Improving Last-Mile Service Delivery using Phone-Based Monitoring*

Karthik Muralidharan† UC San Diego

Paul Niehaus‡ UC San Diego

Sandip Sukhtankar§ University of Virginia

Jeffrey Weaver¶ University of Southern California

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Abstract

Improving "last-mile" public-service delivery is a recurring challenge in developing countries. Could the widespread adoption of mobile phones provide a scalable, cost-effective means for improvement? We use a large-scale experiment to evaluate the impact of phone-based monitoring on a program that transferred nearly a billion dollars to 5.7 million Indian farmers. In randomly selected jurisdictions, officials were informed that program implementation would be measured via calls with beneficiaries. This led to a 7.8% reduction in the number of farmers who did not receive their transfers. The program was highly cost-effective, costing 3.6 cents for each additional dollar delivered.

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†UC San Diego, JPAL, NBER, and BREAD. kamurali@ucsd.edu.

‡UC San Diego, JPAL, NBER, and BREAD. pniehaus@ucsd.edu.

§University of Virginia, JPAL, and BREAD. sandip.sukhtankar@virginia.edu.

¶University of Southern California. jbweaver@usc.edu.

The low quality of public service delivery in developing countries adversely affects the lives of billions of people (World Bank, 2003). As a result, improving "last mile" service delivery has been a recurrent theme in recent research, from incentivizing employees to show up to work (Duflo, Hanna and Ryan, 2012) to ensuring that beneficiaries receive money they are entitled to (Muralidharan, Niehaus and Sukhtankar, 2016). One common challenge is that, like any organization, a government can only manage its personnel to the extent that it can *measure* their performance (Bloom and Van Reenen, 2007). Yet measuring service delivery is difficult, especially when front-line work takes place in many remote and dispersed communities.

Existing mechanisms for measuring and monitoring last-mile service delivery are limited in different ways. Internal reporting by the lower layers of bureaucracy is often distorted, as individuals exaggerate their own performance or overstate problems with initiatives they want to undercut.[1] Mechanisms through which citizens can report problems in service delivery, such as grievance redressal phone numbers, yield non-representative data and are often heavily underutilized, perhaps because beneficiaries do not think they will help.[2] Finally, periodic surveys such as the Living Standards Measurement Surveys or India's National Sample Survey yield independent and representative data, but are typically too small, infrequent, and delayed in their release to be of use for management purposes.

In this paper, we test whether governments can improve last-mile service delivery using a simple approach to measuring whether people get what they are due: calling and asking. This approach leverages the rapid increase in mobile phone penetration in low-income countries, from 1 mobile subscription per 100 people in 2002 to 62 in 2017 (World Bank, 2018). Phone calls bypass bureaucrats to obtain information directly from beneficiaries, without the cost and delay associated with field surveys. Managers can use phone-based monitoring data to track and incentivize worker performance in close to real-time. Governments have begun to use outbound phone calls to gather data on service delivery, with Pakistan's Citizen Feedback Monitoring Program and Andhra Pradesh's Real Time Governance initiative as prominent examples (Masud, 2015; World Bank Global Report, 2016; Gelb, Mittal and Mukherjee, 2019). However, there is limited evidence to date on the impact of phone-based monitoring by governments on the quality of service delivery.

We examine whether phone-based measurement can in fact improve service delivery in the context of a high-stakes government initiative in India. Beginning in May 2018, the

[1] As an example of the former, Singh (2019) finds evidence of substantial over-reporting of primary student learning levels in official data compared to an independent retest of the same students.

[2] For example, only 8,292 complaints were registered between 2012 and 2016 regarding the Public Distribution System in India (Department of Food and Public Distribution, 2018), despite the fact that it serves hundreds of millions of people and has an all-India leakage rate of 32% (Dreze and Khera, 2015).

government of Telangana state (GoTS) attempted to distribute \$0.9B, or around 3.5% of the state's annual budget, as lump-sum payments to farmers. Responsibility for implementing the scheme rested primarily with Mandal (sub-district) Agricultural Officers (MAOs), who managed the distribution of physical checks to the 5.7 million farmers in the state. An important goal of the program was to reduce the debt taken on by farmers to finance the upfront costs of cultivation. The government therefore made it a priority to deliver transfers to farmers before the onset of the monsoon rains to facilitate timely agricultural investments, such as purchasing seeds and fertilizer, and hiring workers to prepare their fields.

Working with the government, we implemented an experimental, at-scale test of phone-based performance measurement. We randomly assigned around a quarter of the 498 MAOs in the state to a treatment condition, in which they were told that a call center would call at least 100 of the farmers for whom they were responsible, collect information on implementation outcomes (including whether and when farmers received their checks, and corruption during the process), and produce reports visible to them and their supervisors. These details were communicated to treated MAOs by the state Commissioner of Agriculture personally via a video conference. This communication was then reinforced with a formal letter to treatment MAOs. The government contracted a call center that surveyed 22,127 farmers within two weeks. The data from these calls were used to create reports on MAOs' absolute and relative performance.

To evaluate the impact of the announcement of phone-based monitoring to MAOs, we use farmer-level administrative bank records of whether and when these checks were encashed as a reliable measure of MAO performance. This high-quality administrative data on the entire universe of 5.7 million potential program beneficiaries allows us to precisely estimate the impact of the phone-based monitoring treatment on the entire population - including beneficiaries with and without cell phones. It also provides the opportunity to test the reliability of phone data as compared to administrative data. Thus, the data from the phone calls were used to *implement* the intervention (creating MAO reports), while administrative data were used to *evaluate* the intervention. Taken together, the design and data allow us to "experiment at scale".[3]

We find that phone-based monitoring significantly improved the likelihood of farmers ever receiving their transfer, as well as receiving it "on-time", meaning prior to the arrival of the monsoon on June 8th, 2018. On-time delivery of transfers was 2.4 percentage points higher in

[3]Specifically, the study meets all three aspects of "experimentation at scale" identified in Muralidharan and Niehaus (2017). The treatment is randomized across all 5.7 million land-owning farm households in a state of 35 million people (and outcomes are observed for all of them); the intervention was implemented by government at scale, with 1.3 million farmers in treated areas; and the unit of randomization was large enough for treatment effects to be inclusive of spillovers.

treated mandals (with a mean of 69% in control mandals), which is a 0.25 standard deviation increase relative to the distribution of mandal-level means in control mandals. The likelihood of checks ever being delivered was 1.3 percentage points higher in treated areas (the control mean was 83%), or a 0.16 standard deviation increase. Expressed differently, the intervention led to a 7.8% reduction in the number of beneficiaries who did not receive their benefits. These effects correspond to a $3.9 million increase in transfers that were delivered on-time, a $1 million increase in amount ever delivered, and 17,771 additional farmers encashing their checks.[4] If phone-based monitoring had been extended to the entire state over both agricultural seasons, our estimates suggest that $33.1 million more would have been delivered on time, an additional $8.6 million would ultimately have been delivered, and around 155,000 additional farmers would have received their payments.

The incidence of the intervention was mildly progressive, although the transfer program itself was regressive (since check sizes were proportional to landholdings). For farmers in the bottom quartile of landholdings, there was a 2.2 percentage point (3.3%) increase in check encashment rate, around twice the overall effect. We do not find evidence that MAOs skewed their efforts towards farmers with phones. Although the phone-based monitoring system only assessed MAO performance by calling the approximately 60% of farmers with listed cell phone numbers, we cannot reject that the measured improvements in performance are the same across beneficiaries with and without phones.

Next, we estimate that phone-based measurement was highly cost-effective. Costing the intervention at the price paid to the call-center vendor, we estimate that the incremental cost per additional dollar of benefits ever delivered to beneficiaries was 3.6 cents. Turning to on-time delivery, the cost per dollar of benefits delivered on time was less than one cent. These costs per additional dollar delivered are lower than the administrative costs of almost any anti-poverty program for which such data is available (see Section III.C). In addition, we also calculate the economic returns to phone-based monitoring. To do this, we define benefits as the difference between the estimated return on capital held by farmers rather than the government. Even under relatively conservative assumptions, we estimate a benefit of four times the cost.

Finally, we compare the phone and administrative data to assess the quality of the data obtained from the phone calls.[5] We find that the match rate between phone and adminis-

[4] Among larger landowners, the treatment effect is larger for "on-time" delivery than "ever delivered". Since they received larger transfers, the impact on on-time delivery relative to ever delivered is bigger for *value* of funds than for *fraction* of checks.

[5] In this particular case, we were fortunate to have high-quality administrative data on outcomes. However, in other cases, the data from the phone calls may be the only source of data available, which makes it policy-relevant to compare phone and administrative data.

trative data is high (88.6%). Comparing measures of MAO performance using both phone and administrative data, our results suggest that the phone-based data is reliable enough to identify which MAOs should be pushed for more effort, but perhaps not reliable enough for more serious personnel actions without data over multiple cycles.

Turning to mechanisms, monitoring systems of the kind we study could affect outcomes through two main channels. First, the anticipation of reports generated by the system could alter the *incentives* of the agents being monitored. In our context, there was no explicit link between the measurement and future benefits such as advantageous postings (Khan, Khwaja and Olken, 2019). However, the MAOs may have anticipated that the monitoring would increase the visibility of their performance to senior officials and potentially be used for future rewards. They may also care about how their performance looked relative to their peers. This would plausibly incentivize exertion of additional effort, although monitoring systems explicitly linked to formal incentives might provide an even stronger push.

Second, phone-based monitoring could affect service delivery by providing the principal monitoring the agent, or the monitored agent with *information* that affects how they deliver services. For example, a principal could use the information generated by the system to determine which agents required nudging to work harder, or an agent could adjust his own effort based on what he learns about his performance as well as that of others.[6]

Our results are best explained by the incentive mechanism. Because the check distribution was implemented quickly, MAOs and their supervisors received performance reports only after nearly 95% of all checks that would be delivered had been delivered. At that point, it was too late for the information contained in the reports to affect MAO behavior. Consistent with this, we find significant treatment effects emerge shortly after the *announcement* of the monitoring (reflecting the incentive effect), but no change in the treatment-control gap after performance reports were issued. In this sense, we view the results as a lower bound on the long-term potential effects of the approach as compared to contexts where it would also be possible to use information from the reports for officials to do their jobs better.[7]

This paper complements recent work testing more specialized approaches for using technology to improve governance such as monitoring worker attendance with time clocks (Banerjee et al., 2010; Dhaliwal and Hanna, 2017) or with custom smart-phone applications (Callen et al., 2018). Relative to these specialized approaches, measurement by phone has the ad-

[6] Although this was not a component of the intervention we study, phone-based monitoring systems could in principle also provide new information to the monitored agent, such as geographically disaggregated data on service delivery. For example, the MAO may lack information on village-level delivery of services by agricultural extension workers whom they supervise. If the reports had contained village-level information, the MAO could use it to identify and allocate resources towards the most underserved communities.

[7] In addition to these two mechanisms, there could also be a direct effect of the phone calls to farmers, such as encouraging them to encash their checks. We test for and find no evidence for such an effect.

vantages of (i) low fixed and variable costs and time to deploy, as call center services are typically cheap and available quickly, (ii) the flexibility to scale across an unusually wide number range of places, programs and outcomes, and (iii) scope to adapt quickly as challenges and circumstances on the ground change. We also complement recent work by Aker and Ksoll (2018), who test a phone-based monitoring intervention implemented by an NGO in an adult education program in 134 villages in Niger and find learning gains.

Empirical evidence on service delivery in developing countries suggests that increased top-down administrative monitoring can be an effective tool for improving last-mile service delivery (Olken, 2007; Muralidharan et al., 2017). However, a practical barrier to the broader use of top-down monitoring has been the challenge of obtaining credible high-frequency data on last-mile service delivery at a sufficiently spatially disaggregated level to hold appropriate officials accountable. Our results suggest that calling and surveying representative samples of beneficiaries, who increasingly have access to a phone, may provide a promising solution.[8]

Specifically, using outbound call centers for measuring the quality of last-mile service delivery can expand state capacity for measurement on several policy-relevant margins including scale, cost, and speed. First, the intervention was successfully implemented by the government at a scale affecting nearly 6 million households.[9] Second, the cost per phone survey is substantially lower than the cost of a field survey, which allowed for a much larger sample size within a fixed budget. This made it possible to generate credible estimates of program performance at a lower-level unit of governance than would typically be possible, and improve the accountability of officials who are closer to the last-mile of service delivery. Third, the intervention was both simple to set up (taking one month from agreement to implementation) and quickly generated usable data on MAO performance, within two weeks of starting to make the phone calls.

Of course, this new measurement technology should only be expected to deliver improved governance when there is also political interest in doing so (as was true in this case). Thus, our results should be interpreted as demonstrating the value of expanding the feasible set of measurement for governments. Historically, innovations in measurement technologies have been a foundation for improved productivity in several settings by enabling better coordination, contracting, and management (Landes, 1983; Baker and Hubbard, 2004; Duflo, Hanna and Ryan, 2012). High-frequency and low-cost measurement of last-mile service delivery us-

[8]While prior studies of monitoring have looked at impacts on reducing corruption or provider absence, we focus on improving bureaucratic effort and service delivery quality. The value of doing so is seen for instance in the results from Bandiera, Prat and Valletti (2009) who show that inefficiencies may be a much larger source of welfare loss in government than corruption per se.

[9]Testing at scale under government implementation is especially important for policy given recent evidence that interventions implemented by governments have systematically smaller effects than those implemented by academics or NGO's (Vivalt, 2019).

ing phone-based monitoring could similarly enable productivity improvements in the delivery of public services.

The rest of the paper is organized as follows. Section 2 describes the setting and nature of the phone-based monitoring intervention. Section 3 describes the research design and data. Section 4 presents the results and a cost-effectiveness analysis. Section 5 concludes.

I Setting and intervention

Telangana is India's newest state, created in 2014 from Andhra Pradesh. It has a population of 35 million, with around 60% living in rural areas, and is relatively well-off, with per capita income 53% higher than the all-India average as of 2016-2017 (Government of Telangana, 2016). It has maintained the same administrative infrastructure as when it was part of Andhra Pradesh, including many of the same bureaucrats and the same capital city. As a result, it is thought to be relatively well-administered; Andhra Pradesh ranked 3rd out of 19 major states in the most recent Government Performance Index (Mundle, Chowdhury and Sikdar, 2016).

Although the state is relatively well administered as compared to other Indian states, last-mile service delivery continues to be a challenge as seen by Telangana's performance on India's two largest welfare programs - the Public Distribution System (PDS) for food security and the National Rural Employment Guarantee Scheme (NREGS) for employment security. Dreze and Khera (2015) estimate that 22% of subsidized food provided under the PDS does not reach intended beneficiaries, and Muralidharan, Niehaus and Sukhtankar (2016) find leakage of 18 to 30.7% of NREGS funds in the state. These facts reflect both corruption as well as weak state capacity to monitor and motivate effort by its workers. Thus, finding low-cost scalable ways of improving state capacity for measuring last-mile service delivery and using this to improve monitoring of workers is likely to be of broad use and interest.

I.A The Rythu Bandhu scheme

The Government of Telangana (GoTS) introduced the "Rythu Bandhu Scheme" (meaning "Friend of the Farmer", hereafter abbreviated as RBS) in May of 2018 to provide capital for the purchase of agricultural inputs prior to the main agricultural season. The RBS was hailed by economists as a more efficient response to widespread farmer economic distress than common alternatives such as raising crop procurement prices or waiving farmers' loans (Subramanian, 2018). It authorized payments of Rs. 4,000 ($55) per acre to every farmer registered as a landholder in each of the two yearly agricultural seasons.[10] A farmer who owned half an acre of land, for example, would receive one payment of Rs. 2,000 prior to the *kharif* cycle (monsoon/fall) and another payment of Rs. 2,000 prior to the *rabi* season (winter/spring). The mean check amount was Rs. 8,817, while the median was Rs. 5,280 (see figure A.1 for the full distribution of check amounts).

In total, the expected outlay for RBS was approximately $0.9B per cropping season or $1.8B annually – equivalent to 7% of the annual state budget and 1.6% of the GDP of Telangana (PRS Legislative Research, 2018). As a fraction of GDP, this is more than three times as large as two of the most well-known cash transfer programs – Progresa in Mexico (0.4% of GDP; Dávila Lárraga, 2016) and Bolsa Familia in Brazil (0.5% of GDP; Gazola Hellman, 2015). The RBS has also contributed to a broader trend in India towards such large cash transfer programs for farmers, where there are now similar programs at the national level ($11B annually) and in two other Indian states (Odisha and Andhra Pradesh, worth $1.9B annually in each state) (Outlook India, 2019; Business Standard, 2019; LiveMint, 2019).

GoTS had never previously attempted this type of cash transfer program, so even aside from the unprecedented size of the transfer, figuring out implementation was non-trivial. Since GoTS did not have bank account information for landholders, they could not transfer money into the farmer's bank accounts. They instead elected to distribute physical checks to each farmer in the form of "order checks" associated with a particular bank. Farmers could go to any branch of the bank listed on the "order check" and exchange it for cash, whether or not they held an account with that bank (conditional on providing official ID matching the name on the check). The government allocated all the mandals in the state among 8 banks, assigning all farmers in a given mandal to the same bank. The Department of Revenue managed the printing of checks (including confirming eligibility of beneficiaries), which occurred in April and May, after which the checks were transferred to the Department of Agriculture for distribution.

The government had originally intended to distribute checks in April and May so that farmers had enough time to apply them towards agricultural investments prior to the onset of the monsoon in June. Typical investments include purchasing seeds and fertilizer, and hiring workers to prepare their fields. It is optimal for farmers to plant soon after the monsoon rains arrive; if they wait, seeds fail to benefit from the rain that has already fallen and yields are lower.[11] However, due to delays in implementation, the check distribution

[10] The government's land registry was updated and digitized in late 2017 and early 2018 to reflect the identity of current landholders.

[11] Giné, Townsend and Vickery (2007) measure expenditure patterns of farmers in the Mahbubnagar district of Telangana and find that 37.4% of all expenditures for a season occur prior to the arrival of the monsoon. This includes 30-40% of all spending on bullock, manual, and tractor labor, 90.1% of all spending on manure,

process did not begin until May 8, creating additional pressure for speedy delivery.[12]

The Department of Agriculture managed the distribution of checks, with Mandal Agricultural Officers (MAOs) responsible for distribution in their respective mandals. In most cases, a MAO was responsible for one mandal, though due to vacant MAO positions, there were a few cases in which a single MAO oversaw implementation across multiple mandals (498 MAOs covered 548 mandals). An average mandal contains roughly 60,000 individuals living in 20 villages, of whom around 10,000 are landholders receiving checks. The program was widely discussed in the media, and beneficiary awareness about the program was high. MAO's were assisted by junior officials in informing village-level leaders regarding the dates of the village-level meetings for distributing these checks, and in implementing the large-scale check distribution process.

The MAOs scheduled one meeting in each village. During those meetings, the MAOs supervised teams of government employees who verified the identities of landowners residing in that village and distributed their checks to them. The first village meetings were held on May 8. Each mandal then had 1-2 village meetings per day, with meetings every day of the week except Sunday, until all villages had been served. There was only one meeting per village, so if a landholder did not get their check then, they had to pick it up at the mandal-level Department of Agriculture offices. To do so, however, they needed signed approval from both the MAO and another bureaucrat in the revenue department, which could be a substantial barrier if for example one or both were absent; beneficiaries may also not have understood the process to follow for picking up a check outside of the village meetings.

Implementing RBS well was a major priority for the government given the sum disbursed, the number of recipients (5.7 million), the high media profile of the scheme, the fact that the government had never before done anything comparable, and upcoming elections. Anticipated risks included (i) non-issue of checks, (ii) non-delivery of checks, (iii) late delivery of checks, which would force farmers to reduce investment or borrow at high rates to finance time-sensitive agricultural inputs, and (iv) corruption during the distribution process (e.g. bribe demands).[13]

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and 59.4% of spending on irrigation. Additional fertilizer is typically purchased and applied later in the season.

[12]The two main delays were in the printing of checks and ensuring that bank branches held sufficient cash to permit encashment of nearly a billion dollars in checks over a short period of time.

[13]While this is of course a particular type of service (delivery of cash), the problems are similar to the delivery of many other services in the last mile. Even when there is no corruption per se, limited state capacity for measuring and monitoring the effectiveness of service delivery staff can contribute to slack in their effort (as seen for instance in the absence of workers).

I.B Phone-based monitoring intervention

The state government had previously collected phone numbers for farmers as part of land record digitization. Overall, 3.5 million (61%) of the 5.7 million entries in the registry listed a contact number. GoTS contracted a call center to collect data from beneficiaries between 29 May and 15 June.[14] The call center attempted to reach a random sample of 46,263 farmers representative of those with listed phone numbers in the GoTS administrative records. A sample of 150 farmers per treatment mandal and 50 per control mandal was randomly selected, stratified by quintile of the mandal-specific land size distribution to reduce variability across mandals due to sampling.[15]

The call center placed calls to the mobile phone numbers of sampled farmers. If a call did not connect, the call center would attempt to reach that number up to five more times over the following two days before giving up. If connected, the call center operator verified the respondent's identity and identified themselves as conducting a survey on behalf of the Government of Telangana to understand the respondent's experience with the Rythu Bandhu Scheme. Calls collected information on whether, where, and when the farmer received their check, whether and when they encashed it, any problems receiving or encashing the check (including time costs and bribes), how they used the funds, suggestions for future rounds of RBS, and overall satisfaction with RBS. Sampled farmers were surveyed once, and the average completed survey lasted four minutes. The call center successfully completed calls with 22,127 (47.8%) of the sampled farmers, began but did not complete surveys with another 24%, had 10% decline to participate, and could not reach the remaining 18% for other reasons (e.g. phone number was no longer valid).[16] The rate of phone call completion did not differ across treatment and control mandals (47.6% in control, 48.0% in treatment; $p = 0.39$).

Prior to the distribution of checks and calls to beneficiaries, the Telangana Department of Agriculture informed treatment MAOs that their mandals had been selected by lottery to take part in a pilot of the phone-based monitoring system. During a special video conference with the treatment MAOs (on 2 May 2018), the state Commissioner of Agriculture explained the initiative and the data that would be collected. He informed them that reports from the

[14]The intervention was designed by the research team, but implemented by GoTS.

[15]Control MAOs were not informed about the phone calls to farmers in their mandals since data from these calls was not used to prepare reports or even shared with the Department of Agriculture. The data from control mandals was collected solely for research purposes.. As discussed in the pre-analysis plan, we had originally anticipated using the phone data to compare between treatment and control areas on outcomes such as corruption and satisfaction that were not available in the administrative data. Although we report these outcomes for completeness in Table A.2, this analysis turned out to be highly underpowered due to extremely low rates of corruption and high rates of beneficiary satisfaction.

[16]The vendor also piloted automated calls (IVR), but these had a high error rate in capturing responses, and were therefore discontinued.

phone call data would be provided to them and their supervisors, including a performance rating for their mandal. The MAOs were told which outcomes the report would cover, but not the specific formula for calculating ratings. On 10 May 2018, the Department of Agriculture sent treatment MAOs a follow-up letter containing the same information. To reduce the risk of spillovers, treatment MAOs were explicitly told the identity of other treatment MAOs in their district and that no other MAOs in their districts were part of the pilot.

In order to give MAOs time to distribute checks and thus have their performance accurately reflected in the call data, phone calls to farmers commenced on May 29th, three weeks after check distribution began, and continued through June 15th. Reports based on the phone data were issued to treatment MAOs and to their supervisors between 9 and 13 July. The reports listed five metrics: the proportion of farmers who reported receiving their check, receiving it before 20 May (a measure of speed of delivery), successfully encashing it at the bank, being asked for a bribe, and being satisfied with the program overall. They rated performance on these metrics for the mandal in question relative to other mandals within the same district, and relative to the state overall. They also showed a simple, color-coded categorical rating ("Poor," "Fair," "Good," or "Excellent") based on absolute performance, motivated in part by the finding of Callen et al. (2018) that "flagging" of high or low performers can make performance data more accessible. A redacted example report is in Appendix B. Table 1 provides the full timeline of the Rythu Bandhu Scheme and phone-based monitoring intervention.

The treatment of phone-based monitoring in principle includes both a monitoring component (which potentially altered incentives) and an information component (which could help MAOs or the MAOs themselves do their jobs better). In practice, and because of the program's compressed time frame, the information provided by the reports came too late for District Agricultural Officers (DAOs) or MAOs to react to it. The program aimed to distribute all checks between early May and mid-June, whereas reports were issued in early July (by when 95% of checks ever issued had already been issued). Our estimates thus reflect the impact of MAOs knowing they were being monitored, but do not capture the potential additional benefits of DAOs and MAOs having timely information from the calls.

The Department of Agriculture did not inform control MAOs about the existence of the pilot. If asked, it said that the initiative might be extended to their areas in the future, but not during the current season. While the call center collected phone data from control mandals, it did not generate reports using these data or inform control MAOs of their existence. Of course, the interpretation of reduced-form intent-to-treat effects depends on treatment and control MAOs' beliefs, which we discuss later.

As discussed in the introduction, MAOs may respond to this monitoring and the creation of reports, even in the absence of formal incentives, for several reasons. These include wanting to look good on rankings of performance relative to their peers, and not wanting to get admonished for poor performance (which was made more likely by the availability of data on absolute and relative performance).

There were several ways in which MAOs and their staff could potentially improve their performance in response to this additional monitoring. They could improve processes to ensure that checks were distributed to all eligible beneficiaries. They could work harder to find recipients – both before the village meetings by publicizing them more thoroughly, during the meetings by extending the meeting length, or after them by following up with those who did not attend. They could also demand fewer bribes.[17]

Yet one might also reasonably expect phone-based monitoring to have limited effects in this setting, as government scrutiny of RBS implementation was already high: MAOs digitally recorded whether checks were distributed, and banks recorded check encashment in order to claim reimbursement. The availability of high-quality administrative data on outcomes makes the RBS an unusually low-cost setting in which to measure effects of phone-based monitoring, but it could also lead to the effects being lower than in other settings where phone data are the *only* performance information available. Our estimates should thus be interpreted as the effects of adding an incremental, independent source of monitoring, and making this salient to implementing officials.

II Research methods

Our design and methods follow a registered pre-analysis plan.[18]

II.A Experimental design

The study population consists of nearly all households eligible to receive RBS, i.e. all land-holding households in Telangana. We excluded one largely urban district (Hyderabad) as it had very few program beneficiaries, leaving 30 remaining districts. Since we randomized across nearly the universe of mandals in the state (outside Hyderabad), the study sample is representative of the rural population of the state.

Within these districts, we randomly assigned treatment at the level of the MAO, since

[17]MAOs were only responsible for check distribution. A different government department (Revenue) printed checks *before* our intervention, after verifying farmer eligibility, and banks independently checked farmer identity before cashing the checks. Improvements in the delivery of benefits are thus unlikely to have come at the cost of lower scrutiny of eligibility requirements.

[18]See https://www.socialscienceregistry.org/trials/2942.

some MAOs oversee multiple mandals. We randomly selected approximately 25% of MAOs for treatment, yielding a total of 122 treatment MAOs and 376 control MAOs. This corresponded to 131 treatment and 417 control mandals. We stratified randomization within each district on an indicator for whether an MAO oversees multiple mandals, the only MAO-level covariate available to us at the time of randomization (further details of the randomization algorithm are in the pre-analysis plan). Figure 1 shows the geographical distribution of treatment and control mandals.

We test for balance on a broad range of characteristics and report them in Table 2, grouping covariates into four broad categories: 1) characteristics of Rythu Bandhu beneficiaries; 2) general characteristics of the mandal (e.g. demographics, wealth); 3) characteristics of the mandal that may be related to implementation of Rythu Bandhu; and 4) MAO characteristics and past performance. For the characteristics of Rythu Bandhu beneficiaries, the unit of observation is at the individual farmer level, while for the other three categories, the unit of observation is either the mandal or MAO. We regress the variable of interest on treatment status and randomization strata fixed effects.

The first category takes farmer-level characteristics from the Government of Telangana landholder registry (Government of Telangana, 2018). The registry was updated in late 2017, shortly before the implementation of Rythu Bandhu, and includes information on all the land-owning farmers in the state (e.g. landholdings, percent of farmers with mobile phone numbers in the land registry, and thus who could be called for the intervention).

In the second category, we use mandal-level data from the 2011 census of India, the most recent round of India's decennial census of all its residents (Government of India, 2011).[19] Most of these characteristics (e.g. demographics, literacy, levels of irrigation) are slow to change, and so 2011 levels should provide a reasonably good approximation of levels at the time of Rythu Bandhu.

In the third category, we use both the 2011 census of India and some more recent data available from the Government of Telangana that may be directly relevant to Rythu Bandhu implementation. The 2011 census of India contains information on the total number of banks in a mandal, the average distance from each village in the mandal to a bank or ATM, the share of households using banking services, the share of households owning cellphones, and the share of villages with an all-weather road, all of which may affect the ease of receiving and encashing a check.[20] Since income levels may have shifted since 2011, we also check for

[19]Some mandal boundaries shifted after the creation of the state of Telangana. To match present-day mandals to 2011 census data, we take the GPS location of 2011 census villages and towns, as well as present-day mandal boundaries. The 2011 census characteristics of mandals are created from 2011 census data for those village and towns that fall within their boundaries.

[20]While there has been an expansion of banking services across India since 2011, the 2011 levels are still

and reject differences across treatment and control in total rainfall between 2013 and 2017 (Government of Telangana, 2019), as rainfall is an important determinant of income in these areas.

Finally, we test for balance on MAO gender, age (as a proxy for experience) and competence. Since there is no prior period in which we can observe their performance in distributing Rythu Bandhu checks, we measure MAO competence based on their performance in implementing another flagship program of the Government of India: the provision of Soil Health Cards to farmers.[21] We use mandal-level data available from prior to Rythu Bandhu (2016 and 2017) on how many soil samples were collected, how many farmers were covered by those samples, the number of samples that were tested, and the number of soil health cards made available to farmers through the government's online portal (Department of Agriculture, Cooperation and Farmers Welfare, 2018).

Overall, across the 33 tests, only one demographic measure (Scheduled Tribe population share) is significant at the 10% level and the joint p-value is equal to 0.15. As a robustness check, we have run all the main regressions with scheduled tribe population share included as a control, and it does not affect the results. Thus, we can be confident that any observed differences between treatment and control mandals in Rythu Bandhu performance outcomes can be attributed to the treatment of announcing phone-based monitoring to randomly-selected MAOs.

II.B Data

We primarily measure outcomes using administrative data, including (i) the register of all agricultural landholders in the state, including names, village, acres held, and a contact phone number; (ii) a farmer-level record of check distribution maintained by the MAOs;[22] and (iii) farmer-level bank records of check encashment. Our analysis focuses on encashment, as getting the money is the ultimate outcome of interest to policy-makers. Bank reports of encashment were recorded in real-time and were the basis for reimbursement from the government; manipulating them would constitute serious fraud and could jeopardize a bank's

informative, as the relative level of banking services across space, which is what matters for balance, has remained fairly static: there is a correlation of 0.98 between the number of banks per district in Telangana in 2011 and 2018 (Reserve Bank of India, 2018).

[21] Under this program, the Department of Agriculture collects a large number of soil samples from across each mandal, tests the samples to assess soil health (e.g. macro and micronutrients, pH levels), and provides farmers with Soil Health Cards that contain recommendations on usage of fertilizer and other agricultural inputs. To date, over a million soil samples have been collected from Telangana farmers, with an average of slightly less than one thousand per mandal annually

[22] We treat the records of check distribution maintained by the MAOs with caution as there are a number of problems with this data. See the Appendix C for further description of the issues with the MAO data.

operating license. We find that they closely match encashment as reported by the surveyed farmers, with agreement in 88.6% of cases.

We focus on check encashment status at two dates. The first (8 June) captures on-time delivery. This was exactly a month after the start of distribution, and is the date on which the monsoon arrived in Telangana in 2018 according to the Indian Meteorological Department (New Indian Express, 2018). Most planting activities begin with the arrival of the monsoon, and the government's goal was to ensure that farmers had funds in place at the start of planting to buy seeds and hire labor. This was a high priority for GoTS since a key goal of the program was to break the cycle of farmer debt, which was widely believed to be a driver of farmer suicides. The second (26 September) captures if the checks were *ever* encashed. This is after the last date (15 August) on which the checks were valid for encashment and thus should well approximate the final distribution of checks.[23]

We also use data from phone calls conducted by the call center as a secondary data source. These data were collected over the phone from 22,127 program beneficiaries as described above. This source of data provides a picture of encashment up until the phone survey was completed (June 15th), but not after that. The administrative data, which covers all encashments through September 26th, is more reliable in determining whether and when checks were encashed, but the phone data contains information on additional outcomes of interest, such as beneficiary satisfaction and corruption.[24]

Finally, we use data from a short phone survey of MAOs. We surveyed 88 of 122 treatment MAOs and a sample of 54 control MAOs.[25] Surveys covered their awareness of the pilot and beliefs about their treatment status. We were concerned that surveying control MAOs might cause them to believe incorrectly that they were being monitored by the phone-based monitoring system and thus affect their behavior (Hawthorne effects). As a result, we only conducted these surveys with a small sample of randomly selected control MAOs after the distribution was mostly complete.[26]

[23] Checks were printed in four tranches, on 19 April, 1 May, 10 May and 15 May, and were valid for three months from the date of printing. It was possible to get checks reprinted, but by 26 September, encashment activity had largely ceased.

[24] In principle, the bank data could have also been used for accountability purposes, but in practice, such high-quality administrative data are unlikely to be available for other programs. Thus, while the bank data is clearly better for research (since it has data on the universe of checks issued), the phone-based measurement of beneficiary experience is a more generalizable intervention for governments to deploy in other sectors. Thus, our *intervention* focuses on the more scalable phone-based measurement and monitoring, while our *evaluation* uses the higher-quality bank data.

[25] We attempted surveys with all of the treatment MAOs (among whom the response rate was 72%) and a random sample of 2 control MAOs per district (60% response rate).

[26] On the survey date, 84% of checks that would ever be encashed had already been encashed. Table A.1 tests for and rejects the presence of Hawthorne effects, as control MAOs randomly selected for the survey (or actually surveyed among the set who were selected) do not have any higher or lower rates of encashment

II.C Estimation

We report intent-to-treat estimates, comparing mean outcomes in treatment and control areas. We discuss MAO beliefs and their implications for interpretation in our cost-benefit analysis below. We thus estimate

$$y_{ivmsd} = \alpha + \beta T_{msd} + \delta_{sd} + \gamma \mathbf{X_{ivmsd}} + \epsilon_{ivmsd} \tag{1}$$

where y is an outcome, T an indicator for assignment to treatment, and X a vector of pre-specified covariates. In practice, there is only one covariable: the size of landholdings, binned into 40 evenly sized bins (i.e. 0 to 2.5^{th} percentile of landholdings, 2.5^{th} to 5^{th} percentile, etc.).[27] Indices denote individual i in village v in mandal m in stratum s in district d. Treatment is strictly exogenous conditional on the randomization stratum fixed effects δ_{sd}. We cluster standard errors at the level of treatment assignment (the MAO) and conduct randomization inference as a robustness check. When using call center data, we reweight estimates by the inverse probability of being sampled.

III Results

III.A Effects on overall program performance

Overall, RBS implementation was imperfect but still fairly successful compared to many other similar programs. 4.03 million farmers (69% of target) encashed their checks before monsoon onset on June 8 (Table 3: Column 2). After 5 months, 4.8 million farmers, 83% of the total, had encashed their checks (Column 4). It appears that corruption was not a major issue, with only 2% of farmers reached by phone reporting that they had to pay a bribe to obtain their checks.[28] The median farmer had a lag of only 6 days between receiving a check

than other control MAOs. Given that being surveyed has no effect on outcomes, and that the surveys were conducted after the treatment effects are seen (see subsection III.A), the small difference in survey response rates between treatment and control MAOs for the survey cannot have affected overall encashment outcomes.

[27] Our pre-analysis plan specifies that we will control for the size of landholdings. We implement this with indicators for percentile of landholding as opposed to exact landholding to preserve anonymity of individual farmers in the data. The results are nearly unchanged if we include the exact landholding as a control instead.

[28] We also find slightly higher reported encashment rates (75.5%) in our phone call data than in the corresponding administrative records (73.6%), suggesting that officials did not collude with banks to encash beneficiary's checks without their knowledge. The call center provides further evidence against such collusion, with only 0.02% of beneficiaries reporting that someone else was given their check and only 1.5% stating that the check wasn't available when they tried to pick it up (which could be consistent with either fraud or poor organization by the MAOs). There is no effect of the treatment on the prevalence of either of these problems.

and successfully encashing it at the bank.

Despite good program implementation to begin with, phone-based monitoring led to further improvements. Treatment increased the probability of "on-time" check encashment (defined as prior to the arrival of the monsoon on June 8th) by 2.4 percentage points, ($p = 0.002$, Table 3)[29], which is equal to a 0.25 standard deviation improvement (standard deviation of the mandal-level means of on-time check encashment in control mandals was 9.7 percentage points). The probability that farmers ever encashed their checks also increased by 1.3 percentage points ($p = 0.037$), a 0.16 standard deviation improvement (standard deviation of mandal-level means in control mandals was 8.1 percentage points).[30] Given the relatively high-quality implementation in the control group, an alternative way of quantifying the impact of the program is that it reduced the fraction of farmers not receiving their checks on time by 7.9% (2.4 percentage points on a base of 31%) and ever receiving their checks by 7.8% (1.3 percentage points on a base of 17%). These are non-trivial rates of improvement - especially given the "light touch" nature of the intervention and its low cost.

Multiplied by the large number of affected farmers (1.3 million), this translates into 31,828 additional farmers encashing their checks prior to the monsoon in treatment areas, or more than one additional farmer per phone call completed by the call center. Furthermore, an additional 17,771 farmers ever encashed their transfers as a result of the treatment. If the program had been extended to the entire state, our estimates suggest that an additional 77,657 farmers would have received the transfer to which they were entitled during this agricultural cycle (and double that amount annually). These gains highlight the policy impact of programs like phone-based monitoring that can be feasibly implemented at large scale, where even modest rate improvements translate into meaningful absolute gains.

As seen in Table 4, conditional on ever encashing, the treatment lowered the mean number of days that passed before recipients encashed their checks by three-fourths of a day ($p = 0.039$). Figure 2 summarizes the main effects visually. The top panel plots the proportion of checks encashed by date in the treatment and control groups separately, while the bottom panel plots regression estimates of the treatment effect by date. The gap between treatment and control peaks at 2.8 percentage points (5.2%) on 25 May ($p = 0.004$) and then narrows, asymptoting to 1.3 percentage points by the end of September.

[29]The onset of the monsoon changes from year to year — if we instead used the dates of monsoon onset from 2015 to 2017 (June 13th in 2015, June 19th in 2016, and June 12th in 2017), the p-values would be nearly the same ($p = 0.002, 0.005, 0.002$ respectively) and our conclusion would not change.

[30]For completeness, we also report effects on check distribution in Appendix C, based on MAO records of whether each check was distributed and the date of distribution. We treat these data with caution as they were uploaded by MAOs with substantial lags, causing date of distribution to be mismeasured, and were not subject to penalties for misreporting like those banks faced. See Appendix C for further description of the issues with the MAO data. That said, the results are broadly consistent with those for encashment.

The larger gap between treatment and control at the end of May is likely a better estimate of the effect of phone-based monitoring on bureaucratic effort because MAOs had the greatest ability to affect check delivery outcomes during the meetings in each village, nearly all of which occurred in May. After that, households who did not get their checks had to visit government offices to pick them up. Thus, the shrinking gap over time reflects a natural catch-up in which control households who had not received their checks in village meetings had to exert costly effort to get their checks.

Data from the call center only capture the experiences of a small and non-representative sample of the farmers (0.4%), but sheds light on the process changes underlying these gains. Consistent with the largest gap between treatment and control being at the end of May, respondents in treated areas were more likely to receive their checks during village meetings ($p = 0.09$) (Table A.2). Since this was far less costly than the alternative (traveling to mandal headquarters and going through an additional layer of bureaucracy to get their check), this is another channel through which the treatment improved beneficiary outcomes. We find no treatment effects on either the likelihood that phone call respondents were asked to pay a bribe or on their overall satisfaction with the scheme (columns (2) and (3)). This is not surprising given that baseline rates of corruption were extremely low (2% of respondents), and satisfaction with the Rythu Bandhu Scheme was high (93% of respondents).[31]

Recall that there are two channels through which phone-based monitoring may affect beneficiary outcomes: an incentive channel, where MAOs exert more effort when they know they are being monitored, and an information channel, where they use the information provided by the report to reallocate effort more optimally. Given that reports were issued after nearly all encashment had occurred (July 5-9), it seemed a priori unlikely that it would affect outcomes through information effects. As shown in Figure 2, the treatment effects are concentrated during May and June, well before the phone-based monitoring reports were shared. There is also no evidence of a differential change in encashment rates following the distribution of the reports themselves.[32] Overall, the timing of the effects implies that they

[31] We can also examine the treatment effect on check encashment in the call center data. While this is a non-representative and substantially smaller sample than the administrative records, it does let us check for consistency between estimates of impact in the two data sources. Taking the sample of farmers who were reached by phone, the estimated effect on encashment as measured by phone calls is not significantly different from the estimated treatment effect on encashment as measured using administrative records (Table A.8, $p = 0.46$). Note that for this test, we adjust the administrative records to the date of the call (e.g. if the call center collected information from the respondent on June 7th, whether the check was recorded as encashed in the administrative data on or before June 7th). The estimated treatment effect is not statistically significantly different from zero in either data set, reflecting both the selected nature of the sample reached by phone and its much smaller size (22,537 as opposed to 5.7 million). It is reassuring to note that the control means are nearly identical, suggesting that the administrative data and phone call data are of comparable quality.

[32] In hazard models, an indicator for post 9 July is not a significant predictor of encashment (table A.3).

were driven by treated MAOs' knowledge that they were being monitored and anticipation of future reports being made available to their superiors rather than by the information in the reports themselves.

One concern is that these estimates may be biased due to spillovers across mandals. Knowledge of the intervention may reallocate the attention of those who supervise MAOs towards treatment mandals and away from control mandals, if supervisors do not wish to have their subordinates who are being monitored look bad. We directly test for this possibility by taking advantage of random variation in treatment intensity within bureaucratic units. Districts in Telangana are divided into "revenue divisions" (up to 5 per district), and then into mandals (4 to 15 per revenue division). Although roughly the same fraction of mandals were treated in each district, we did not stratify the randomization at the revenue division level. As a result, there is random variation in the fraction of MAOs within each revenue division that are treated. If there were diversion of revenue division supervisor-level attention, we should expect worse performance among control MAOs with more treated MAOs in their revenue division, as these control MAOs would get less attention paid to them. We find no evidence of this (Table A.4).

A second concern is that we are picking up a mechanical effect of the phone calls themselves rather than the impact on officials. Receiving a call may have acted as a "reminder" for respondents to pick up their checks, and so the treatment effect could be a result of more individuals in treatment mandals receiving phone calls. Any such effect would necessarily be too small to fully explain our results, as calls only reached 0.82% of treatment farmers and 0.27% of control farmers, a difference that is smaller than the observed treatment effect.[33] Since farmers were randomly selected for phone calls, we directly test the effect of being called in Table A.5. Farmers who were randomly selected for calls are not more likely to encash their checks or pick up their checks more quickly; if anything, they are slightly less likely to encash their checks on time, though the difference is economically small. This is most likely because most calls occurred post-encashment, so it would not have been possible to act as a reminder. We also confirm that the results are unchanged when we drop the individuals who were called in both treatment and control groups from the administrative data sample (Table A.6).

A third concern is that the improved performance of treatment MAOs may have caused worse performance on other tasks (multi-tasking). In practice, this was not likely to be a concern because MAOs were primarily focused on RBS check distribution during the month of May 2018 and other tasks were deprioritized by the government. We also check that an increase in effort intensity during this period did not lead to increased slacking at other

[33]Calls were attempted with approximately 150 farmers per treatment and 50 per control mandal.

periods by testing for differences in the issue of soil health cards (as described earlier, this was an effort intensive task that MAOs were responsible for). In Table 5, we find that treatment MAOs perform no worse on those tasks. Although we cannot observe all of the tasks that MAOs perform, this is at least suggestive that there was limited effort diversion away from other tasks..

III.B Distributional consequences

The baseline allocation of benefits under RBS was regressive, as check size was proportional to registered landholdings. A wealthier farmer who owned 10 acres of land, for example, would have received a check for Rs 40,000 ($570), while a poorer farmer who owned only half an acre would have received a check for only Rs. 2,000 ($30). This pattern was exacerbated by differences in distribution and encashment rates. As of 26 September, 89% of farmers in the top quartile of the landholding distribution (holding more than 3.1 acres of land) had encashed their checks, declining monotonically to 68% of farmers in the bottom quartile (holding fewer than 0.4 acres). This could reflect differences in the effort made by government officials, or differences in farmers' motivation and ability to collect and encash their checks. For example, a farmer at the fifth percentile of the land size distribution (0.09 acres of land) would receive a check worth just Rs. 370 ($6), possibly less than the time and money costs of encashment.

We find that the effect of announcing phone-based monitoring to MAOs on on-time delivery was significant and nearly identical across farmers of different landholding sizes. Figure 3 plots the distribution of treatment effects by quartile of landholding, and the middle panel of Table 3 reports the corresponding estimates. However, by the end of September, some suggestive distributional effects begin to emerge. The treatment effect on encashment continued to be statistically significant for farmers in the lower three quartiles of landholdings, but not for farmers in the top quartile.[34]

Our interpretation is that the treatment initially improved delivery of the transfer among all types of farmers. Among the set of wealthier farmers, the control group eventually caught up with their treatment counterparts as they used their resources and better networks to ensure that they got their transfers. It may also be more likely to be worth it for them to put in time and effort to track down the checks (since the amounts were larger). Among the set of poorer farmers, the control group did not catch up, and treatment effects remain significant over time. Wealthier farmers still benefited from the treatment, which sped up the process and lessened their cost of accessing the transfer (Table 4), but the gains along the

[34] We reject equality of treatment effects between the top and bottom quartiles ($p = 0.045$), but do not reject a joint test of equality across all four quartiles at conventional significance levels ($p = 0.11$).

margin of ever getting the transfer (which may matter more for the poor) are concentrated among the poor.

A further distributional concern about measurement by phone is that it could skew MAO effort towards those who own phones or have phone numbers. MAOs had access to the land registry, so could see which farmers had numbers listed and thus would be able to provide information to the call center. However, we find significant positive impacts on on-time encashment for both those with and without phones, and cannot reject that these effects are the same (Table 3: Bottom panel). The difference in ever-encashed between those with and without phones is also not statistically significant ($p = 0.59$), but time to encashment seems to have improved more for the group without a phone ($p = 0.03$). Thus, despite MAO performance being measured only in the population with mobile phones, the resulting increase in MAO effort appears to have led to improvement in program performance for all beneficiaries.[35]

III.C Tallying costs and benefits

We next examine cost-effectiveness of the intervention at delivering money to farmers, and its overall welfare consequences. We cost the intervention at Rs. 2.5 million ($36,000), the price GoTS paid the call-center vendor pro-rated by the proportion of calls made to treatment areas. This is a conservative estimate, as the government paid a premium to complete the procurement process quickly, and conversations with the vendor indicate that the call center could be operated for roughly half this cost. On the other hand, this figure does not include the relatively small sunk costs of time spent by government employees or members of the research team designing the intervention (e.g. sampling protocols).[36]

At this cost we estimate that phone-based monitoring was a highly cost-effective means of delivering cash transfers. The estimated impact on money ultimately delivered to farmers

[35] As seen in the control dependent variable means in the table, overall encashment rates were higher for those with phones (90%) than those without (72%). This reflects in part the higher level of wealth among individuals with listed phone numbers, as controlling for landsize shrinks this gap. The remainder of the gap is likely explained by unobserved higher levels of ability among this group, as well as the fact that even in the absence of the treatment, if a farmer had a phone number listed, then they could be contacted and encouraged to pick up their check.

[36] We also do not cost incremental MAO effort, which is likely to be small or at least below the wage premium enjoyed by public employees (Finan, Olken and Pande, 2017). As a conservative and rough estimate, if we assumed the increased cost of effort to MAOs amounted to 15% of MAO monthly salary, the cost of effort would amount to a total of approximately $9,000 among the 132 treatment MAOs, which does not noticeably change the cost-benefit calculations. Similarly, the total cost of analyzing the data and sending the reports was minor at less than $2,000. Finally, the marginal cost of collecting beneficiary phone numbers was virtually zero, since it had been done as an incidental part of the process of land record digitization. In other settings where phone numbers are not available, then this cost would need to be included; however, it is now fairly common for governments to have databases of citizen phone numbers as in this context.

was Rs. 69 million, or roughly $1 million, and on money delivered on time was $3.9 million.[37] Since the funds for the program were already earmarked, the relevant benchmark for cost-effectiveness of an intervention seeking to improve program implementation (like the one we study) is the administrative cost of delivering anti-poverty programs. The cost per incremental dollar delivered was 3.6 cents, which compares favorably to the administrative costs of well-implemented government cash transfer programs such as Progresa in Mexico (estimated administrative costs of 8.9 cents for every dollar delivered (Coady, 2000)).[38] Focusing on the government's objective of getting transfers to farmers on time, the cost per dollar delivered on time was less than one cent.

To estimate an economic cost-benefit ratio we must price the value of putting capital in the hands of farmers during the planting season as opposed to leaving it on the government's books. In this framework, higher rates of distribution and faster distribution create social benefits by increasing the amount of capital earning the higher rate of return among farmers. We assume that prior to receiving the transfer, farmers finance the purchase of inputs at a rate r_f, either by borrowing or by substituting capital from alternative uses. Capital held by the government earns a lower return r_g.

Time runs from the start of the program ($t = 0$) to the date T on which farmers' investments pay off and debt is repaid. The total value of a unit of capital held by the government until time t and then by the farmer from time t until T is thus

$$v(t) = e^{r_g t} e^{r_f (T-t)} \tag{2}$$

Given a distribution F of check encashment dates, total social value is

$$W(F) = \int v(t) dF(t) \tag{3}$$

Faster and broader distribution of transfers shifts F (as seen in Figure 2), increasing the amount of capital earning the higher rate r_f. We calculate $W(F)$ for both treatment and control groups using administrative records and conduct hypothesis testing using randomization inference.

We value capital on the government's books at the rate it earns on deposits ($r_g = 5\%$ annu-

[37] The treatment effects on amount ever delivered and amount delivered on time were Rs. 54 ($0.77) and Rs. 203 ($2.9) per farmer respectively, and so multiplied by the 1.3 million farmers in the treated mandals, this gives the estimates $1 million and $3.9 million.

[38] Many other programs are much less cost-effective. For example, Caldés et al. (2006) estimates administrative costs of between 6.8 to 16.1% and 21.2 to 24.5% for two poverty alleviation programs similar to Progresa. In locations with high rates of corruption, the implied costs of social protection schemes are even higher (Olken, 2006; Niehaus and Sukhtankar, 2013).

ally),[39] and capital held by farmers at the going rate for short-term farm loans ($r_f = 25\%$).[40] We conservatively assume that investments are realized and debt is repaid immediately at harvest, so T equals 4 months; in practice, farmers' may continue to earn higher returns on capital that has been transferred to them, so this is a conservative assumption.

Using these estimates, phone-based monitoring generated Rs. 10.6M ($140,000) in benefits, or roughly four times its cost. We reject the null of no benefit ($p = 0.04$) using randomization inference. This result is reasonably robust to variation in T and δ. At $\delta = 20\%$, benefits exceed costs for any T longer than 26 days, while at $T = 4$ months benefits exceed costs for any $\delta \in [5\%, 25\%]$ (Figure 4). Even under conservative parameter assumptions, the intervention was cost-effective.

These calculations may also be conservative in the sense that they reflect intent-to-treat estimates, while awareness in the treatment group was incomplete. Among treatment MAOs we surveyed, 90% had heard of the intervention, but only 28% were sure that the initiative had rolled out in their area; 28% were unsure and 35% thought it had not. This may partly reflect strategic misrepresentation, such as if MAOs believed they could excuse poor results by feigning ignorance. In the control group, 52% of MAOs had heard about the intervention, but only 4% believed themselves treated, with another 8% unsure. While the control group was relatively "uncontaminated" by misperceptions of being treated, treatment effects may have been even larger if awareness of phone-based monitoring were universal in the treatment group.[41] Overall, these benefit-cost estimates suggest that phone-based monitoring can cheaply be applied to large-ticket programs at scale and create substantial economic value.

III.D Comparing call center with administrative records

One concern with phone-based monitoring is whether this type of data provides an accurate picture of bureaucratic performance. Although the fact that MAOs responded to phone-based monitoring implies that they believed it would at least partially reflect their true performance, this incentive could become stronger or weaker in the longer run as they learn more about the accuracy of phone-based monitoring. Further, the ability of senior officials to take meaningful follow-up action based on phone-based data will depend crucially on its

[39] In principle, the government could use funds for other productive investments. In practice funds appropriated for the program would not be reallocated till the next fiscal year and would only earn interest.

[40] This is the rate charged by registered micro-finance organizations who are subject to a regulated interest-rate cap; informal moneylenders typically charge much higher rates.

[41] Despite potential confusion about treatment status, MAOs understood the nature of the intervention well. Among MAOs who had heard of the intervention, 89% correctly identified that the intervention would collect information from farmers over the phone, and 93% said that this data would be used for issuing reports on performance to MAOs/DAOs.

reliability and accuracy. In most cases, it would be difficult to assess how accurately phone-based data measures performance without an independent data source to compare it to. In this case, we can take advantage of the existence of administrative data and examine the accuracy of phone data by comparing measured MAO performance in phone call data to the administrative data.

As a first pass, we check whether phone call and administrative data agree on whether a given check was encashed, which they do in 88.6% of cases.[42] However, to see if the data can be reasonably used for personnel management, the key metric of interest is whether aggregated phone and administrative data agree on performance at the MAO level. We calculate how often phone and administrative data rank the relative performance of a pair (m, m') of MAOs within a district the same way. For example, suppose the call center rates MAO A as 3rd and MAO B as 4th best. If the administrative data rates them as 2nd and 3rd best respectively, then since both sources ordered MAO A as performing better, we consider the sources to be in agreement. On the other hand, if the administrative data rated them as 3rd and 2nd best respectively, then we would not consider them to be in agreement: MAO A is ranked better than MAO B by the call center, but not by the administrative data.

These rankings disagree in 31% of cases. This disagreement is largely due to the relatively small sample of calls per mandal rather than inaccuracy in the phone call data per se — 22% can be explained by sampling variation, with a relatively low true rate of disagreement between the two data sources of 9% (see notes to Table A.7 for details on calculation). The key implication is that much of the disagreement could be eliminated if the government chose to increase the number of phone calls placed. We illustrate this in Figure A.2, where we calculate the rate of disagreement due to sampling variation for different sample sizes — increasing the sample size to around 300 calls per mandal reduces the disagreement from sampling variation to only 10%.[43]

Overall, these results suggest that managers can reasonably use phone data to help decide which officials to push for more effort or acknowledge for good performance. However, the reliability of phone-based data may not be high enough to justify using them to determine more serious administrative actions (e.g. suspensions or promotions) with samples of the size that we observe here. Future work should consider the use of data over multiple years as well as decision-theoretic modeling of the costs and benefits of various follow up actions under different levels of precision of measurement.[44]

[42] To calculate the rate of agreement, we take the encashment status reported over the phone and check whether it matches the administrative record of whether the check was encashed by that date.

[43] Another way of assessing the accuracy of the phone-based data is to compare their level of agreement on the worst MAOs. For the 20% of MAOs who were ranked as the worst performers in the phone data, 47% are also among the worst 20% of MAOs in the administrative data, while 80% are in the bottom 50%.

[44] Creating protocols for the optimal use of sample-based performance measures for high-stakes personnel

IV Conclusion

We find evidence that the cheap, simple, and flexible approach of monitoring beneficiary experiences using outbound phone calls can be a cost-effective tool for improving last-mile service delivery. Further, unlike smaller-scale studies of interventions to improve monitoring of bureaucrats, our results suggest that phone-based monitoring can be rapidly deployed at the scale of an entire state of 35 million people. Beyond the case studied here, a unique feature of phone-based monitoring is its potential to be scaled across an unusually wide range of locations, programs and outcomes. In general, phone-based monitoring can provide a rich picture of service delivery, as it can collect information on any outcome that can be observed by beneficiaries and described verbally.

In principle, the specific intervention we studied (of measuring MAO performance, creating report cards, and announcing to MAO's that their performance would be measured and reported) may not have required the phone calls because the key metric in the report cards could have been created with the administrative data on check encashment from the banks.[45] However, in most cases, there is no similarly high-quality administrative data on last-mile service delivery. Thus, the attraction of phone-based monitoring is its wide applicability as a treatment that can be easily deployed and scaled across settings and programs. Consistent with wanting to study a scalable and widely applicable intervention, we use only the data from the phone-calls to *implement* the intervention, and only use the administrative data to *evaluate* it. Our set up has the dual advantage of studying an implementation protocol that is inclusive of practical problems (including non-representative phone ownership and responses), while evaluating it using a different administrative data set that does not have these problems, and also has adequate power to detect treatment effects.

While the approach we studied here is itself adaptable to other settings and programs, this does not mean that its effects will be the same. It would therefore be useful to test phone-based monitoring in other settings. For instance, it may perform better for outcomes that beneficiaries experience more directly (e.g. check distribution) than indirectly (e.g. public good maintenance). It would also be useful to test this approach in a setting where the scope for improvement is greater than in the RBS, which was relatively well-implemented.

action is a complex topic. The relevant issues are well illustrated by the literature on using estimates of teacher value-added for teacher retention and tenure decisions (Chetty, Friedman and Rockoff, 2014; Koedel, Mihaly and Rockoff, 2015; Rothstein, 2017).

[45] The report cards also included information on beneficiary satisfaction and bribe requests, which would not have been possible with only administrative data. It may be that MAOs increased effort in order to satisfy these multiple performance targets, leading to better outcomes than would have been possible with just administrative data. However, we have no evidence on the potential differences in impact based on reports using phone versus administrative data.

In general, Telangana is a relatively well-administered state, which may mean that the effects are larger than other areas, as the bureaucracy is more responsive. But it may also mean that the effects would be larger if phone-based monitoring were implemented in other locations where there is more margin for improvement.

Similarly, it would be valuable to examine how effects evolve over time in settings where bureaucrats perform similar functions repeatedly. As with all monitoring technologies, the officials being monitored would learn about the consequences of performing at different levels and might develop new strategies – both productive and counterproductive – to influence their ratings. But over time, phone-based monitoring could also inform officials in real-time on what locations are most in need of their targeted intervention, as well as motivating them to increase effort. It could provide inputs for improving personnel management, which has been identified as the most important component of organizational management quality, and is systematically worse for public organizations (Bloom and Van Reenen, 2010). It could be tuned in many ways to improve performance, evolving statistical protocols for different types of follow-up action reflecting the cost of different kinds of Type I and Type II errors. Optimal monitoring protocols would take into account the need for whistleblower protection in small samples (Chassang and i Miquel, 2018) and the motivations of the respondents answering the phone (Fiorin, 2018). One could even consider making the results publicly available, trading off the costs and benefits of transparency. Such public availability of data may also increase the effectiveness of bottom-up community and citizen monitoring interventions (such as the ones studied by Reinikka and Svensson (2011)).

Overall, phone-based measurement of beneficiary experiences opens up a broad set of possibilities for improving the quality of service delivery in developing countries. There is much exciting future research to be done in improving the design of such measurement systems, developing protocols for the optimal follow-up actions based on such measurement, and studying the impact of doing so. Social scientists can play an important role in improving the quality of governance by doing such work (Duflo, 2017).

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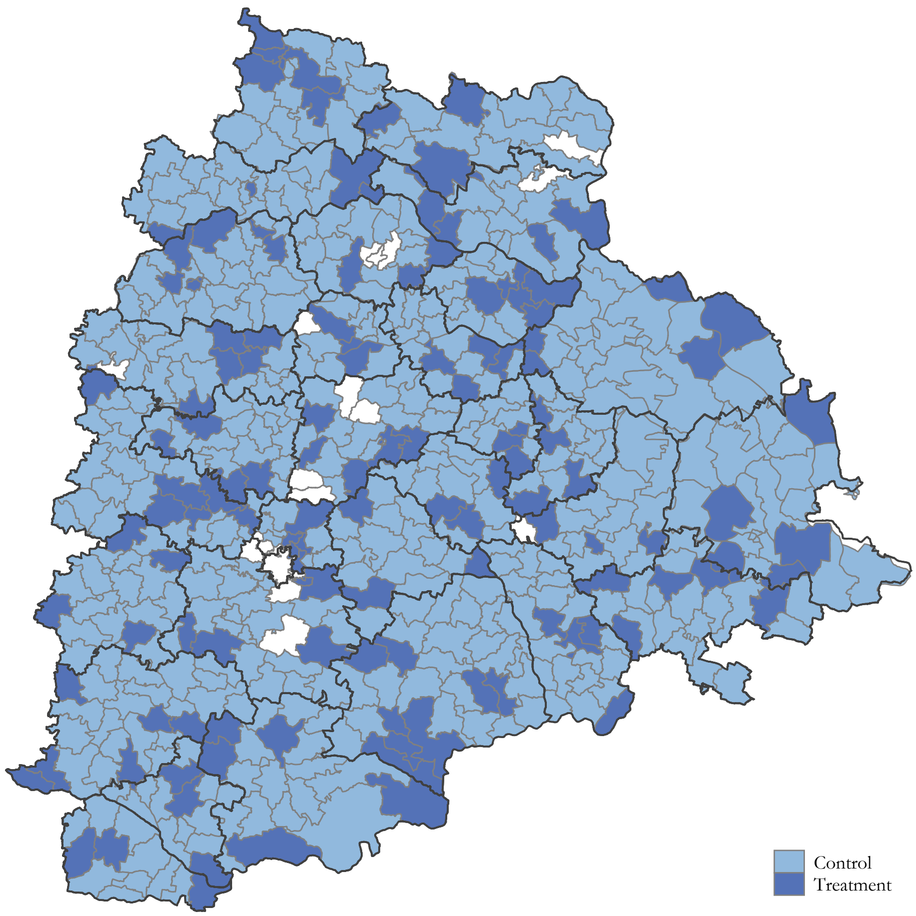
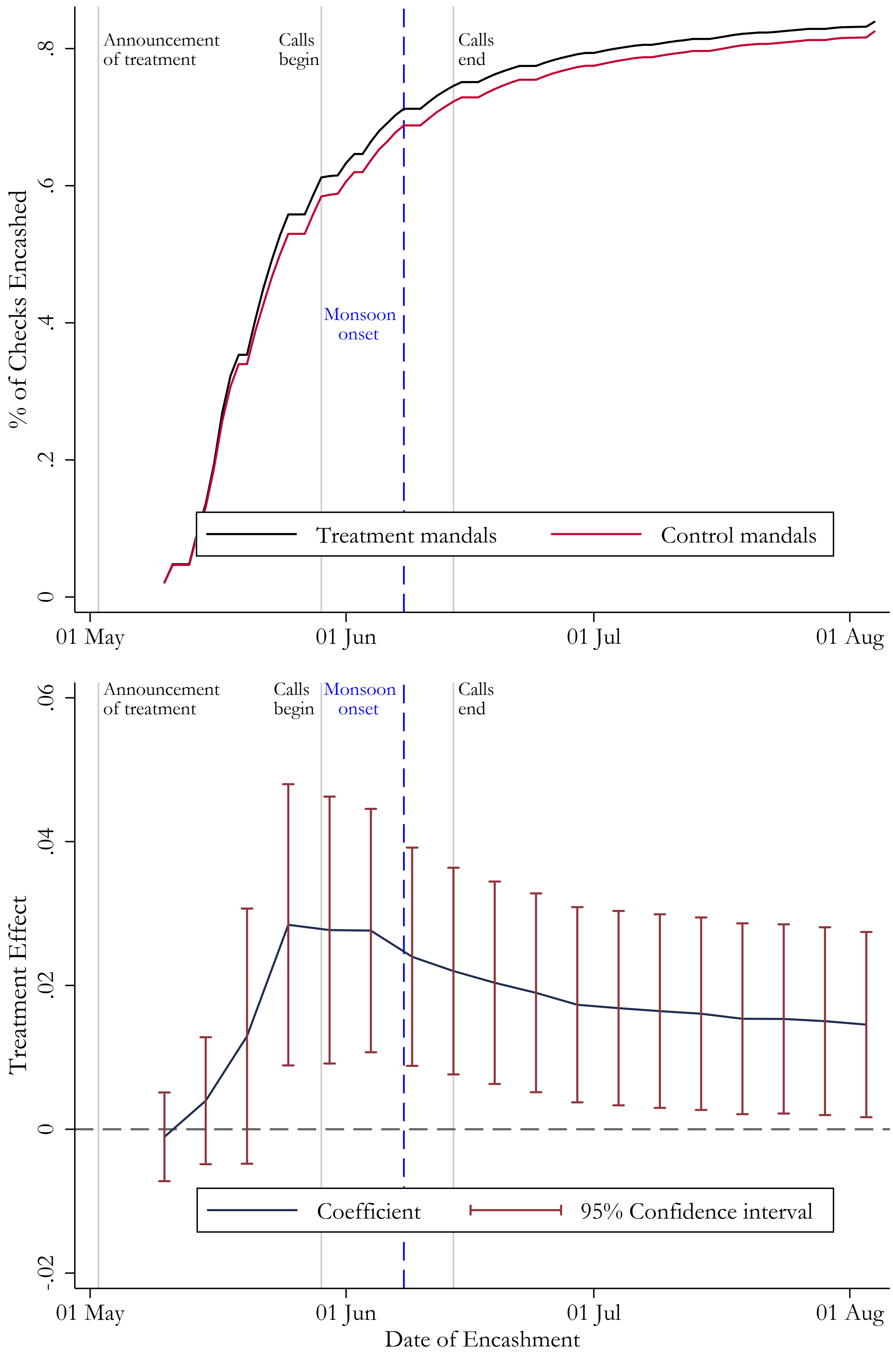
Figure 1: Study areas with treatment and control mandals



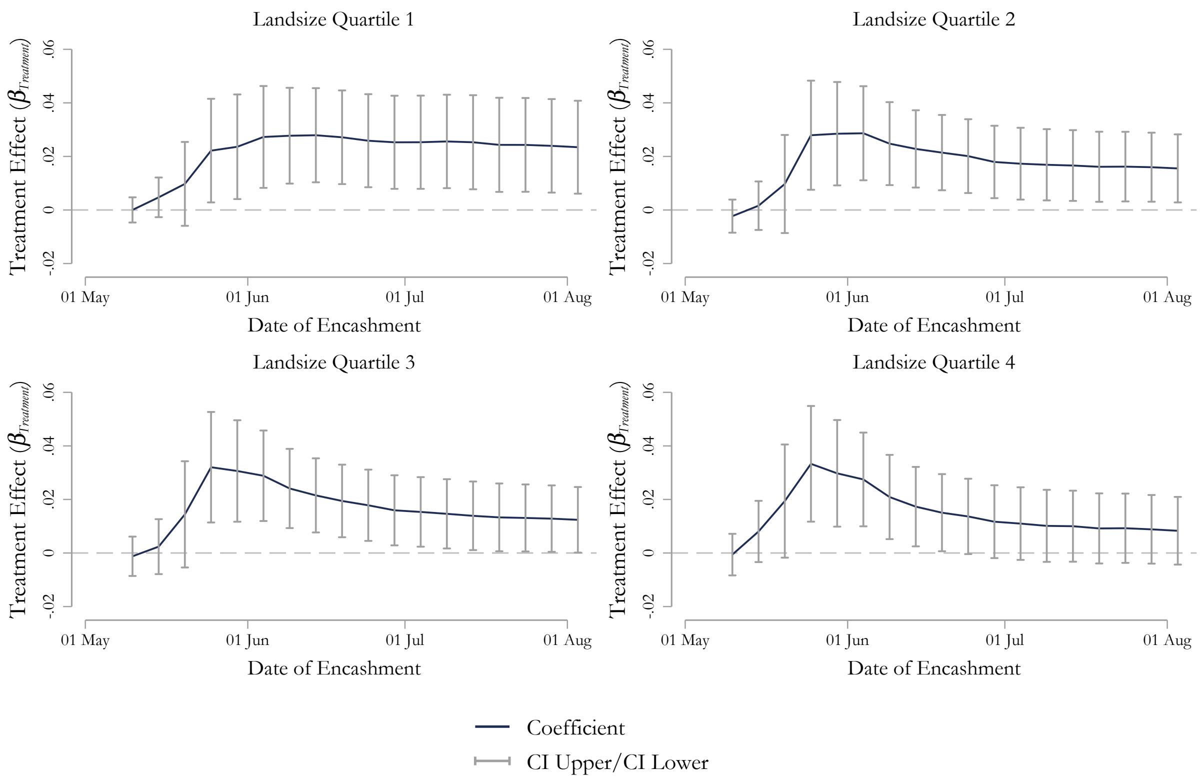
This map shows the geographical distribution of treatment and control mandals (sub-districts) across the entire state. Dark black lines indicate district boundaries, whereas gray lines are mandal boundaries. Randomization was stratified by district, and occurred at the mandal agricultural officer level. Mandals in white were not included in the randomization and study. This typically occurred because the mandal is urban, such as those around Hyderabad, or did not have an MAO assigned to it, so it was not possible to implement the treatment. Note that since there are 10 cases where a treatment MAO oversees multiple geographically contiguous mandals, there is slightly more geographical clustering of treatment mandals than would occur due to chance.

Figure 2: Treatment effect, by date



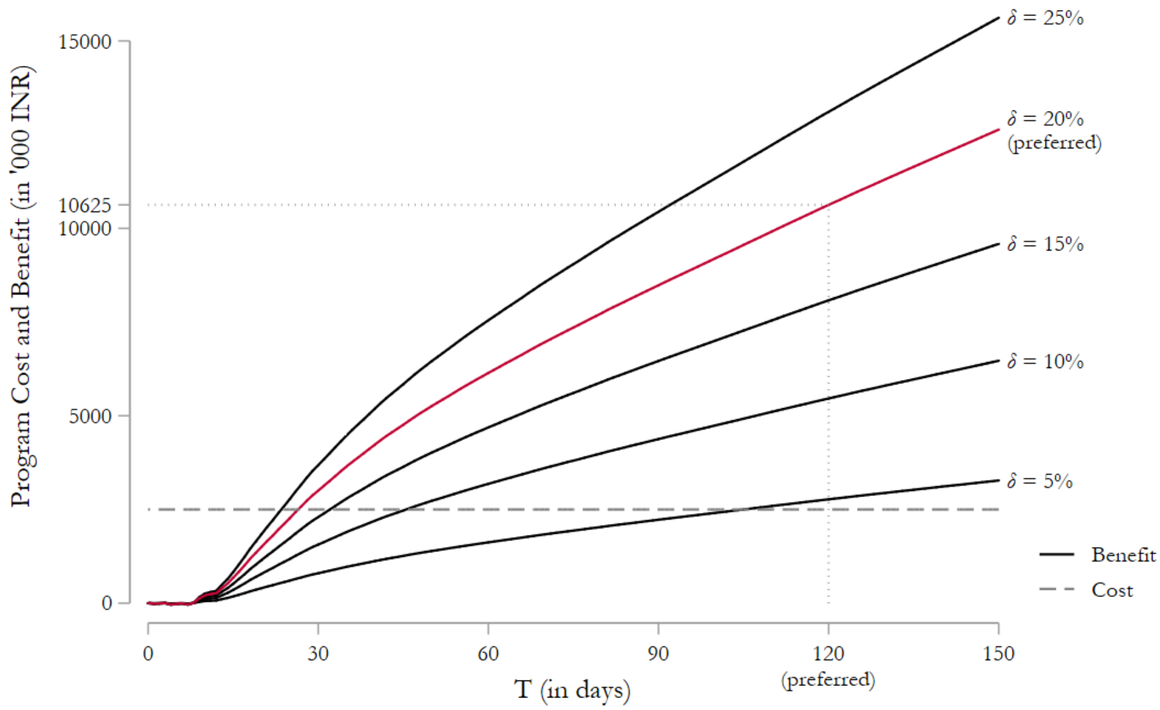
The two graphs in this figure report (a) the cumulative rate of encashment in treatment and control mandals by day, and (b) the coefficient of treatment effect on the cumulative rate of encashment over the period of check distribution in our data. The coefficient in the bottom graph are estimated through regressions with fixed effects at the randomization strata level and standard errors clustered at the MAO level. Less than 1% of checks were encashed after August 4 or before 10 May, so the axis is restricted to those time periods. The monsoon arrived on June 8th, and so we consider this the date of "on-time" encashment.

Figure 3: Treatment Effect Over Time, by Landsize Quartile



The graphs in this figure report the coefficient of treatment effect on the cumulative rate of encashment over the period of check distribution, across the four land size quartiles, as well as the 95% confidence interval. The first graph is of treatment effects among the quartile of farmers with the smallest farms, while the fourth is for the quartile of farmers with the largest farms. The coefficient in the bottom graph are estimated through regressions with fixed effects at the randomization strata level and standard errors clustered at the MAO level. Less than 1% of checks were encashed after August 4 or before 10 May, so the axis is restricted to those time periods.

Figure 4: Sensitivity of cost-effectiveness estimates



Sensitivity of cost-effectiveness estimates tested with respect to the total time period of consideration (T) and the differential rate of return (δ, i.e., $r_f - r_g$). The interest earned by the government (r_g) is 5% annually, and the short-term annual interest rate for farmers (r_f) varies from 10% to 30% annually. The preferred specification for these parameters is T = 120 days and δ = 20% since we think T=120 corresponds most closely to the planting season and that value of δ is relatively conservative as described in the text.

Table 1: Timeline of Project

Date(s)	**Activities**
September to December 2017	Land records in Telangana are updated and digitized
February 28, 2018	Rythu Bandhu Scheme is announced
April 1	First meeting between JPAL research team and Government of Telangana
April to May 2018	Rythu Bandhu checks printed and distributed to MAOs
May 2	Treatment MAOs informed of intervention (via video-conference)
May 8	Check distribution begins
May 23	At this point, 50% of all checks have been distributed and encashed (60.2% of all checks that will ever be encashed)
May 29 to June 15	Call center collects data
June 8	Monsoon rains arrive in Telangana
June 9 to June 11	Phone survey of MAOs is conducted
June 15	At this point, 75% of all checks have been distributed and encashed (90.4% of all checks that will ever be encashed)
July 9	Reports sent to treatment MAOs and their supervisors. At this point, 79.4% of all checks have been distributed and encashed (95% of all checks that will ever be encashed)
September 26	Research team receives bank records of check distribution through this date

Table 2: Balance tests

Variable	Control mean	Treatment mean	Difference	SE	p-value
1. Characteristics of Rythu Bandhu beneficiaries					
Land Ownership (acres)	2.22	2.18	-0.02	0.05	0.72
Median Land Size	1.57	1.54	-0.00	0.05	0.92
Land Size - 25th Percentile	0.65	0.65	0.02	0.04	0.63
Land Size - 75th Percentile	2.97	2.87	-0.06	0.06	0.33
Registered Mobile Numbers	0.61	0.61	-0.01	0.02	0.67
Farmers per MAO	10,311.52	10,016.79	-287.57	362.70	0.43
2. General mandal characteristics					
Mandal population	46,714.75	49,894.53	-3,264.32	2,603.48	0.21
Share of female population	0.50	0.50	-0.00	0.00	0.57
Share of SC population	0.18	0.18	-0.00	0.01	0.74
Share of ST population	0.13	0.14	0.02	0.01	0.06
Literacy rate in mandal	0.59	0.59	-0.00	0.01	0.56
Share of working population	0.52	0.52	0.01	0.00	0.24
Share of villages with paddy as main crop	0.68	0.65	-0.01	0.04	0.86
Share of irrigated land	0.52	0.51	-0.01	0.04	0.76
Share of electrified villages	0.95	0.94	-0.02	0.02	0.36
Average village distance from Hyderabad	135.91	134.76	0.32	2.09	0.88
3. Mandal characteristics related to Rythu Bandhu					
Number of banks in mandal	3.52	4.12	-0.26	0.35	0.47
Average distance to nearest ATM	12.72	12.43	-0.18	0.47	0.70
Share of HHs using banking services	0.45	0.43	-0.01	0.02	0.45
Average distance to nearest bank	7.51	7.22	-0.15	0.31	0.62
Share of villages with all-weather road	0.91	0.91	0.00	0.01	0.79
Share of HHs owning mobile phones	0.52	0.50	-0.01	0.01	0.70
Average rainfall in mandal 2013-17 (mm)	707.35	714.26	8.76	10.19	0.39
4. MAO characteristics					
Age of MAO	35.57	36.36	0.89	0.76	0.24
Gender of MAO (Female = 1)	0.30	0.33	0.02	0.05	0.65
Number of SHC samples (2016)	993.66	911.41	-68.33	69.74	0.33
No. of farmers covered by SHCs (2016)	4,321.85	4,268.36	-64.72	310.09	0.83
SHC tests conducted (2016)	211.28	180.75	1.05	39.21	0.98
SHCs available on portal (2016)	806.44	770.22	50.26	154.57	0.75
Number of SHC samples (2017)	1,030.01	961.11	-64.65	54.10	0.23
No. of farmers covered by SHCs (2017)	4,470.67	4,572.42	34.66	251.47	0.89
SHC tests conducted (2017)	976.04	924.91	-44.77	52.32	0.39
SHCs available on portal (2017)	4,176.72	4,332.26	85.98	240.81	0.72
Joint p-value					0.15
Observations	417	131	548		

Differences in (3) are estimated through regressions on a treatment indicator, with fixed effects at the randomization strata level. Standard errors are clustered at the MAO level and reported in parentheses. For distance to nearest bank or ATM, the census records the distance to the nearest bank and ATM as one of three categories (less than 2.5km, 5-10km, or more than 10km). We substitute 2.5km, 7.5km, and 20km respectively for each of these categories, and calculate the average distance, weighting each village according to population. This produces the average distance across all farmers in the mandal.

Table 3: Effect on encashment outcomes

	Encashed before June 8th		Ever encashed		
	(1) Treatment	(2) Control mean	(3) Treatment	(4) Control mean	(5) Observations
Overall	0.0240***	0.69	0.0134**	0.83	5645937
	(0.00774)		(0.00640)		
Land quartiles					
Quartile 1	0.0277***	0.52	0.0223**	0.68	1449482
	(0.00913)		(0.00885)		
Quartile 2	0.0248***	0.71	0.0144**	0.85	1460294
	(0.00791)		(0.00632)		
Quartile 3	0.0241***	0.76	0.0112*	0.88	1443788
	(0.00755)		(0.00601)		
Quartile 4	0.0209***	0.77	0.00710	0.89	1443836
	(0.00803)		(0.00620)		
Test of H_o : $\beta_{Q1} = \beta_{Q2} = \beta_{Q3} = \beta_{Q4}$	*.72*	*(.54)*	*2.03*	*(.11)*	
Phone coverage					
Non Phone Owners	0.0252**	0.57	0.00913	0.72	2254142
	(0.00989)		(0.0103)		
Phone Owners	0.0207**	0.76	0.0131**	0.90	3543258
	(0.00810)		(0.00553)		
Test of H_o : $\beta_{No-Phone} = \beta_{Phone}$	*.12*	*(.70)*	*.29*	*(.59)*	

All specifications include fixed effects at the randomization strata fixed level. Standard errors are clustered at the MAO level and reported in parentheses. The bottom row of each panel reports the F-statistic and p-value from a test of the null that coefficients are statistically similar across categories. Models are estimated using administrative data at the individual check level, as a handful (0.8%) of individuals in the database were issued multiple checks. According to the Revenue Department, amounts above Rs. 50,000 (12.5 acres of land) were split into multiple checks. Outcomes are essentially perfectly correlated within individual, as farmers either picked up and encashed all or none of their checks, which is accounted for by clustering at the mandal level. Farmers with less than 0.025 acres of land (less than 1% of the sample) were still issued checks, but in the amount of Rs. 100.

Table 4: Effect on time to encashment

	Days to encashment		
	(1) Treatment	(2) Control mean	(3) Observations
Overall	-0.785**	20.16	4663678
	(0.381)		
Land quartiles			
Quartile 1	-0.689	24.00	984273
	(0.508)		
Quartile 2	-0.675*	20.08	1239638
	(0.383)		
Quartile 3	-0.841**	18.71	1278121
	(0.359)		
Quartile 4	-0.989***	18.80	1284764
	(0.368)		
Test of H_o : $\beta_{Q1} = \beta_{Q2} = \beta_{Q3} = \beta_{Q4}$	*1.33*	*(.26)*	
Phone coverage			
Non Phone Owners	-1.340***	22.14	1614191
	(0.453)		
Phone Owners	-0.493	19.13	3172605
	(0.392)		
Test of H_o : $\beta_{No-Phone} = \beta_{Phone}$	*5.02*	*(.03)*	

All specifications include fixed effects at the randomization strata fixed level. Standard errors are clustered at the MAO level and reported in parentheses. The bottom row of each panel reports the F-statistic and p-value from a test of the null that coefficients are statistically similar across categories. Models are estimated using administrative data at the individual check level, as a handful (0.8%) of individuals in the database were issued multiple checks. According to the Revenue Department, amounts above Rs. 50,000 (12.5 acres of land) were split into multiple checks. Outcomes are essentially perfectly correlated within individual, as farmers typically encashed all of their checks at the same time, which is accounted for by clustering at the mandal level.

Table 5: Effect on other MAO activities

	(1) Number of SHC samples entered	(2) Number of farmers covered by SHCs	(3) SHC tests conducted	(4) SHCs available on portal
Treatment	-43.54	-138.8	-26.55	-124.9
	(47.03)	(205.9)	(45.31)	(203.5)
Control Mean	906.02	4259.53	873.73	3891.78
Observations	514	514	514	514

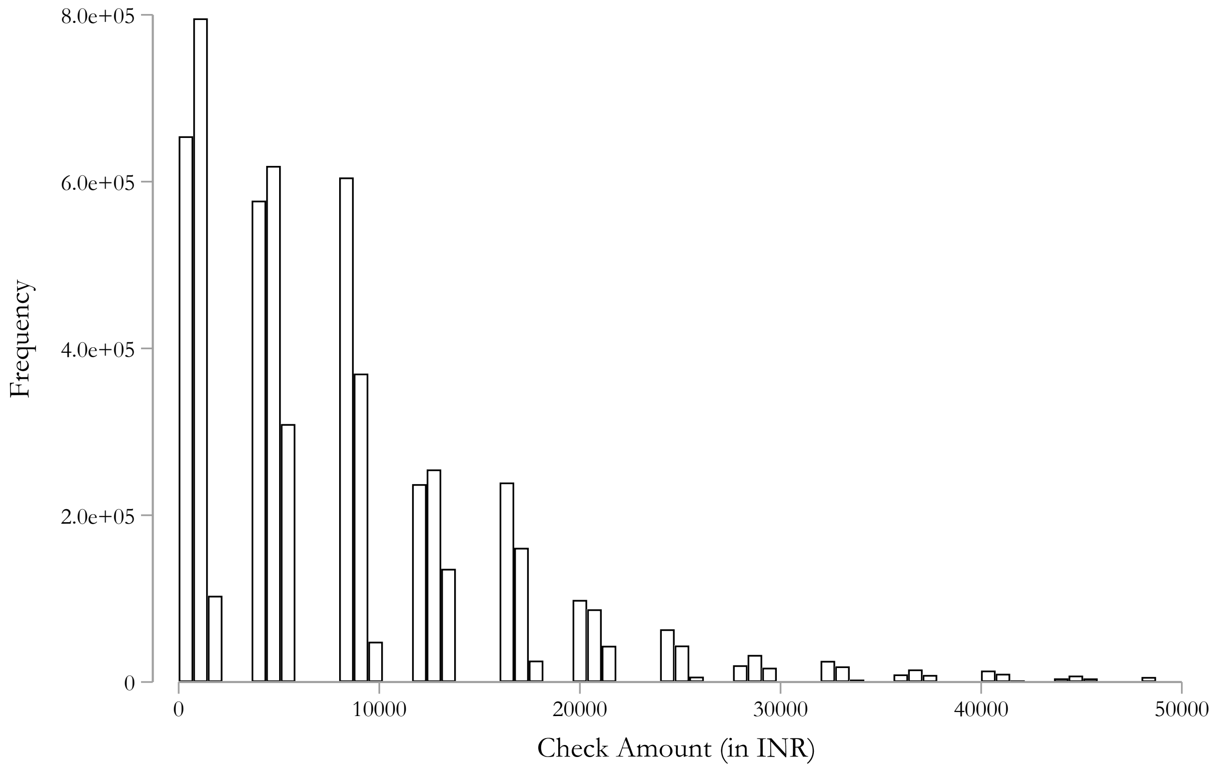
All specifications include fixed effects at the randomization strata fixed level. Standard errors are clustered at the MAO level and reported in parentheses. Models are estimated using administrative data on Soil Health Cards at the mandal level from the Department of Agriculture, Cooperation and Farmers Welfare.

Online Appendix
Improving Last-Mile Service Delivery using Phone-Based Monitoring

Karthik Muralidharan Paul Niehaus Sandip Sukhtankar
Jeffrey Weaver

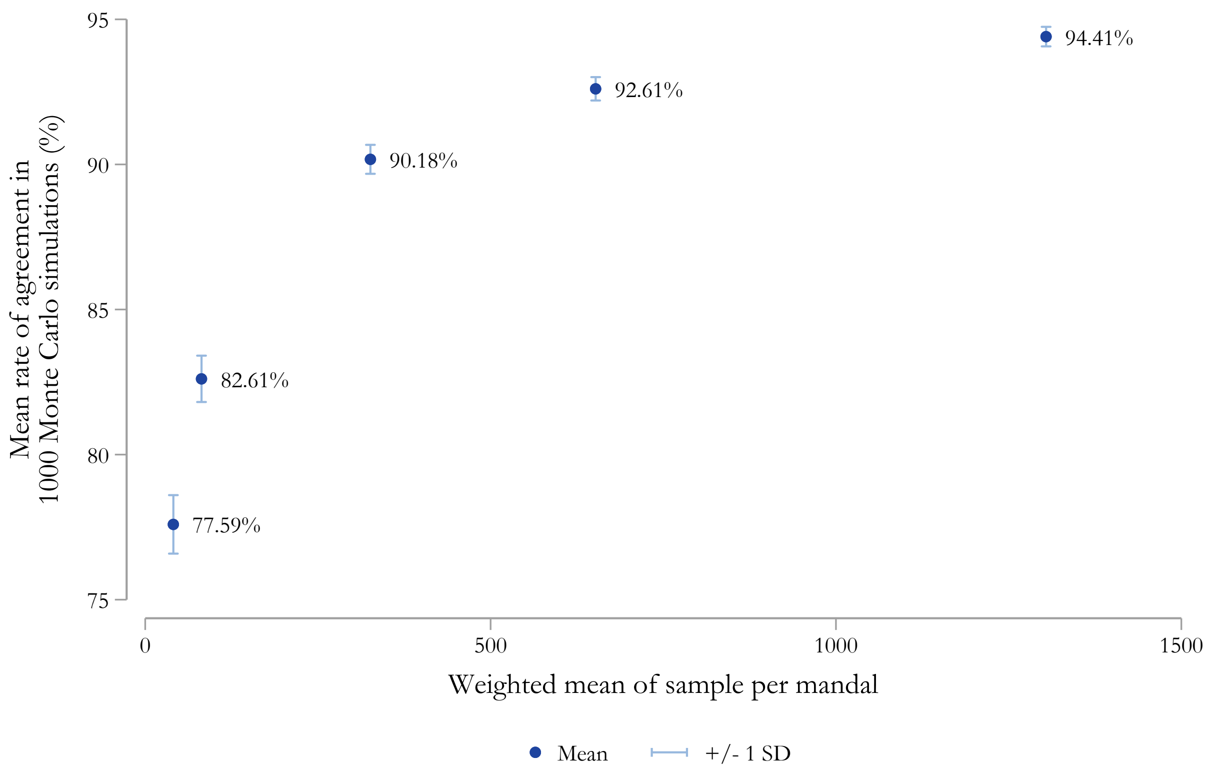
A Appendix Tables and Figures

Figure A.1: Distribution of Check Amounts Under Rythu Bandhu



This graph shows the frequency of check amounts in our sample, excluding the top 1% of landowners, corresponding approximately to amounts greater than INR 50,000.

Figure A.2: Phone Call Sample Size and Agreement Rate



To produce the above graph, we ran 1000 Monte Carlo simulations in which we selected (with replacement) X farmers per mandal from the full sample of farmers, calculated the rate of check encashment in each mandal in the simulated sample using the bank database, and produced a ranking of MAOs in each district in the simulated sample. We then calculate how often the simulated sample and full administrative data rank the relative performance of a pair (m, m') of MAOs within a district the same way. For example, suppose the call center rates MAO A as 3rd and MAO B as 4th best. If the administrative data rates them as 2nd and 3rd best respectively, then since both sources ordered MAO A as performing better, we consider the sources to be in agreement. On the other hand, if the administrative data rated them as 3rd and 2nd best respectively, then we would not consider them to be in agreement: MAO A is ranked better than MAO B by the call center, but not by the administrative data. This graph shows the rate of agreement for each sample size, where as the sample size increases, the rate of agreement will naturally increase.

Table A.1: Effect of MAO Survey Among Control MAOs

	Ever encashed		Encashed on time		Days to encashment	
	(1)	(2)	(3)	(4)	(5)	(6)
Sampled for MAO survey	0.00876		0.0112		-0.0214	
	(0.00767)		(0.0107)		(0.438)	
Completed MAO survey		0.00219		0.00127		0.249
		(0.00839)		(0.0124)		(0.534)
Observations	4348248	4348248	4348248	4348248	3575083	3575083

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Outcome in header. All specifications include district fixed effects since a fixed number of MAOs in each district were sampled for the survey. Standard errors in parentheses and clustered at the MAO level.

Table A.2: Impact on beneficiary experience

	(1) Received at Gram Sabha	(2) Asked to Pay Bribe	(3) Satisfied with Scheme
Treatment	0.00759*	0.00108	0.00232
	(0.00457)	(0.00230)	(0.00359)
Control Mean	0.94	0.02	0.93
Observations	19890	19830	22329

Outcomes in header. Estimates are weighted using (inverse) sampling probability, as pre-specified, based on the probability that an individual was sampled for an attempted call. All specifications include randomization strata fixed effects. Standard errors are clustered at the MAO level and in parentheses. The number of observations varies due to lower rates of response on some questions, which were asked later in the phone survey.

Table A.3: Direct Effect of Reports on Encashment (Hazard Model)

	Exponential Proportional Hazard		Stratified Cox	
	(1)	(2)	(3)	(4)
Treatment	0.074**	0.034**	0.044**	0.041**
	(0.030)	(0.017)	(0.021)	(0.020)
Treatment × Report		-0.045		-0.023
		(0.079)		(0.039)
Observations	5527320	5527320	4560929	4560929

Model specification in header. All specifications include fixed effects at the randomization strata fixed level. Standard errors are clustered at the MAO level and reported in parentheses.

Table A.4: Testing for spillovers

	(1) Ever distributed	(2) Ever encashed
Number of treatment mandals in revenue division	0.000679	0.00847
	(0.00473)	(0.00551)
Observations	399	399

As pre-specified, this table tests for the possibility that these results could be explained by supervisors of MAOs focusing more attention on treatment MAOs. Districts in Telangana are divided into "revenue divisions," which each contain several mandals. Although roughly the same fraction of mandals were treated in each district, we did not stratify the randomization at the revenue division level. As a result, there is random variation in the fraction of MAOs within each revenue division that are treated. If there were diversion of revenue division supervisor-level attention and attention matters for performance, we should expect worse performance among control MAOs with more treated MAOs in their revenue division, as these control MAOs would get less attention paid to them. This table does not find this to be the case. Outcome in header. All specifications include fixed effects for districts and number of mandals in the revenue division. Standard errors in parentheses and clustered at the revenue division level. 17 mandals could not be matched to revenue divisions, so were not included.

Table A.5: Effect of Being Randomly Selected for Calls

	(1) Ever encashed	(2) Encashed on time	(3) Days to encashment
Sampled for phone survey	-0.000059	-0.003	0.016
	(0.0014)	(0.002)	(0.083)
Observations	3500017	3500017	3134515

Outcome in header. Each specification contains mandal fixed effects, as this is the unit at which random sampling of farmers to be called by the call center was done. Standard errors are clustered at the farmer level. These observations only include farmers who had a listed phone number, and hence a non-zero chance of being sampled for the phone survey.

Table A.6: Main Outcomes, Excluding Those Randomly Selected for Calls

	(1) Ever encashed	(2) Encashed on time	(3) Days to encashment
Treatment	0.013^{**}	0.023^{***}	-0.770^{**}
	(0.006)	(0.008)	(0.382)
Observations	5537302	5537302	4569539

Outcome in header. All specifications include fixed effects at the randomization strata fixed level. Standard errors are clustered at the MAO level and reported in parentheses. These observations do not include farmers who were randomly sampled to be called by the call center.

Table A.7: Comparing encashment outcomes in phone and administrative data

	(1)	(2)	(3)
	Actual agreement rate	Agreement rate from sampling variation	Residual disagreement rate
Pair-wise order of rankings	68.5%	77.6%	9.1%
Bottom 20% in PD found in bottom 20% of AD	43%	61.7%	18.7%
Bottom 20% in PD found in bottom 50% of AD	83%	92.7%	9.7%

AD (Administrative Data). PD (Phone Data). The actual rate of agreement between phone and administrative data is reported in (1). Next, a comparison is made between the entire population of administrative data and 1,000 random draws of farmers sampled from the administrative data, where each draw is the size of the phone call sample. The mean of these 1,000 agreement rates is reported in (2), showing the amount of disagreement that we would expect due simply to sampling variation in which farmers were selected for the phone call sample. The residual disagreement rate after accounting for (2) is reported in (3).

Table A.8: Comparing encashment outcomes in phone and administrative data

	(1) Phone Data	(2) Admin Data
Treatment	0.00219	0.0131
	(0.00982)	(0.0111)
Observations	22005	22005
Control mean	0.75	0.73
Chi-squared test p-value (vs. (1))		0.46

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Reports Sent to District and Mandal Agricultural Officers

II.A Report to Mandal Agricultural Officers - Example

Report on Rythu Bandhu Scheme in (Mandal name)[46]

Dear (MAO Name),

As you were informed at the beginning of May, the Department of Agriculture carried out a pilot program in your mandal during the Rythu Bandhu Scheme. For this program, a call center called farmers from all of the villages in your mandal and collected information about their experience with the Rythu Bandhu scheme. The below table gives the results of this survey.

The first row gives the percentage of farmers who received their cheques in your mandal, as well as in the whole district and the state. The second row is the percent of farmers who received their cheques before May 20th. The third row is the percent of farmers who reported being satisfied or very satisfied. The fourth row is the percent who have successfully encashed their cheques, if they got them. The fifth row is the percent of farmers who reported having a payment demanded for receiving their cheques.

This report will also be shared with district and state level agricultural officials.

We hope that the availability of this data on program performance is helpful to you. If you have any questions about this report, you can call (Name of member of field team) (Mobile number), an outside consultant who is working with Government of Telangana to implement the pilot.

(Name of bureaucrat)
Commissioner of Agriculture

[46] The report was sent out in the working language of the state (Telugu). This is the original pre-translation version of the report.

Category	In Mandal	District Average	State Average	Overall Rating
Farmers Received Cheque	90.5%	82.2%	83.3%	Excellent
Cheque Received Before May 20	43.9%	38.9%	45.1%	Fair
Satisfied Farmers	91.5%	93.4%	92.2%	Excellent
Successfully Encashed Cheque	81.1%	76.8%	75.4%	Excellent
Money Requested for Cheque	3.7%	2.0%	1.5%	Fair

GUIDE:
Farmers Received Cheque:
0-75% (Poor), 75%-80% (Fair), 80-85% (Good), 85-100% (Excellent)

Cheque Received Before May 20:
0-40% (Poor), 40%-50% (Fair), 50-60% (Good), 60-100% (Excellent)

Satisfied Farmers:
0-80% (Poor), 80%-85% (Fair), 85-90% (Good), 90-100% (Excellent)

Successfully Encashed Cheque:
0-55% (Poor), 55%-60% (Fair), 60-75% (Good), 75-100% (Excellent)

Money Requested for Cheque:
>4% (Poor), 2%-4% (Fair), 1-2% (Good), 0-1% (Excellent)

Village	Cheque Received (%)	Cheque Encashed (%)
(VILLAGE 1)	84.1%	75.8%
(VILLAGE 2)	88.0%	48.6%
(VILLAGE 3)	88.4%	74.7%
(VILLAGE 4)	89.6%	79.6%
(VILLAGE 5)	90.4%	77.1%
(VILLAGE 6)	91.3%	77.9%
(VILLAGE 7)	91.6%	84.1%
(VILLAGE 8)	91.7%	80.4%
(VILLAGE 9)	91.9%	78.5%
(VILLAGE 10)	92.6%	80.4%
(VILLAGE 11)	93.4%	82.6%
(VILLAGE 12)	94.0%	85.4%
(VILLAGE 13)	94.3%	80.3%
(VILLAGE 14)	95.3%	82.3%
(VILLAGE 15)	96.1%	81.4%
(VILLAGE 16)	96.7%	82.6%
(VILLAGE 17)	96.7%	79.1%
(VILLAGE 18)	97.0%	76.6%
(VILLAGE 19)	97.1%	83.1%
(VILLAGE 20)	97.9%	74.5%

II.B Report to District Agricultural Officers - Example

Report on Rythu Bandhu Scheme in (District name)[47]

Mandal	MAO Name	Farmers Received Cheque	Cheque Received Before May 20	Satisfied Farmers	Successfully Encashed Cheque	Money Requested for Cheque
		79.3%	38.0%	91.3%	65.2%	1.1%
(Mandal 1)	(MAO 1)	Fair	Poor	Excellent	Good	Good
		78.7%	36.0%	88.0%	73.3%	2.7%
(Mandal 2)	(MAO 2)	Fair	Poor	Good	Good	Fair
		80.5%	43.9%	91.5%	78.1%	3.7%
(Mandal 3)	(MAO 3)	Good	Fair	Excellent	Excellent	Fair

[47]The report was sent out in the working language of the state (Telugu). This is the original pre-translation version of the report.

C Discussion of disbursement and encashment data

The Agriculture Department at the Government of Telangana (GoTS) wanted to collect administrative data on the progress of the Rythu Bandhu program using two independent databases. The first database was maintained by MAOs. MAOs were given tablets on which they were supposed to input which checks had been distributed on a given date. The second database was maintained by banks, and tracked whether each check was encashed and the date of encashment. The bank database was updated in real-time as checks were cleared.

Clearly, the reliability of these systems may differ: the MAO-based system depends on clerical and administrative processes, while the bank-based system is linked with existing payment systems and largely mechanized. Banks were required to maintain updated data to ensure they received payments from the government, while we observed that the MAO-based system was updated in a more haphazard and inconsistent manner. We did not know this during the study design phase, but after observing the distribution process, we came to suspect that the MAO-based data on distribution of checks may not be as reliable as bank-based systems on encashment.

We conducted a number of checks and found that the suspected unreliability of MAO-based distribution indicators was borne out in the data itself, such as in the following instances:

- We received the up-to-date MAO and bank-based databases at three points in time: once in July, once in August, and once in September 2018. When we compare the September data to the previous two rounds, we see that for 1% of the observations, disbursement status was revised from "distributed" to "not distributed". This indicates an error was caught in these cases, but is worrisome since there may be other errors that were missed. The bank-based data had no such revisions.
- The September round of data lists dates of distribution for 700K checks that are also recorded as never having been distributed.
- From our conversations with GoTS, there was a misunderstanding on the part of MAOs regarding the date of distribution indicator. A significant number of MAOs updated the "date of distribution" field with the date on which they uploaded their data, which may be weeks after the actual date of distribution. This is obvious in the data, where many MAOs are recorded as distributing an impossibly large number of checks on a given day, with no checks delivered on other working days. Based on this, we do not believe the date of distribution field to be a usable indicator for speed of distribution.
- There are substantial revisions in the indicator for date of distribution in the MAO-based database. Between the three rounds of data, there are differences in dates of distribution in 1-5% of the observations.

Overall, there are significant reasons to distrust the MAO-based data on check distribution, including that MAOs might have tried to overstate their performance by recording that they distributed more checks than they actually did. The bank-based data on check encashment, on the other hand, do not have these concerns and closely match data from the phone surveys. We report results below in tables A.9 and A.10 based on the MAO database since we committed to this in the pre-analysis plan, but believe the outcomes to be noisily measured in that dataset as compared to the check encashment data.

Table A.9: Effect on check distribution outcomes (MAO reports)

	Distributed before June 8th		Ever distributed		
	(1) Treatment	(2) Control mean	(3) Treatment	(4) Control mean	(5) Obs.
Overall	0.00924	0.81	0.00793*	0.87	5,645,937
	(0.00653)		(0.00468)		
Land quartiles					
Quartile 1	0.0177*	0.67	0.0165*	0.74	1,449,482
	(0.00984)		(0.00878)		
Quartile 2	0.00955	0.83	0.00910**	0.89	1,460,294
	(0.00634)		(0.00417)		
Quartile 3	0.00742	0.87	0.00654**	0.92	1,443,788
	(0.00568)		(0.00319)		
Quartile 4	0.00546	0.87	0.00371	0.93	1,443,836
	(0.00569)		(0.00334)		
Test of H_o : $\beta_{Q1} = \beta_{Q2} = \beta_{Q3} = \beta_{Q4}$	*0.43 (0.73)*		*1.18 (0.32)*		
Phone coverage					
No listed phone	0.00814	0.69	0.00673	0.76	2,254,142
	(0.0114)		(0.0104)		
Listed phone	0.00544	0.89	0.00498*	0.94	3,543,258
	(0.00536)		(0.00269)		
Test of H_o : $\beta_{No-Phone} = \beta_{Phone}$	*0.10 (0.75)*		*0.08 (0.78)*		

All specifications include fixed effects at the randomization strata fixed level. Standard errors are clustered at the MAO level and reported in parentheses. The bottom row of each panel reports the F-statistic and p-value from a test of the null that coefficients are statistically similar across categories.

Table A.10: Effect on time to distribution (MAO reports)

	Days till distributed		
	(1) Treatment	(2) Control mean	(3) Observations
Overall	-0.125	11.70	4,930,113
	(0.310)		
Land quartiles			
Quartile 1	-0.220	13.55	1,082,824
	(0.386)		
Quartile 2	-0.0543	11.53	1,302,380
	(0.312)		
Quartile 3	-0.104	18.71	1,334,261
	(0.299)		
Quartile 4	-0.232	11.23	1,343,004
	(0.297)		
Test of H_o : $\beta_{Q1} = \beta_{Q2} = \beta_{Q3} = \beta_{Q4}$		*0.61 (0.61)*	
Phone coverage			
No listed phone	-0.128	13.85	1,729,723
	(0.403)		
Listed phone	-0.0826	10.57	3,332,746
	(0.286)		
Test of H_o : $\beta_{No-Phone} = \beta_{Phone}$		*0.05 (0.83)*	

All specifications include fixed effects at the randomization strata fixed level. Standard errors are clustered at the MAO level and reported in parentheses. The bottom row of each panel reports the F-statistic and p-value from a test of the null that coefficients are statistically similar across categories.