of the total number of lists posted in the intervention GPs over these months, which enables us to assess the causal effect of the intervention on last mile delays.

Figure 4: Extent of different types of delay (in days) across blocks
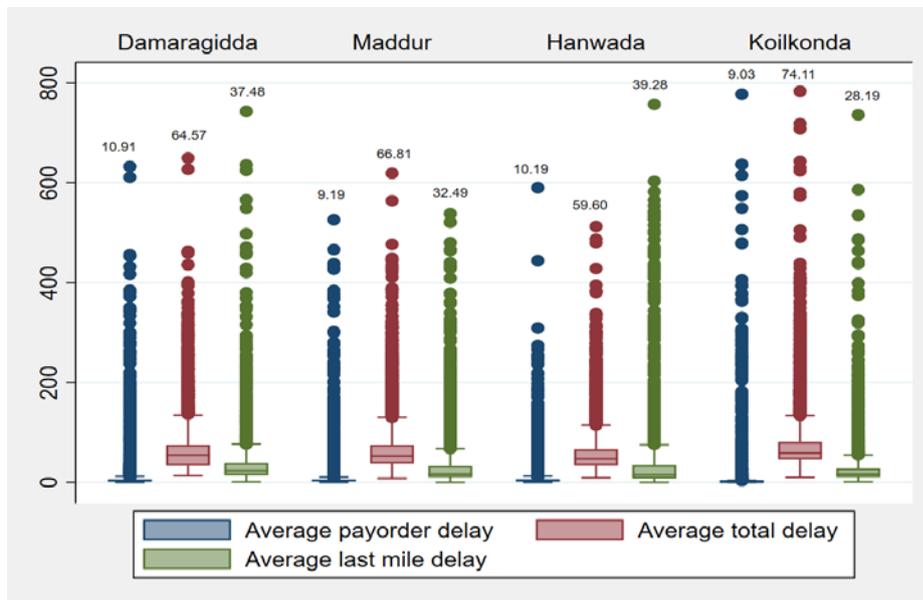
Note: box plot showing average days of FTO delay, total delay, and last mile delay in days per job card during 2017 for all the studied blocks. Values shown at the top signify the mean of the respective delays. The upper and lower hinges of the box correspond to the 75th and 25th percentiles of the distribution and the line across the box indicates the median. The Y axis represents the duration in days of the respective delays.

Source: authors' illustration from administrative data from the Telangana NREGA website.

Figure 5: Difference in last mile delay between intervention and control GPs (in days)
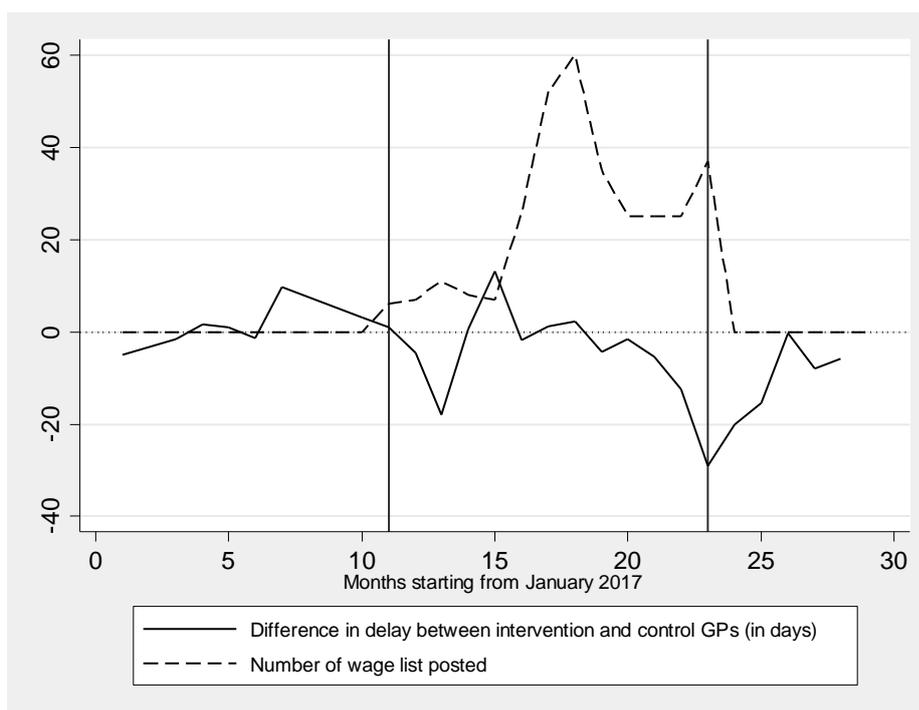
Note: the monthly mean level of last mile payment delays (in days) is calculated for intervention (treated) and control GPs and their difference is plotted. The months are plotted on the X axis, from January 2017 till April 2019; hence, '1' indicates January 2017, '12' indicates December 2017, '20' indicates August 2018, and so on. The period between the vertical lines is the period of the intervention (November 2017 to November 2018). The dashed line plots the number of wage credit lists posted in all the intervention GPs combined across the intervention period.

Source: authors' illustration.

The findings reveal a sizeable positive impact, as the difference in last mile delays between the treated and the control GPs shows a massive fall during the intervention period. Before the start of the intervention, the difference in last mile delays across the treated and control GPs was close to zero. However, from November 2017 (month number 11), when the intervention started, the difference starts reducing and it continues to do so as the total number of wage list postings increases. In November 2018, we observe a reduction of last mile delays in the treated GPs of about 28 days on average in comparison with the control GPs, which clearly indicates that the intervention had a substantial impact.

Despite this clear impact of the intervention on last mile delays, its impact on FTO generation and wage credit delay is likely to be limited. This is because the responsibility for these delays lies with the block and the central/state level authorities, who are not targeted through our intervention—unlike the last mile delay, for which the local-level post offices can be held responsible. To test this we plot the monthly difference in mean FTO generation delay (in days) at the GP level between the treated and control GPs from January 2017 till April 2019 along with the monthly difference in wage credit delay. Figure 6 presents these plots. As one would expect, there is an inconsistent and marginal rise and fall in the FTO as well as wage credit delay during the intervention period, indicating its negligible impact. Notably, this acts as a falsification test (discussed later) in which we find a negligible effect of the intervention on related outcomes that we hypothesized would not be significantly impacted.

14

Figure 6: Difference in FTO delay and total delay between intervention and control GPs (in days) from January 2017 to April 2019
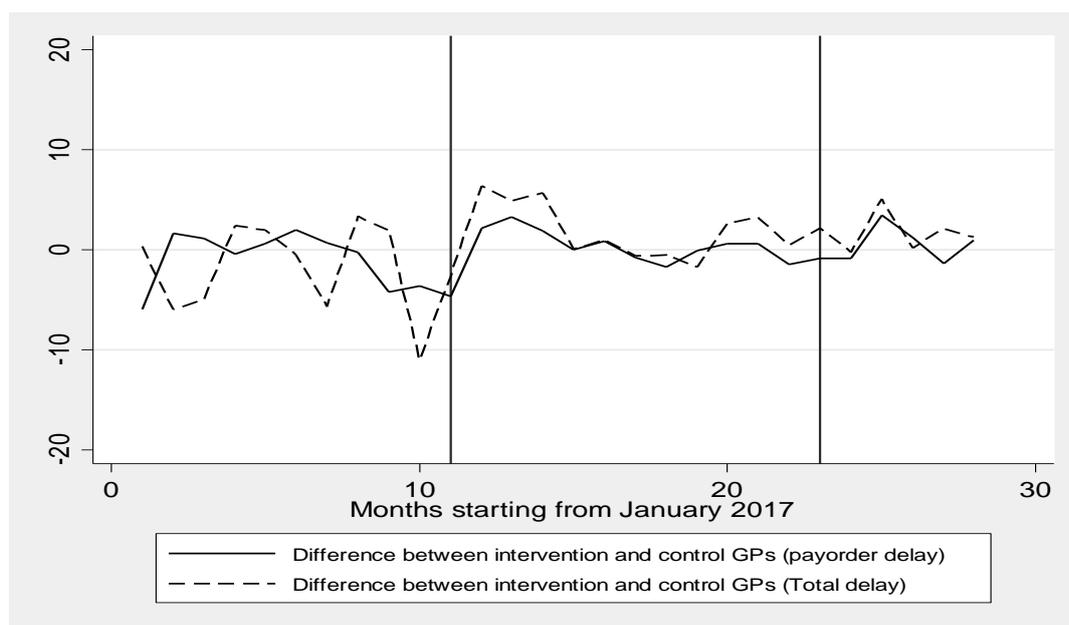

Note: the monthly mean level of last mile payment delays (in days) is calculated for intervention (treated) and control GPs and their difference is plotted. The months are plotted on the X axis, from January 2017 till April 2019; hence, '1' indicates January 2017, '12' indicates December 2017, '20' indicates August 2018, and so on. The period between the vertical lines is the period of the intervention (November 2017 to November 2018).

Source: authors' illustration.

It may be argued that the intervention had a limited effect on payment delays as it affected only the last mile, local-level delays while having limited impact on FTO and wage credit delays. However, our observations indicate that last mile payment delays are significant, especially when we consider that the programme was designed to target the poorest population during the lean agricultural season. The average last mile delay before the intervention in all the GPs from the four surveyed blocks is found to be about 37 days, and the delay goes up to about 80 days for about 10 per cent of the job cards (Figure 4). The fact that we are able to register a gain of about 28 days in terms of the last mile delay is noteworthy, and it is here that our intervention assumes importance.

Indeed, our qualitative work during the midline survey indicates that messages received by the beneficiaries of the programme when their wages were credited in their bank/postal account resulted in a reduction of last mile payment delays. One of the respondents reported:

> Before, we were not aware of the amount of money credited to our account. We used to ask the FA but he was not able to answer. Therefore, we had to make multiple trips to the bank. Now we get the information through phone calls. Even if we miss the call, we can see our names on the list posted on the wall of the GP office. This has helped us a lot.

A similar picture emerges from the endline survey data, which show that 68 per cent of respondents think that their bank/post office transactions have got easier as compared with the previous year and about 63 per cent of them believe that delay in payment has reduced in comparison with previous year.

15

6.2 Impact on work days

To examine the impact on uptake, we use data on the number of days of work for each job card from the treated and control GPs. As in the earlier case, we plot the monthly mean difference in the average number of days' work between the intervention and control GPs from January 2017 to December 2018 (Figure 7). We observe that the difference in work days between the treated and control GPs hovers around zero not only before but also during the intervention, indicating a limited impact of the intervention on uptake.

Figure 7: Difference in mean uptake between intervention and control GPs (in days) starting from January 2017
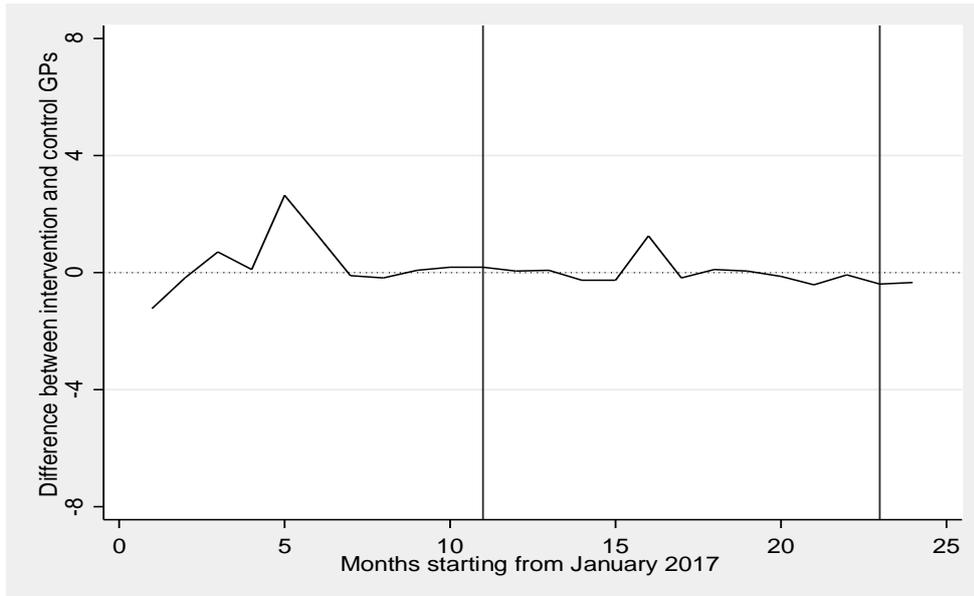
Note: the monthly mean level of uptake in days is calculated for the intervention and control GPs and the difference in days of work is plotted on the Y axis. The months are plotted on the X axis, from January 2017 till April 2019; hence, '1' indicates January 2017, '12' indicates December 2017, '20' indicates August 2018, and so on. The period between the vertical lines is the period of the intervention (November 2017 to November 2018).

Source: authors' illustration.

Arguably, uptake of work also depends on a set of household and other confounding factors that need to be controlled for before making any causal interpretation. For this, we use a simple difference-in-differences (DID) regression method for the sampled job cards, comparing the baseline and endline differences in uptake for job cards from treated and control GPs against the set of possible confounding factors.[15] Table 2 presents the regression results on the logarithmic value of uptake in days.[16] The marginal effects of the treatment indicate no significant difference in uptake, as is also observed in Figure 7, where we compare the treated GPs with the control GPs. To measure the spillover in terms of uptake (if any), we compare the control GPs with the additional control GPs from the other blocks (Hanwada and Koilkonda). Our results indicate no significant change, indicating limited spillover effects.

---

[15] See Angrist and Pishcke (2008) for more information on DID regression.

[16] We add 1 to the number of days to avoid missing values when zero days of work is transformed to its logarithmic value.

Table 2: Impact of treatment on uptake and spillover effects

	Treated vs. control GPs	Control GPs vs. additional control GPs
Treatment (Reference: control GPs)	0.153	
	(0.272)	
Post	-0.320**	
	(0.159)	
Treatment*Post	-0.214	
	(0.208)	
Control (Reference: additional control GPs)		-0.316
		(0.344)
Post		-0.323***
		(0.107)
Control*Post		0.003
		(0.193)
Observations	1314	1982
R-squared	0.045	0.036

Note: the following control variables were incorporated in all the regressions: Scheduled Caste/Scheduled Tribe, number of adults in the household, type of house (non-cemented or not), land cultivated in acres, total number of livestock (cows, bullocks, and oxen), whether household has a toilet, and whether its members watch TV, along with main occupation of the household and block dummies. The outcome variable is log (days of work+1). Since the outcome variable is defined at household level, we use only the household-level control variables. The marginal effects from double difference regressions are reported and the bootstrapped standard errors clustered at the GP level run with 500 replications are reported in parentheses. Post is a variable that indicates the endline period. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The regression tables with all the control variables can be provided on request.

Source: authors' calculations.

6.3   Impact on intermediate outcomes

As discussed, we further examine the effect of the intervention on intermediate outcomes using ANCOVA regressions given by equations (1) and (2). It should also be noted that information about awareness of households' entitlement to delayed payment compensation was not collected during the baseline survey. Hence, to estimate the impact of the intervention on this indicator, we use a pooled probit model but did not control for baseline-level awareness of delayed compensation. The assumption is that at the baseline, there is no significant difference in awareness levels between respondents in the treatment and control arms. Intuitively, this is justified, as we did not find a significant difference between the treatment and control arms for any of the other five indicators of entitlement awareness, which makes it less likely that we will observe a significant difference in this awareness indicator specifically (Table 1).

The estimation results are presented with two different specifications for estimating equations (1) and (2). The first specification incorporates treatment as a dummy and takes the value of 1 for the treated GPs. The second specification categorizes the control GPs into two groups: the control group and the additional control group as discussed. The additional control GPs are here taken as the reference group. As stated, the second specification helps us to gauge the spillover impact. We also present the estimates from a comparison of the sampled households from the treated GPs with those from the control GPs.

Table 3 presents the estimation results of the pooled regression as depicted in equations (1) and (2). The coefficients of the probit model are changed to the marginal effects, which are calculated at the mean value of the independent variables and presented as such. The findings indicate a definite positive and significant impact of the intervention on awareness. We find about a 15–30

percentage point increase in the probability of being aware of the different entitlements. Notably, our results indicate a significant spillover impact on some of the indicators of awareness. However, the effect size is found to be lower, as we observe that the probability of being aware for respondents from a control GP is 10–15 percentage points more than that for respondents from the additional control GPs. Net of spillover effect, the effect size of increase in probability of being aware of these entitlements lies in the range of about 12–36 percentage points.

This finding is substantiated by the qualitative discussions during the midline survey. In three out of the four treated GPs that we visited, villagers seemed to be aware of the current MGNREGS wage rate and work application procedure. Some of them specifically attributed this awareness to the mobile phone calls from the intervention team. One stated: 'We came to know of various provisions of MGNREGS through the Upadhi Hami Phone Radio which we otherwise would not have known. This has helped us to demand correct wages from the FA.'

Table 4 shows the results from the pooled probit regression to estimate the impact on the application process and attendance at community meetings. The findings reveal a consistent significantly positive impact on the probability of receiving a receipt for a work application (at the 5 per cent level) as we find around a 10–13 percentage point increase in the probability as a result of the intervention. Similarly, a 10–14 percentage point reduction in the probability of travelling more than once to banks/post offices for collection of wages is observed. The impact on attendance at GS and social audit meetings seems to be robust and the findings indicate a 12–14 and 16–27 percentage point increase, respectively. The probability of raising concerns over MGNREGS at GS meetings also seems to be significantly higher in the treated GPs. Unlike the earlier case, we find no spillover effect on these process variables, though a significant effect on the chances of participation in social audit meetings and of MGNREGS being discussed at GS meetings is observed.

Table 3: Impact of treatment on awareness

	Work entitlement (1)	Work application (2)	Unemployment allowance (3)	Payment duration (4)	Wage rate (5)	Delay compensation (6)
	Comparison of treated GPs with control GPs and additional control GPs					
Treatment	0.121***	0.211***	0.145***	0.206***	0.205***	0.164**
	(0.039)	(0.047)	(0.024)	(0.055)	(0.038)	(0.071)
	Comparison of treated GPs with additional control GPs only					
Ref. additional controls						
Treatment	0.117**	0.362***	0.263***	0.272***	0.218***	0.230***
	(0.050)	(0.058)	(0.032)	(0.067)	(0.037)	(0.026)
Control	-0.004	0.150**	0.117***	0.065	0.012	0.065**
	(0.054)	(0.063)	(0.033)	(0.067)	(0.039)	(0.031)
	Comparison of treated GPs with control GPs only					
Treatment	0.107***	0.211***	0.248***	0.207***	0.247***	0.291***
	(0.038)	(0.051)	(0.038)	(0.052)	(0.038)	(0.032)

Note: the following control variables were incorporated in all the regressions: respondent gender, age, education, Scheduled Caste/Scheduled Tribe, number of adults in household, type of house (non-cemented or not), land cultivated in acres, total number of livestock (cows, bullocks, and oxen), whether household has a toilet, and whether its members watch TV, along with main occupation of the household and block dummies. The marginal effects from the ANCOVA pooled probit regression are reported, along with the bootstrapped standard errors clustered at the GP level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The regression tables with all the control variables can be provided on request.

Source: authors' calculations.

Table 4: Impact of treatment on process-related variables and attendance at meetings

	Job card updated by FA	Got receipt for work	Travelled more than once for wages	Attended GS meetings	Attended social audit meetings	Raised issue on MGNREGA
	(1)	(2)	(3)	(4)	(5)	(6)
Comparison of treated GPs with control GPs and additional control GPs						
Treatment	-0.015	0.096**	-0.099**	0.125***	0.156***	0.085**
	(0.073)	(0.039)	(0.048)	(0.048)	(0.047)	(0.033)
Comparison of treated GPs with additional control GPs only						
Ref. additional controls						
Treatment	0.136*	0.128**	-0.134***	0.144**	0.272***	0.317***
	(0.077)	(0.055)	(0.051)	(0.062)	(0.053)	(0.046)
Control	0.151	0.031	-0.035	0.019	0.116**	0.231***
	(0.099)	(0.058)	(0.052)	(0.058)	(0.054)	(0.049)
Comparison of treated GPs with control GPs only						
Treatment	-0.017	0.103**	-0.102**	0.137***	0.183***	0.132***
	(0.075)	(0.042)	(0.048)	(0.048)	(0.048)	(0.042)

Note: the following control variables were incorporated in all the regressions: respondent gender, age, education, Scheduled Caste/Scheduled Tribe, number of adults in household, type of house (non-cemented or not), land cultivated in acres, total number of livestock (cows, bullocks, and oxen), whether household has a toilet, and whether its members watch TV, along with main occupation of the household and block dummies. The marginal effects from the ANCOVA pooled probit regression are reported, along with the bootstrapped standard errors clustered at the GP level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The regression tables with all the control variables can be provided on request.

Source: authors' calculations.

To sum up, we observe that the intervention was instrumental in increasing awareness of the basic provisions of the MGNREGS programme and also in improving process mechanisms. But this increase did not lead to higher uptake through increased days of work under the programme. The spillover effect, as one would expect, is also found to be negligible. This suggests a limited impact of generalized information campaigns through meetings and phone calls. However, the personalized information campaigns seem to be effective, as we find a considerable impact of wage credit list posting on reducing last mile delays in wage payments.

6.4 Robustness and falsification checks

We conduct a series of robustness checks to ensure that our causal estimates are qualitatively correct. First, the inferences drawn so far from the pooled regressions rest on the assumption that between the baseline and endline there were no changes in the villages that could systematically influence the outcome variables. Accordingly, we gather data on these changes (if any) from the panchayat officials and the FA. The officials and FA report that there have not been any new NGOs working on MGNREGS or related programmes established during the intervention period. It is also found that there have not been any systematic changes in the way MGNREGS has functioned during the year of the intervention. Incidentally, in four GPs, the FA was changed during the intervention period. Hence as a robustness check, we drop these four GPs and re-run the regressions. Qualitatively, the marginal effects for all the variables across specifications remain unchanged.[17]

In addition, we conduct a number of falsification tests, in which we examine the effect of the intervention on non-equivalent dependent variables (NEDV) to test for potential internal validity

---

[17] The regression results can be obtained on request.

threats. In other words, are there any 'placebo' effects of the treatment on outcomes that are generally considered to be unrelated to the intervention? An insignificant causal effect here indicates that the change in the original outcome variables is due to the intervention and not to other confounders (Cohen-Cole and Fletcher 2009; Coryn and Hobson 2011). Accordingly, we consider three outcome variables that should not be related with our intervention: (i) whether the household has a toilet funded partially or fully by the government; (ii) whether the local drinking water services are funded partially or fully by the government; and (iii) whether the household uses improved cooking facilities that include liquefied petroleum gas (LPG) or an induction/hot plate.[18] Our regression results indicate that the impact on these unrelated variables is indistinguishable from zero at the 5 per cent level of significance (Table 5).

Table 5: Impact of the treatment on unrelated variables (falsification test)

	Government-funded toilet	Government-funded water services	Improved cooking facilities
Comparison of treated GPs with control GPs and additional control GPs			
Treatment	-0.038	-0.018	-0.023
	(0.072)	(0.037)	(0.030)
Comparison of treated GPs with control GPs only			
Treatment	-0.046	-0.017	-0.020
	(0.071)	(0.035)	(0.026)

Note: outcome variables are toilet partially or fully funded by the government (Government-funded toilet), water services partially or fully funded by the government (Government-funded water services), and whether the household is using LPG/biogas or an induction/hotplate (Improved cooking facilities). The following control variables were incorporated in all the regressions: Scheduled Caste/Scheduled Tribe, number of adults in the household, type of house (non-cemented or not), land cultivated in acres, total number of livestock (cows, bullocks, and oxen), whether household members watch TV, and main occupation of the household, along with block dummies. Since the outcome variable is defined at household level, we use only the household-level control variables. The marginal effects from the ANCOVA pooled probit regression are reported, along with the bootstrapped standard errors clustered at the GP level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The regression table with all the control variables can be provided on request.

Source: authors' calculations.

Next, we implement a placebo test where we randomly categorize all the GPs into fake treatment and control GPs with dummies. Hence, out of the 96 GPs, 48 GPs were grouped into a fake treatment group and the remaining 48 into a control group. The difference in uptake between the fake treated and control GPs since January 2017 is plotted in Figure 8. No significant difference during the intervention can be observed. Similar plots are presented for last mile delays for the 70 GPs that use postal accounts for payments by randomizing them into treated and control groups. As can be observed, no significant difference is found during the period of intervention. Notably, we also did not find any significant placebo effect on the intermediate outcome variables, which tends to indicate that our causal estimates are immune to potential internal validity threats.[19] Finally, instead of an ANCOVA pooled probit regression, we use a DID regression to estimate

[18] Toilets and improved cooking facilities have been used for falsification since arguably two of the biggest welfare programmes started by the central government of India during this period were the Swachh Bharat Abhiyaan sanitation programme, which aimed to provide toilets in all households, among other benefits (https://swachhbharatmission.gov.in/sbmcms/index.htm), and the Ujjwala Yojana, which aimed to provide subsidized improved cooking facilities to poor households (https://pmuy.gov.in/) (both websites accessed 21 May 2020.)

[19] The regression results are given in Table E1 in Appendix E.

the causal impact of the intervention on the intermediate outcomes. The direction of the marginal effects for most of the variables remains same.[20]

Figure 8: Difference in mean last mile delays and uptake between fake intervention and control GPs (in days) from January 2017 till December 2018
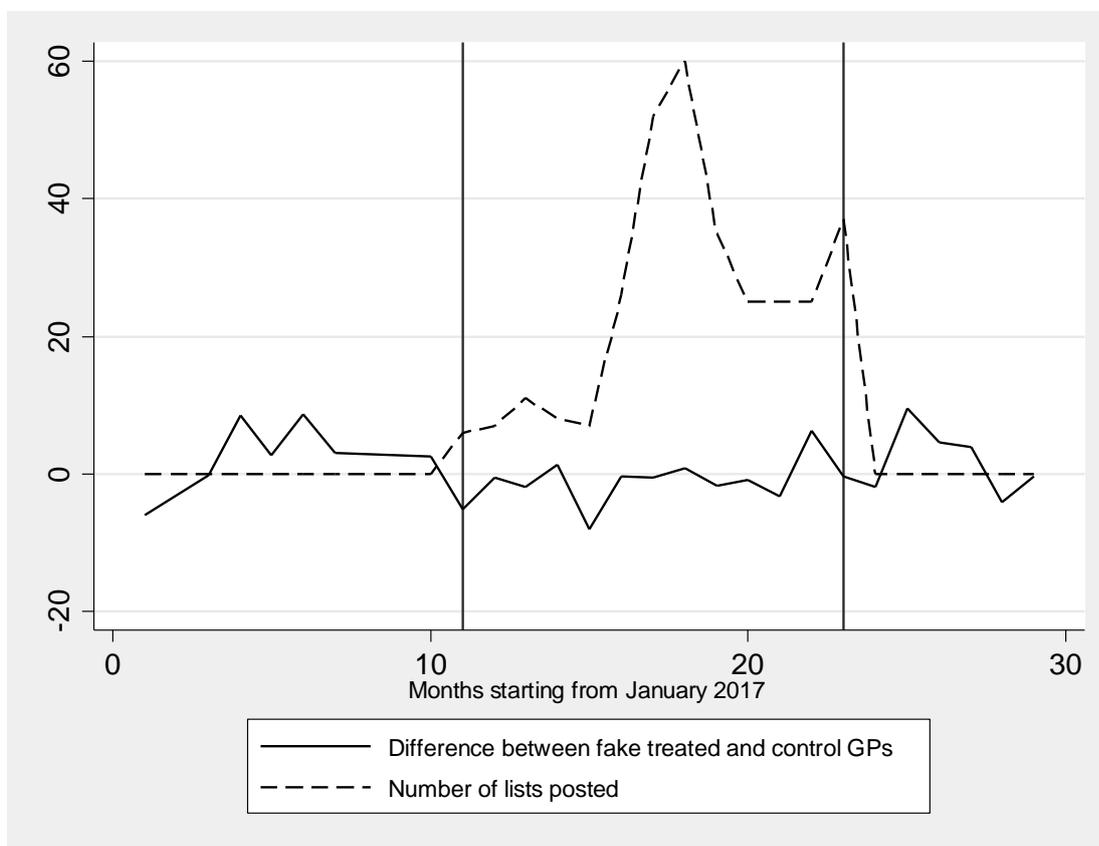
Note: the monthly mean level of uptake and last mile payment delays in days are calculated for fake intervention and control GPs and the difference in days of work is plotted on the Y axis. The months are plotted on the X axis, from January 2017 till December 2018. Hence '1' indicates January 2017, '12' indicates December 2017, '20' indicates August 2018, and so on. The period between the vertical lines is the period of the intervention (November 2017 to November 2018). The dashed line plots the number of wage credit lists posted in all the intervention GPs combined across the intervention period.

Source: authors' illustration.

6.5 Heterogeneous impact on SC/STs, educated households, and those owning mobile phones

One of the major features of the intervention was the additional effort made to reach out to households from the SC/ST community. Hence, it is likely that the marginal treatment gains would be disproportionately higher for these households. Accordingly, we examine the plot of differences in last mile delays between the treated and control GPs since January 2017, as was done earlier separately for the SC/ST and non-SC/ST households. As is evident from Figure 9, which presents the findings, a reduction in last mile delays is observed during the intervention period. Importantly, we observe a reduction in last mile delays for non-SC/ST households as well, but the effect size for SC/ST households is found to be substantially higher. For example, while the largest monthly average reduction in last mile delays for non-SC/ST households is found to be about 25 days, that

---

[20] The regression results can be provided on request.

for SC/ST households is found to be 40 days. This seems to indicate that the intervention had a higher effect on reduction in last mile payment delays for SC/ST households than for other households.

Figure 9: Last mile payment delays for SC/ST and non-SC/ST households
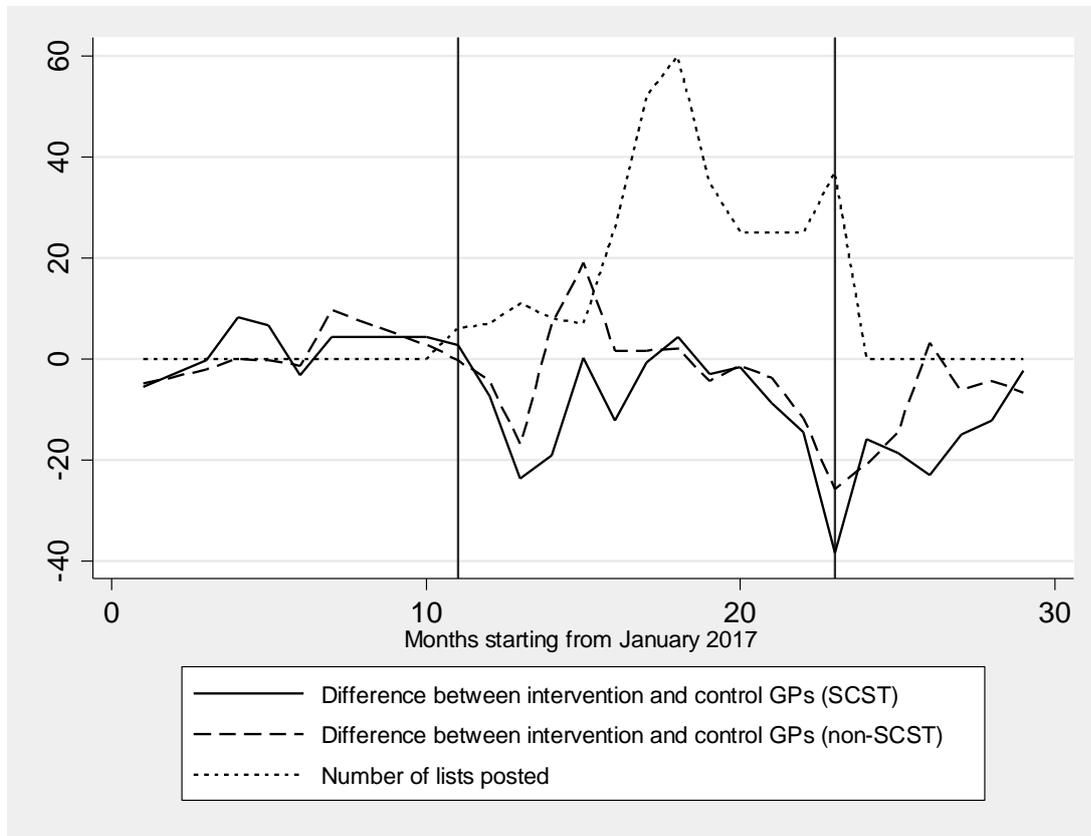
Note: the monthly mean level of last mile payment delays in days is calculated for treated and control GPs and the difference in days of work is plotted on the Y axis separately for SC/ST and non-SC/ST households. The months are plotted on the X axis, from January 2017 till December 2018. Hence '1' indicates January 2017, '12' indicates December 2017, '20' indicates August 2018, and so on. The period between the vertical lines is the period of the intervention (November 2017 to November 2018).

Source: authors' illustration.

We present a similar plot of differences in uptake for households from treated and control GPs separately for SC/ST households and others in Figure 10. Unlike the case for last mile delays, no significant effect on uptake was found for SC/ST households when compared with non-SC/ST households. Notably, findings from the regression to estimate the marginal effect for SC/ST households on the intermediate outcomes of awareness and improved process mechanisms compared with non-SC/ST households indicate no significant gains.[21] This again points to a limited impact of generalized information campaigns even on groups for whom exposure to the intervention is higher.

---

[21] The regression results can be provided on request.

Figure 10: Uptake for SC/ST and non-SC/ST households
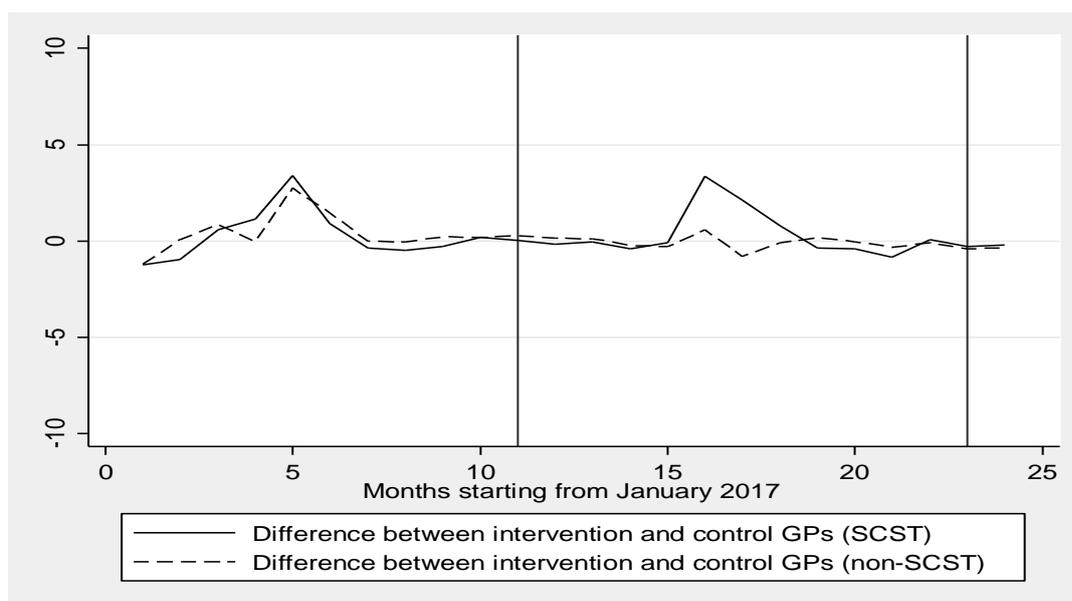

Note: the monthly mean level of uptake in days is calculated for treated and control GPs and the difference in days of work is plotted on the Y axis separately for SC/ST and non-SC/ST households. The months are plotted on the X axis, from January 2017 till December 2018. Hence '1' indicates January 2017, '12' indicates December 2017, '20' indicates August 2018, and so on. The period between the vertical lines is the period of the intervention (November 2017 to November 2018).

Source: authors' calculations.

Since the intervention primarily involved information dissemination and mobile phones are an important component of this, it is possible that the potential gains from the intervention are will be higher for mobile phone owners or literate households. However, findings from the regressions indicate no such gains for these households. This is true not only for uptake but also for intermediate outcomes, possibly indicating that the effects of the intervention are inclusive of the potentially deprived households, who are possibly less educated or without access to a mobile phone.[22]

6.6   Effect on uptake through 'encouragement effect'

The literature has indicated that because workers are uncertain of securing jobs from the local authorities and concerned about associated delays in payment, they are often discouraged from applying for work under MGNREGS (Mukhopadhyay et al. 2015; Narayanan et al. 2017). If this holds, it is possible that a reduction in payment delays would encourage workers to apply for more work under the programme. In other words, a substantial reduction in last mile payment delays, which is observed during the intervention, could lead to a higher uptake of jobs in the following period. In this section, we test whether this holds true, given that we found a considerable reduction in last mile delays due to the intervention.

For this we consider the period from January to December 2019 and calculate the monthly average uptake in the treated and control GPs. We show these two plots for all months starting from January 2018 in Figure 11. A gain of about 3–5 days is observed starting from April 2019, which is close to 10–15 percentage points during the peak working season of MGNREGS from May. This should be placed alongside two important findings already discussed. First, the reduction in

---

[22] The regression results are given in Tables E2–E6 in Appendix E.

payment delays is seen to revert to original levels within three or four months of the end of the intervention. Second, we did not find any significant increase in uptake during the intervention. These observations indicate that the increase in uptake that we observe is potentially due to the reduction in last mile delays during the intervention period and hence suggest an 'encouragement effect' for beneficiaries to apply for more work under the programme.

Figure 11: Uptake in treated and control GPs from 2019
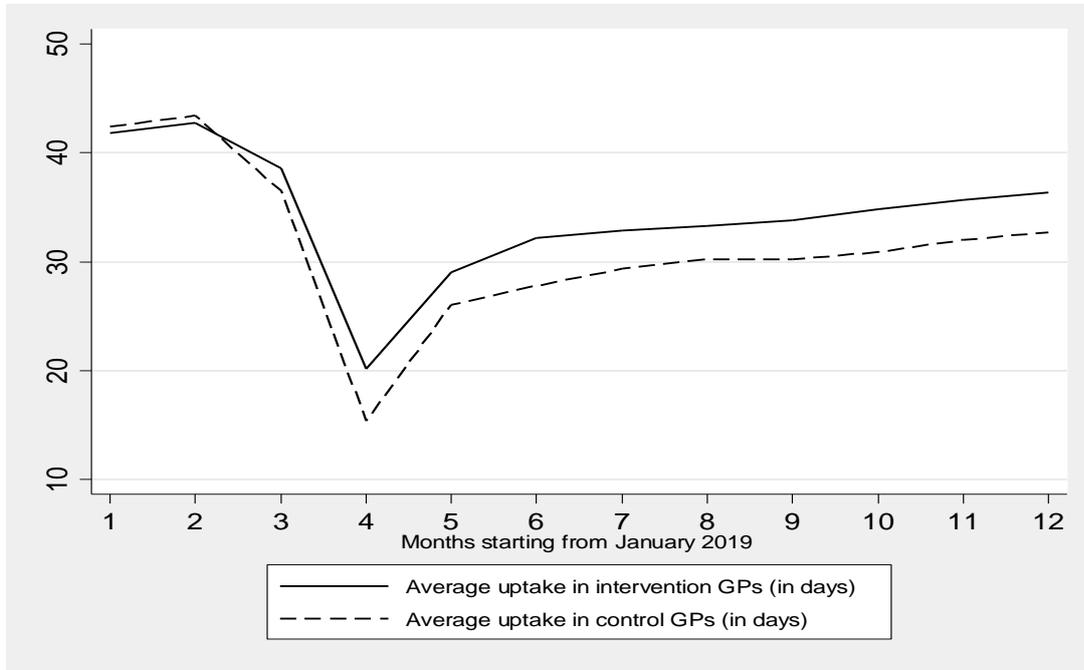
Note: the monthly mean level of uptake in days calculated for treated and control GPs is plotted on the Y axis. The months are plotted on the X axis, from January 2019 till December 2019. Hence '1' indicates January 2019, '12' indicates December 2019, and so on.

Source: authors' illustration.

7   Cost-effectiveness

The evidence from this paper indicates that a personalized information campaign can have a considerable impact by reducing last mile delays and potentially encouraging higher uptake in the subsequent period. On the other hand, the effects of generalized information campaigns are limited. In terms of policy recommendations, however, one may argue that the former is costly and hence not effective through the lens of a cost–benefit analysis. In order to examine this question in detail, we need to estimate the difference between the amount of delay compensation the government has to pay to beneficiaries in the absence of the intervention and the total cost incurred to implement the intervention at local level, which includes both the personalized and generalized campaigns.

As estimated, we observe an average drop of around 25 days in last mile delay per job card during the peak MGNREGA month in the treated GPs in comparison with the control GPs. With an average of about 370 active job cards in every GP in Telangana, the total drop in last mile delays is close to 9,250 days. In accordance with the Guidelines on Compensation for delayed wage payments, dated 12 June 2014, which states that the compensation amount to be paid is calculated at a 'rate of 0.05 per cent of the unpaid wages per day for the duration of the delay', and given that the minimum daily wage under MGNREGS in the state during the period of the intervention was

25

around INR200 (~US\$3), the compensation for each delayed day is INR10 (~US\$0.14).[23] This amounts to a cost of INR92,500 (~US\$1,322) incurred by the government in every GP each month at least during the three peak months of work if it has to pay compensation for the last mile delays. So, given an average of 25 GPs in each block, the monthly cost per block amounts to US\$33,000.

To calculate the total cost of the intervention, we need to gauge both the fixed and the variable costs. The fixed costs include a one-time lump sum payment of INR5,000 (~US\$70) for devising the phone call application to send out calls in the treated GPs. This covers the entire duration of the intervention over all 26 treated GPs (which is approximately the size of a block). The variable costs include remuneration for local people to crawl online administrative data and disseminate them in the form of posters to the designated GP locations. One person is required per block and, according to the local wage rate, their monthly salary is INR20,000 (~US\$300), including travel expenses. Posters should be put up as many times as wages are disbursed from the central office, and the intervention team reported that for each GP a maximum of five posters were needed to cover all the prime locations. Given the average printing cost per poster of around INR100 (~US\$1.4), the total monthly cost per GP is around INR500 (~US\$7), which equates to around US\$175 per block. The total cost of implementing the intervention per peak month per block is therefore US\$545. With other miscellaneous costs of US\$455, which include advertising job openings, this amounts to US\$1,000 per block. Moreover, this cost covers both the generalized and the personalized campaigns.

It should be noted that this estimate does not include the sunk costs of time spent by the research team to design the intervention and learn how to use the phone-calling application. Nevertheless, this indicates a monetary gain for the government of close to US\$32,000 every month for each block on average if it applies our intervention for the reduction of last mile delays. This is significant: the marginal gain for every dollar spent is close to US\$32. Please note that from the second month onwards, because the server fixed cost need not to be paid, this gain would be close to \$34.5. Additional benefits would include the estimated improvement in awareness of entitlements and process mechanisms that can be used to extract other benefits from the programme. The reduction in last mile payment delays could also encourage workers to apply for more work under the programme instead of migrating out for employment. Hence, to sum up, we argue that this intervention is highly cost-effective as well as effective.

8   Discussion and conclusion

One of the keys to the success of any social welfare programme is how it has been implemented at the local level. Implementation failures may undermine the programme and the beneficiaries may end up not getting optimal benefits from it. However, the delivery of complete and correct information to beneficiaries may bridge this implementation gap, which often arises because of information asymmetry. Information asymmetry in various contexts can be utilized by the local authorities for their own benefits at the cost of the intended beneficiaries.

This paper, which is based on a randomized experimental design, evaluates a novel intervention that accesses information from a public website and disseminates it to the beneficiaries of the MGNREGS programme. Of the final outcome variables, we observe a substantial drop in last mile payment delays due to the personalized information campaign of wage credit list posting.

---

[23] The circular is available at: https://nrega.nic.in/Circular_Archive/archive/Guidelines_Compensation_delayed_wages_pay.pdf (accessed 17 January 2021).

Generalized awareness campaigns had a limited impact, as we find no effect on uptake, though a modest positive impact on intermediate outcomes like improvements in awareness of entitlements under the programme and in the process mechanism itself is found. We also find no gain in terms of uptake through spillover effects, though we do find evidence of an 'encouragement' effect through an increase in uptake in the following year potentially due to the reduction in delays during the intervention period. In addition, a higher reduction of last mile delays for the deprived SC/ST population is observed due to the higher focus of the intervention on these groups.

One of the novelties of the intervention is the use of two dissemination channels: generalized and personalized. Generalized campaigns operated through phone calls and meetings, whereas personalized campaigns functioned through wage credit list posting. The results of the study show how effective these two channels were in impacting on the outcome variables. As hypothesized, the generalized channel can have an impact in terms of enhancement of awareness, which can improve the process mechanism and ultimately increase uptake. However, despite finding a significant effect on awareness and improvement of the process mechanism, we did not observe an associated increase in uptake from this channel. In contrast, the personalized channel had a direct causal impact on last mile delays, which might in turn increase uptake in the following year. This is contrary to the set of literature that finds no substantial effect of generalized awareness campaigns on welfare programmes and indicators, thus emphasizing the need for personal campaigns (Alik-Lagrange and Ravallion 2019; Seimetz et al. 2016; Staats et al. 1996).

Our finding that personalized campaigns are more effective is supported by the intensity of the intervention (a greater reduction in delays was observed in the months when list postings were highest) and also complemented by qualitative evidence. However, we acknowledge the possibility that the generalized interventions are driving part of the results, since the current experimental setting does not allow us to tease out the different drivers. Therefore, future researchers might consider using separate treatment arms, which we were unable to do in our experiment, largely for logistical reasons. This would open up the possibility of comparisons of impacts between targeted and generalized interventions.

The novelty of the intervention and the findings also revolve around two other positives. First, apart from programme benefits during the intervention period, we found evidence of a positive 'encouragement' effect of the intervention through increased uptake of the programme, which is pertinent since this is largely an indirect or a side benefit. Second, the intervention need not, of course, be limited to programmes like MGNREGS in Telangana and could be replicated for any other welfare programmes that generate publicly available micro-level data. For example, the Public Distribution System (PDS) in India offers public data that can be similarly used to empower beneficiaries. Hence, we recommend that similar interventions be used by CSOs, who can thereby engage with local stakeholders and disseminate information more efficiently. We expect that the gains from such interventions will be even greater, given the already established organizational structures at the local level of the CSOs.

References

Afridi, F., V. Iversen, and M.R. Sharan (2017). 'Women Political Leaders, Corruption, and Learning: Evidence from a Large Public Program in India'. *Economic Development and Cultural Change*, 66(1): 1–30. https://doi.org/10.1086/693679

Alik-Lagrange, A., and M. Ravallion (2019). 'Estimating Within-Cluster Spillover Effects using a Cluster Randomization with Application to Knowledge Diffusion in Rural India'. *Journal of Applied Econometrics*, 34(1): 110–28. https://doi.org/10.1002/jae.2658

Angrist, J.D., and J.S. Pischke (2008). *Mostly Harmless Econometrics: An Empiricist's Companion*. Cambridge, MA: Princeton University Press.

Banerjee, A., R. Hanna, J. Kyle, B.A. Olken, and S. Sumarto (2018). 'Tangible Information and Citizen Empowerment: Identification Cards and Food Subsidy Programs in Indonesia'. *Journal of Political Economy*, 126(2): 451–91. https://doi.org/10.1086/696226

Bardhan, P.K., and D. Mookherjee (2000). 'Capture and Governance at Local and National Levels'. *American Economic Review*, 90(2): 135–39. https://doi.org/10.1257/aer.90.2.135

Basu, P., R.R. Natarajan, and K. Sen (2020). 'Administrative Failures in Anti-Poverty Programmes and Household Welfare'. WIDER Working Paper 2020-41. Helsinki: UNU-WIDER. https://doi.org/10.35188/UNU-WIDER/2020/798-9

Benati, L. (2001). 'Some Empirical Evidence on the "Discouraged Worker" Effect'. *Economics Letters*, 70(3): 387–95. https://doi.org/10.1016/S0165-1765(00)00375-X

Björkman, M., and J. Svensson (2009). 'Power to the People: Evidence from a Randomized Field Experiment on Community-Based Monitoring in Uganda'. *The Quarterly Journal of Economics*, 124(2): 735–69. https://doi.org/10.1162/qjec.2009.124.2.735

Cameron, A.C., J.B. Gelbach, and D.L. Miller (2008). 'Bootstrap-Based Improvements for Inference with Clustered Errors'. *The Review of Economics and Statistics*, 90(3): 414–27. https://doi.org/10.1162/rest.90.3.414

Census of India (2011). 'Provisional Population Totals'. New Delhi: Office of the Registrar General and Census Commissioner. Available at: https://www.censusindia.gov.in/ (accessed 23 December 2020).

Chong, A., M. Gonzalez-Navarro, D. Karlan, and M. Valdivia (2013). 'Effectiveness and Spillovers of Online Sex Education: Evidence from a Randomized Evaluation in Colombian Public Schools'. NBER Working Paper 18776. Cambridge, MA: National Bureau of Economic Research.

Clark, K.B., and L.H. Summers (1981). 'Demographic Differences in Cyclical Employment Variation'. *Journal of Human Resources*, 16(1). https://doi.org/10.2307/145219

Cohen-Cole, E., J.M. Fletcher, Steptoe, and D. Roux (2009). 'Detecting Implausible Social Network Effects in Acne, Height, and Headaches: Longitudinal Analysis'. *British Medical Journal*, 338I: 28–31.

Coryn, C.L., and K.A. Hobson (2011). 'Using Nonequivalent Dependent Variables to Reduce Internal Validity Threats in Quasi-Experiments: Rationale, History, and Examples from Practice'. *New Directions for Evaluation*, 2011(131): 31–39. https://doi.org/10.1002/ev.375

Dal Bó, E., and F. Finan (2020). 'At the Intersection: A Review of Institutions in Economic Development'. In *The Handbook of Economic Development and Institutions*. Princeton: Princeton University Press. https://doi.org/10.1515/9780691192017-003

Das, S. (2016). 'Television is More Effective in Bringing Behavioral Change: Evidence from Heat-Wave Awareness Campaign in India'. *World Development*, 88: 107–21. K. https://doi.org/10.1016/j.worlddev.2016.07.009

Das, U. (2015). 'Can the Rural Employment Guarantee Scheme Reduce Rural Out-Migration? Evidence from West Bengal, India'. *The Journal of Development Studies*, 51(6): 621–41. https://doi.org/10.1080/00220388.2014.989997

Dasgupta, A., Gawande, and D. Kapur (2017). '(When) Do Antipoverty Programs Reduce Violence? India's Rural Employment Guarantee and Maoist Conflict'. *International Organization*, 71(3): 605–32. https://doi.org/10.1017/S0020818317000236

Deininger, K., and Y. Liu (2013). 'Welfare and Poverty Impacts of India's National Rural Employment Guarantee Scheme: Evidence from Andhra Pradesh'. Washington, DC: The World Bank. https://doi.org/10.1596/1813-9450-6543

Deshpande, A. (2011). *The Grammar of Caste: Economic Discrimination in Contemporary India*. New Delhi: Oxford University Press. https://doi.org/10.1093/acprof:oso/9780198072034.001.0001

Drèze, J., and A. Sen (2013). 'An Uncertain Glory: India and its Contradictions'. New York: Princeton University Press. https://doi.org/10.23943/9781400848775

Dutta, P., R. Murgai, M. Ravallion, and D. Walle (2012). 'Does India's Employment Guarantee Scheme Guarantee Employment?'. *Economic and Political Weekly*, 47(16): 55–64. https://doi.org/10.1596/1813-9450-6003

Haushofer, J., M. Chemin, C. Jang, and J. Abraham (2020). 'Economic and Psychological Effects of Health Insurance and Cash Transfers: Evidence from a Randomized Experiment in Kenya'. *Journal of Development Economics*, 144: 102416. https://doi.org/10.1016/j.jdeveco.2019.102416

Hidrobo, M., A. Peterman, and L. Heise (2016). 'The Effect of Cash, Vouchers, and Food Transfers on Intimate Partner Violence: Evidence from a Randomized Experiment in Northern Ecuador'. *American Economic Journal*, 8(3): 284–303. https://doi.org/10.1257/app.20150048

Imbert, C., and J. Papp (2015). 'Labor Market Effects of Social Programs: Evidence from India's Employment Guarantee'. *American Economic Journal: Applied Economics*, 7(2): 233–63. https://doi.org/10.1257/app.20130401

Jensen, R. (2007). 'The Digital Provide: Information (Technology), Market Performance, and Welfare in the South Indian Fisheries Sector'. *The Quarterly Journal of Economics*, 122(3): 879–924. https://doi.org/10.1162/qjec.122.3.879

Kaufmann, C., T. Müller, A. Hefti, and S. Boes (2018). 'Does Personalized Information Improve Health Plan Choices when Individuals are Distracted?'. *Journal of Economic Behavior & Organization*, 149: 197–214. https://doi.org/10.1016/j.jebo.2018.03.013

Khera, R., and N. Nayak (2009). 'Women Workers and Perceptions of the National Rural Employment Guarantee Act'. *Economic and Political Weekly*: 44(43): 49–57.

Liu, Y., and C.B. Barrett (2013). 'Heterogeneous Pro-Poor Targeting in the National Rural Employment Guarantee Scheme'. *Economic and Political Weekly*, 48(10): 46–53. Available at: https://www.epw.in/journal/2013/10/special-articles/heterogeneous-pro-poor-targeting-national-rural-employment (accessed 6 January 2021).

McKenzie, D. (2012). 'Beyond Baseline and Follow-up: The Case for More T in Experiments'. *Journal of Development Economics*, 99(2): 210–21. https://doi.org/10.1016/j.jdeveco.2012.01.002

Miguel, E., and M. Kremer (2004). 'Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities'. *Econometrica*, 72(1): 159–217. https://doi.org/10.1111/j.1468-0262.2004.00481.x

Mukhopadhyay, A., A.M. Himanshu, and M.R. Sharan (2015). 'The National Rural Employment Guarantee Scheme in Rajasthan: Rationed Funds and Their Allocation across Villages'. *Economic and Political Weekly*, 50(6). Available at: https://www.epw.in/journal/2015/6/special-articles/nregs-rajasthan.html (accessed 19 January 2021).

Muralidharan, K., P. Niehaus, S. Sukhtankar, and J. Weaver (forthcoming). 'Improving Last-Mile Service Delivery Using Phone-Based Monitoring'. *American Economic Journal*.

Nagavarapu, S., and S. Sekhri (2016). 'Informal Monitoring and Enforcement Mechanisms in Public Service Delivery: Evidence from the Public Distribution System in India'. *Journal of Development Economics*, 121: 63–78. https://doi.org/10.1016/j.jdeveco.2016.01.006

Nair, M., P. Ariana, E.O. Ohuma, R. Gray, B. De Stavola, and P. Webster (2013). 'Effect of the Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA) on Malnutrition of Infants in Rajasthan, India: a Mixed Methods Study'. *PloS One*, 8(9): p.e75089. https://doi.org/10.1371/journal.pone.0075089

Narayanan, S., U. Das, Y. Liu, and C.B. Barrett (2017). 'The "Discouraged Worker Effect" in Public Works Programs: Evidence from the MGNREGA in India'. *World Development*, 100: 31–44. https://doi.org/10.1016/j.worlddev.2017.07.024

Narayanan, R., S. Dhorajiwala, and R. Golani (2019). 'Analysis of Payment Delays and Delay Compensation in MGNREGA: Findings Across Ten States for Financial Year 2016–2017'. *The Indian Journal of Labour Economics*, 62(1): 113–33. https://doi.org/10.1007/s41027-019-00164-x

Pritchett, L. (2009). 'Is India a Flailing State?: Detours on the Four-Lane Highway to Modernization'. Harvard Kennedy School Working Paper RWP-09-013. Cambridge, MA: Harvard Kennedy School. https://doi.org/10.2139/ssrn.1404827

Protik, A.E., I. Nichols-Barrer, J. Berman, and M. Sloan (2018). 'Bridging the Information Gap between Citizens and Local Governments: Evidence from a Civic Participation Strengthening Program in Rwanda'. *World Development*, 108: 145–56. https://doi.org/10.1016/j.worlddev.2018.03.016

Ravallion, M., D. van de Walle, P. Dutta, and R. Murgai (2013). 'Testing Information Constraints on India's Largest Antipoverty Program'. Policy Research Working Paper. Washington, DC: The World Bank. https://doi.org/10.1596/1813-9450-6598

Seimetz, E., S. Kumar, and H.J. Mosler (2016). 'Effects of an Awareness Raising Campaign on Intention and Behavioural Determinants for Handwashing'. *Health Education Research*, 31(2): 109–20. https://doi.org/10.1093/her/cyw002

Skoufias, E. (2005). 'PROGRESA and Its Impacts on the Welfare of Rural Households in Mexico'. Research Report 139. Washington, DC: International Food Policy Research Institute.

Staats, H.J., A.P. Wit, and C.Y.H. Midden (1996). 'Communicating the Greenhouse Effect to the Public: Evaluation of a Mass Media Campaign from a Social Dilemma Perspective'. *Journal of Environmental Management*, 46(2): 189–203. https://doi.org/10.1006/jema.1996.0015

Stigler, G.J. (1961). 'The Economics of Information'. *Journal of Political Economy*, 69(3): 213–25. https://doi.org/10.1086/258464

Sundaram, K., and S.D. Tendulkar (2003). 'Poverty among Social and Economic Groups in India in 1990s'. *Economic and Political Weekly*: 37(50): 5263–76.

Appendix A: Theoretical framework of in payment delays

The theoretical framework can be conceptualized as follows. Consider M the amount of money that should be disbursed by the BPM but is instead held for time period t before being distributed to the beneficiaries. Hence, the BPM's earnings are the interest accruing, given by $I(t) = M(1 + r)^t - M$, where r is the interest rate and $r > 0$. Here, $I(t)$ is a convex function of t. Now consider that the probability of the BPM being caught and punished is given by $p(t)$, where $p'(t) > 0$, $p''(t) > 0$, and $p(t) \rightarrow 1$ for large t. The fine imposed is also assumed to be a function of t and is denoted by $F(t)$ such that $F'(t) > 0$ and $F''(t) > 0$. Hence, the expected fine at t would be $p(t).F(t)$. The BPM would delay till time t if $I(t) > p(t).F(t)$. The graphical representation of the same is shown in Figure A1.
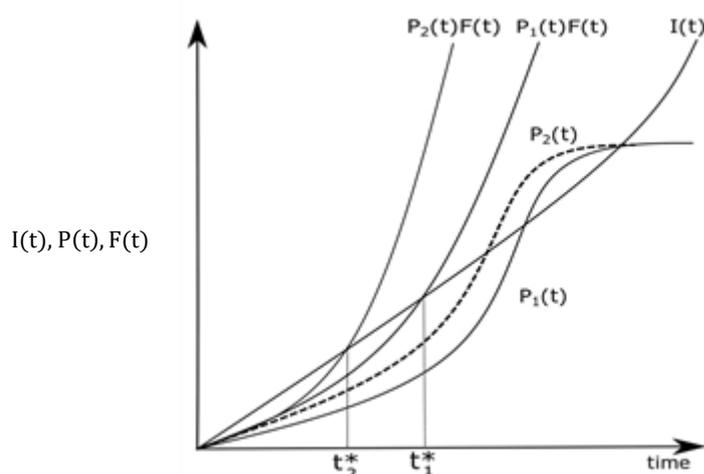
Figure A1: Theoretical framework of delay in payments

Source: authors' illlustration.

Here we consider two periods: pre-intervention and post-intervention, denoted by the subscript 1 and 2, respectively. t^*_1 is the equilibrium time until which the BPM would hold the money that needs to be distributed in the absence of treatment. Since the intervention essentially increases the level of $p(t)$, there would be an inward shift of $p(t).F(t)$ as well; hence, t^*_2 would be the new equilibrium during the intervention, which would shift to the left as the number of list postings in the GPs increases.

Appendix B: Basic characteristics of the selected blocks

Table B1: Basic characteristics of the selected blocks

Block	Proportion of SC/ST	Proportion of literates	Proportion of agricultural labourers	Proportion of casual labourers
Damaragidda	0.185	0.439	0.181	0.208
Maddur	0.164	0.459	0.160	0.167
Hanwada	0.147	0.489	0.222	0.143
Koilkonda	0.133	0.507	0.238	0.160

Note: SC/ST = Scheduled Caste/Scheduled Tribe. The four blocks were situated in the Mehbubnagar district of Telangana.

Source: Census (2011): https://censusindia.gov.in/2011-common/censusdata2011.html (accessed 2 July 2020)

Appendix C: Balance tests

Figure C1: Kernel density plots

(i) Total Scheduled caste population
kernel = epanechnikov, bandwidth = 166.7750

(j) Total female population
kernel = epanechnikov, bandwidth = 368.8292

(k) Distance from the nearest town
kernel = epanechnikov, bandwidth = 3.6658

(l) Nearest distance from block
kernel = epanechnikov, bandwidth = 3.5636

---------- Treatment ————Control
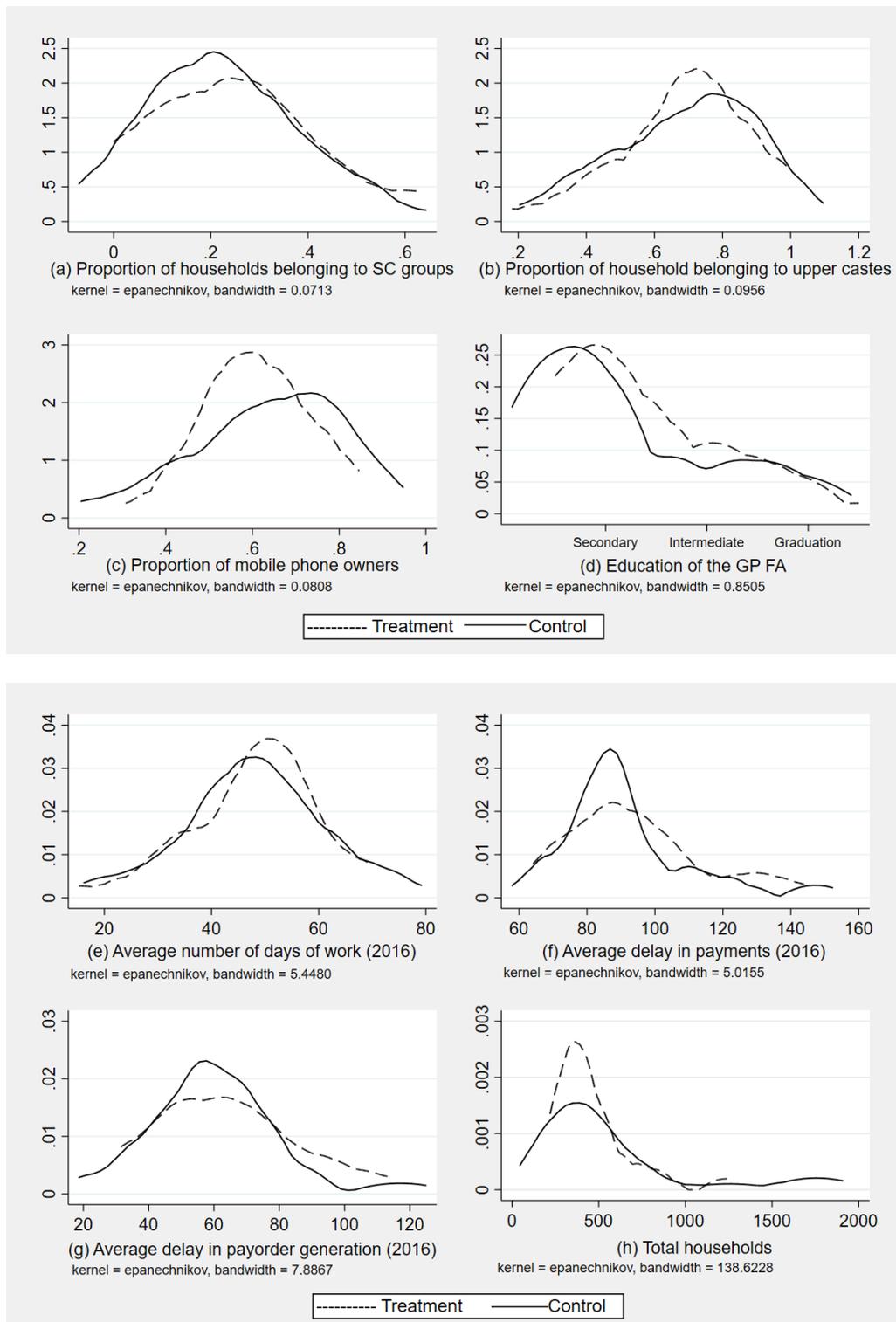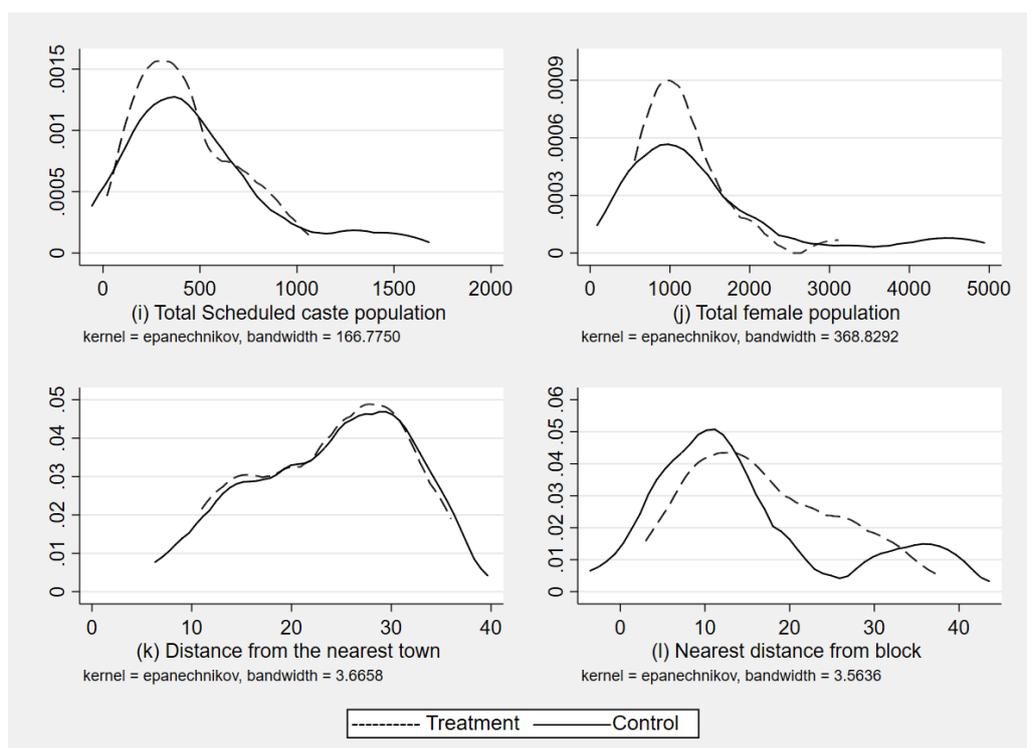
Note: 'Nearest distance from block' indicates the distance to the nearest block headquarters.

Source: authors' illustrations.

Table C1: Kolmogorov-Smirnov test results

Variables	Combined K-S statistic	P-value
From survey		
Average proportion of Scheduled Castes	0.142	0.966
Average proportion of Upper Castes	0.127	0.989
Average ownership of mobile phones	0.224	0.572
Education of FA	0.253	0.418
Average GP-level days of work	0.224	0.572
Average GP-level delay in payment	0.278	0.304
Average GP-level delay in FTO generation	0.139	0.973
From Census 2011		
Total number of households	0.141	0.973
Total SC population	0.192	0.779
Total female population	0.225	0.594
Distance from the nearest town	0.092	0.981
Distance from the block office	0.232	0.525

Note: we perform the Kolmogorov-Smirnov (K-S) test to examine if the distributions of the two groups are equal separately for the treated and control GPs. The null hypothesis is that the two groups are equal and we are unable to reject the null for any of the variables presented. SC = Scheduled Caste; FA = Field Assistant; GP = Gram Panchayat. The command ksmirnov in STATA 14 is used to obtain the K-S statistics and p-values.

Source: authors' calculations based on Census (2011) data (https://censusindia.gov.in/2011-common/censusdata2011.html (accessed 2 July 2020)) and data from the baseline survey conducted from September to October 2017.

Appendix D: Difference between resurveyed and non-resurveyed households

Table D1: Difference between resurveyed and non-resurveyed households

Variables	Not resurveyed in endline	Mean	Resurveyed in endline	Mean	Mean difference (2) - (4)
	(1)	(2)	(3)	(4)	(5)
Selected outcome variables and household characteristics					
Work entitlement	92	0.685	1352	0.632	0.052
Work application	92	0.207	1352	0.237	-0.031
Unemployment allowance	92	0.033	1352	0.048	-0.015
Payment duration	92	0.152	1352	0.104	0.048
Wage rate	92	0.033	1352	0.037	-0.004
Job card updated by Field Assistant	65	0.246	992	0.298	-0.052
Got receipt for application	92	0.217	1352	0.180	0.037
Travelled more than once for wages	92	0.880	1352	0.866	0.014
Attended Gram Sabha meeting	87	0.299	1281	0.335	-0.036
Attended social audit meetings	91	0.165	1341	0.192	-0.028
Raised issues on MGNREGA	92	0.076	1352	0.095	-0.019
Non-cemented house	92	0.413	1352	0.385	0.028
Gender of the respondent	84	1.512	1352	1.488	0.024
Education of the respondent	84	0.571	1347	0.868	-0.296
MGNREGS work days of the respondent	84	25.09	1347	28.99	-3.893
Occupation: agriculture worker	92	0.815	1352	0.797	0.018
Occupation: casual laborer	92	0.707	1352	0.696	0.011
Scheduled Caste/ Scheduled Tribe	92	0.315	1352	0.258	0.057
Livestock	92	0.989	1352	1.457	-0.468**
Watches television	92	0.522	1349	0.560	-0.039
Flush toilet	92	0.207	1352	0.245	-0.038
Government/Private toilet	92	0.293	1352	0.246	0.048
Main water source	92	0.935	1352	0.918	0.017
Main cooking source	92	0.098	1352	0.107	-0.009
Number of boys	92	0.891	1351	0.967	-0.076
Number of girls	92	0.913	1352	0.757	0.156

Note: mean difference test using ttest command in STATA 14 is used for computation. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Source: authors' calculations based on data from baseline survey conducted from September to October 2017.

Appendix E: Robustness checks

Table E1: Placebo test: regression results

Awareness indicators

	Work entitlement	Work application	Unemployment allowance	Payment duration	Wage rate	Delay compensation
	(1)	(2)	(3)	(4)	(5)	(8)
Fake randomized treatment	0.013	0.006	-0.003	0.017	0.008	0.015
	(0.030)	(0.043)	(0.010)	(0.032)	(0.020)	(0.013)

Process mechanism improvement and meeting attendance indicators

	Job card updated by FA	Got receipt for work	Travelled more than once for wages	Attended GS meetings	Attended social audit meetings	Raised issue on MGNREGS
	(1)	(2)	(4)	(5)	(6)	(7)
Fake randomized treatment	0.006	0.01	-0.011	0.059*	0.017	-0.027
	(0.057)	(0.033)	(0.032)	(0.033)	(0.034)	(0.027)

Note: the following control variables were incorporated in all the regressions: respondent gender, age, education, SC/ST, number of adults in the household, type of house (non-cemented or not), land cultivated in acres, total number of livestock (cows, bullocks, and oxen), whether household has a toilet, and whether its members watch TV, along with main occupation of the household and block dummies. The regressions were run on sampled job cards from all the control GPs. The marginal effects from the ANCOVA pooled probit regression are reported and the bootstrapped standard errors clustered at the GP level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, *$p<0$.

Source: authors' calculations.

Table E2: Heterogeneous impact on uptake for mobile phone owners and literates

	Impact on literates	Impact on mobile phone owners
Treatment	0.153	0.204
	(0.263)	(0.319)
Post	-0.396***	-0.372**
	(0.150)	(0.163)
Literate	-0.539**	
	(0.256)	
Post*Treatment	-0.253	-0.192
	(0.208)	(0.227)
Literate*Treatment	0.077	
	(0.309)	
Literate*Post	0.327	
	(0.231)	
Literate*Post*Treatment	0.132	
	(0.313)	
Mobile*Treatment		-0.083
		(0.239)
Mobile*Post		0.081
		(0.192)
Mobile*Post*Treatment		-0.032
		(0.238)
Constant	3.054***	2.939***
	(0.327)	(0.362)
Observations	1308	1314

Note: since the outcome variable is defined at household level, we used only the household-level control variables. The following control variables were incorporated in all the regressions: education, SC/ST, number of adults in the household, type of house (non-cemented or not), land cultivated in acres, total number of livestock (cows, bullocks, and oxen), whether household has a toilet, and if its members watch TV, along with main occupation of the household and block dummies. The marginal effects from the ANCOVA pooled probit regression are reported along with the bootstrapped standard errors clustered at the GP level in parentheses.
*** $p<0.01$,** $p<0.05$, * $p<0.1$.

Source: authors' calculations.

Table E3: Heterogeneous impact on awareness for literate population

	Work entitlement (1)	Work application (2)	Unemployment allowance (3)	Payment duration (4)	Wage rate (5)	Delay compensation (8)
Comparison of treated GPs with control GPs and additional control GPs						
Treatment	0.119***	0.241***	0.139***	0.225***	0.193***	0.166***
	(0.043)	(0.050)	(0.022)	(0.057)	(0.032)	(0.022)
literate	-0.015	0.124***	-0.030	0.070**	-0.018	0.010
	(0.029)	(0.039)	(0.034)	(0.033)	(0.025)	(0.023)
Interaction	0.011	-0.121*	0.048	-0.074	0.053	0.007
	(0.074)	(0.066)	(0.036)	(0.065)	(0.043)	(0.026)
Comparison of treated GPs with control GPs only						
Treatment	0.117***	0.229***	0.236***	0.232***	0.260***	0.358***
	(0.040)	(0.051)	(0.036)	(0.058)	(0.041)	(0.081)
literate	0.052	0.071	-0.062	0.121*	0.108**	0.027
	(0.061)	(0.092)	(0.086)	(0.063)	(0.048)	(0.039)
Interaction	-0.051	-0.077	0.101	-0.11	-0.049	-0.069
	(0.088)	(0.106)	(0.086)	(0.085)	(0.067)	(0.054)

Note: the marginal effects from the ANCOVA pooled probit regression are reported along with the bootstrapped standard errors clustered at the GP level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Source: authors' calculations.

Table E4: Heterogeneous impact on process mechanisms and attendance in meetings for literate population

	Job card updated by FA (1)	Got receipt for work (2)	Travelled more than once for wages (3)	Attended GS meetings (4)	Attended social audit meetings (5)	Raised issue on MGNREGS (6)
Comparison of treated GPs with control GPs and additional control GPs						
Treatment	-0.023	0.070*	-0.100**	0.127**	0.139***	0.085***
	(0.070)	(0.038)	(0.045)	(0.051)	(0.047)	(0.032)
literate	-0.103**	0.007	0.004	0.088**	0.064**	0.034
	(0.049)	(0.028)	(0.033)	(0.044)	(0.031)	(0.032)
Interaction	0.028	0.097*	0.01	-0.016	0.062	0.002
	(0.079)	(0.052)	(0.055)	(0.071)	(0.060)	(0.047)
Comparison of treated GPs with control GPs only						
Treatment	-0.030	0.089**	-0.115**	0.116**	0.170***	0.132***
	(0.070)	(0.042)	(0.050)	(0.054)	(0.048)	(0.045)
literate	-0.089	0.056	-0.032	-0.014	0.064	0.054
	(0.089)	(0.061)	(0.091)	(0.093)	(0.051)	(0.081)
Interaction	0.051	0.054	0.063	0.089	0.053	-0.002
	(0.107)	(0.074)	(0.097)	(0.104)	(0.069)	(0.086)

Note: the marginal effects from the ANCOVA pooled probit regression are reported along with the bootstrapped standard errors clustered at the GP level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Source: authors' calculations.

Table E5: Heterogeneous impact for mobile phone owners on awareness

	Work entitlement	Work application	Unemployment allowance	Payment duration	Wage rate	Delay compensation
	(1)	(2)	(3)	(4)	(5)	(8)
Comparison of treated GPs with control GPs and additional control GPs						
Treatment	0.132**	0.255***	0.181***	0.208***	0.160***	0.190***
	(0.057)	(0.070)	(0.029)	(0.068)	(0.047)	(0.036)
Possess a mobile phone	0.131***	-0.033	0.063**	0.112***	0.063***	0.038
	(0.027)	(0.037)	(0.028)	(0.033)	(0.022)	(0.024)
Interaction	-0.02	-0.071	-0.054*	-0.001	0.069	-0.038
	(0.066)	(0.068)	(0.030)	(0.063)	(0.044)	(0.026)
Comparison of treated GPs with control GPs only						
Treatment	0.068	0.212**	0.329***	0.208***	0.111*	0.337***
	(0.061)	(0.084)	(0.061)	(0.073)	(0.057)	(0.095)
Possess a mobile phone	0.024	-0.1	0.135*	0.136**	-0.063	-0.043
	(0.041)	(0.072)	(0.073)	(0.057)	(0.047)	(0.059)
Interaction	0.068	-0.001	-0.122*	-0.001	0.214***	0.007
	(0.069)	(0.091)	(0.073)	(0.076)	(0.063)	(0.066)

Note: the marginal effects from the ANCOVA pooled probit regression are reported along with the bootstrapped standard errors clustered at the GP level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Source: authors' calculations.

Table E6: Heterogeneous impact for mobile phone owners on process mechanisms and attendance in meetings

	Job card updated by FA	Got receipt for work	Travelled more than once for wages	Attended GS meetings	Attended social audit meetings	Raised issue on MGNREGS
	(1)	(2)	(3)	(4)	(5)	(6)
Comparison of treated GPs with control GPs and additional control GPs						
Treatment	-0.126	0.078	-0.051	0.112*	0.100	0.112**
	(0.086)	(0.057)	(0.055)	(0.064)	(0.066)	(0.048)
Possess a mobile phone	0.182***	0.169***	-0.051	0.065*	-0.025	-0.011
	(0.046)	(0.033)	(0.033)	(0.036)	(0.035)	(0.031)
Interaction	0.186**	0.027	-0.072	0.019	0.089	-0.044
	(0.091)	(0.062)	(0.065)	(0.069)	(0.072)	(0.056)
Comparison of treated GPs with control GPs only						
Treatment	-0.236***	0.078	-0.066	0.101	0.217***	0.260***
	(0.090)	(0.076)	(0.068)	(0.069)	(0.071)	(0.064)
Possess a mobile phone	-0.007	0.172**	-0.051	0.052	0.145**	0.119**
	(0.074)	(0.081)	(0.069)	(0.067)	(0.067)	(0.047)
Interaction	0.368***	0.035	-0.054	0.055	-0.054	-0.205***
	(0.096)	(0.100)	(0.092)	(0.086)	(0.093)	(0.076)

Note: the marginal effects from the ANCOVA pooled probit regression are reported along with the bootstrapped standard errors clustered at the GP level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Source: authors' calculations.