Using information and technology to improve efficacy of welfare programs:

Evidence from a field experiment in India

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Using information and technology to improve efficacy of welfare programs: Evidence from a field experiment in India *

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Abstract

Does information dissemination among beneficiaries of welfare programs mitigate implementation failures that undermine these programs? We present experimental evidence on this question in the context of the rural public works program in India. A noble intervention that involves accessing micro level online administrative information of the program and disseminating it to the beneficiaries was implemented in parts of the state of Telangana. Using baseline and endline survey as well as administrative data, we evaluate the impact of this intervention on awareness of the provisions, process, delayed payments and uptake of the program in terms of days worked. The design of the intervention ensured us to examine the effect of spillovers from the program as well as look at the impact of heterogeneous intensity of treatment. The findings indicate a positive and significant impact on raising awareness levels, improving the process mechanism and reducing last mile payment delays. However no significant impact was observed on delays that do not occur at the local level and on uptake as well. The impact of spillovers is also found to be largely positive, however no major difference in impact is found because of the heterogeneous treatment intensity.

Keywords: Information, Welfare program, implementation, randomization, delay of payments, MGNREGS

JEL Codes: C93, D73, H53

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1 Introduction

"Knowledge is power. Information is liberating. Education is the premise of progress, in every society, in every family." – Kofi Annan

It has been well recognized that welfare interventions including conditional cash transfer programs, public work programs and food distribution programs have been effective in providing social security to the deprived population across developing countries (Von Braun, 1995; Subbarao, 2001; Gilligan et al., 2009; Miller et al., 2011; Glewwe and Kassouf, 2012; De Brauw and Hoddinott, 2011). Despite success of these programs in producing sizable impact on welfare outcomes, it has been argued that much of it also depends on how the programs are implemented at the local level. Limited institutional capacity to implement these programs often is cited as the reason for them to fail or not produce the desirable impact (Pritchett, 2009). For example, elite capture of welfare programs due to multiple market failures has been evident where the benefits are disproportionately enjoyed by the better off population at the cost of the deprived ones (Bardhan and Mookherjee, 2000). Similarly, implementation deficiencies, transaction cost and previous experience of the beneficiaries including delayed payments may actually discourage the poor to take optimal benefits out of these programs (Skoufias, 2005; Narayanan et al., 2017).

The implementation failures often undermine the program and one key reason for prevalence of such failures arguably has been the dearth of the correct information among the beneficiaries which makes it impossible for them to hold the functionaries accountable (Drèze and Sen, 2013). Literature has emphasized the pivotal role of information in efficient functioning of the markets and proper provisioning of public goods and services (Stigler, 1961; Jensen, 2007; Gisselquist and Niño-Zarazúa, 2013; Dal Bo and Finan, 2016; Protik et al., 2018). Notably as Banerjee et al. (2018) indicate, information plays an important role in better rural public service delivery, which suffers because of rent seeking behavior of the implementing authorities. This largely happens due to information asymmetry that can often be utilized by them for their own benefit resulting in hefty welfare losses for the intended deprived population.

However, it is not exactly clear if providing information to the citizen acts like a magic bullet. It may happen that citizens would not be able to make use the information to demand their entitlements. Further even if the information is provided, the implementing authorities may not care about the demands. Hence, gauging whether dissemination of information improves service delivery is an empirical question (Banerjee et al., 2018). Literature has found mixed evidence on this. For example, Banerjee et al. (2018) found dissemination of information increased receipts of benefits in a subsidized rice program in Indonesia. However, Ravallion et al. (2013) found no such effects on similar outcomes related to the Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS) in India apart from enhancing awareness.

This paper experimentally evaluates an intervention based on accessing information from public website and passing the same to the beneficiaries of the MGNREGS, which is a public works program implemented in India since 2005. In other words, the intervention harnesses public micro-level administrative records available online on MGNREGS and disseminate the information to beneficiaries or a group of beneficiaries. The main component of the intervention is as follows: after the information on wages, once it gets credited to the bank or postal accounts gets updated, the names of the relevant individuals are listed and then pasted at core junctions of the village. In addition, the intervention attempted to spread awareness on various provisions of the program through local meetings and mobile phone calls. The 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 on different outcomes related to the program including awareness, process mechanism, participation in local meetings, delayed payments and uptake in terms of days worked among others. The design of the intervention and survey also allowed us to look at the effect of spillovers from the intervention and evaluate differential impact of the heterogeneous treatment intensity.

The findings reveal that the intervention has been successful in enhancing awareness and encouraging the beneficiaries to attend local meetings and raising concerns about MGNREGS in these meetings. We did not find significant impact of the intervention in reducing delay in

payments that occur at higher levels but a definite impact on the last mile delay in payments because of the wage credit list pasting. Interestingly, the gains are found to converge to the pre-intervention level within three months of conclusion of the intervention. Finally, we did not find significant impact on uptake possibly because of the short duration of the intervention and local level conditions, which prompted the authorities to take decisions that discouraged workers from demanding work. In terms of the effects of spillover, we found positive results on some of these indicators while being negligible for other indicators.

Notably we look at the possible impact of differential treatment intensity. However we deviate from the existing studies on randomized experiments that intend to examine the heterogeneous impact of differential intervention intensity. Unlike these studies, which randomly allocate units to heterogeneous intensity of treatment, we do not randomize it. Rather we allow the intensity to grow organically or naturally differing on account of variations in the characteristics of the units (in our case, the unit is GP). Because of randomized selection of GPs, the distribution of these characteristics is likely to be similar and hence there would be no specific GP level characteristics that may systematically alter the outcome variables. We argue such an approach would enhance the external validity of our intervention without introducing biasness in the estimates. The findings indicate no significant difference in the impact of heterogeneous treatment intensity. Notably we did not find higher effect for mobile phone owners and the literate population possibly indicating that the nature of the intervention is inclusive.

The paper contributes to four strands of literature. Firstly, it provides evidence that technology based interventions can be effective in improving efficacy of safety net programs. This works through direct dissemination of information to the beneficiaries as well as encouraging them to hold the implementing authorities to be more accountable (Björkman and Svensson, 2009; Nagavarapu and Sekhri, 2016). With respect to this, improving last mile service delivery becomes important and our paper complement that by Muralidharan et al. (2018), who find significant gains in reduction of delay in payments under a cash transfer program implemented in Telangana in 2018. Secondly, the paper extends the growing literature on mobile phone

usage, which has been found to be effective in increasing income, improving better farming practices and reducing price dispersion among others (Jensen, 2007; Labonne and Chase, 2009; Aker, 2010; Aker and Mbiti, 2010; Cole and Fernando, 2012; Debnath and Sekhri, 2016). This paper gives evidence on the role of mobile phones in improving accountability and efficacy of public works programs. Thirdly, 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 (Miguel and Kremer, 2004; Chong et al., 2013; Alik-Lagrange and Ravallion, 2019). Lastly, the study contributes to the growing research on MGNREGS and shows how a simple intervention can be effective in improving implementation and service delivery under the program. On this note, the significance of the study lies in finding ways to increase accountability among local level implementers. Our intervention can be a useful alternative for Civil Society Organizations (CSO) and other program implementing authorities to engage in for better public service delivery in MGNREGS and even other programs.

The structure of the paper is as follows. Section 2 describes the MGNREGS program in brief. Section 3 describes the intervention and then discusses the design or roll out of the intervention with respect to the evaluation strategy. The next section describes the survey and section 4 discusses the estimation strategy. Section 5 presents the main findings from the regressions and analysis. The final section concludes with a discussion.

2 MGNREGS

The project examines the impact of information dissemination to the beneficiaries of a public works programme called the MGNREGS. Introduced on 23rd August 2005, initially it was implemented in 200 rural districts of India and then was extended to the whole country since 2008. Under this Act, any adult from a household living in rural areas, willing to do unskilled

manual labour at statutory minimum wage is entitled to be employed for at least 100 days a year on public works. For this the members willing to work would have to apply for registration. After verification of the place of residence and age of the adult members, the household is issued a job card, which is mandatory under the program. An application has to be made if the household wants work, indicating the time and duration of the work. Against this application, work would be provided within 15 days, failing to which an unemployment allowance has to be paid. Further, the wages have to be given within 15 days after completion of the work otherwise a delayed compensation needs to be paid. The democratically elected village head and his/her office is responsible for implementation of the program 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 welfare impacts of the program. These include positive impact on poverty, women empowerment, infant nutrition, dietary intake, education, and reduction in distress-led migration and violence among others, which underlie the importance of the program (Afridi et al., 2017; Das, 2015a; Dasgupta et al., 2017; Deininger and Liu, 2013; Imbert and Papp, 2015; Nair et al., 2013; Khera and Nayak, 2009). Despite these positive effects, studies have presented a number of administrative problems that the program has suffered from, which often undermined the potential benefits. Firstly, substantial proportion of rural households is found not to have got work despite seeking (Dutta et al., 2012; Liu and Barrett, 2013; Das, 2015c). In addition, delay in payments has been massively prevalent which often discourages workers from demanding work under the program (Narayanan et al., 2017, 2019). Arguably, one major reason for these implementation failures has been dearth of correct information among the beneficiaries because of which the local authorities cannot be made accountable. Studies have found abysmally low levels of awareness on the entitlements of the program across the country (Dreze and Khera, 2009; Das et al., 2012; Das, 2015b). Accordingly our intervention intervenes in enhancing awareness, giving information about process

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

delays and disseminating online public data that can help the beneficiaries to hold the local authorities accountable. We expect this would improve uptake of the program, resolve some of the process related problems and finally reduce delays in payments. A detailed explanation of the intervention is presented in the following section.

3 Intervention design and data

3.1 Intervention description

The intervention has been developed by the LibTech team, which consists of researchers, social activists and engineers interested in improving public service delivery in India [2]. The project started back in 2012 under the aegis of the Program on Liberation Technology at the Stanford University and covered parts of the deprived areas of Chhattisgarh, Jharkhand and Telangana.

The current intervention that is being evaluated has been rolled out in randomly selected GPs of the Damaragidda and Maddur blocks from the Mahbubnagar district in Telangana with the name *Upadhi Hami Phone Radio*. The intervention was carried out for 13 months from November 2017 to November 2018. The different ingredients of the intervention are as follows. Firstly, information about various rights and entitlements guaranteed under MGNREGS were disseminated through periodic voice broadcasts over mobile phones. These broadcasts included information on different procedures and processes that can help workers to access their entitlements. Local level meetings are also arranged with the intervention team to discuss these provisions. In addition, the intervention team arranges special broadcasts by eminent and well known people on important days like the national Independence Day and the state formation day to attract their attention. For instance, on the MGNREGS implementation day, the Project Director of the Mahbubnagar district addressed the workers over the *Upadhi Hami Phone Radio* broadcast system.

One important part of the intervention involved pasting the wage credited information in

[2] More details on the team can be found from http://libtech.in/ (accessed on June 30, 2019)

core junctions of the villages (GP headquarters or market place) and publicizing the information through voice broadcasts over mobile phones. The objective of the exercise is to reduce the last mile time delay in disbursement of MGNREGS wages after it is credited in to workers accounts. The delay in this last mile wage disbursement happens because the workers are often not aware when their wages are credited in their accounts and the officials use it to exploit the workers. The Branch Post Master (BPM) often collects cash from their office and may keep it with herself for her personal needs for extended period before disbursing the wages. The bank officials on the other hand may refuse the workers to pay the money to avoid overload. Further in the absence of the information on whether wages have been credited in their account, the beneficiaries make multiple visits to banks or post offices in anticipation of the money. In this situation, timely dissemination of such information when the wage is credited increases transparency and hence accountability of the BPMs in the fear of backlash. With this information, the workers can also avoid making multiple trips to the banks thus allowing her not to compromise on her daily wage work.

3.2 Intervention design

The intervention was rolled out randomly at the GP level in the Damaragidda and Maddur blocks of the Mahbubnagar district of Telangana. Because of the possible differences across the blocks, the randomization is 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 left out Mogala Madaka GP of the Damaragidda block from evaluation as it is adopted by the local Member of Parliament. Hence the 26 GPs form our intervention group and the remaining GPs in these two blocks constitute the control group (23 GPs). We further consider two other blocks within the Mahbubnagar district, Hanwada and Koilkonda, broadly based on the similar geographic and demographic characteristics. These two blocks are close to Damaragidda and Maddur block and in terms of the population characteristics, they are similar as well. Since these blocks were not at all intervened, the GPs

in these two blocks are the other set of controls. The basic characteristics of these four blocks taken from the Census 2011 are presented in table 1. The block map of the Mahbubnagar district is shown in figure 1, which also marks the four studied blocks. The intervened GPs along with the two set of control GPs from these four blocks are shown in figure 2.

[Table 1 here]

[Figure 1 here]

[Figure 2 here]

It may be noted that the control GPs located within the same intervening block are closer to the treated GPs and there is a possibility of flow or spillover of the intervention into these GPs. The spillover may come from the beneficiaries as well as the local functionaries including the FA. For example, the information disseminated as a part of the intervention may get shared with the villagers in the adjoining GPs which are not intervened because of the proximity of the two set of GPs. Similarly the FA from an intervention GP may learn certain provisions about the program and spread it across her network that includes FAs from the control GPs located within that block. This is particularly possible because FAs within a block often meet at the block office for monthly meetings and have to mandatorily maintain WhatsApp group among themselves. Hence gains from some of the interventions in the intervention GPs may flow to the adjoining control GPs within the same block. Because of these possible spillovers, the control GPs within the same block are likely to be contaminated and hence they are referred as "Contaminated Control GPs". However, in the control GPs that are located in Hanwada and Koilkonda, the chances of spillovers can be assumed to be negligible. Because of being located in the non-intervention blocks, the GPs in these blocks are far off from the intervention GPs. Further, the chances of interaction between FAs within these blocks with those from the intervention blocks are less, which has been confirmed from the survey as well. Out of the 96 FAs, only 2 reported of having some connection with FAs from other blocks. Therefore we assume that spillover can flow across GPs within the same block and not across the blocks. Accordingly, we refer these GPs from Hanwada and Koilkonda as the "Pure Control GPs".

One of the criticisms of randomized experiments in social science has been external validity which raises concerns about the validity of the causal estimates in a different setting (Angrist and Pischke, 2008; Deaton and Cartwright, 2018; Athey and Imbens, 2017). An obvious concern revolving this issue is that the protocols and the different components of the intervention may not be carried out in a different setting because of various factors including those related to local politics, geography and social dynamics among others. For example, any community led intervention may not work well in a community where social ties and networks are not as strong (Cameron et al., 2019). Hence, moving from a controlled setting to a more natural one, the validity of the treatment effect may be compromised. Accordingly, to ensure and improve scalability and external validity, we did not randomize the treatment intensity across GPs but rather let it grow organically and naturally within the GP. In other words, the intensity of the intervention was allowed to vary depending on the local dynamics as discussed. Except for the list of interventions, nothing was pre-fixed and the intensity depended on the GP centric factors including how cooperative the FA is, location of the GP, and the caste composition and dynamics within the GP. Hence it was higher in a GP which is geographically accessible or has a supportive FA. In other words, the intensity varied across the GPs that were intervened, but remained uniform within the GP. The advantage is such design ensures that the intervention is reasonably scalable and hence our estimations and inferences might be more valid externally without them being biased.

Notably in appendix fig. A1, we show that the distribution of a set of GP level characteristics (caste and gender composition, population, proportion of mobile phone owners, education of the FA, pre-intervention average days of work under MGNREGS and average delays along with distance from the nearest town) is similar across the intervention and the contaminated control GPs. Further, we also statistically test the equality of distribution through Kolmogorov Smirnov test and for none of the tests, we are able to reject the null hypothesis of equality of the distributions (Appendix table A.1)[3].

[3]Details about the test can be accessed from https://ocw.mit.edu/courses/mathematics/18-443-statistics-for-applications-fall-2006/lecture-notes/lecture14.pdf (accessed on August 3, 2019)

3.3 Survey Design

The data used in the paper to evaluate the impact of the intervention is based out of two waves of household survey. The baseline survey was conducted in September to October, 2017 before the start of the intervention. The endline survey was conducted from December 2018 to February 2019 after thirteen months of exposure to the intervention. The same households and respondents that were surveyed in the baseline survey were also surveyed during the endline survey. Additionally a midline survey was conducted to get a stock of the nature and status of the intervention. We use the midline survey to contextualize the empirical findings from the regressions.

For the baseline survey, within each Gram Panchayat (GP), among the job card holders, approximately 15 households (after adequate power calculations) were randomly chosen among the list of households who have worked at least once in 2016-17. The total number of GPs surveyed from the four blocks in each wave is 96 and the total number of households surveyed is 1444 and 1352 in baseline and endline survey respectively. Some households are left out in the second phase since the respondents were not found even after three visits. A week long (mid line) visit was made to the respective treatment blocks to understand and to take stock of the intervention process from both the beneficiaries and intervention implementing authorities. Further, we gathered qualitative information about the implementation of MGNREGS. More specifically, we visited two GPs each from Damaragidda and Maddur blocks and conducted twelve interviews which included four qualitative group discussions with the beneficiaries. We also conducted separate interviews with the two local intervention functionaries at the block level and the one who heads both of them.

The survey questionnaire covered a wide range of household information starting from demographic, socio-economic and a detailed set of information on MGNREGS. Apart from the general questions on the program, some specific questions were asked to get a clear picture on awareness of the beneficiaries about the scheme and the entitlements (including delay compensation, unemployment allowance, minimum days of work entitlement and wage rates among

others), process related information including that on bank/ postal accounts and also their attendance in local level meetings. During endline, in addition to these common set of questions to all the respondents of the respective households, we ask a set of additional intervention related questions only to the households belonging to the GPs that are intervened. These include qualitative/ subjective questions on their perception about the impact of MGNREGS in the last one year. It also included some objective questions on the intervention including those on getting mobile phone calls and attendance in local meetings among others. Apart from the household survey, information on the FA from each GP was gathered. Besides, a GP questionnaire was administered to capture salient characteristics of these GPs.

The tablet based survey was executed by using Google form in the first phase. However, in the second phase we have used KoBoToolbox, an android based ODK-interface application developed by the Harvard humanitarian initiative[4]. The survey team consisted of enumerators who have completed at least higher secondary education and were conversant in Telugu as well as the local dialects. However, the midline survey was conducted by the authors.

3.4 Outcome Variables

The paper evaluates the impact of the intervention discussed on a set of outcome variables that gives an indication of the level of awareness, process mechanism, delay in payments and uptake of the program in terms of the number of days of work. The first set of variables included six indicators of awareness levels: (i) whether the respondent knows about the work entitlement of 100 days every year to each household (ii) whether the respondent knows about the process of work application in MGNREGS (iii) whether the respondent knows about unemployment allowance been given in case of not receiving work (iv) whether the respondent knows about the number of days after completion of work within which the payment has to be made (15 days); (v) whether the respondent knows the correct wage rate (Rs. 197 for baseline and Rs. 205 for endline) and (vi) whether the respondent knows about delay compensation. Please note

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

that these outcomes are binary in nature[5].

Further, we calculate an index of awareness using two methods for each respondent based on the responses on the first five questions. We consider the first five variables as we did not collect the relevant information for the sixth variable (on delay compensation) in the endline survey. The first method is a simple average of all the five responses and hence can take five values: 0, 0.2, 0.4, 0.8, 1. The second method is a weighted average where the weights are proportional to the inverse of the probability of being aware about that particular indicator among the sampled respondent in the baseline. Hence if large proportion of the respondents is not aware of a particular entitlement, being aware of that entitlement gets a higher weightage. In our case, the probability of knowing about unemployment allowance among the respondents is lowest among the five indicators of awareness and hence gets the highest weightage. Most respondents know about the entitlement of 100 work days under MGNREGS every year and this indicator accordingly gets the lowest weightage. Both these indices of awareness are continuous in nature.

The next set of variables indicates process related information and the respondent's attendance in local meetings and raising concerns about MGNREGS. The objective is to examine the impact of the intervention on improvement in the process of the program implementation and attendance in meetings and raising concerns about the program. These variables include: (i) whether the jobcard is updated by FA in the last one year before the survey; (ii)) whether a receipt was received for work application in the last one year before the survey (iii) whether the respondent had to travel more than once for withdrawing wages from bank/ postal accounts last time they worked; (iv) whether any members attended the Gram Sabha (GS) meetings[6] ; (v) whether any members attended the social audit meetings and (vi) whether concerns about MGNREGS was raised in the GS meetings. All these six indicators are binary in nature.

The variables on delay and uptake are used directly from the online administrative data on

[5]Based on the distribution, we consider the range of Rs. 180 to 200 as the correct wage during baseline and Rs. 202 to 220 during the endline.

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

MGNREGS. The specifics of these variables have been discussed in the "Results" section.

3.5 Control variables

In the regressions, we include a set control variables measured during baseline to increase the precision of the estimates. The control variables allow us to control for minor differences in the characteristics of the individuals and their households that may account for the differences in the outcomes variables. The list of control variables measured includes gender, age and education of the respondent since they are directly associated with the outcome variables. The economic conditions of the household matter as well since economically better off households are expected to be better aware and extract relatively higher gains from the intervention. Accordingly, we include variables like type of households (non-cemented or not), land cultivated by the household in acre, number of livestock that include ox, bullocks and cows, main occupation of the household (casual labour or not) and whether the household has a toilet or not. An increase in the number of adult members in the household may indicate more individuals who can work under MGNREGS and hence we include this as a control variable in the regression. Further, because of the fact that media can influence awareness levels and transparency in the proceedings of any programs locally, we include whether the household members watch television. In addition, since caste is one of the major barriers of social inclusion, we introduce whether the respondent belongs to the Scheduled Caste/ Schedule Tribe (SC/ST) community (Sundaram and Tendulkar, 2003; Deshpande, 2011). Possession of mobile phones is also added as a control variable.

3.6 Randomization process

As indicated earlier, the intervention was randomized at the GP level in two blocks. The randomization process is stratified across blocks to control for the systematic differences across the two blocks. To ensure success of this randomization for the sample of 1352 respondents that were surveyed across two waves, we compare the baseline characteristics across the respondents

from treated and contaminated control GPs. Table 2 gives the results from the difference in means test between the respondents from the two groups. We find that the mean levels of none of the thirteen outcome variables are statistically significant at 5% level. We look at 17 control variables that include the characteristics of the respondent and their households. Four variables that include proportion of respondents who are illiterate, mean age, proportion of houses which are non-cemented and 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 estimate, our regression strategy controls for these household and respondent characteristics and also the outcome variable measured during the baseline along with the block fixed effects. Hence we minimize the bias when we estimate the impact of the treatment.

[Table 2 here]

4 Estimation Strategy

To estimate the impact of the intervention on the outcomes variables pertaining to MGNREGS as discussed, we make use the randomized experimental design. The advantage of the randomized experiment is to control the bias that may occur due to selection in and out of the program. In other words, since the intervention is randomly allocated across the GPs, there are no underlying variables that may alter the outcome variables and also affect whether the GP is treated or not. In this way, we would get rid of the selection or omitted variable bias that may yield biased estimates of the impact.

Despite our survey design having a baseline and endline survey, we do not use the Difference in Difference methodology to obtain the causal estimate. Instead we make use of the Analysis of Covariance (ANCOVA) to estimate the treatment effect which controls for the lagged value of the outcome variables that we gathered during the baseline survey. McKenzie (2012) indicates when autocorrelation of outcomes is low, ANCOVA estimates can be preferred over the

Difference in Difference estimates. Such approach averages out the noise and increases power. Intuitively, the Difference in Difference estimate overcorrects for the baseline imbalances when the autocorrelation is low. However, ANCOVA leads to more efficient causal estimation as it adjust for these imbalances according to the degree of correlation (Hidrobo et al., 2016). 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:

$$Prob(Y_{ij1} = 1) = \phi(\alpha + \beta.T_j + \chi.Y_{ij0} + \lambda.X_i + \delta.B_{ij}) \tag{1}$$

Here Y_{ij1} is the binary outcome variable of interest for individual i, from GP, j, which is the cluster in our case at endline. Y_{ij0} is the same variable at baseline. These binary outcomes include a set of awareness and process related variables as discussed. T_j is the treatment dummy variable which equal 1 if the GP, j is in the treatment arm. X_i is the vector of control variables that include baseline individual and household level characteristics of individual, i. B_{ij} is the vector of block level dummies. In all the regressions, the standard errors are robust and clustered at the GP level, which is the level of randomization in our case. β is the estimate of the causal impact of the intervention.

Notably, for the some of the outcome variables of interest, the type of the variable is continuous instead of binary. For estimating the impact of treatment on these variables, we use pooled Ordinary Least Square (OLS) regressions.

To calculate the effect of the spillover, we categorize the control GPs into two groups: the control GPs within the intervening blocks of Damaragidda and Maddur, which we refer as contaminated control and the controls GPs from the non-intervening blocks also referred as pure control GPs (in the Hanwada and Koilkonda block). Accordingly two dummy variables are generated for control GPs: one for the contaminated control and the other for the pure control. We specifically make this adjustment to estimate the impact of the spillovers and pure treatment effect. If the pure control GPs are taken as the reference group, the marginal effect associated with the contaminated control GP dummy gives us the estimate of the spillover

effect and that association with the treatment dummy would give us the estimate of the pure treatment effect. Formally we estimate the following probit model:

$$Prob(Y_{ij1} = 1) = \phi(\alpha + \beta_T.T_j + \beta_s.C_j^c + \chi.Y_{ij0} + \lambda.X_i + \delta.B_{ij}) \tag{2}$$

Here everything remains same except C_j^c which is the dummy for contaminated GPs. The β_T and β_S are the estimators and they measure the pure treatment effect and the spillover effect respectively. To test whether these estimators are statistically different from each other, the test of equality has been conducted and the resulting p-values have been reported. This test tests whether the treatment effect is indeed greater than the spillover effect.

To calculate the treatment intensity, we take five indicators at the GP level for all the GPs in the treatment arm. These indicators of intervention intensity are as follows:

(i) Number of phone calls made per phone number collected within the GP

(ii) Number of phone calls received per phone number collected within the GP

(iii) Number of messages sent per phone number collected within the GP

(iv) Number of times, the list with the names of wage beneficiaries has been pasted in the GP

(v) Proportion of respondents within the GP who knows at least one of the three main persons involved in the intervention process.

Using these five indicators, we calculate the intensity of treatment using the Principal Component Analysis (PCA) and normalize it in the range of [0,1]. To measure the impact of these two categories of intervention intensity, we estimate the following pooled probit model:

$$Prob(Y_{ij1} = 1) = \phi(\alpha + \beta_T.T_j + \beta_{int}.I_T + \chi.Y_{ij0} + \lambda.X_i + \delta.B_{ij}) \tag{3}$$

Here I_T indexes the normalized intensity of treatment calculated and β_{int} gives the average estimate of the impact of intensity of treatment controlling for the treated GPs. Please note that for the non-intervened GPs, the value of I_T is taken to be 0.

It should be noted that though the assignment of GPs in the treatment and control arm within the Damaragidda and Maddur block has been random, the intensity of the treatment has not been randomly fixed. As discussed, it depends on GP level factors including the geography, accessibility and cooperativeness of the local level implementers. However, we argue since the assignment has been random, the distribution of these GP level factors is similar across the treatment and control arm. In fact we show that the distribution across the GPs in the intervention and contaminated control arm is similar (Fig A.1 and Table A.1). In terms of location, 20 (out of 23) contaminated control GPs are situated in the roadside whereas for treatment GPs, it is 22 (out of 26). Further, in the regression model, we are controlling for the lagged value of the outcome variable, along with the economic and social characteristics of the individuals and households which are likely to be correlated with these GP level factors. It is because of these reasons that β_{int} is the unbiased estimators of heterogeneous intensity of treatment.

5 Results

5.1 Impact of pooled treatment

As discussed, we first show the effect of the intervention on the discussed six outcomes of awareness of the respondents controlling for their responses in the baseline survey as well as their own and household characteristics along with the two awareness indices. The six outcome variables of awareness are binary in nature and accordingly we run probit regression to estimate the impact of the treatment on these indicators. For the awareness indices, we use pooled OLS regression to estimate the impact controlling for the index value during the baseline and the individual and household characteristics. It should also be noted that the information about awareness of the entitlement of 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 the baseline level awareness of

delayed compensation. The assumption is at the baseline, there is no significant difference in awareness levels between respondents in the treatment and control arm. Intuitively this makes sense as we did not find significant difference between the treatment and the control arm for all the other five indicators of entitlement awareness (Table 2).

The estimation results are presented with two different specifications to estimate equation 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 contaminated control group and the pure control group. The pure control GPs are taken as the reference group. Further, we also compare the treatment GPs with the contaminated control GPs.

Table 3 presents the estimation results from pooled regression as depicted in equation 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 first five columns show the impact of the intervention of the five questions on awareness of the entitlements. The next two columns (6 and 7) show the causal estimates on the two indices of awareness and the last column shows the impact on awareness on delayed payment compensation. The findings indicate a definite positive and significant impact of the intervention on awareness. We find about 20 to 30 percentage point increase in the probability of being aware of the different entitlements. Notably, our results indicate significant spillover impact on some of the indicators of awareness. However, the effect size is found to be lower as we observe the probability of being aware for respondents from a contaminated control GP is 7 to 15 percentage points more than the probability of the respondent from a pure control GP. Net of spillover effect, the effect size of increase in probability of being aware in these entitlements lies in the range of about 12 to 36 percentage points.

This finding is substantiated by the qualitative discussions that we had during the midline survey in May 2018. In three out of the four intervention GPs that we visited, the villagers seem to be aware of the existing MGNREGS wage rate and work application procedure. Some

among them attributed this to the mobile phone calls from the intervention team. One among them echoed *"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."*

In the next table (table 4), the results from the pooled probit regression to estimate the impact on the process and attendance in community meetings is documented. Column 1 to 3 show the results of the impact of three process mechanism variables already discussed. Column (4), (5) and (6) show the causal impact on community meetings namely attendance in GS meetings, social audit and raising concerns on MGNREGS in the GS meetings respectively.

The findings reveal consistent and significantly positive impact on the probability of receiving a receipt for work application (at 5% level) as we find around 10-13 percentage points increase in the probability because of the intervention. Similar effect was found on the probability of travelling to banks/ post offices as we find 10-14 percentage points reduction in the probability due to the intervention. The impact on attendance in Gram sabha and social audit meetings seem to be robust and the findings indicate a 12-14 and 16-27 percentage point increase respectively. The probability of raising concerns on MGNREGS in the GS meeting also seems to be significantly higher in the treatment GPs. Unlike earlier case, we find no spillover effect on these process variables.

However, it is worthwhile to note that the chances of participation in social audit meetings and MGNREGS being discussed in the GS meetings is higher for the contaminated control GPs in the intervention blocks in comparison to the pure control GPs of the non-intervention blocks. This might be because of discussions among villagers within and across adjacent GPs about MGNREGS and the GS meetings being a platform to discuss the grievances. In other words, it is possible that some households benefitted discussing MGNREGS concerns in the GS meetings in the treatment GPs and this information got spread in the adjacent control GPs. Accordingly, in control GPs of the intervention blocks, beneficiaries started raising concerns on MGNREGS in their GS meetings. Such type of spillovers are unlikely to take place across

GPs located in different blocks and hence we do not see such an effect in the pure control GPs located in the non-intervention blocks.

[Table 3 here]

[Table 4 here]

5.2 Impact of treatment intensity

As discussed, we use pooled probit and OLS regression model to estimate the causal impact of the heterogeneous levels of treatment (equation 3). Like in the other case, we show results for two samples of respondents: (i) treatment GPs and all control GPs and (ii) treatment GPs and contaminated control GPs from the intervention blocks.

The results from the regression which show the impact of the treatment levels on awareness of entitlements and process mechanisms are shown in table 5 and 6 respectively. For most of the indicators, no significant impact of intensity of treatment is found controlling for the other confounders as well as whether the respondent comes from a treated GP. However for couple of variables on awareness and frequency of travel to banks/ post offices, we find a significant impact. One standard deviation in the intensity of treatment is found to increase the probability of the respondents being aware of work entitlement and process of work application by close to 17 and 20 percentage points respectively while the same increase is found to decrease the probability of traveling more than once to banks and post offices by close to 13 percentage points. We find similar effect when we compare the treated GPs with the contaminated control GPs. In other words, although the intervention is found to have a significant effect of awareness, process related mechanisms and participation in local meetings, the impact of intensity of treatment seems to be limited.

[Table 5 here]

[Table 6 here]

5.3 Impact on delay

One of the main ingredients of our intervention has been crawling of publicly available micro level administrative data on MGNREGS and using it to improve implementation of the program. A major problem that has been raised time and again in MGNREGS has been the prevalence of delayed payment. Extant literature and field reports across the country has reported extensive delayed payments under the program which undermines its efficacy (Basu and Sen, 2015; Masiero and Maiorano, 2018; Narayanan et al., 2017).

The system of payment under MGNREGS in Telangana is as follows. After the work gets completed, there is a physical verification of the work largely by the office of the Block Development Officer (BDO). Post verification, a Fund Transfer Order (FTO), which is analogous to a payorder 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 electronic transfer of wages. The final payment status is reflected in the public website and gives information on the credited date of the wages in the relevant bank or postal office for payments which are not rejected [7].

As evident, large part of this payment process is done at the block and the central/ state level. Hence gains on reduction of delay in payment because of the intervention are likely to be limited. However it can still facilitate the verification process of the works. Though the verification of work is done at the block level, the local level FAs can influence the block level authorities to speed up the process of verification. Because of the fact that the intervention team works alongside the FA on many issues, the process of verification by the FA may speed up though the team do not directly intervene on this. Therefore, generation of the FTO or the payorder may be faster. Accordingly using jobcard level data from all the GPs of the four blocks, we test if the intervention has been influential in earlier generation of payorder.

For this purpose, we collect the data on work completion date starting from January 2017 till April 2019 and the corresponding payorder generation date for all job cards from the four

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

blocks. Next, for all the payments made, we calculate the month-wise mean time difference between the work completion date and the payorder generation date for each job card starting from January 2017 till April 2019. We call this time difference as the payorder delay and is measured in days.

The average monthly difference in payorder delay between the intervention and the two types of control GPs (contaminated and pure) is calculated and then plotted against the months starting from January 2017 till April 2019 (Figure 3). The period of intervention from November 2017 till November 2018 is also marked in the plot. A relative drop in reduction in payorder generation delay indicates a corresponding drop in the intervention GPs in comparison to the corresponding type of control GPs. However what we observe is a inconsistent drop in the payorder delay during the intervention that seems to indicate the negligible effect of the intervention. Our discussions with the FA and the intervention supervisors indicate that much of the responsibility of payorder generation remains with the officials at the block level. Even if an FA wants to fasten this process, it would be largely dependent on block level officials who are directly responsible for verification of the works only after which the payorder is generated. Since the intervention is highly localized and did not involve block level officials, as expected the impact is found to be negligible.

[Figure 3 here]

Similarly, we examine if the intervention actually had an effect on total delay in payments, which we define as the number of days between the work completion date till the date when the wage got credited in the bank or postal account. Figure 4 presents similar plots as shown in figure 3 for every month starting from January 2017. As one would expect, no consistent drop in total delay is observed during the intervention period in either of the two plots indicating absence of impact of the intervention on total delay.

[Figure 4 here]

5.4 Impact on last mile delay

Apart from the delay in payments which is largely dependent on the higher level officials at the block, state and the central level, one further problem surrounding payments is the associated last mile delays. Due to various reasons including corruption and lack of information among others, the money is not debited from bank/postal accounts once it is credited though the beneficiaries would want to. In other words, it is often the case that the beneficiaries are not informed about the money after being credited in their account and hence the beneficiaries do not immediately get the money for various reasons. This last mile delay is not trivial as data shows a mean last mile delay of more than 44 days in 2016 for beneficiaries who maintain postal accounts in these four blocks with delays extending for months in some cases.

Our intervention allows the technical team to crawl the available public data and provide information on the credited date that is the exact date when the money is credited in the bank or postal account of the beneficiary. Once the payment is credited in the postal/ bank accounts, the intervention team pastes the list of beneficiaries in places that are easily accessible to all in the GP. Further the broadcast is made through mobile phones to inform the beneficiaries that the payments have been credited in their account. This piece of intervention is expected to reduce last mile delays as the beneficiaries having postal accounts would start demanding the credited wages from the BPMs. They would also have to travel less to the banks for getting the wage. The fact that our intervention has been found to have a significant effect on reduction in number of trips to the banks/ post offices for wage collection indicates its success.

The theoretical framework can be conceptualized as follows. Consider M amount of money has to be disbursed by the BPM but she holds it for time period, t before distributing it to the beneficiaries. Hence her earnings is 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 till time period, t if $I(t) > p(t).F(t)$. The graphical representation would look as follows:

[Figure 5 here]

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 till when would the BPM 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 outward shift of $p(t).F(t)$ as well and hence t_2^* would be the new equilibrium during the intervention, which would shift towards left as the intensity of treatment increases.

Notably, the crawled RN6 data allows us to get information on the credited and debited date for all jobcards from the GPs which use postal accounts to disburse the MGNREGS money to the beneficiaries[8]. We use this data to calculate at month-wise mean difference in the credited and debited dates across all these GPs that use postal accounts starting from January 2017 from when the data is available[9]. Next, the difference between the intervention GPs and contaminated control GPs and pure control GPs are plotted for all the month starting from January 2017 and presented. Figure 5 presents the difference between the intervention and contaminated control GPs and figure 6 gives the difference between the intervention and pure control GPs. This difference is observed across all the months starting from January 2017. Further we plot the total number of list pastings done in the intervention GPs over these months to look at the possible causal effect on last mile delays.

The findings reveal sizeable positive impact of the intervention when the intervention and the contaminated control GPs are compared on reduction of these delays. The observations from figure 6 reveal prior to the intervention, the difference in delay across the treatment and the control GPs remained close to zero. However after November 2017 (month number 11) when the intervention started, the number of lists pasted in the early months have been low and ranged between 6 to 11 every month till March 2018. Since April 2018, the number of

[8]The RN6 table in the micro level public data gives the information on credited and debited date.
[9]Respondents in 70 out of the 96 GPs surveyed have postal accounts.

list pasted increased to 26 and went up to 60 till November 2018. It is during this period that we observe consistently lower last mile delay in the intervention GPs in comparison to the control GPs in the same blocks. Interestingly, we find a positive association of the number of list pastings and the reduction in last mile delay. When we compare the intervention GPs with the pure control GPs in Hanwada and Koilkonda blocks (figure 7), we also observe a decrease in levels of last mile delayed payments. This becomes evident when the last mile delay during months just before the intervention (June to October 2017) is compared to that in June to October in 2018 during the implementation phase of the intervention.

[Figure 6 here]

[Figure 7 here]

One may argue that the intervention had limited effect on delay in payments as it only affects the last mile local level delays. While we agree the intervention had limited impact on reduction in delayed payment at higher levels, a reduction in last mile delay is also significant from the point of view of the beneficiary. A look into last mile delays before the start of the intervention in postal accounts shows an average delay of about 23 days from December 2016 to October 2017 which crosses 100 days in some cases. In fact in December 2016 and July 2017, the average last mile delay was 41 days and 35 days respectively. Hence reduction in delay duration even if it is last mile in nature is significant and it is here that our intervention assumes importance.

Indeed our qualitative work during the midline survey seem to indicate that many beneficiaries of the program received messages related to the money credited in their bank/postal account which can result in reduction of 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'.* We seem to get a similar picture from the endline survey data as well which gathered information from the respondents

from the treated GPs on their subjective perception of the benefits out of the intervention. We found about 68 percent of the individuals think that the bank/post office transactions have got easier as compared to previous year and about 63 percent of the respondents believe that delay in payment has reduced in comparison to previous year.

5.5 Impact on work days

Literature and field level reports has been able to establish the effect of delayed payments which may discourage the workers to demand more work under MGNREGS. Narayanan et al. (2017) using data at all-India level showed the evidence of "discouraged worker effect" primarily because of two reasons: higher experience of delayed payment and prevalence of higher rationing (demanding work but not getting it from the local authorities). Because the intervention has been able to improve awareness and raise discussions around MGNREGS in the GS meetings along with reducing last mile delayed payments, one might hypothesize that uptake of work in terms of number of work under the program would rise because of our intervention.

To examine this, we look at data on number of days of work job card wise from these four blocks. As in the earlier case, we calculate the total number of days of work by each job card from January 2017 to April 2019 after which we compute the difference between the average days of work between the intervention and contaminated control GPs and then the intervention and the pure control GPs. This difference is plotted against the months leading up to the intervention commencement and then during the intervention and even the post intervention period. Figure 8 presents the plots for difference in work days for intervention and the two types of control GPs. An increase during the intervention period would suggest a definite impact of the intervention on uptake. However we observe that the difference in the work days between the treatment and contaminated control GPs coming from the same blocks hovers around zero even after the commencement of the intervention. This remains similar when the intervention GPs are compared with the pure control GPs and it indicates negligible impact of the intervention on uptake under the programme.

[Figure 8 here]

Interestingly we find the work days in the pure control GPs to be higher than that in the intervention GPs after the intervention. This indicates that the uptake of work has been higher since the start of 2018 in the two pure control blocks (Hanwada and Koilkonda). In fact, the average work days dropped from 34 days to less than 26 days from 2017 to 2018 in the intervention blocks (Damaragidda and Maddur) whereas for the control blocks (Hanwada and Koilkonda), it went up from 33 days to 34 days. Further, for the sampled jobcards we analysed the days of work in 2017 and 2018. To estimate the impact of the intervention and its intensity on days of work in 2018, we applied the ANCOVA pooled OLS regression model using the total days of work in 2017 as a control variable along with the other household level characteristics discussed. The marginal effects of the treatment and the treatment intensity on uptake are presented in table 7. The findings from the regression indicate something similar to what has been observed from figure 7.

[Table 7 here]

Our qualitative investigation indicated that there was a push from the Mahbubnagar district magistrate office in some of the blocks within the district to include farm ponds as the only work to be done under MGNREGS. Both, Damaragidda and Maddur, which are among the most deprived blocks within the district were included in these list of blocks and Hanwada and Koilkonda were excluded. The idea is to replenish groundwater through the farm ponds which may raise agricultural productivity in these regions. However evidence from our qualitative research suggested since working on farm ponds is difficult, many of the households preferred not to work under the program or work for lesser number of days. The respondents especially the female workers complained about the hard nature and the physical intensiveness involved in the work. One of them reported *'the work available is very difficult and physically demanding, it becomes even harder to perform the work, given the only tools available with us, to dig the rocky hills to build trenches for rainwater harvesting, are pickaxe and shovel'*. People in other GPs echoing the same voice and emotions mentioned that the hard and undesirable nature of

MGNREGS work (farm ponds) acted as a demotivating factor for most of them, as a result of which they decided to opt out of MGNREGS work last year as well as this year. On asking, if they would have chosen to work had the nature of the work been easier, all of them replied affirmatively. These qualitative evidences corroborate the findings from the regression exercise which seems to indicate no significant effect of the intervention on uptake of the program.

Hence, to sum up even when the intervention might have laid a platform to raise the uptake of the program, we did not find any impact most likely because of the discouragement faced by the workers due to the nature of work that farm ponds entail. Further it cannot be denied that sustained incentivization and enabling for larger duration of time may be required to generate substantial interest in the program. In addition, since the intervention was unable to tackle the higher level delayed payments, no gains in uptake is found.

5.6 Heterogeneous Impact

One of the major components of the intervention as discussed has been voice broadcast over mobile phones. Further, information disseminated through the intervention is more likely to be used by the educated individuals. Hence it is essential to gauge if the intervention had a differential impact for mobile phone owners and the literate ones. Accordingly we ran the regressions of the outcome variables on the treatment dummy and the dummy indicating whether the respondent is literate and whether he/she is a mobile phone owner separately along with the interaction term keeping all the other control variables intact. The marginal effect of the interaction term would yield the estimate for the heterogeneous impact of the intervention on the literate population and the mobile phone owners.

The results are presented in appendix table A.2-A.5 (not presented here due to paucity of space). The estimations seem to suggest very limited heterogeneous impact of the intervention on literate population and the ones owning mobile phones. This indicates that the gains is uniform across population and hence to some extent can be called inclusive in nature. Our interaction with the intervention team suggests that they took immense care not to restrict

the impact to the better off literate population or the ones who own a mobile phone. In the local meetings, the provisions were discussed with those who attended which in turn did not necessarily depended on owning a mobile phone or being educated. One reason why the pasting of list after wage credit was given utmost importance despite similar information being broadcasted through mobile phones is to cater to the population who did not possess a mobile phone.

5.7 Robustness and falsification checks

We conduct a series of robustness checks to ensure that the results are not sensitive to alternative ways of estimation. Firstly, the inferences drawn so far out of the pooled regressions rest on the assumption that in between the endline and baseline there has not been changes in the villages that can systematically influence the outcome variables. If there have been systematic changes, one may argue that the changes in the outcome variables is because of these changes and not because of the treatment. Accordingly we gather data on these changes (if any) from the panchayat officials and the FA. The officials and FA reported that there are no new NGOs that have been established that works on anything related to MGNREGS. It is also found that there have been no systematic changes in the way MGNREGS functioned in this one year. Incidentally the local body election was held across Telangana in January 2019. Before this, there were no elected GP head in these villages. However, in four GPs, the FA got changed in between. Hence as a robustness chance, we drop these four GPs and run the regressions. The direction and signs of the marginal effects for all the variables across specifications remain unchanged[10].

Further, instead of ANCOVA pooled probit regression, we use difference in difference regression to estimate the causal impact of the intervention. The signs of estimates of the causal impact for most of the variables remain same including those indicating the awareness, getting receipts for work demand and uptake of works among others. In addition, we conduct a num-

[10]The regression results can be obtained on request

ber of falsification tests where we examine the effect of the intervention on outcome variables, which are unlikely to be causally related. In other words, are there any "placebo" effects of the treatment on outcomes that are largely considered to be unrelated with the intervention? An insignificant causal effect here indicates that the change in the original outcome variables is because of the intervention and not due to some other factors. For example, potential social network effects have been examined on outcomes like obesity and smoking (Christakis and Fowler, 2007, 2008). However using similar methodology, Cohen-Cole and Fletcher (2008) found "implausible" social network effects on acne, height and headache and concluded that researchers should be cautious in associating health outcomes with social network effects especially when confounding factors related to environment are not adequately controlled for in the analysis.

Accordingly, we consider three outcome variables that we should not be related with our intervention: (i) whether the household has a toilet funded partially or fully by the government; (ii) whether the drinking water services are funded partially or fully by the government and (iii) whether the household uses improved cooking facilities that includes Liquefied Petroleum Gas (LPG) or induction/hot plate[11]. More specifically, we look at the impact of the intervention on these variables. The expected hypothesis is that the effect on these outcomes would largely be insignificant. Our results indicate the impact on these unrelated variables have been indistinguishable from zero at 5% level of significance (Table 8).

[Table 8 here]

Finally we implement a placebo test where we randomly categorize all the control GPs (contaminated and pure) into fake treatment and control GPs. Hence out of the 70 control GPs, 35 GPs were grouped into fake treatment group and the remaining 35 in control group. Accordingly, 505 surveyed households belonged to the fake treatment group and 499 belonged to the control group. We ran the same regressions and present the estimations in table 9. As one would expect, we observe no significant difference in neither of the variables related

[11] Toilets and improved cooking gas has been used for falsification since two of the arguably biggest welfare programs started by the central government of India during this period has been the sanitation program called the Swachh Bharat Abhijaan, which among others aimed at provided toilets in all households and the Ujjwala yojana, which aims at provided subsidized improved cooking facilities to poor households

to awareness, process mechanism, participation or uptake because of this fake assignment of treatment among the actual control GPs.

[Table 9 here]

6 Discussions and Conclusion

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

This paper based on randomized design evaluates a noble intervention that accesses information from public website and disseminate the same to the beneficiaries of the MGNREGS. The outcome variables are those related to awareness, different aspects of implementation and uptake under the program. The findings indicate a significant and positive impact of the intervention on various indicators of awareness. Overall, a treatment effect by more than 20 percentage points is found. The effect size also seems to be slightly higher in GPs with higher treatment intensity though no statistically different effect was found when compared with treatment GPs of lower intensity. Significant spillover effects have also been observed as we find the awareness of a set of entitlements to be higher in untreated GPs in the vicinity of the treated GPs (located in the same blocks) in comparison to the untreated GPs which are located in different blocks altogether. This effect is found to be robust after controlling for a variety of household and respondent level factors as well as baseline values of the awareness levels indicating a definite volume of spillovers. This is possible because Indian villages are often socially connected where villagers may exchange information that they gain from different sources. Further the FAs within the block are socially connected and spillovers may be prevalent from their

end as well.

Our findings also indicate significant effect of the program on process related variables as we find the causal estimate for the probability of getting receipts after demanding work is higher and that for making more than one trip to the banks/ post offices for wage collection is lower for the respondents located in the intervention GPs. The probability of attending GS and social audit meetings are also found to be higher as well as the chances of raising concerns over MGNREGS in the GS meetings. Further, we did not find any impact on the large scale delays but observed evidence for a possible reduction in last mile local level delays for postal payments because of the intervention. Finally, we did not find any such impact on increasing uptake of the program. This probably is because of an unwritten order by the district collector to carry out works on farm ponds in the two intervention blocks, which are largely considered to extremely difficult. As a result, uptake is found to be lower because of this exogenous decision.

To sum up, we observe definite positive impact of the intervention on many of the indicators that otherwise can undermine the success of the program in just about thirteen months of exposure out of which the first five months were used to build the process that included establishment of contacts and collection of phone numbers among others. We expect higher returns of the intervention with higher duration of exposure whereby positive impact on uptake can also be observed.

The nobility of the intervention and the paper revolves around on three facts. Firstly the intervention has been rolled out organically. Depending on the local conditions, treatment intensity varied and not externally controlled as often is the case with other randomized experiments. Hence concerns about external validity which is often held as the one of reasons for criticisms of randomized control trials in social science is minimized in our setting. Secondly, we find some evidence of positive spillover effects, which is important for any intervention to maximize its benefits across wider range of population. Working at a larger scale, if interventions produce positive spillovers across the adjoining areas, the benefits from it can be optimized. Thirdly, the intervention is not limited to MGNREGS in Telangana and can be replicated for

any other welfare programs that give publicly available micro level data. For example, the Public Distribution System (PDS) in India offers public data that can be used similarly and empower the beneficiaries. The intervention can largely be used by the CSOs that can engage with the local stakeholders and disseminate the information more efficiently. We expect in such an arrangement, the gains from the intervention may actually be higher given the already established organization at the local level of the CSOs.

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Table 1: Basic characteristics of the selected blocks

Block	Proportion of SC	Proportion of literates	Proportion of agricultural labourer	Proportion of casual labourer
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	0.16

Source: Census (2011)

Table 2: Comparison of means for the treated and contaminated 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
Jobcard 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 in banks/POs	302	0.901	316	0.915	-0.014
Attendance in GS meetings	282	0.319	324	0.34	-0.02
Attendance in social audit meetings	282	0.319	324	0.34	-0.02
Raised issue on MGNREGS	312	0.096	348	0.112	-0.016
Number of days of work	312	40.042	348	40.816	-0.774
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
Non-cemented house	312	0.333	348	0.247	0.086*
Land cultivated in acre	312	3.128	348	3.205	-0.077
Cows, Oxes and buffaloes	312	1.558	348	1.612	-0.054
Has a flush toilet	312	0.135	348	0.098	0.037
Casual Laborer	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 TV	310	0.571	347	0.506	0.065
Owns a mobile	312	0.635	348	0.612	0.023

The mean level of the baseline characteristics is presented; hh. stands for household.
* represents significance at 5% level.

Table 3: Impact of treatment on awareness

	Work entitlement	Work application	Unemployment allowance	Payment duration	Wage rate	Awareness (simple average)	Awareness (weighted average)	Delay compensation
	Comparison of treatment GPs with all GPs							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.121***	0.212***	0.145***	0.207***	0.206***	0.215***	0.048***	0.164***
	(0.038)	(0.045)	(0.021)	(0.051)	(0.031)	(0.034)	(0.008)	(0.021)
	Comparison of treatment GPs with other types of controls							
Ref. pure controls								
Treatment	0.117**	0.362***	0.263***	0.272***	0.218***	0.215***	0.048***	0.230***
	(0.047)	(0.053)	(0.030)	(0.062)	(0.034)	(0.034)	(0.008)	(0.030)
Contaminated control	-0.005	0.150**	0.117***	0.065	0.012	0.110***	0.094**	0.066*
	(0.049)	(0.060)	(0.030)	(0.063)	(0.035)	(0.036)	(0.044)	(0.037)
Treatment=contaminated control (p-value)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	Comparison of treatment GPs with control GPs of intervention block (contaminated)							
Treatment	0.105***	0.213***	0.247***	0.205***	0.251***	0.291***	0.048***	0.291***
	(0.037)	(0.046)	(0.035)	(0.051)	(0.036)	(0.031)	(0.008)	(0.032)

Note: The following control variables have been 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 acre, total number of livestock (cows, bullocks and oxes), whether household has a toilet in the household and if its members watches TV along with main occupation of the household and block dummies. The marginal effects from ANCOVA pooled probit regression are reported in column (1) to (5) and (8). In columns (6) and (7), marginal effects from pooled OLS regressions are reported and the standard errors clustered at the GP level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

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

	Jobcard update by FA	Got receipt for work	Travelled more than once for wages	Attendance in GS meetings	Attendance in social audit meetings	Raised issue on MGNREGA
	(1)	(2)	(3)	(4)	(5)	(6)
Comparison of treatment GPs with all GPs						
Treatment	-0.016	0.096***	-0.099**	0.125***	0.156***	0.085***
	(0.069)	(0.036)	(0.042)	(0.046)	(0.044)	(0.029)
Comparison of treatment GPs with other types of controls						
Ref. pure controls						
Treatment	0.136*	0.128**	-0.134***	0.144**	0.272***	0.317***
	(0.072)	(0.051)	(0.050)	(0.057)	(0.050)	(0.042)
Contaminated control	0.151*	0.031	-0.035	0.02	0.116**	0.231***
	(0.088)	(0.053)	(0.048)	(0.056)	(0.049)	(0.045)
Treatment=contaminated control (p-value)	0.82	0.01	0.02	0.01	0.00	0.00
Comparison of treatment GPs with control GPs of intervention block (contaminated)						
Treatment	-0.018	0.103***	-0.102**	0.137***	0.183***	0.132***
	(0.069)	(0.039)	(0.043)	(0.044)	(0.044)	(0.038)

Note: The following control variables have been 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 acre, total number of livestock (cows, bullocks and oxes), whether household has a toilet in the household and if its members watches TV along with main occupation of the household and block dummies. The marginal effects from ANCOVA pooled probit regression are reported and the standard errors clustered at the GP level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Impact of treatment intensity on awareness

	Work entitlement	Work application	Unemployment allowance	Payment duration	Wage rate	Awareness (simple average)	Awareness (weighted average)	Delay compensation
Comparison of treatment GPs with all GPs by treatment intensity								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.044	0.09	0.141***	0.155**	0.193***	0.158***	0.040***	0.157***
	(0.052)	(0.058)	(0.029)	(0.075)	(0.038)	(0.048)	(0.012)	(0.028)
Treatment intensity	0.308**	0.459***	0.014	0.191	0.044	0.211**	0.031	0.024
	(0.131)	(0.145)	(0.040)	(0.242)	(0.068)	(0.098)	(0.024)	(0.037)
Comparison of treatment GPs with control GPs of intervention block (contaminated)								
Treatment	0.04	0.091	0.242***	0.151**	0.231***	0.152***	0.039***	0.277***
	(0.049)	(0.060)	(0.049)	(0.074)	(0.044)	(0.049)	(0.012)	(0.046)
Treatment intensity	0.262**	0.446***	0.019	0.203	0.054	0.210**	0.031	0.041
	(0.127)	(0.145)	(0.070)	(0.240)	(0.079)	(0.098)	(0.024)	(0.068)

Note: The following control variables have been 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 acre, total number of livestock (cows, bullocks and oxes), whether household has a toilet in the household and if its members watches TV along with main occupation of the household and block dummies. The marginal effects from ANCOVA pooled probit regression are reported in column (1) to (5) and (8). In columns (6) and (7), marginal effects from pooled OLS regressions are reported and the standard errors clustered at the GP level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Impact of treatment intensity on process related variables and attendance in meetings

	Jobcard update by FA	Got receipt for work	Travelled more than once for wages	Attendance in GS meetings	Attendance in social audit meetings	Raised issue on MGNREGA
Comparison of treatment GPs with all GPs						
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.024	0.076*	-0.033	0.142**	0.131**	0.064*
	(0.078)	(0.043)	(0.047)	(0.064)	(0.058)	(0.035)
Treatment intensity	0.032	0.074	-0.237**	-0.065	0.089	0.077
	(0.121)	(0.071)	(0.096)	(0.109)	(0.112)	(0.074)
Comparison of treatment GPs with control GPs of intervention block (contaminated)						
Treatment	-0.023	0.083*	-0.034	0.155**	0.153**	0.101**
	(0.078)	(0.048)	(0.049)	(0.062)	(0.060)	(0.044)
Treatment intensity	0.021	0.073	-0.246**	-0.067	0.108	0.113
	(0.121)	(0.078)	(0.100)	(0.111)	(0.111)	(0.092)

Note: The following control variables have been 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 acre, total number of livestock (cows, bullocks and oxes), whether household has a toilet in the household and if its members watches TV along with main occupation of the household and block dummies. The marginal effects from ANCOVA pooled probit regression are reported and the standard errors clustered at the GP level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Impact of treatment and its intensity on uptake

	All control GPs		Only contaminated GPs	
	(1)	(2)	(3)	(4)
Treatment	-0.119	-0.024	-0.123	-0.042
	(0.174)	(0.268)	(0.173)	(0.264)
Treatment intensity		-0.351		-0.297
		(0.654)		(0.633)
N	1338	1338	657	657
R-squared	0.279	0.279	0.312	0.313

Note: The following control variables have been incorporated in all the regressions: SC/ST, number of adults in the household, type of house (non-cemented or not), land cultivated in acre, total number of livestock (cows, bullocks and oxes), whether household has a toilet in the household and if its members watches TV along with main occupation of the household and block dummies. The outcome variable is log (days of work+1). The marginal effects from pooled OLS regression are reported and the standard errors clustered at the GP level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Impact of the treatment on the unrelated variables (Falsification test 1)

	Government funded toilet	Government funded water facilities	Improved cooking facilities
Comparison of treatment GPs with all GPs			
Treatment	-0.037	-0.02	-0.021
	(0.072)	(0.037)	(0.028)
Comparison of treatment GPs with control GPs of intervention block			
Treatment	-0.046	-0.021	-0.018
	(0.068)	(0.037)	(0.025)
Comparison of treatment GPs with control GPs of non-intervention block			
Treatment	-0.128	0.077*	-0.052
	(0.081)	(0.047)	(0.035)

Note: The following control variables have been incorporated in all the regressions: SC/ST, number of adults in the household, type of house (non-cemented or not), land cultivated in acre, total number of livestock (cows, bullocks and oxes), whether household members watches TV and main occupation of the household along with block dummies. The marginal effects from ANCOVA pooled probit regression are reported and the standard errors clustered at the GP level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

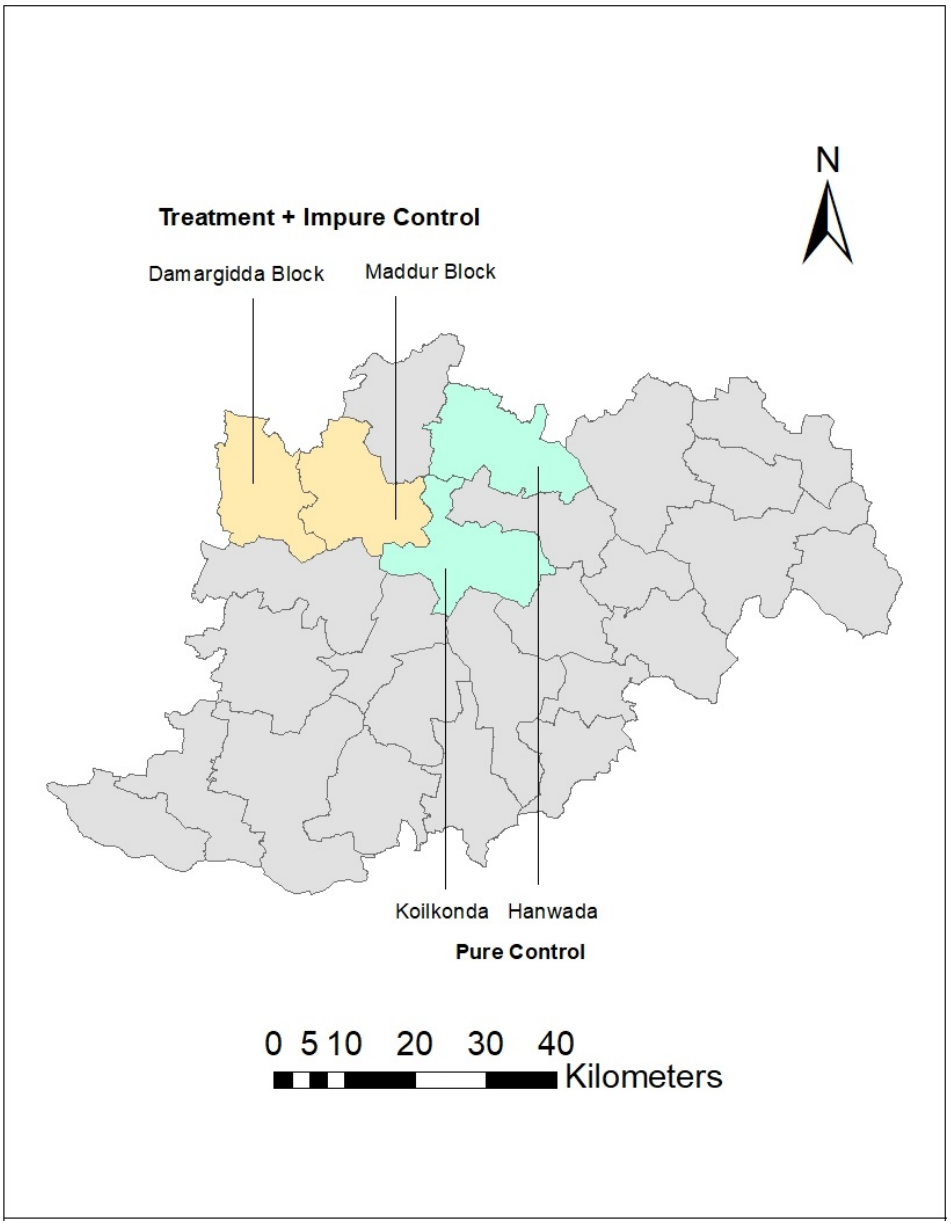
Table 9: Placebo test

	Awareness indicators							
	Work entitlement	Work application	Unemployment allowance	Payment duration	Wage rate	Awareness (simple average)	Awareness (weighted average)	Delay compensation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fake randomized treatment	0.013	0.006	-0.003	0.017	0.008	0.008	0.001	0.015
	(0.030)	(0.043)	(0.010)	(0.032)	(0.020)	(0.015)	(0.002)	(0.013)

	Process mechanism and meeting attendance indicators					
	Jobcard update by FA	Got receipt for work	Travelled more than once for wages	Attendance in GS meetings	Attendance in social audit meetings	Raised issue on MGNREGA
	(1)	(2)	(3)	(4)	(5)	(6)
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)

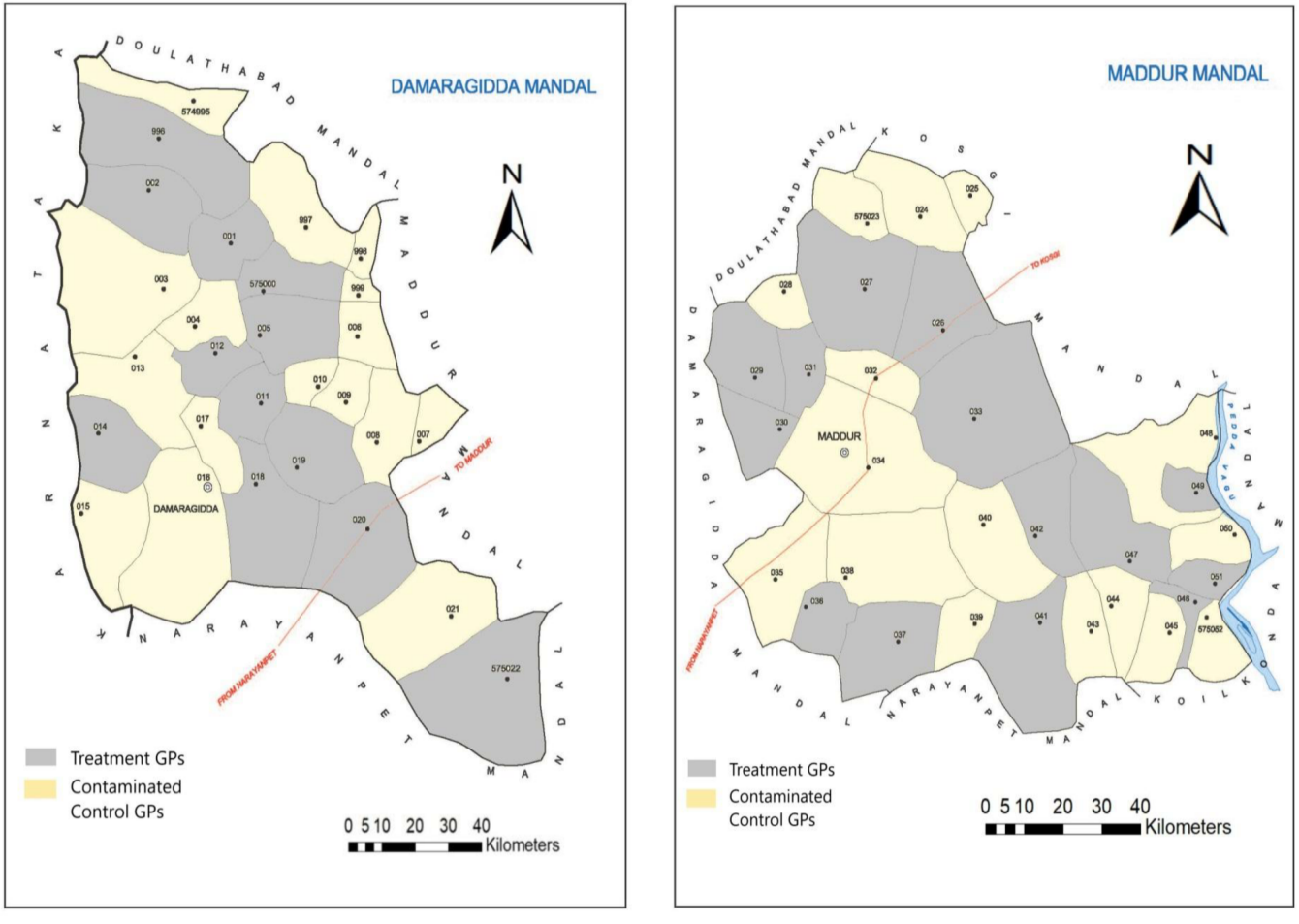
Note: The following control variables have been incorporated in all the regressions: respondent gender, age education, SC/ST, number of adults in the household, type of house (non-cemented or not), land cultivated in acre, total number of livestock (cows, bullocks and oxes), whether household has a toilet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

Figure 1: Geographical location of the selected blocks



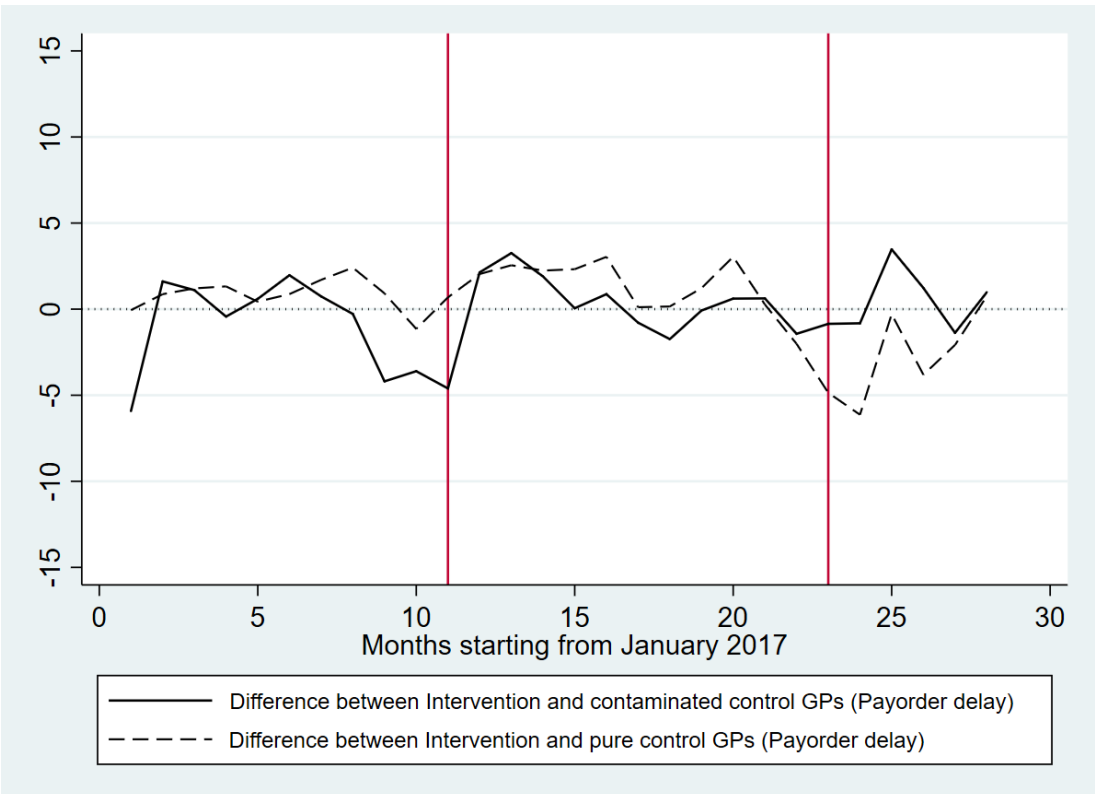
Source: Census of India (2011). Maps not to scale

Figure 2: Geographical location of the GPs getting the intervention



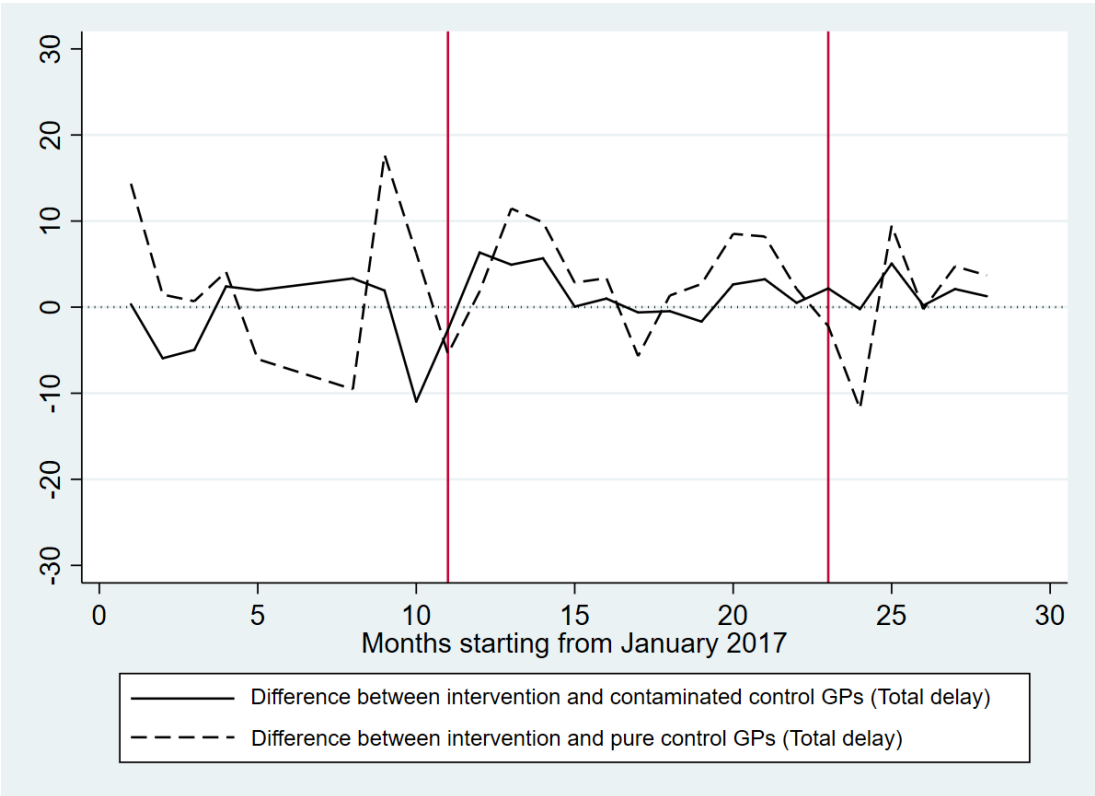
Source: Census of India (2011). Maps not to scale

Figure 3: Difference in payorder generation delay between intervention and control GPs (in days) starting from January 2017 to April 2019



Note: The month wise mean payorder delay (in days) is calculated by calculating the time difference in days between the date of completion of work and payorder generation date. The Y-axis shows the month-wise mean difference in payorder generation delays between intervention and types of control GPs. In X-axis, the months (of work completion) are plotted starting from January 2017. Hence "1" indicates January 2017; "12" indicates December 2017; "20" indicates August 2018 and so on. The period between the red lines is the period of intervention (November 2017 to November 2018).

Figure 4: Difference in total delay between intervention and control GPs (in days) starting from January 2017 to April 2019.



Note: The month wise mean total delay (in days) is calculated by calculating the time difference in days between the date of completion of work and the date of credit of wage in the bank/ postal account. The Y-axis shows the month-wise mean difference in total delays between intervention and types of control GPs. In X-axis, the months (of work completion) are plotted starting from January 2017. Hence "1" indicates January 2017; "12" indicates December 2017; "20" indicates August 2018 and so on. The period between the red lines is the period of intervention (November 2017 to November 2018).

Figure 5: Theoretical framework for impact on last mile delays.

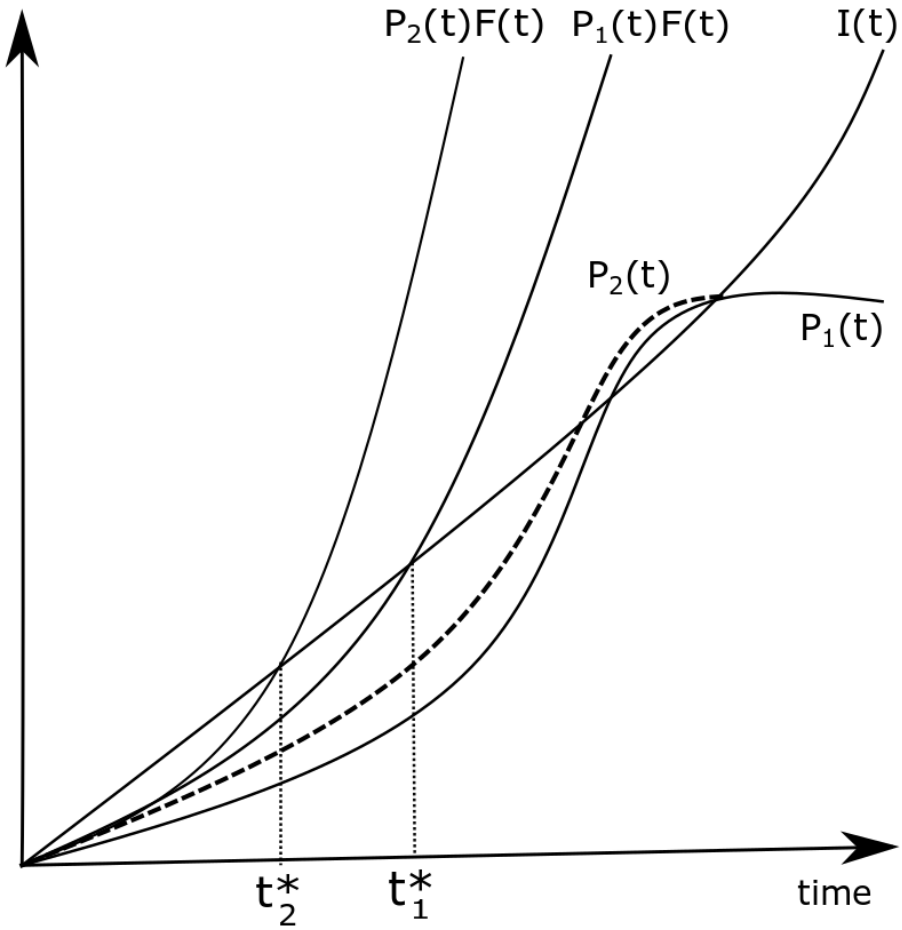
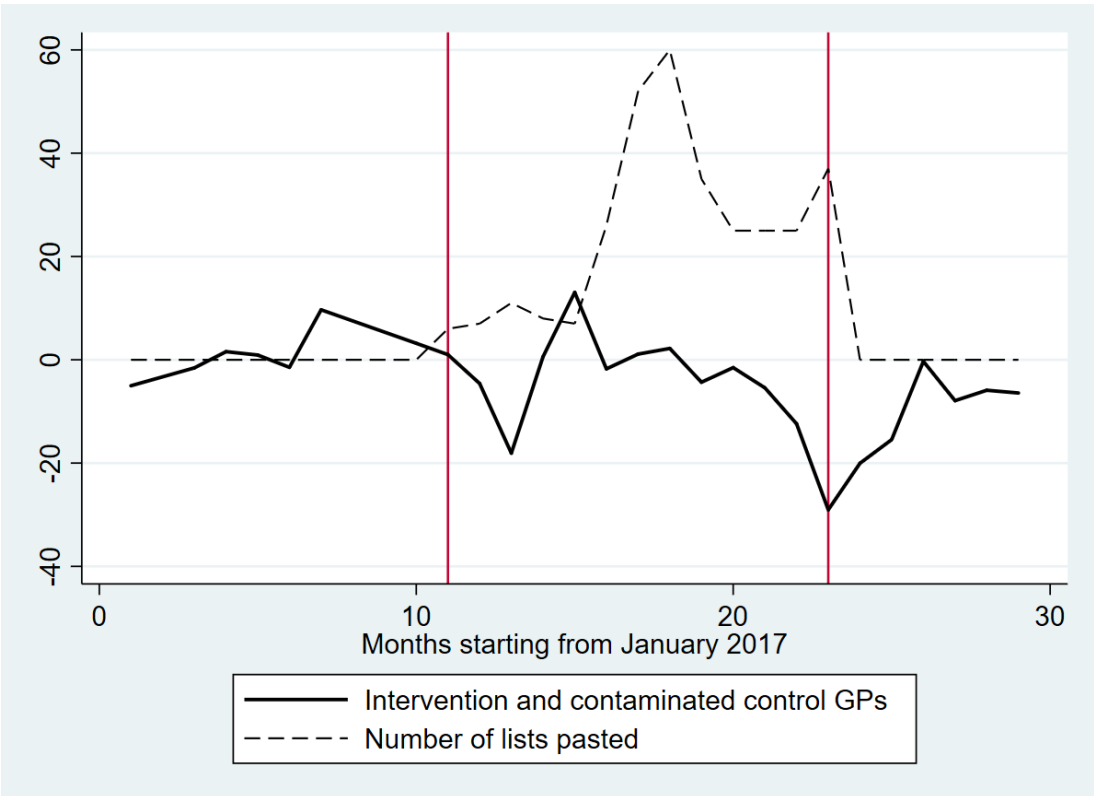
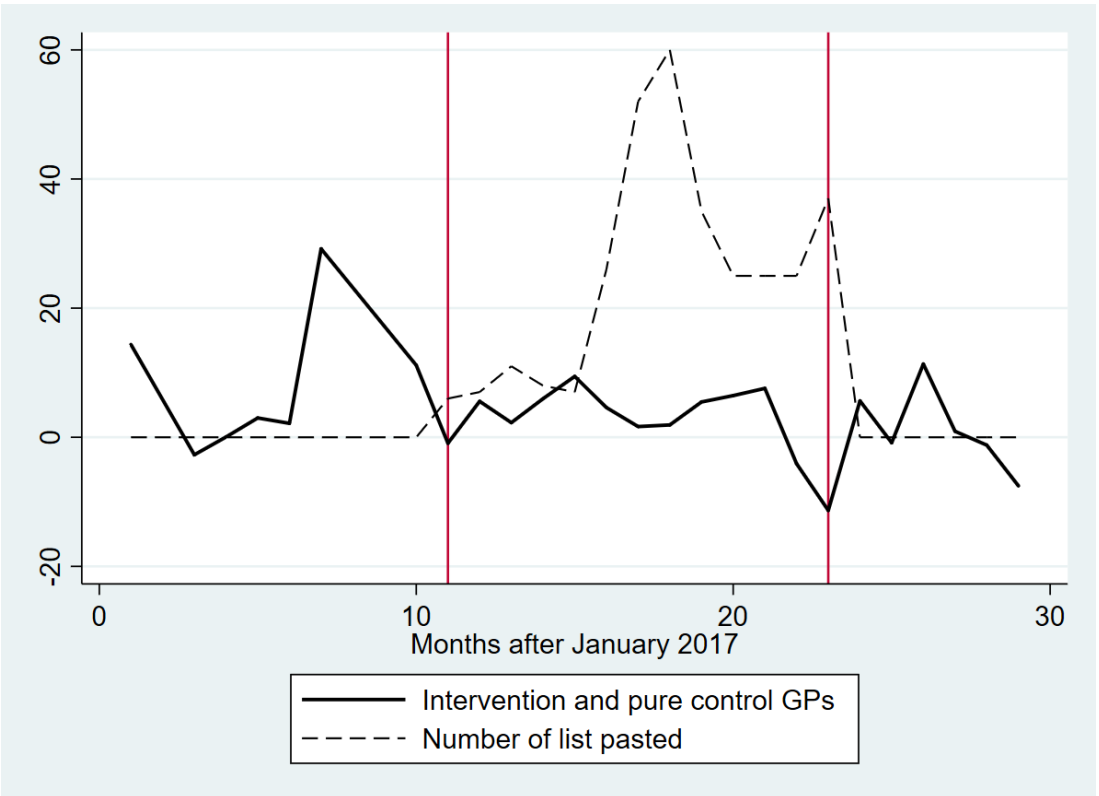


Figure 6: Difference in last mile delay between intervention and contaminated control GPs (in days) starting from January 2017 to April 2019.



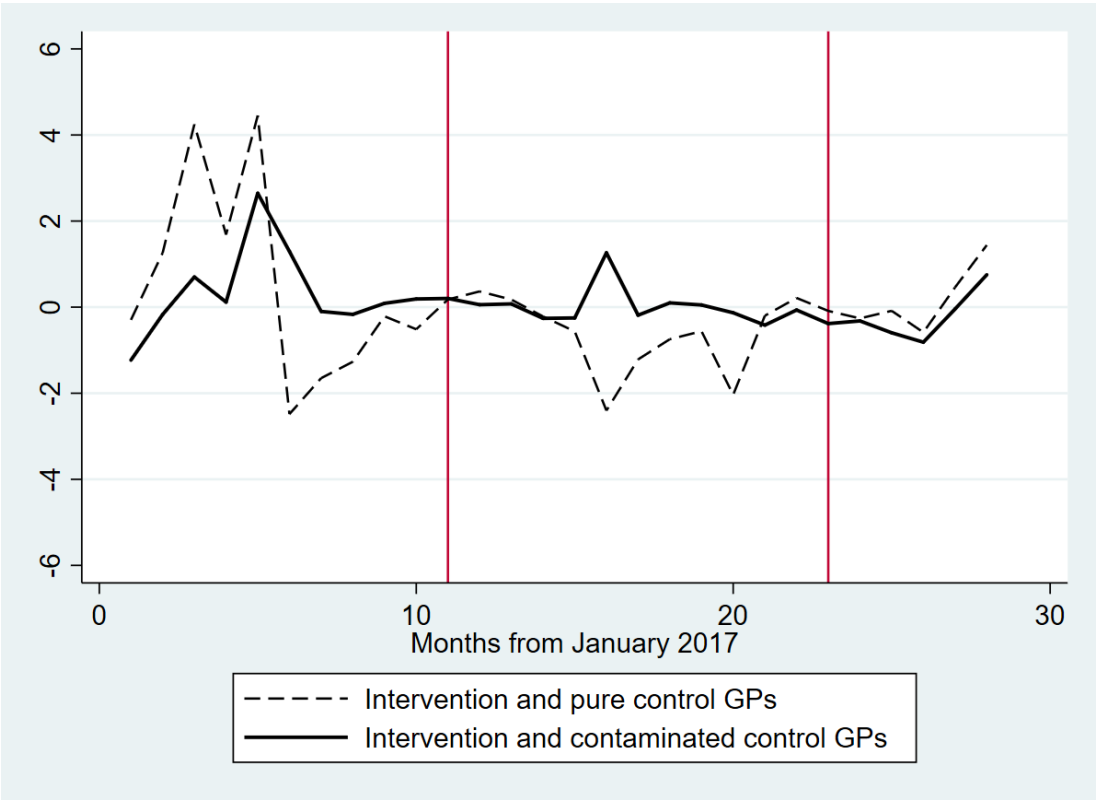
Note: The month wise mean level of last mile payment delay (in days) is calculated for intervention and contaminated control GPs and their difference is plotted. In X-axis, the months are plotted starting from January 2017. Hence "1" indicates January 2017; "12" indicates December 2017; "20" indicates August 2018 and so on. The period between the red lines is the period of intervention (November 2017 to November 2018). The dashed line plots the number of wage credit list pasted in all the intervention GPs combined across the intervention period. This gives an indication of intervention intensity related to reduction in last mile payment delays.

Figure 7: Difference in last mile delay between intervention and pure control GPs (in days) starting from January 2017 to April 2019.



Note: The month wise mean level of last mile payment delay (in days) is calculated for intervention and pure control GPs and their difference is plotted. In X-axis, the months are plotted starting from January 2017. Hence "1" indicates January 2017; "12" indicates December 2017; "20" indicates August 2018 and so on. The period between the red lines is the period of intervention (November 2017 to November 2018). The dashed line plots the number of wage credit list pasted in all the intervention GPs combined across the intervention period. This gives an indication of intervention intensity related to reduction in last mile payment delays.

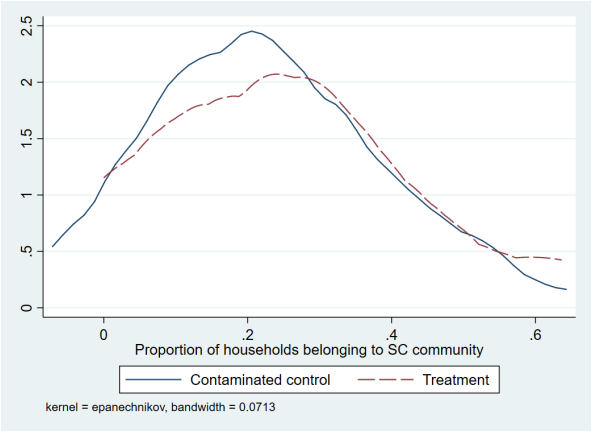
Figure 8: Difference in mean uptake between intervention and control GPs (in days) starting from January 2017 till April 2019.



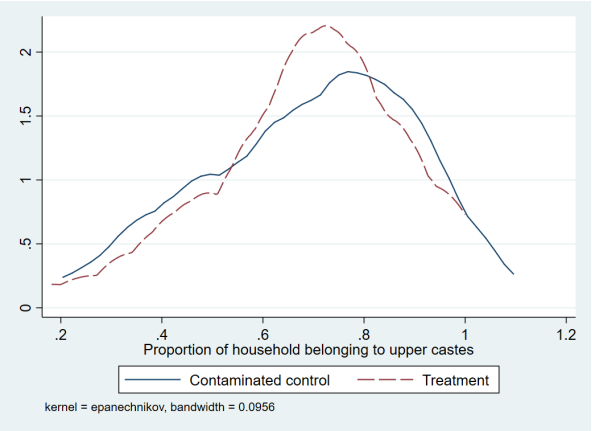
Note: The month wise mean level of uptake in days is calculated for intervention, contaminated control GPs and the pure control GPs is calculated and the difference between intervention and contaminated control GPs and the difference between intervention and pure control GPs are plotted in Y-axis. In X-axis, the months are plotted starting from January 2017. Hence "1" indicates January 2017; "12" indicates December 2017; "20" indicates August 2018 and so on. The period between the red lines is the period of intervention (November 2017 to November 2018).

7 Appendix

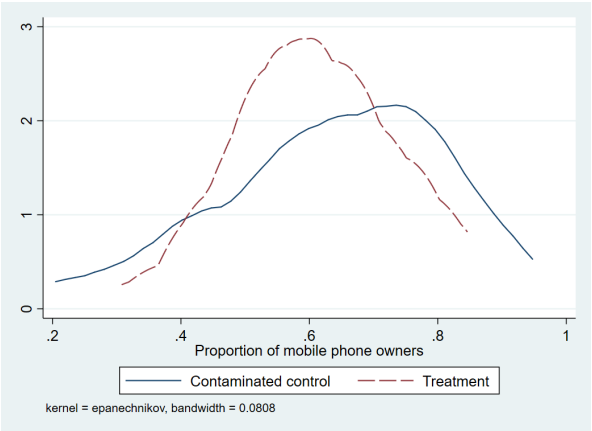
Fig A.1(a-l): Kernel density plots



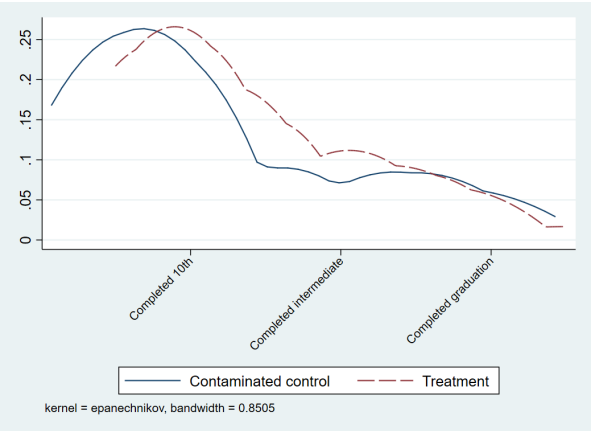
(a) Proportion of households belonging to SC community



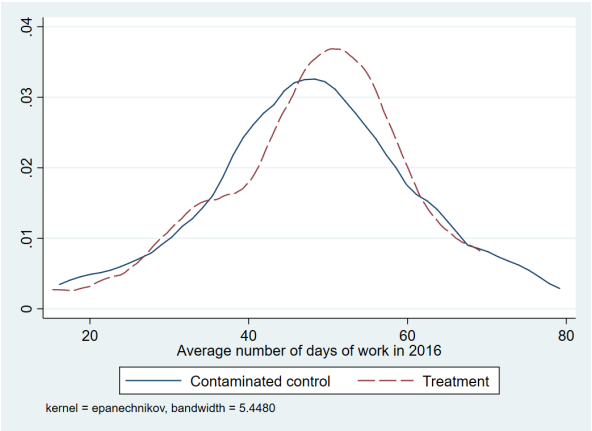
(b) Proportion of households belonging to Upper castes group



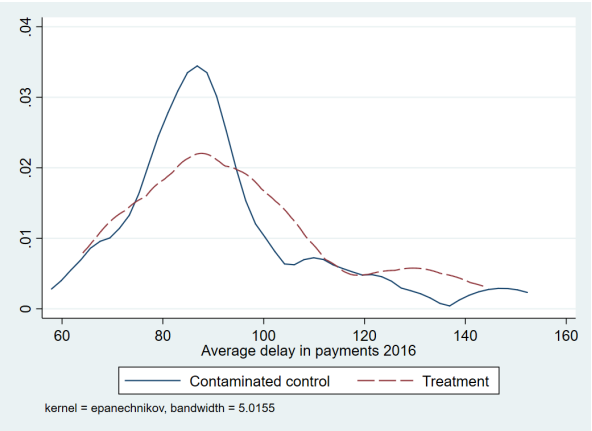
(c) Proportion of respondents with mobile phones



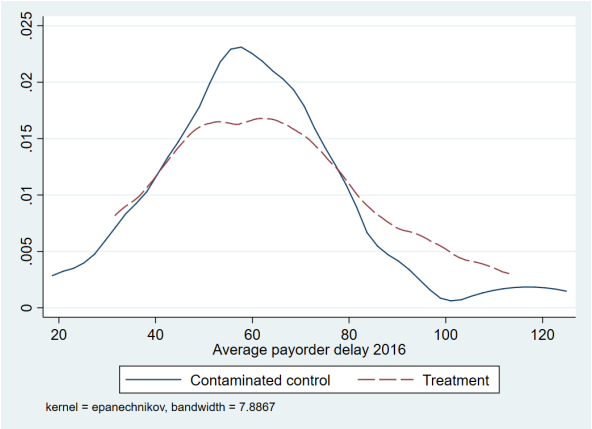
(d) Education of the GP FA



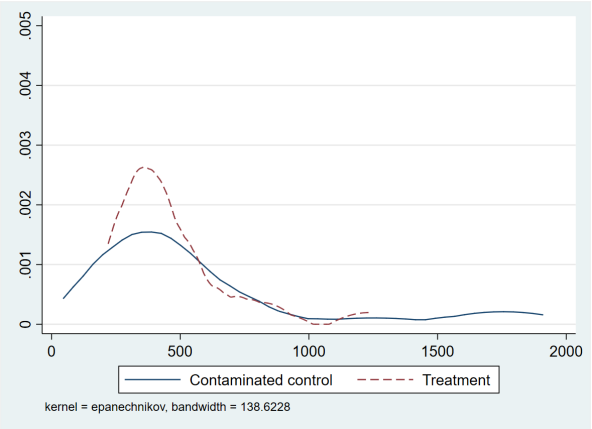
(e) Average number of days of work (2016)



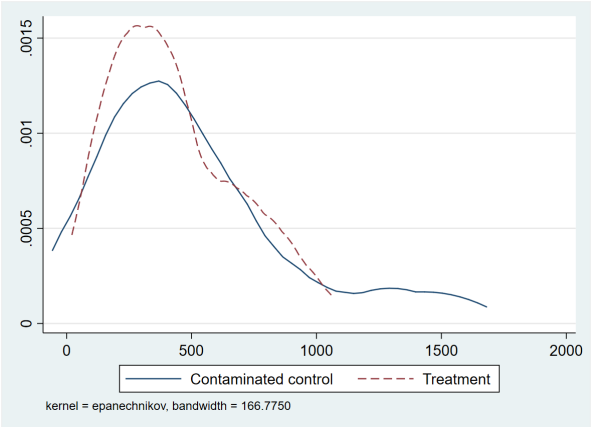
(f) Average delay in payments (2016)



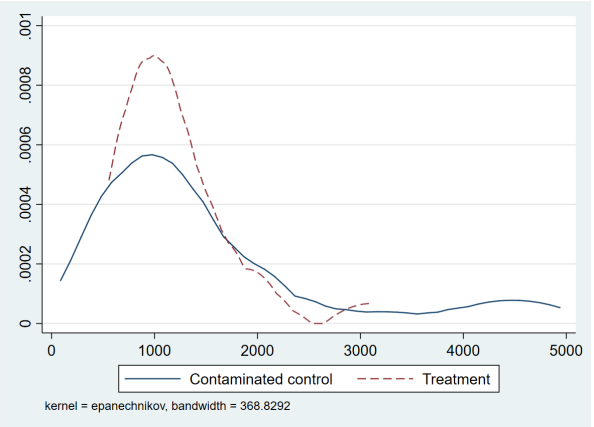
(g) Average delay in payorder generation (2016)



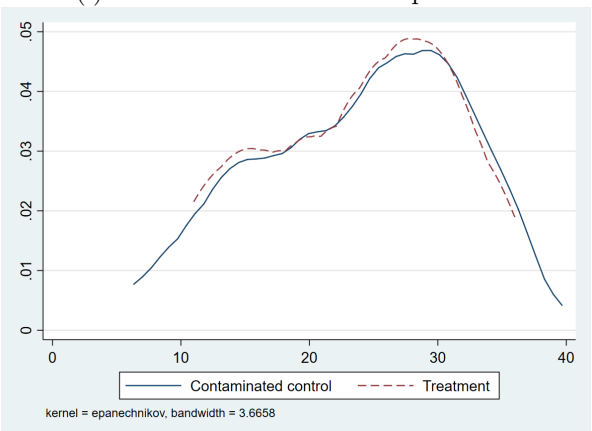
(h) Average delay in payorder generation (2016)



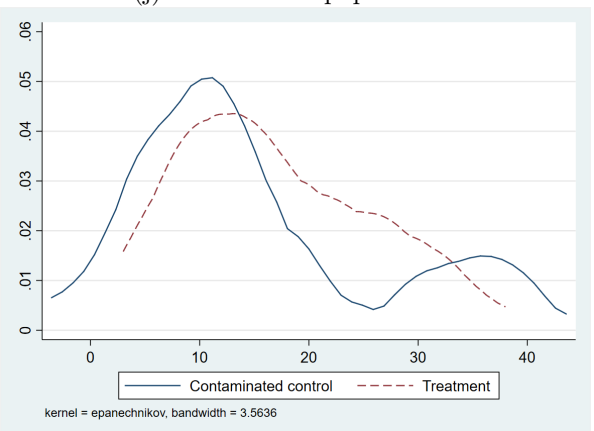
(i) Total Scheduled Caste Population



(j) Total female population



(k) Distance from the nearest town



(l) Nearest distance from block

Table A.1: Kolmogorov Smirnov tests

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 payments	0.278	0.304
Average GP level delay in payorder 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	1.00
Distance from the block office	0.232	0.525

Table A.2: Heterogeneous impact on awareness on literate population

	Work entitlement	Work application	Unemployment allowance	Payment duration	Wage rate	Awareness (simple average)	Awareness (weighted average)	Delay compensation
Comparison of treatment GPs with all GPs								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.119***	0.241***	0.139***	0.225***	0.193***	0.223***	0.049***	0.166***
	(0.043)	(0.050)	(0.022)	(0.057)	(0.032)	(0.038)	(0.009)	(0.022)
literate	-0.015	0.124***	-0.03	0.070**	-0.018	0.034**	0.003	0.01
	(0.029)	(0.039)	(0.034)	(0.033)	(0.025)	(0.016)	(0.002)	(0.023)
Interaction	0.011	-0.121*	0.048	-0.074	0.053	-0.033	-0.002	0.007
	(0.074)	(0.066)	(0.036)	(0.065)	(0.043)	(0.037)	(0.008)	(0.026)
Comparison of treatment GPs with contaminated control GPs								
Treatment	0.117***	0.229***	0.236***	0.232***	0.260***	0.222***	0.049***	0.358***
	(0.040)	(0.051)	(0.036)	(0.058)	(0.041)	(0.038)	(0.009)	(0.081)
literate	0.052	0.071	-0.062	0.121*	0.108**	0.069*	0.011*	0.027
	(0.061)	(0.092)	(0.086)	(0.063)	(0.048)	(0.035)	(0.006)	(0.039)
Interaction	-0.051	-0.077	0.101	-0.11	-0.049	-0.059	-0.007	-0.069
	(0.088)	(0.106)	(0.086)	(0.085)	(0.067)	(0.049)	(0.010)	(0.054)

Table A.3: Heterogeneous impact on process mechanisms and attendance in meetings on literate population

	Jobcard update by FA	Got receipt for work	Travelled more than once for wages	Attendance in GS meetings	Attendance in social audit meetings	Raised issue on MGNREGA
Comparison of treatment GPs with all GPs						
	(1)	(2)	(3)	(4)	(5)	(6)
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 treatment GPs with control GPs of intervention block						
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)

Table A.4: Heterogeneous impact on awareness on mobile phone owners

	Work entitlement	Work application	Unemployment allowance	Payment duration	Wage rate	Awareness (simple average)	Awareness (weighted average)	Delay compensation
Comparison of treatment GPs with all GPs								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.132**	0.255***	0.181***	0.208***	0.160***	0.217***	0.046***	0.190***
	(0.057)	(0.070)	(0.029)	(0.068)	(0.047)	(0.048)	(0.010)	(0.036)
Possess a mobile phone	0.131***	-0.033	0.063**	0.112***	0.063***	0.060***	0.009***	0.038
	(0.027)	(0.037)	(0.028)	(0.033)	(0.022)	(0.014)	(0.002)	(0.024)
Interaction	-0.02	-0.071	-0.054*	-0.001	0.069	-0.002	0.004	-0.038
	(0.066)	(0.068)	(0.030)	(0.063)	(0.044)	(0.041)	(0.008)	(0.026)
Comparison of treatment GPs with contaminated control GPs								
Treatment	0.068	0.212**	0.329***	0.208***	0.111*	0.186***	0.043***	0.337***
	(0.061)	(0.084)	(0.061)	(0.073)	(0.057)	(0.052)	(0.011)	(0.095)
Possess a mobile phone	0.024	-0.1	0.135*	0.136**	-0.063	0.022	0.007	-0.043
	(0.041)	(0.072)	(0.073)	(0.057)	(0.047)	(0.026)	(0.005)	(0.059)
Interaction	0.068	-0.001	-0.122*	-0.001	0.214***	0.037	0.007	0.007
	(0.069)	(0.091)	(0.073)	(0.076)	(0.063)	(0.046)	(0.009)	(0.066)

Table A.5: Heterogeneous impact on process mechanisms and attendance in meetings on mobile phone owners

	Jobcard update by FA	Got receipt for work	Travelled more than once for wages	Attendance in GS meetings	Attendance in social audit meetings	Raised issue on MGNREGA
Comparison of treatment GPs with all GPs						
	(1)	(2)	(3)	(4)	(5)	(6)
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 treatment GPs with contaminated control GPs						
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)