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Using information and technology to improve the efficacy of welfare programmes: evidence from a field experiment in India

Upasak Das[1]

[1] GDI, University of Manchester, UK; Center For Social Norms and Behavioural Dynamics, University of Pennsylvania, USA

Email: upasak.das@manchester.ac.uk

Amartya Paul[2]

[2] Centre for Development Studies, Trivandrum, India

Mohit Sharma[3]

[3] Collaborative Research and Dissemination (CORD), New Delhi, India

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Abstract

Does information dissemination among beneficiaries of welfare programmes mitigate such programmes' failures of implementation? We present experimental evidence on this in the context of the rural public works programme in India. We assess the impact of an intervention that involved dissemination of publicly available micro-level data on last mile delays in payments and on the uptake of work, along with a set of intermediate outcomes. We found a substantial reduction in last mile payment delays along with improvements in awareness of the basic provisions of the programme and its process mechanisms, while observing a limited effect on the uptake of work. However, we found a considerable increase in uptake in the subsequent period, potentially indicative of an 'encouragement' effect as a result of the reduction in last mile delays. A comparatively higher impact of payment delays was found in deprived communities. The findings lay a platform for an innovative information campaign that could be used by government and civil society organisations as transparency measures to improve efficiency.

Keywords

Information, welfare programme, implementation, randomisation, delay of payments, MGNREGS

JEL Codes

I30, I38, H75

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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 not producing the desired impact (Bardhan & Mookherjee, 2000; Skoufias, 2005; Pritchett, 2009; Narayanan et al, 2017). One key reason identified for the prevalence of such failures has been the dearth of correct information among the beneficiaries, which makes it difficult for them to hold functionaries accountable (Drèze & Sen, 2013). It is often argued that information plays an important role in better public service delivery that otherwise suffers because of the rent-seeking behaviour of the implementing authorities. This largely happens as a result of multiple information asymmetries, often utilised by the latter for their own benefit, resulting in hefty welfare losses for the intended beneficiaries (Banerjee et al, 2018). Accordingly, the literature has emphasised the pivotal role of information in the efficient functioning of the markets and proper provisioning of public goods and services (Stigler, 1961; Jensen, 2007; Bó & Finan, 2020; Protik et al, 2018).

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 such demands without the right incentive mechanism or sanctions. Hence, gauging whether dissemination of information improves service delivery depends on the context, along with how the information gets disseminated. The literature has found mixed evidence on this. For example, Banerjee et al (2018) found dissemination of information increased receipts of benefits in a subsidised rice programme in Indonesia. However, Ravallion et al (2013) found no such effect 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), a public works programme implemented in India since 2005. More specifically, the intervention harnesses the public micro-level administrative records of the programme available online and disseminates personalised information to its putative beneficiaries or groups of beneficiaries. The main component of the intervention is as follows: once information on wages is updated when these are credited to people's bank or postal accounts, the names of the relevant individuals are listed and then pasted at core junctions in the village. In addition, through local meetings and mobile phone calls, messages on various provisions of the programme are sent. This intervention was rolled out randomly in parts of the southern state of Telangana. We make use of this randomised design and examine the impact of the intervention on two main outcomes related to the programme, namely delayed payments and uptake in terms of days worked in addition to the associated intermediate outcomes. The design of the intervention and survey also allowed us to look at the effect of spill-overs from the intervention.

The findings reveal a substantial reduction in the last mile (ie final stage of the process) delay in payments thanks to wage credit list pasting but only a limited impact in reducing payment delays occurring at higher levels. Interestingly, the gains were found to converge to the pre-intervention level within three months of its conclusion. The average effect on uptake of the programme during the intervention was found to be insignificant. Nevertheless, we found a significant gain in average uptake in the period after the intervention – potentially because of a reduction in last mile payment delays – through a plausible 'encouragement effect'. In terms of intermediate outcomes, we observed a significantly positive impact on including awareness indicators and those related to the process mechanism, among others. Further, we found modest spill-over effects from the intervention on these intermediate outcomes, although no impact was observed on uptake. Notably, a higher reduction in the last mile delay for deprived communities was observed than for other communities.

The paper contributes to five strands of literature. First, it provides evidence that technology-based interventions can be effective in improving the efficacy of safety net programmes. This works through direct dissemination of information to the beneficiaries, as well as encouraging them to hold the implementing authorities to greater account (Björkman & Svensson, 2009; Nagavarapu & Shekri, 2016). In this respect, improving last mile service delivery becomes important and our paper complements that by Muralidharan et al (forthcoming), who found significant gains from the reduction in payment delays under a cash transfer programme implemented in Telangana in 2018. Second, we contribute to the existing literature on the impact of the type of information campaign that is effective (Das, 2016; Banerjee et al, 2018; Kaufmann et al, 2018; Alik-Lagrange & Ravallion, 2019). Our findings reveal the limited effect of direct generalised awareness campaigns while observing positive evidence on the effectiveness of the more personalised ones. Third, the design of the survey and randomisation allowed 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 spill-overs from the treatment. Hence, the paper contributes to the set of literature that examines the spill-over effects of welfare interventions (Miguel & Kremer, 2004; Chong et al, 2013; Alik-Lagrange & Ravallion, 2019). Fourth, it presents indicative evidence of a potential 'encouragement effect' similar to the 'discouraged worker effect' that has been discussed in the literature (Miner, 1966; Clark & Summers, 1981; Benati, 2001; Narayanan et al, 2017). This emanates from the fact that we observed an increase in uptake in terms of days of work in the subsequent period, potentially because of the reduction in last mile payment delay during the intervention period. Finally, the study also contributes to the growing research on MGNREGS and related welfare programmes and shows ways to improve its implementation and service delivery. On this note, the significance of the study lies in finding ways to increase accountability among local-level implementers. Accordingly, the intervention may be a useful alternative for civil society organisations (CSOs) and other programme implementing authorities to engage in better public service delivery.

The structure of the paper is as follows. Section 2 describes the MGNREGS programme in brief. Section 3 gives a description the intervention's design and of the mechanisms through which it may lead to the desired outcomes, while section 4 presents the study design along with a discussion on the data, variables and process of randomisation. Section 5 discusses the estimation strategy and the next section (6) presents the main findings from the regressions and the analysis. Section 7 examines the intervention in terms of its cost-effectiveness. The final section (8) concludes with a discussion on the potential takeaways and policy recommendations.

2 MGNREGS

MGNREGS was introduced on 23 August 2005 and initially implemented in 200 rural districts in India. Since 2008, it has been extended to all the rural parts of the country. Under the programme, any adult from a household living in rural areas, 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. Persons willing to work in this way have to apply for registration. After verification of the place of residence and age of the relevant adult, the household is issued a job card, which is mandatory under the programme. An application has to be made if the household wants work, indicating the time and duration of the work. Against this application, work is to be provided within 15 days, failing which an unemployment allowance must be paid. Further, the wages have to be paid within 15 days after completion of the work; otherwise delayed payment compensation needs to be paid. The democratically elected village head and his/her office is normally responsible for implementation of the programme at the Gram Panchayat (GP) level.[1] However, in the state of Telangana, the responsibility lies with an employee of the state government called the Field Assistant (FA).

A number of studies have examined the programme's welfare impacts on indicators related to poverty, women's empowerment, nutrition, education and reduction in distress-driven migration, among others (Khera & Nayak, 2009; Deininger & Liu, 2013; Nair et al, 2013; Das, 2015; Imbert & Papp, 2015; Afridi et al, 2017; Dasguptaet al, 2017). Nevertheless, studies have also documented the programme's administrative problems, including high unmet demand and delayed payments, which have undermined its potential benefits (Dutta et al, 2012; Liu & Barrett, 2013; Narayanan et al, 2017; Narayanan et al, 2019). Because a dearth of information stands as a major reason why such failures occur, this intervention intends to enhance awareness, providing information about process failures and disseminating personalised information on wage credits that can enable the beneficiaries to hold the local authorities accountable. A

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

detailed explanation of the intervention, along with its various mechanisms and potential outcomes, 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 in the Damaragidda and Maddur blocks, part of the Mahbubnagar district in Telangana. It was called *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 were as follows. First, information about various rights and entitlements guaranteed under MGNREGS were disseminated through periodic voice broadcasts over mobile phones. These broadcasts included information on different general processes that could help workers to access their entitlements. Local-level meetings were also arranged with the intervention team to discuss these provisions in detail.

One important part of the intervention involved pasting personalised wage credited information at core junctions in the villages (GP headquarters or market place) and publicising the information through voice broadcasts over mobile phones. The objective of the exercise was to reduce the last mile delay in disbursement of MGNREGS wages after these were credited to workers' accounts. The delay in this last mile wage disbursement was happening because workers were often not aware when their wages were credited to their accounts and officials were using this to their own advantage. Branch Post Masters (BPMs) might take the opportunity to collect cash from their office and keep it themselves for an extended period before disbursement, in order to meet their personal needs. Further, in the absence of the information on whether wages had been credited to their account, the beneficiaries were making multiple visits to banks or post offices in anticipation of the money, foregoing the labour market wages that they would otherwise have earned had they not visited. In this situation, timely dissemination of personalised information when the wage is credited can potentially enhance transparency and hence accountability among the BPMs, while also enabling the workers to avoid making multiple trips to the bank. Notably, along with the pastings at core junctions, the wage lists were also pasted in the localities where deprived communities – the Scheduled Castes and Scheduled Tribes (SCST) – are located. This was to ensure inclusiveness, since households belonging to the SCST community have historically been found to lag behind non-SCST households in terms of various indicators of welfare (Sundaram & Tendulkar, 2003). An attempt was thus made to avoid the possibility of these socially ostracised communities not receiving the information because of their lower access to the core junctions.

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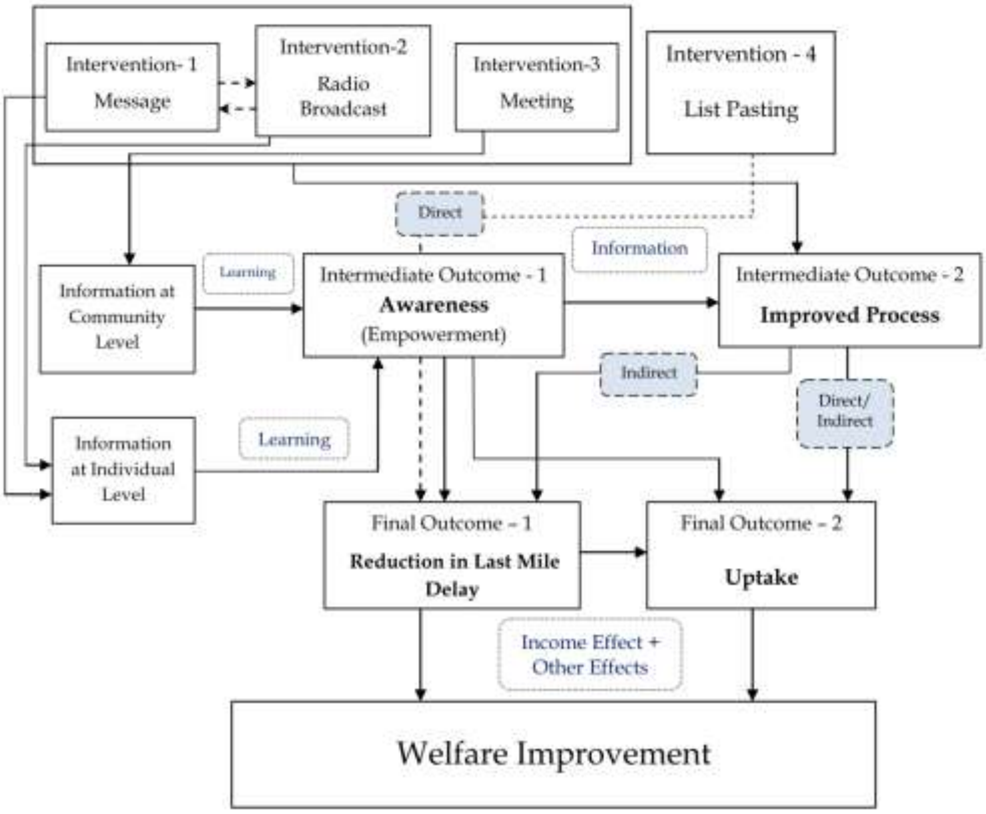
[2] Currently these blocks come under the Narayanpet district. More information on LibTech can be found on the website http://libtech.in/. Accessed: 10 July 2020.

3.2 Mechanisms

As discussed, the intervention consisted of four components: (1) radio broadcasts via a phone call; (2) delivery of phone messages; (3) local meetings; and (4) pasting personalised wage credit lists. The first three components are the more generalised items, which discussed the basic provisions of the programme and means of grievance redress. In terms of their possible impact on outcomes, these three components could potentially increase awareness about programme entitlements through individual and community-level interactions. This would act as a catalyst to improve process mechanisms through the channel of higher accountability and learning. For example, a more aware individual might raise more grievances about the way MGNREGS was implemented in the village. This, in turn, might encourage the beneficiaries to demand more work and also insist on payments being made on time. Both these outcomes – uptake and reduction in delay in payments – could lead to improvements in welfare outcomes.

The fourth type of intervention, which involved pasting a list of wage credits and was more personalised in nature, was also able to have a bearing on uptake and reduction in delayed payments through a number of direct and indirect channels. The direct channel through which it could lessen delayed payments lay in reducing information asymmetry among the beneficiaries with regards to wage credit information. An indirect channel of collective bargaining power can also be hypothesised, because the list pasting exercise may have enabled the beneficiaries to collectively demand faster payments. In fact, a reduction in delays in payment could then potentially encourage workers to demand more work under the programme and perhaps in the subsequent period. A detailed portrayal of these mechanisms is presented in Figure 1.

Figure 1: Mechanisms from intervention to outcomes

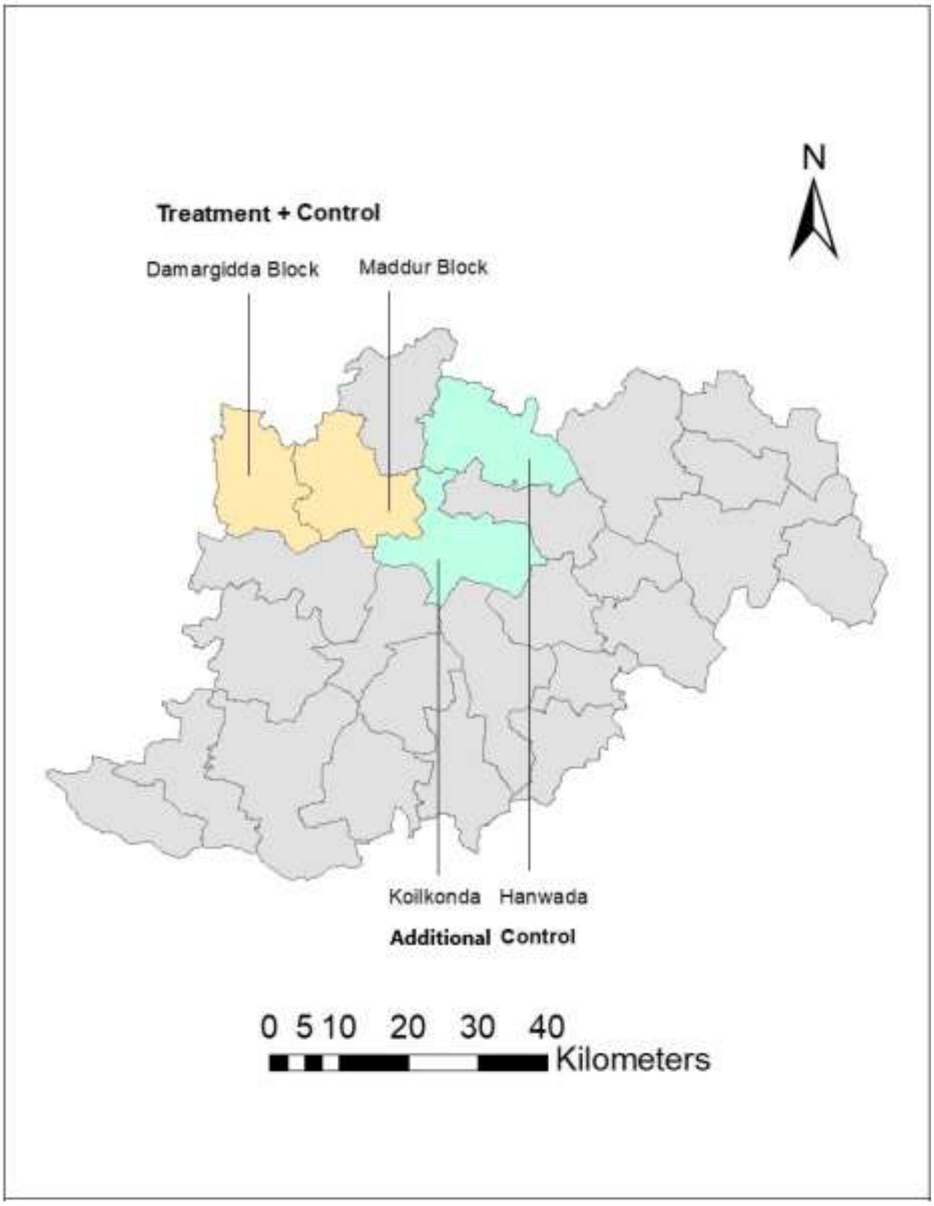


4 Study design, data, variables and randomisation process

4.1 Study design

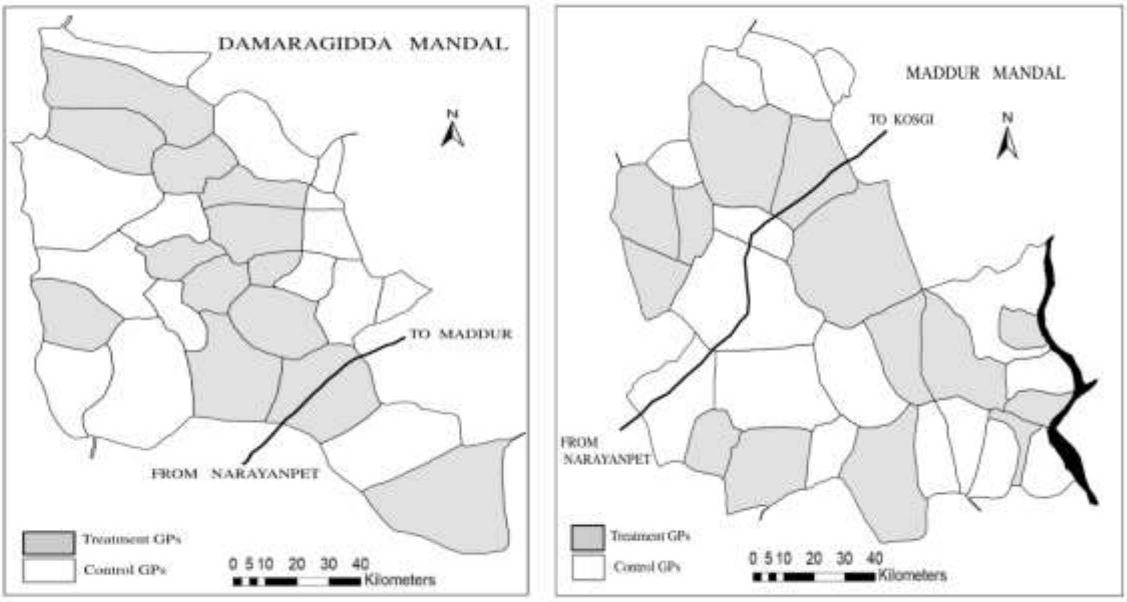
The intervention was rolled out randomly at the GP level in the Damaragidda and Maddur blocks, where the randomisation was stratified across the blocks. Accordingly, the intervention was implemented in 12 randomly selected GPs out of the 22 GPs in the Damaragidda block and 14 out of 27 GPs in the Maddur block. It should be noted that we left out Mogala Madaka GP in the Damaragidda block from evaluation as it has been adopted by the local Member of Parliament. Hence, the 26 GPs formed our intervention group and the remaining GPs in these two blocks constituted the control group (23 GPs). We further considered two other blocks within the Mahbubnagar district, Hanwada and Koilkonda, broadly based on their similar geographic and demographic characteristics. These two blocks are close to Damaragidda and Maddur and, in terms of population characteristics, they are similar as well. Since these blocks did not undergo any intervention at all, the GPs in them form our other set of controls, which we refer to as 'additional controls'. The basic characteristics of these four blocks, taken from the 2011 Census conducted by the Indian government, can be provided on request. The block map of the Mahbubnagar district is shown in Figure 2, which also highlights the four studied blocks. The GPs intervened in, along with the two sets of control GPs from these four blocks, are shown in Figure 3.

Figure 2: Geographical location of the selected blocks



Source: Census (2011). Maps not to scale.

Figure 3: Geographical location of the GPs receiving the intervention



Source: Census (2011). Maps not to scale.

It may be noted that the control GPs located within the same intervention block were closer to the treated GPs, leading to a possibility of flow or spill-over of the intervention from the beneficiaries into these GPs. For example, the information disseminated as a part of the intervention could have been shared with the villagers in the adjoining, non-intervened GPs because of the proximity of the two sets of GPs. Hence, gains from some of the interventions in the treatment GPs may have flowed to the adjoining control GPs within the same block. However, the chances of spill-overs in GPs located in Hanwada and Koilkonda blocks were negligible because of the lower level of interactions between individuals from two different blocks. Therefore we assumed that spill-overs could flow across GPs within the same block and not across the blocks. Notably, spill-overs would only be possible for generalised messages disseminated during the local meetings or voice broadcasts over phones, not through personalised wage credit lists, because of the nature of the intervention.

4.2 Data

We used data primarily from the administrative website of the programme in Telangana from the period January 2017 to December 2018.[3] Specifically, data on delays in payments along with days of work for all the job cards from the intervention and control GPs (28,984 job cards in total) were used. In addition, we also conducted two waves of household survey to gauge the impact on intermediate outcomes. The baseline survey was conducted from September to 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 surveyed in the

[3] The website used is at http://www.nrega.telangana.gov.in/Nregs/. Last accessed: 8 June 2020.

baseline survey were also surveyed during the endline survey. Additionally, a midline survey was conducted to take stock of the nature and status of the intervention and also to contextualise the empirical findings from the regressions.

For the baseline survey, among job card holders within each GP, roughly 15 households (calculated from power calculations) were randomly chosen from a list of households that had worked at least once in 2016–17. The total number of GPs surveyed from the four blocks in each wave was 96 and the total number of households surveyed in the baseline and endline surveys was 1,444 and 1,352, respectively. Some households were left out in the second phase, since the respondents could not be found, even after three visits. To ensure that the sample of non-resurveyed households was random, we compared their characteristics with those that had been resurveyed. The results, which can be provided on request, indicate no major difference in the characteristics; hence, the sample of households which we were able to resurvey can be treated as a random sample. One could argue that our sample size is too low to yield unbiased estimates; however, we were using the full population of job card holders to determine the effect on last mile payment delays and uptake. We used 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 was adequately powered (power=0.8).

The survey questionnaire covered a wide range of household information, eg demographic and socioeconomic, with a detailed set of information on MGNREGS. Apart from general questions on the programme, some specific questions were asked in order to get a clear picture of beneficiaries' awareness of the scheme and their entitlements (such as compensation for payment delays, the unemployment allowance, minimum days of work entitlement and wage rates, among others). We also endeavoured to obtain process-related information, including that on bank and postal accounts and households' attendance at local-level meetings. In addition, we collected information about the FAs, as well as on the salient characteristics of the GPs. During the endline survey, we gathered further information on intervention-related questions from those households in the GPs undergoing the intervention. This included qualitative and subjective questions on their perceptions of the intervention and its effects on MGNREGA participation and delays in payments.

The tablet-based survey was executed by using a Google form in the first phase. However, in the second phase we used KoBoToolbox, an android based Open Data Kit (ODK)-interface application developed by the Harvard humanitarian initiative.[4] 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. A midline qualitative survey was conducted in two treatment GPs each from the Maddur and Damaragidda blocks to understand and take stock of the intervention process from the beneficiaries' viewpoint as well as from that of the intervention implementing authorities.

[4] More information can be obtained at https://www.kobotoolbox.org/. Accessed: 30 June 2019.

Separate interviews with the two local intervention functionaries at the block level were conducted, as well as one with the person who supervised them.

4.3 Variables

As indicated, the two main outcome variables were last mile delay in payments and uptake under the programme in terms of days of work. Uptake in terms of number of extra days worked was obtained directly from the online administrative data portal for every job card across the months and years. The last mile payment delay was calculated as the difference in days between the date of wage debit and the credit date in the post office or bank account (detailed explanation in section 6.1). This revolved around the assumption that, whenever the beneficiaries knew that wages had been credited, they would go ahead and withdraw the money. We made this assumption because the beneficiaries are poor and have a high marginal utility of money, a disproportionately high disutility of delay and a low propensity to save. Nevertheless, even if we consider some bias in our measurement of last mile delay, the causal estimates remain unbiased thanks to the random assignment of the intervention across the GPs.

The intermediate outcomes included six indicators of awareness levels: (1) whether the respondent knew about the work entitlement of 100 days every year for each household; (2) whether the respondent knew about the process of applying for work in MGNREGS; (3) whether the respondent knew about the unemployment allowance given if they did not receive work; (4) whether the respondent knew about the number of days after completion of work within which the payment had to be made (15 days); (5) whether the respondent knew the correct wage rate (Rs197 = ~US$2.8 for the baseline and Rs205 = ~$3) for the endline); and (6) whether the respondent knew about compensation for delays. These outcomes are binary in nature.[5]

The other set of intermediate variables involved process-related information about MGNREGS and included: (1) whether the job card had been updated by the FA in the year before the survey; (2) whether a receipt was received for a work application in the year before the survey; (3) whether the respondent had to travel more than once to withdraw wages from a bank or postal account the last time they worked; (4) whether any household members attended the Gram Sabha (GS) meetings;[6] (5) whether any members attended the social audit meetings; and (6) whether concerns about MGNREGS were raised in the GS meetings. All these six indicators are dichotomous in nature.

In the regressions to estimate the impact on intermediate variables, we included a set of control variables measured during the baseline survey to increase the precision of the estimates. To capture the economic conditions of the households, we used variables like

[5] Based on the distribution, we considered the range of Rs180–200(~ $2.5–2.9) as the correct wage during the baseline and Rs202–220 (~ $2.9–$3) during the endline survey.

[6] The Gram Sabha (GS) is a forum which is used by villagers to discuss local governance and make needs-based plans for the village.

type of household (cemented or non-cemented), land cultivated by the household in acres, number of livestock such as oxen, bullocks and cows, main occupation of the household (casual labour or not) and whether the household had a toilet or not. In addition, whether household members watched television and the number of adult members in the household were included in the regression. Since caste is one of the major barriers to social inclusion, we also asked whether the household belonged to the SCST community (Sundaram & Tendulkar, 2003; Deshpande, 2011). For the estimation of intermediate variables, which are at the respondent level, we controlled for gender, age and education of the respondent, along with possession of mobile phones.

To ensure success of the randomisation procedure for the sample of 1,352 respondents surveyed across two waves, we compared the baseline characteristics across the respondents from the treated and control GPs. Table 1 presents the results from the difference in means test between the respondents from the two groups. We found none of the mean levels of the 12 outcome variables to be statistically significant at the 5% level. We looked at 17 control variables, including the characteristics of the respondent and their household. Four variables (proportion of respondents who were illiterate, mean age, proportion of non-cemented houses and proportion of households whose main occupation was casual labour) were found to be significantly different in the treatment and control arms. While this imbalance is likely to have biased the estimate, our regression strategy controls for these household and respondent characteristics and also the outcome variable measured during the baseline survey, along with the block fixed effects. Hence we minimised the bias when we estimated the impact of the treatment. We conducted other tests for balance, including kernel density plots and the Kolmogorov Smirnov tests, the results of which can be provided on request.

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

	Observations	Control	Observations	Treatment	Difference
	(1)	(2)	(3)	(4)	(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 update 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 banks/post offices	302	0.901	316	0.915	-0.014
Attendance at GS meetings	282	0.319	324	0.34	-0.02
Attendance at 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
SCST	312	0.244	348	0.276	-0.032
Number of adults in hh	312	3.875	348	3.92	-0.045
Non-cemented house	312	0.333	348	0.247	0.086**
Land cultivated (acres)	312	3.128	348	3.205	-0.077
Cows, oxen and buffaloes	312	1.558	348	1.612	-0.054
Has a flush toilet	312	0.135	348	0.098	0.037
Casual labourer	312	0.519	348	0.443	0.077**
Highest education in the household					
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	312	0.635	348	0.612	0.023

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

5 Estimation strategy

We made use of the randomised 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 relied on the monthly average difference in last mile delay and work uptake between the job cards in the treatment and control GPs. We compared this difference during the pre-intervention period with that during the intervention and also in the post-intervention period. In essence, this is similar to the Difference in Difference (DID) comparison, which assumes that the indicators in the intervention GP would have shown similar values to that in the control GPs in the absence of the treatment. The observed difference between the two post-intervention phases can then be causally linked to the intervention.

Further, for estimation of the impact on the intermediate variables, we used Analysis of Covariance (ANCOVA) to estimate the treatment effect, which controls for the baseline value of the outcome variables. The literature indicates that this leads to an improvement in statistical power, especially when autocorrelation of outcomes is low (McKenzie, 2012; Hidrobo et al, 2016; Haushofer et al, 2020). Since the autocorrelation of the outcome variables was low and most of the variables of interest are binary in nature, we estimated the following probit model:

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

Here 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 effect of the spill-over, we categorised the control GPs into two groups: those within the intervening blocks of Damaragidda and Maddur, and the additional control GPs in the non-intervention blocks of Hanwada and Koilkonda. Accordingly, two dummy variables were generated for control GPs: one for the normal control and the other for the additional control GPs. We specifically made this adjustment to estimate the impact of the spill-overs and pure treatment effect. 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 spill-over effect, while the association with the treatment dummy would give us the estimate of the treatment effect adjusted for spill-overs. Formally we estimated the following probit model:

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

Here everything remains the same except the dummy for the normal control GPs, with that for the additional control GPs as the reference group. The β_T and β_S are the estimators and they measure the pure treatment effect and the spill-over effect, respectively. Bootstrapped standard errors with 500 replications, clustered at the GP level, were used (Cameron et al, 2008).

6 Results

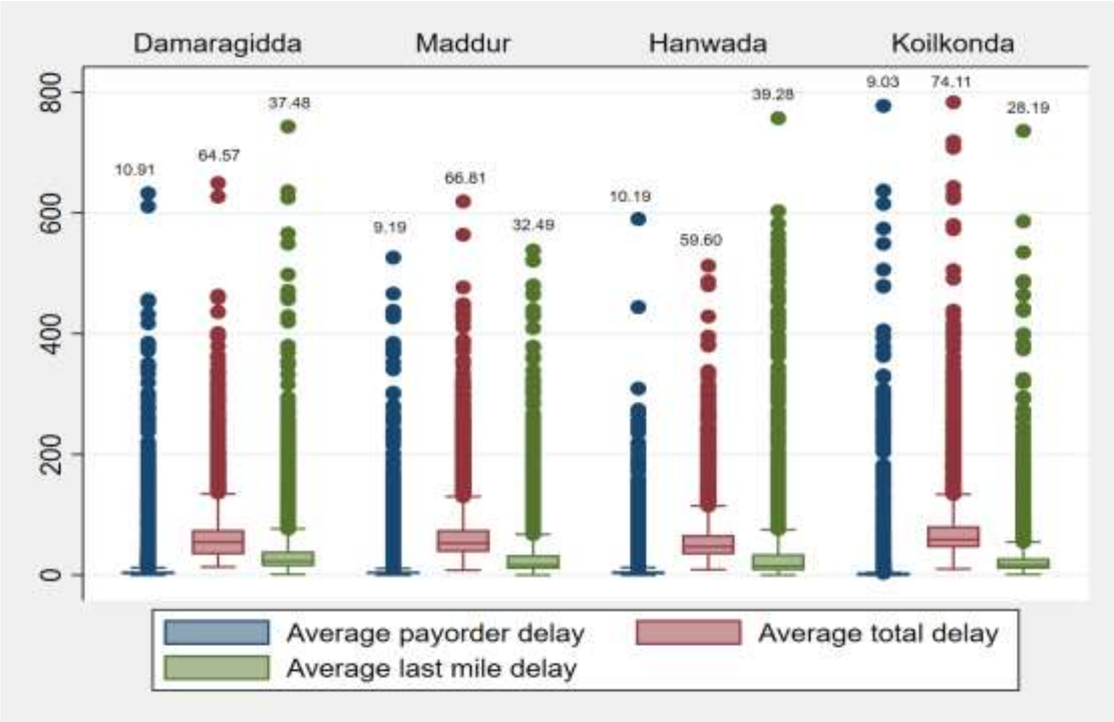
6.1 Impact on delays

The system of payment under MGNREGS in Telangana is as follows. After the work is completed, it is physically verified, largely by the office of the Block Development Officer (BDO). Post-verification, a Fund Transfer Order (FTO), which is analogous to a payment order, is generated at the local level. The FTO is then approved by the central ministry, which sends its details to payment intermediaries. These payment intermediaries are responsible for the electronic transfer of wages. The final payment status is shown on a public website and gives information on the credited date of the wages at the relevant bank or post office for payments which are not rejected.[7] Hence, in terms of delays in payments, these might arise either as a result of late payment order generation or when the wage is credited from the centre.

In addition to these delays, an associated last mile delay after wages are credited to accounts is also prevalent. More often than not beneficiaries do not receive information when their wages are credited to their account. The postal officials, including managers, make use of this information asymmetry to delay the payment for their personal needs before paying the wages. 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 payment order generation and total delay separately for the four blocks. A delay in payment order generation is defined as the number of days it took for the order to be generated after completion of the work; total delay is the total time taken in days for the wages to be credited to an account after completion of work. The average total delay across the four blocks was about 66 days and, even after the wage had been credited, an average worker had 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, 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: imposition of an implicit consumption tax and a decline in the human as well as financial net worth of the household. This has been noted elsewhere in the literature, which has documented a high prevalence of delayed payments under the programme (Narayanan et al, 2017).

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

Figure 4: Extent of different types of delays (in days) across blocks



Source: Administrative data from Telangana NREGA website.
Notes: Box plot showing job card-wise average days of payment order delay, total delay and last mile delay in 2017 for all the studied blocks. Values posted at the top end signify the mean of respective distribution of delays. The upper and lower hinges of the box correspond to the 75th and 25th percentile of the distribution and the line across the box indicates the median.

The intervention allows one to crawl the available public data and provide personalised information to beneficiaries once their wages have been credited to their accounts, through wage list pasting. The purpose of this information is to reduce the information asymmetry and enable the beneficiaries to demand their credited wages from postal officials. A brief theoretical framework on the set up is provided in Appendix A.

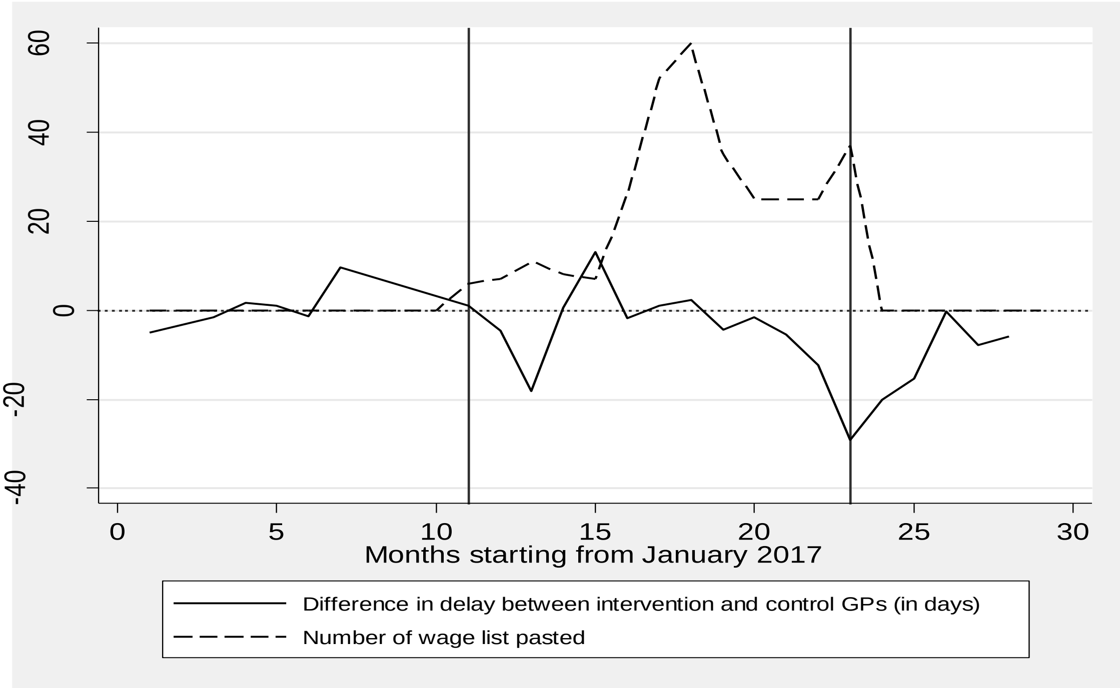
We used information on the credit and debit dates for all active 7,733 job cards from the GPs that were using postal accounts for disbursement of MGNREGS wages to the beneficiaries.[8] We specifically used these data to calculate the monthly mean difference in the credit and debit dates (defined as last mile delay) and then plot the monthly mean difference in last mile delay in the treated and control GPs between January 2017 and April 2019. Figure 5 presents this plot along with that for the total number of lists pasted in the intervention GPs over these months, in order to look at the causal effect on last mile delays.

The findings reveal a sizeable positive impact, as the difference in last mile delays between the treated and control GPs shows a massive fall during the intervention period. In other words, before the start of the intervention, the difference in last mile delay across

[8] The RN6 table from the data portal provides the information on credit and debit dates.

the treatment and control GPs remained close to zero. However, after November 2017 (month number 11) when the intervention started, this difference started to reduce and did so considerably during the months when the total number of wage list pastings increased. In November 2017, we observed a reduction of last mile delays in the treated GPs by about 28 days on average in comparison to the control GPs, clearly indicating the intervention's substantial impact.

Figure 5: Difference in last mile delay between the intervention and control GPs (in days)

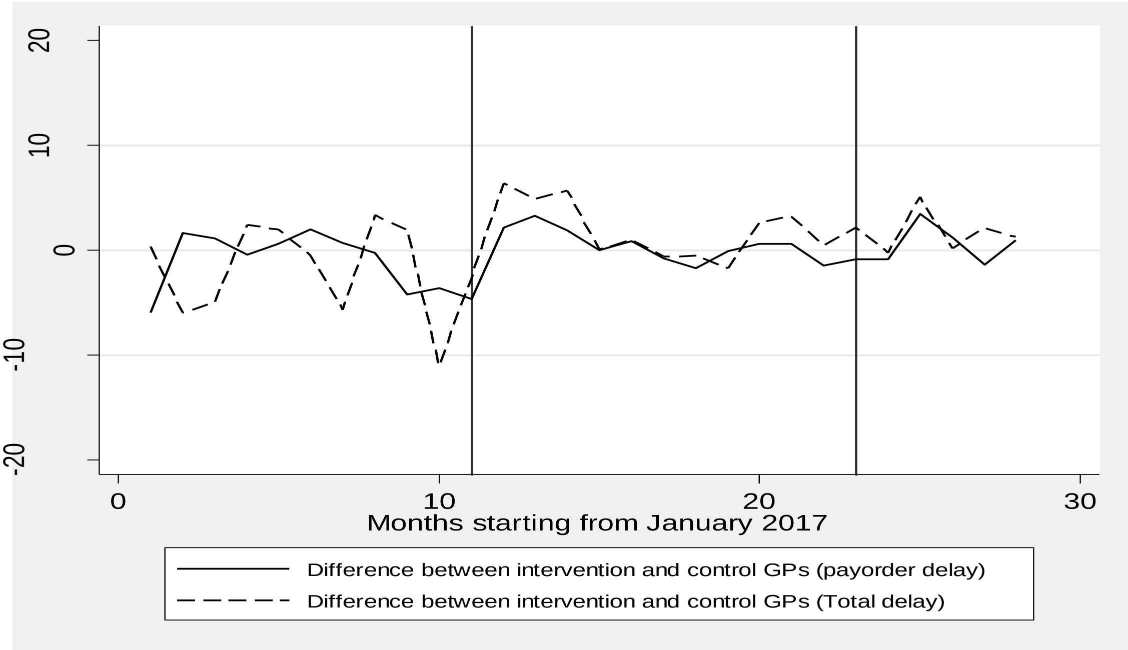


Notes: The monthly mean level of last mile payment delays (in days) was calculated for the intervention and control GPs and their difference plotted. On the X-axis, the months are plotted from January 2017 to April 2019. Hence 1 indicates January 2017, 12 indicates December 2017, 20 indicates August 2018, and so on. The period between the vertical lines is the period of intervention (November 2017 to November 2018). The dashed line plots the number of wage credit lists pasted in all the intervention GPs combined across the intervention period.

Despite observing an impact from the intervention on last mile delays, its impact on payment order generation and wage credit delays was deemed likely to be limited. This is because the responsibility for these delays lies with the block and central or state-level authorities, who were not targeted through our intervention. In contrast, local-level post offices can be held responsible for last mile delays. To test this we plotted the monthly difference in mean payment order generation delays (in days) at the GP level between the treated and control GPs between January 2017 and April 2019, along with the monthly difference in wage credit delays. Figure 6 presents these plots. As one would expect, we observed an inconsistent and marginal rise and drop in the payment order and wage credit delays during the intervention period, indicating its negligible impact.

This acts as a falsification test (discussed below), where we found a negligible effect of the intervention on related outcomes which we hypothesised would not be impacted.

Figure 6: Difference in payment order delay and total delay between the intervention and control GPs (in days) between January 2017 and April 2019



Notes: The monthly mean level of last mile payment delays (in days) was calculated for the intervention and control GPs and their difference plotted. On the X-axis, the months are plotted from January 2017 to 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 intervention (November 2017 to November 2018).

One could argue that the intervention had a limited effect on the delay in payments as it only affected last mile local-level delays, while having a limited impact on payment order and wage credit delays. However, our observations suggest that last mile payment delays are significant, especially when we consider that the programme was designed to target the poorest populations during the lean agricultural season. The average last mile delay before the intervention in all the GPs in the four surveyed blocks was about 37 days, reaching 80 days for about 10% of the job cards (Figure 4). The fact that we were able to register a gain of some 28 days in terms of access to wages is noteworthy and it is here that the intervention assumes importance.

Indeed, our qualitative work during the midline survey indicates that the beneficiaries of the programme were receiving messages when their wages were credited to their accounts, resulting in a reduction of the last mile delay in payments. One of the respondents reported:

> Earlier we were not aware of the amount of money credited in our account. We used to ask the FA but he was not able to respond. Therefore we had to make multiple trips to the bank. Now we get the information through phone calls. Even if we miss

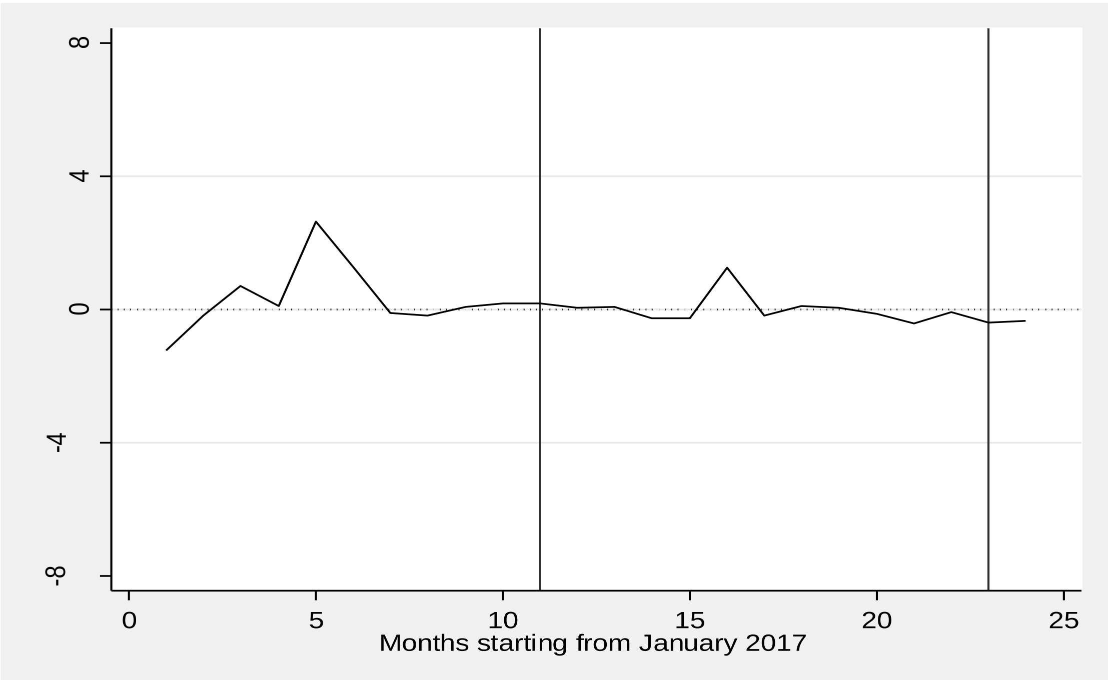
the call, we can see our names through the list pasted in GP office. This has helped us a lot.

A similar picture from the endline survey data was observed, with 68% of the respondents thinking that bank/post office transactions had got easier compared to the previous year and about 63% of them believing that delays in payment had reduced in comparison to the previous year.

6.2 Impact on work days

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

Figure 7: Difference in mean uptake between intervention and control GPs (in days) starting from January 2017



Notes: The monthly mean level of uptake in days was calculated for the intervention and control GPs and the difference in days of work plotted on the Y-axis. On the X-axis, the months were plotted from January 2017 to 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 intervention (November 2017 to November 2018).

Arguably, uptake of work also depends on a set of household and other confounding factors that needs to be controlled for before making any causal interpretation. For this, we used the simple DID regression method for the sampled job cards to compare the baseline and endline difference in uptake for the cards from treated with those from the control GPs against the set of possible confounding factors.[9] Table 2 presents the regression results on the logarithmic value of uptake in days.[10] The marginal effects of the treatment indicate no significant difference in uptake, as is observed in Figure 7, where we compared the treated GPs with the controls. To measure the spill-over in terms of uptake (if any) we compared the control GPs with the additional control GPs from other blocks (Hanwada and Koilkonda). Our results indicate no significant change, suggesting limited spill-over effects.

Table 2: Impact of treatment on uptake and spill-over 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

*Notes: The following control variables were incorporated in all the regressions: SCST, number of adults in the household, type of house (cemented or non-cemented), 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 only used 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 parenthesis. '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.*

9 See Angrist and Pishcke (2008) for more information on DID regression.

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

6.3 Impact on intermediate outcomes

As discussed, we further examined 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 the entitlement to delayed payment compensation was not collected during the baseline survey. Hence, to estimate the impact of the intervention on this indicator, we used a pooled probit model but did not control for the baseline level awareness of delayed compensation. The assumption was that at the baseline there would be no significant difference in awareness levels between respondents in the treatment and control arms. Intuitively, this is justified, as we did not find significant differences between the treatment and control arms for any of the other five indicators of entitlement awareness, which makes observation of a significant difference in this specific awareness indicator less likely (Table 1).

The estimation results are presented with two different specifications to estimate 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 categorises 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 specified, the second specification helps us to gauge the spill-over impact. We also present the estimates comparing the sampled households from the treated GPs with those from the control GPs.

Table 3 presents the estimation results from 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. The findings indicate a definite positive and significant impact of the intervention on awareness. We found a roughly 15% to 30% increase in the probability of being aware of the different entitlements. Notably, our results indicate a significant spill-over impact on some of the indicators of awareness. However, the effect size was lower, as we observe that the probability of being aware for respondents from a control GP was 10% to 15% higher than for a respondent from the additional control GPs. Net of the spill-over effect, the effect size of the increase in probability of being aware of these entitlements lies in the range of 12 to 36 percentage points.

Table 3: Impact of treatment on awareness

	Work entitlement	Work application	Unemployment allowance	Payment duration	Wage rate	Delay compensation
	Comparison of treatment GPs with all control GPs (including additional control GPs)					
	(1)	(2)	(3)	(4)	(5)	(6)
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 treatment GPs with other types of controls					
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 treatment GPs with control GPs					
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)

*Notes: The following control variables have been incorporated in all the regressions: respondent's gender, age education, SCST, number of adults in the household, type of house (cemented or non-cemented), 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 parenthesis.*** p<0.01, ** p<0.05, * p<0.1. The regression tables with all the control variables can be provided on request.*

This finding is substantiated by the qualitative discussions held during the midline survey. In three out of the four intervention GPs that we visited, the villagers seemed to be aware of the existing MGNREGS wage rate and work application procedure. Some among them in fact attributed this to the mobile phone calls from the intervention team. One among them said: "We came to know of different 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 documents the results from the pooled probit regression to estimate the impact on the process and on attendance at community meetings. The findings reveal a consistent and significantly positive impact on the probability of receiving a receipt for a work application (at the 5% level) as we find around a 10%–13% increase in the probability thanks to the intervention. Similarly, a 10%–14% reduction in the probability of having to travel more than once to a bank or post office for collection of wages was 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% point increase, respectively. The probability of raising concerns about MGNREGS at GS meetings also seemed significantly higher in the treatment GPs. Unlike in the earlier case, we found no spill-over effect on these process variables, although a significant effect on the chances of participation in social audit meetings and of MGNREGS being discussed in the GS meetings was observed.

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

	Comparison of treatment GPs with all control GPs (including additional control GPs)					
	Job card update by FA	Got receipt for work	Travelled more than once for wages	Attendance at GS meetings	Attendance at social audit meetings	Raised issue on MGNREGA
	(1)	(2)	(3)	(4)	(5)	(6)
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 treatment GPs with other types of controls					
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 treatment GPs with control GPs					
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)

*Notes: The following control variables were incorporated into all the regressions: respondent's gender, age education, SCST, number of adults in the household, type of house (cemented or non-cemented), 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 parenthesis. *** p<0.01, ** p<0.05, * p<0.1. The regression tables with all the control variables can be provided on request.*

To sum up, we observe that the intervention was instrumental in increasing awareness of the basic provisions of the programme and also in improving process mechanisms. But this increase did not lead to a higher uptake of work through increased days of work under the programme. The effect of spill-overs, as one would expect, was also found to be negligible. This hints at the limited impact of generalised information campaigns conducted through meetings and phone calls. However, the personalised information campaigns seemed to be effective, as we found a considerable impact of the intervention through the pasting of wage credit lists on reducing last mile delays in wage payments.

6.4 Robustness and falsification checks

We conducted a series of robustness checks to ensure our causal estimates were qualitatively correct. First, the inferences drawn so far out of the pooled regressions rest on the assumption that, between the endline and baseline surveys, there were no changes in the villages that might systematically influence the outcome variables. Accordingly, we gathered data on these changes (if any) from the panchayat officials and the FA. The officials and FA reported that there had not been any new NGOs working on MGNREGS or related programmes established during the intervention period. We also found that there had not been any systematic changes in the way MGNREGS functioned in this one year. Incidentally, in four of the GPs, the FA changed. Hence, as a robustness check, we dropped these four GPs and ran the regressions. Qualitatively, the marginal effects for all the variables across specifications remained unchanged.[11]

In addition, we conducted a number of falsification tests where we examined the effect of the intervention on Non-Equivalent Dependent Variables (NEDV) to test for potential internal validity threats. In other words, were there any 'placebo' effects of the treatment on these outcomes that would largely be considered to be unrelated to the intervention? An insignificant causal effect here indicates that the change in the original outcome variables is a result of the intervention and not of other confounders (Cohen-Cole et al, 2009; Coryn & Hobson, 2011). Accordingly, we considered three outcome variables that should not have been related to our intervention: (1) whether the household had a toilet funded partially or fully by the government; (2) whether the drinking water services were funded partially or fully by the government; and (3) whether the household used improved cooking facilities such as Liquefied Petroleum Gas (LPG) or an induction/hot plate.[12] Our regression results indicate that the impact on these unrelated variables was indistinguishable from zero at the 5% level of significance (see Table 5).

[11] The regression results are available on request.

[12] Toilets and improved cooking gas have been used for falsification since two of the arguably biggest welfare programmes started by the central government during this period have been the sanitation programme, called the Swachh Bharat Abhiyaan, which among other things aimed to provide toilets to all households, and the Ujjwala Yojana, which aims to provide subsidised improved cooking facilities to poor households. See https://swachhbharatmission.gov.in/sbmcms/index.htm and https://pmuy.gov.in/. Both accessed: 21 May 2020.

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

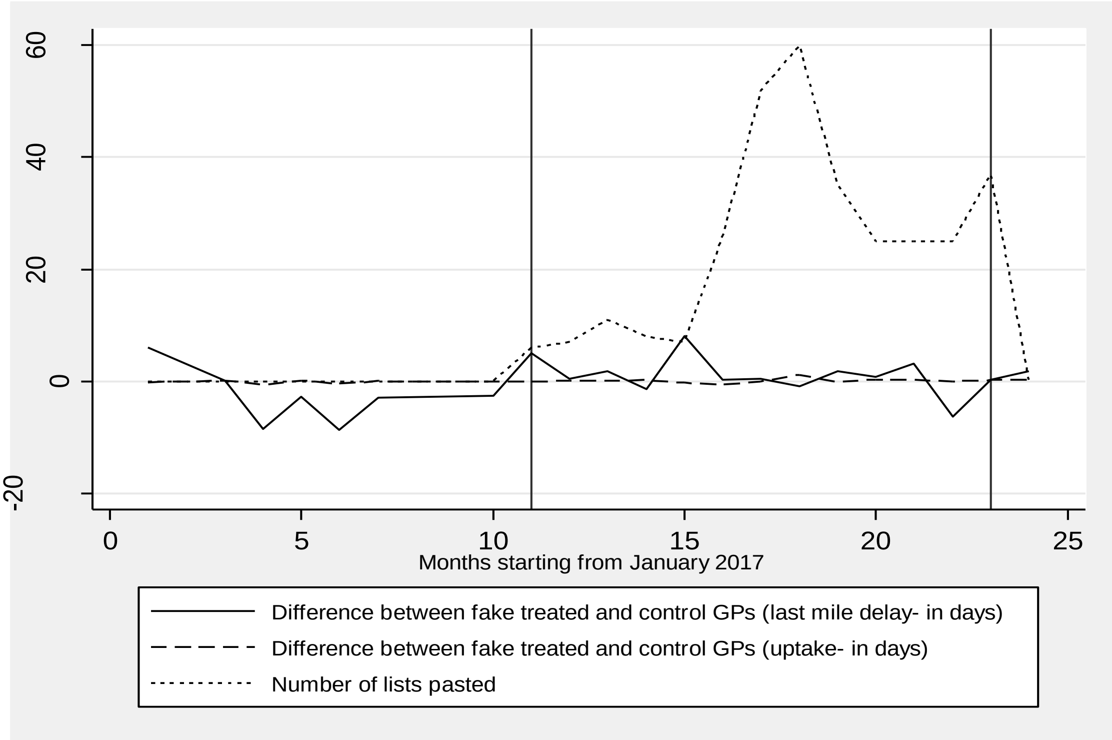
	Comparison of treatment GPs with all control GPs (including additional control GPs)		
	Government funded toilet	Government funded water facilities	Improved cooking facilities
Treatment	-0.038	-0.018	-0.023
	(0.072)	(0.037)	(0.030)
	Comparison of treatment GPs with control GPs		
Treatment	-0.046	-0.017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

*Notes: Outcome variables are toilet partially or fully funded by the government (Government funded toilet), water facilities partially or fully funded by the government (Government funded water facilities) 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: SCST, number of adults in the household, type of house (cemented or non-cemented), 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 the household level, we only used 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 parenthesis. *** p<0.01, ** p<0.05, * p<0.1. The regression tables with all the control variables can be provided on request.*

Next, we implemented a placebo test where we randomly categorised all the GPs into fake treatment and control GPs with dummies. Hence, out of the 96 GPs, 48 were grouped into the fake treatment group and the remaining 48 into the control group. The difference in uptake between the fake treated and control GPs was plotted from January 2017; however, we did not observe any significant difference during the intervention. Similar plots are presented for last mile delays for the 70 GPs that use postal accounts for payments by randomising them into treated and control groups. As can be seen, no significant difference was found during the period of intervention (Figure 8). 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.[13] Finally, instead of an ANCOVA pooled probit regression, we used DID regression to estimate the causal impact of the intervention on intermediate outcomes. The direction of the marginal effects for most of the variables remains the same.[14]

[13] The regression results are available on request.
[14] The regression results are available on request.

Figure 8: Difference in mean last mile delay and work uptake between fake intervention and control GPs (in days) between January 2017 and December 2018

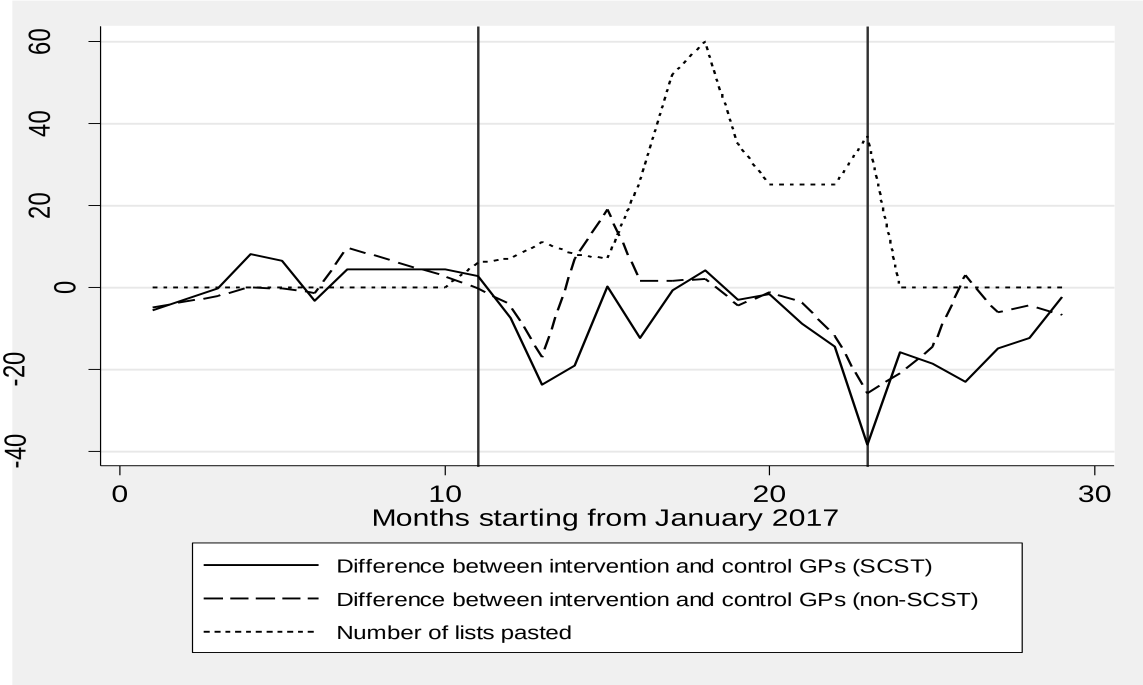


Notes: The monthly mean level of uptake and last mile payment delays in days was calculated for the fake intervention and control GPs and the difference in days of work plotted on the Y-axis. On the X-axis, the months are plotted from January 2017 to December 2018. Hence 1 indicates January 2017, 12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

6.5 Heterogeneous impact on SCSTs, educated households and those owning mobile phones

One of the major features of the intervention has been the additional effort made to reach households in the SCST community. It is likely therefore that the marginal treatment gains will be disproportionately higher for these households. Accordingly, we examined the plot for difference in last mile delay between the treatment and control GPs since January 2017, as done earlier, separately for SCST and non-SCST households. As is evident from Figure 9, which presents the findings, a reduction in last mile delay was observed in the treatment households. Importantly, we also observed a reduction in last mile delay for non-SCST households as well but the effect size for SCST households was substantially higher. For example, while the largest monthly average reduction in last mile delay for non-SCST households was about 25 days, that for SCST households was 40 days. This seems to indicate that the intervention had a higher effect on reduction in last mile delay in payments for SCST households than for other households.

Figure 9: Last mile payment delays for SCST and non-SCST

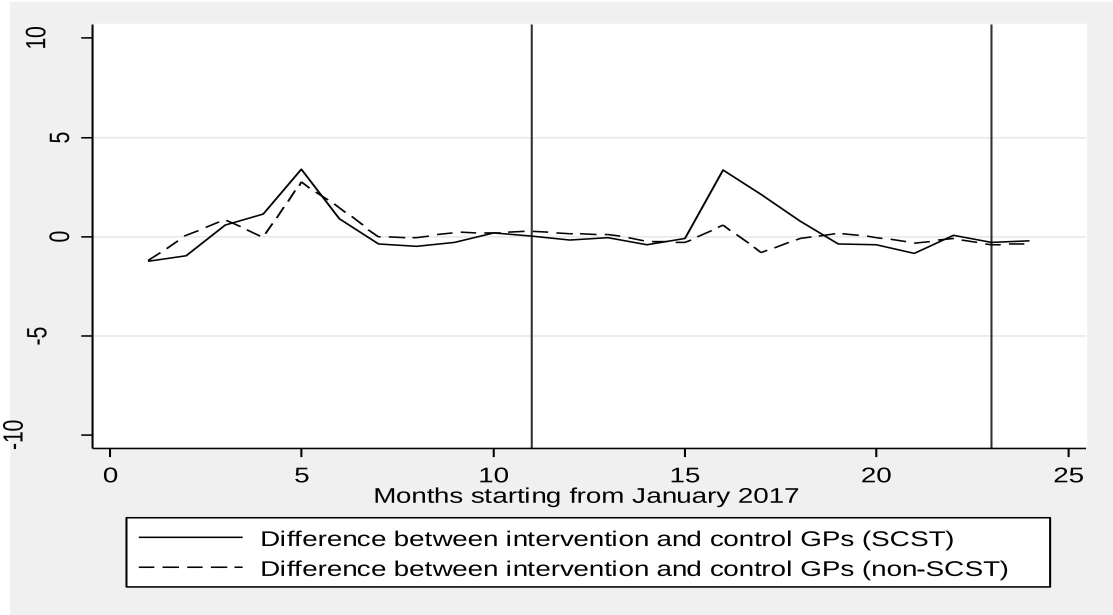


Notes: The monthly mean level of last mile payment delays in days was calculated for the intervention and control GPs and the difference in days of work plotted on the Y-axis separately for SCST and non-SCST households. On the X-axis, the months are plotted from January 2017 to April 2019. Hence 1 indicates January 2017; 12 indicates December 2017; 20 indicates August 2018, and so on. The period between the vertical lines is the period of intervention (November 2017 to November 2018).

We present a similar plot for difference in work uptake for households in the intervention and control GPs separately for SCST households and others in Figure 10. Unlike the case for last mile delay, no significant effect on uptake was found among SCST households when compared to non-SCST households. Notably, the findings from the regression to estimate the marginal effect for SCST households on the intermediate outcomes of awareness or process mechanisms compared to the non-SCST households indicate no significant gains.[15] This again points to the limited impact of generalised information campaigns even on the particular groups with greater exposure to the intervention.

[15] The regression results are available on request.

Figure 10: Uptake of work among SCST and non-SCST households



Notes: The monthly mean level of work uptake in days was calculated for the intervention and control GPs and the difference in days of work plotted on the Y-axis separately for SCST and non-SCST households. On the X-axis, the months are plotted from January 2017 to 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 intervention (November 2017 to November 2018).

Since the intervention was predicated on information dissemination and mobile phones being an important component, it could be assumed that the potential gains from the intervention would be higher for mobile phone owners or literate households. However, the findings from the regressions indicate no such gains in these households. This is true not only for uptake of work but for intermediate outcomes as well, possibly indicating that the effects of the intervention were inclusive of deprived households, who were likely to be less well educated or not to have access to a mobile phone.[16]

6.6 Effect on uptake through the 'encouragement effect'

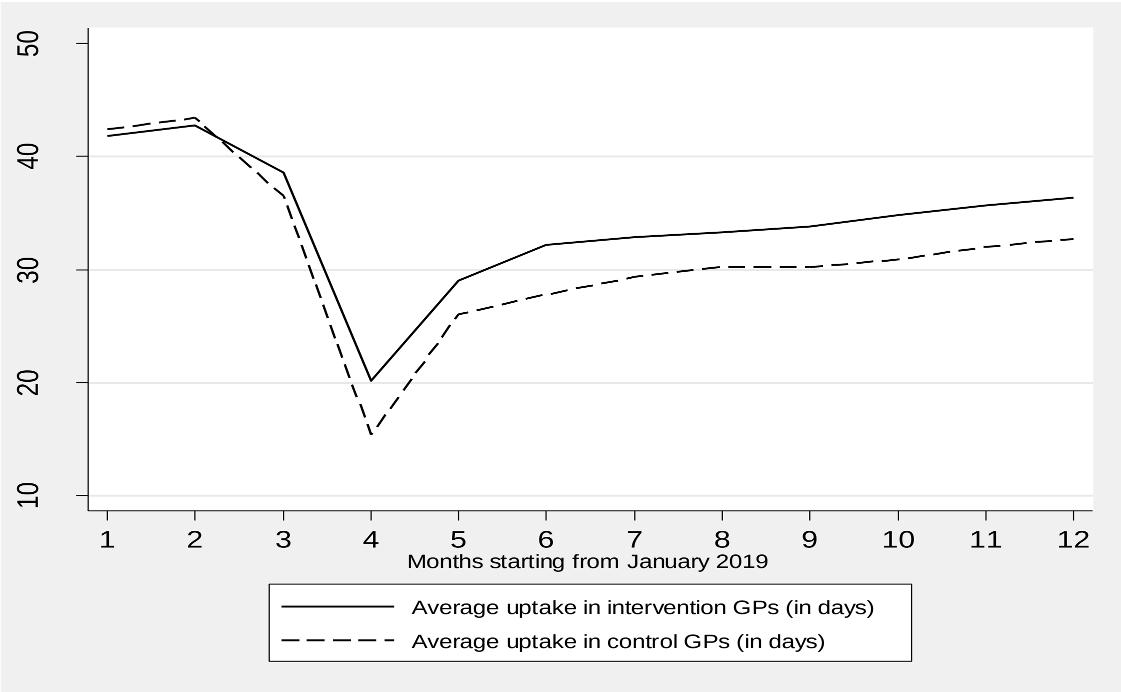
The literature has indicated that, because of the uncertainty of securing jobs from the local authorities and the associated delay in payments, workers are often 'discouraged' from demanding work under MGNREGS (Mukhopadhyay et al, 2015; Narayanan et al, 2017). If this holds, it is possible that a reduction in the delay in payments would encourage workers to demand more work under the programme. In other words, a substantial reduction in last mile delay in payments, as was observed during the intervention, could potentially lead to a higher uptake of jobs in the next period. In this section, we test whether this holds true.

For this, we considered the period from January to December 2019 and calculated the monthly average uptake of work in the treated and control GPs. We show these two plots

[16] The regression results are available on request.

for all months, starting from January 2018, in Figure 11. A gain of about three to five days was observed starting from April 2019, which is close to a 10% to 15% increase during the MGNREGS peak working season starting in May. This should be placed alongside two important findings already discussed. First, the reduction in delay in payments was seen to converge to the original levels three to four months after the intervention. Second, we did not find any significant increase in uptake during the intervention. These observations ensure that any increase in uptake observed arose because the reduction in last mile delays during the intervention period induced an encouragement effect for the beneficiaries to demand more work under the programme.

Figure 11: Uptake of work in treatment and control GPs from 2019



Notes: The monthly mean level of uptake in days was calculated for the intervention and control GPs and plotted on the Y-axis. On the X-axis, the months are plotted from January 2019 to December 2019. Hence 1 indicates January 2019; 12 indicates December 2019.

7 Cost-effectiveness

The evidence presented in this paper indicates the considerable impact of a personalised information campaign, as reflected in a reduction in last mile payment delays and a potentially higher uptake of work in the subsequent period. Nevertheless, the effects of generalised information campaigns have been limited. However, in terms of policy recommendations, one could argue that the former is costly and hence not effective when viewed through the lens of a cost–benefit analysis. In order to examine this in detail, we need to estimate the difference between the amount of compensation for payment delays the government was having to pay the beneficiaries in the absence of the intervention and the total cost incurred to implement the latter at local level, which includes both the personalised and generalised campaigns. As estimated, we observed an average drop of around 25 days in last mile delay per job card during the peak MGNREGA month in the intervention GPs in comparison to the control GPs. With an average of about 370 active job cards in every GP in Telangana, the total drop in last mile delay 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 should be calculated at a "rate of 0.05 percentage of the unpaid wages per day for the duration of the delay", compensation for every delayed day would amount to Rs10 (~$0.14), given that the state's minimum wage under MGNREGA during the span of intervention was around Rs200. This amounts to an expenditure of Rs 92,500 (~$1,322) by the government in every GP every month, at least during the three peak months of work, if it decides to pay compensation for last mile delays. So the monthly cost amounts to $33,000 for every block, assuming 25 GPs situated within a block on average.

To calculate the total cost of the intervention, we need to gauge both the fixed as well as the variable costs. The fixed costs involved paying a onetime lump sum to the web server through which the phone call application was devised. The variable costs included paying remuneration at a contracted price to the person hired for local logistics, ie trawling online administrative data and disseminating these in the form of a poster to the designated GP locations. The poster was expected to be put up as many times as the wage was disbursed from the centre. The intervention team reported that, for each GP, a maximum of five posters was needed to cover all the prime locations and the average printing cost per poster was around Rs100 (~$1.4). Since this needed to be repeated for the number of times the payment was disbursed, the total monthly cost per GP would be around Rs500 (~$7), close to $175 for every block. In addition to this, according to the local wage rate, one person per block could be employed for a monthly salary of Rs20,000 (~$300), including travel expenses. A one-time sum of Rs5,000 (~$70) was also paid as server cost for the entire duration of the experiment, which covered the 26 treated GPs (close to the size of a block). Adding up the latter expenses, the total cost of implementing the intervention every peak month for each block would be $545. With other miscellaneous costs of $455, this amounts to $1,000 for every block. Note that this estimate does not incorporate the sunk costs of time spent by the research team researching and designing the intervention and the mobile phone application.

This indicates a gain for the government of close to $32,000 every month for each block on average if our intervention for reduction of last mile delays is applied. This is significant, as it means that the marginal gain for every dollar spent is close to $32. It should be noted that, from the second month onwards, because the server's fixed cost does not need to be paid, this gain is close to $34.5. Further, the cost includes that for both the generalised and the personalised campaigns, while the potential improvement in awareness of the entitlements or process mechanisms could be used by participants to extract other benefits from the programme. The reduction in the last mile delay in payments may also encourage workers to demand 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.

8 Discussion and Conclusions

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 a programme and its intended beneficiaries may end up not receiving optimal benefits. However, delivery of the correct information to the beneficiaries could bridge this implementation gap, which often arises because of an information asymmetry. Information asymmetry may be utilised in various contexts by local authorities for their own benefit at the cost of the intended beneficiaries.

This paper, based on a randomised experimental design, has evaluated a novel intervention that accessed information from a public website and disseminated it to beneficiaries of the MGNREGS. Of the final outcome variables we observed a substantial drop in the last mile delay in payments, thanks to a personalised information campaign of wage credit list pasting. Generalised awareness campaigns had only limited impact, as we found no effect on increasing the uptake of work, although a modest positive impact on intermediate outcomes, like improvement in awareness of entitlements under the programme and process mechanism, were found. We also found no gains through spill-over effects on uptake, although we found evidence of an 'encouragement' effect through an increase in work uptake the following year, potentially because of the reduction in delays during the intervention period. In addition, higher gains in the reduction of last mile delays for the deprived SCST population were observed thanks to the higher focus of the intervention on these deprived groups.

One of the novelties of the intervention was its use of two channels of the dissemination process: generalised and personalised. The generalised campaigns were run through phone calls and meetings, whereas the personalised campaigns involved wage credit list pastings. The findings of this paper can inform us about how effective these channels have been in enhancing the impact of the outcome variables. As hypothesised, the former channel may have had an impact on enhancement of awareness indicators, which could improve the process mechanism and ultimately increase the uptake of MGNREGS work. However, despite finding a significant effect on awareness and betterment of the process mechanism, we did not observe an associated increase in uptake. In contrast, the latter channel of pasting the list of wage credits had a direct causal impact on reducing last mile delays, which did in effect increase work uptake in the following year, demonstrating the importance of personalised information campaigns. This is in line with other studies that did not find a substantial effect of generalised awareness campaigns on welfare programmes and indicators (Staats et al, 1996; Seimetz et al, 2016; Alik-Lagrange & Ravallion, 2019).

The nobility of the intervention and the findings also revolve around two other positives. First, apart from the programme benefits during the intervention period, we found evidence of positive encouragement effects from the intervention through increased uptake of work under MGNREGS, which is pertinent, since this is largely an indirect or a side benefit. Second, the intervention need not be limited to programmes like MGNREGS in Telangana: it could be replicated for any other welfare programmes that have publicly available micro-level data. For example, the Public Distribution System (PDS) in India offers public data that could be used similarly to empower beneficiaries. We therefore recommend that the intervention be widely used by CSOs, who can engage with local stakeholders and disseminate the information more efficiently. In such an arrangement, we would expect the gains from the intervention actually to be higher, given the already established network of CSOs at the local level.

However, it must be noted that the significance of the findings rests on one caveat. Importantly, implementation of the programme and its delivery structure in Telangana was centralised, which may have contributed to the success of the micro-level dissemination of information to arrest last mile corruption. In states with greater decentralisation, the effects might be attenuated (Maiorano et al, 2018). This is relevant because, as argued by Bussell (2010), initiatives to increase transparency and improve efficiencies may be resisted by the implementing authorities if these disrupt established patterns of rent seeking, especially when much of the power is decentralised. In addition, what might be prominent in decentralised systems is a local power asymmetry. The current literature has highlighted the prevalence of local-level power asymmetries, where the powerful are often able to break contracts and use information to their advantage, something which is dependent on state capacity (Khan & Roy, 2019; Lavers, 2020; Lavers et al, 2020a, 2020b). Here, studying the whole intervention in these heterogeneous contexts should provide important insights into the whole argument. It therefore remains as an area of further research.

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Appendix A: Theoretical framework of the delay in payments

The theoretical framework can be conceptualised as follows. Consider M amount of money has to be disbursed by the Branch Post Master (BPM) but she holds it for time period, t before distributing it to the beneficiaries. Hence her earnings are the interest earned 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 $p(t) \to 1$ 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 until time period, t if $I(t) > p(t).F(t)$. A graphical representation is shown in Figure A1:

Here we consider two situations: pre-intervention and post-intervention periods, denoted by the subscript 1 and 2 respectively. t_1^* is the equilibrium time period up to 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 and hence t_2^* would be the new equilibrium during the intervention, which would shift towards the left as the number of list pastings in the GPs increased.

2. Figure A1: Theoretical framework of the delay in payments