Accountability in the delivery of guaranteed employment through MGNREGA in rural India *

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16 February 2026

Abstract

We use the uneven roll-out of accountability measures to identify their impact on the provision of guaranteed employment in rural Andhra Pradesh and Telangana, India. A public information campaign combined with an NGO supported grievance mechanism resulted in treated households working over 10 additional days per year in the program. We find no immediate impact from the information campaign alone. These estimates, based on three rounds of Young Lives survey data, suggest that the combination of accountability measures enhanced the fulfillment of work entitlements. However, our research design cannot disentangle longer-run effects of the information campaign from the incremental impact of the grievance mechanism. The paper also discusses the implementation of India's workfare program, accountability failures, the design of the interventions, and seasonal variation in program take-up.

Keywords: Administrative processes, Corruption, Information and Knowledge, Labor Supply, Workers' Rights

JEL Code: D73, D83, J22, J83

*We thank the participants of the Trinity College Dublin Economic PhD Working group, Michael King, Tara Mitchell, Andy McKay, and Prudhvikar Reddy for their comments. The main data used in this publication come from Young Lives, a 15-year study of the changing nature of childhood poverty in Ethiopia, India (Andhra Pradesh and Telangana), Peru and Vietnam (www.younglives.org.uk). The author would like to thank CESS, in particular P. Reddy, S. Galab, and S. Sunder, for their help and input. We thank Sanghamitra Bandyopadhyay and two anonymous referees for the substantial improvements during the review process. The authors would like to acknowledge research support from the Irish Research Council and the EU's Horizon 2020 research and innovation program under the Marie Skłodowska-Curie grant agreement No 713279. The views expressed in this paper are those of the author. They are not those of Young Lives, the University of Oxford, CESS, Trinity, or other funders. None of the authors have a conflict of interest to disclose. Data sharing is not applicable to this article as no new data were created or analyzed in this study.

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1 Introduction

Lack of accountability is a common reason for the failure of anti-poverty programs and laws in developing countries (Burguet et al. (2016)). The Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA) in India, is one of the world's biggest public works schemes, covering around 11 percent of the world's population, and accounts for a significant proportion of India's annual public spend ((Niehaus and Sukhtankar, 2013); (Klonner and Oldiges, 2022)). Yet, even over 17 years into its implementation, the Act suffers from leakages, low participation rates and the rationing of work ((Shariff, 2009; Dutta et al., 2014; Sukhtankar, 2016)). Using MGNREGA data from 2009-2012, Liu and Barrett (2013) and Das (2015) find that poorer households face more rationing which is opposed to the intended targeting of the program. Narayanan and Das (2014) show substantial variation in female participation rates between and within states. Therefore, rationing and other (social) barriers are a particular concern for the most vulnerable groups, including widows, single women, and poorer households.

Commonly cited reasons for rationing and elite capture arising from weak accountability include limited beneficiary awareness, a lack of independent monitoring of agent behavior, inadequate grievance redress mechanisms, and disempowered clients (Ravallion et al., 2013; Dutta et al., 2014; Jha et al., 2015). However, the empirical evidence on whether information campaigns mitigate these failures in the delivery of development programs remains mixed. On the one hand, Banerjee et al. (2015) find that providing information directly to citizens is a cost-effective way to improve government performance. Relatedly, DiRienzo et al. (2009) suggest that beneficiary awareness can constrain government discretion by increasing scheme transparency, thereby supporting more efficient delivery. Muralidharan et al. (2021) find that informing program officials that implementation would be verified via phone calls with beneficiaries reduced the share of beneficiaries who did not receive their transfers. In the context of MGNREGA, Banerjee et al. (2020) show that making information available in real time reduced payment leakage and administrative costs, although the resulting gains for recipients were less clear.

On the other hand, Banerjee et al. (2010a) and Ravallion et al. (2013) find that aware-

ness, while necessary, may not be sufficient to overcome accountability failures. Buntaine et al. (2018) find that an information campaign in Uganda on how and when individuals could participate in community-driven development projects had no impact on knowledge or participation. In a study of an MGNREGA information awareness campaign, Ravallion et al. (2013) find that while the information campaign increased knowledge of the Act, it did not translate into improved performance. Ravallion et al. (2013) argue that for information to influence the program implementation, individuals need to be empowered enough to actually put the information to use. Buntaine et al. (2018) argue that information campaigns will not impact participation unless it also promotes collective action. However, there is little empirical evidence on how information in the hands of individuals mobilized to use the information affects the prevalence of corruption. In addition, there is limited evidence on the impact of grievance redress mechanisms on program delivery (Ringold et al. 2012a).

In this paper, we contribute to the empirical evidence on bottom-up monitoring approaches for public works schemes. We focus on a sequence of two accountability measures that aim to empower beneficiaries to access their entitlements, which are central to the functioning of MGNREGA (Drèze, 2023). Households are first exposed to an information campaign, which is subsequently complemented by a grievance mechanism. The grievance mechanism is facilitated by an NGO and can be used when a household's rights are not upheld. While the first measure increases household access to information about their entitlements, the second measure changes how beneficiaries can interact with the system to seek redress when problems arise. Through this, these measures increase the expected cost of deviation (corruption) for the local MGNREGA agent.

Using three rounds of panel survey data, we test the impact of the information campaign and grievance mechanism by observing the total and seasonal distribution of days worked through MGNREGA. Given the sufficient shelf of work projects in Andhra Pradesh (AP) (Leelavathi and Hanumantha Rao (2010)), we follow Sukhtankar (2016) and attribute the initially low number of days worked through MGNREGA to deviations in the

provision of labor at the village level.[1] Therefore, we hypothesize that lower local corruption leads to, first, an increase in the total number of days worked and, second, changes in the timing when households access work over the year.

During the eight months of the agricultural season, local employers have an incentive to pressure agents into withholding work to reduce competition. Reddy et al. (2014) document a "widespread demand by farmers for stopping MGNREGS work during the agricultural peak season." Berg et al. (2018) find that MGNREGA led to an increase in agricultural wages with effects concentrated in the peak season and that the timing of MGNREGA employment is at least partly supply-driven. Therefore, absent this pressure, employment through MGNREGA could be higher and more evenly spread throughout the year as the wage is typically higher than for alternative employment, and work days are heavily concentrated in the lean season. In our data, the four-month lean season accounts for three quarters of days worked. However, the seasonal distribution likely also reflects demand as households use MGNREGA to smooth consumption in the lean season (Reddy et al., 2014; Klonner and Oldiges, 2022) and the impact of rationing on seasonal distribution is, therefore, less clear-cut.

Besides increasing the supply of jobs, the information campaign might also raise demand for work as workers are better informed about the extent of their rights, NGOs could assist in identifying projects on private land, and field agents are less likely to demand a share of the payment. However, an increase in demand does not necessarily increase days worked without a change in supply (Ravallion et al., 2013, 2015; Das et al., 2023).

Our identification strategy exploits spatial variation in the implementation of the two-step accountability intervention, as not all areas were treated by the NGOs. We observe no significant difference between treated and non-treated areas across several outcome variables before treatment, with the average of total days worked through MGNREGA being almost identical (41.45 vs. 41.72 days per year). Using one round of household panel data before the information campaign, one between the two treatments, and one round after implementation of the grievance mechanism, we can compare the development

[1] We define sufficient to mean that there is enough work projects in a locality to meet demand from applicants for MGNREGA work.

of MGNREGA outcomes of those exposed to the accountability intervention with those in non-treatment areas.

Our dataset combines information on treated areas with household data from the Young Lives panel survey for AP and Telangana.[2] The survey collects extensive demographic, consumption, income, assets and job information at the individual and household level since 2002 in 15 mandals. We limit our analysis to 1225 rural households that did not move mandals. This is merged with state government data on the MGNREGA information campaign, which was carried out in 6 of these 15 mandals between 2011 and 2016.

We find that the roll-out of the information campaign had no statistically significant impact on days worked by treated households just over a year after the campaign was completed. This result is in line with findings by Ravallion et al. (2013), which suggest that information by itself may be insufficient to address accountability.

After phase two of the intervention, when the grievance mechanism is added, we find that treated households work more total days in the year compared to non-treated households. This suggests that information combined with a redress mechanism may contribute to empowering citizens to access their entitlements under MGNREGA. However, the increase may also reflect longer-term effects of the information campaign, in line with findings by Das et al. (2023). When we examine heterogeneity by gender, we find that the estimated effects are slightly larger for men though the difference is not statistically significant.

The findings are robust to changes in the sample and estimation assumptions, including restricting the sample to households that held a job card in at least one survey round, household or mandal fixed effects, and estimating both proportional (Poisson) and linear specifications.

This study contributes to three strands of the literature. First, our findings complement studies on information and accountability. We build on Ravallion et al. (2013), by

[2]Prior to June 2, 2014 AP and Telangana were one state. After the split, while the names of the department and NGO coalition changed, their structure did not. Therefore, we expect both the intervention and implementation in both states to have continued similarly.

showing that information combined with a grievance mechanism can result in the realization of entitlements under government programs. By showing an impact on the level of entitlements accessed, we also complement Muralidharan et al. (2021), who find that phone-based monitoring increases the likelihood of beneficiaries receiving transfers for completed work, as well as research by Das et al. (2023) showing how increased transparency on payments after an intervention from November 2017 to November 2018 led to a decrease in payment inefficiencies and – with a lag – to more days worked under MGNREGA in Telangana. Muralidharan et al. (2023) observe an increase in MGNREGA days worked between 2010 and 2012 after the introduction of biometric authentication for payments in Andhra Pradesh. In the timing of the intervention, the program studied in our paper falls between these studies. Relatedly, Nagavarapu and Sekhri (2016) show that increased informal monitoring of (public) agents can improve service delivery.

Second, we add to the literature on what role civil society organizations (CSOs) can play in building accountability. Information programs have been found to increase citizen participation in holding governments to account in certain circumstances (Protik et al., 2018). We show how CSOs' direct delivery of information to beneficiaries and their direct facilitation of a grievance mechanism strengthen program delivery.

Finally, we contribute to the research on welfare programs, including MGNREGA, that examines how we can strengthen implementation via an accountability initiative that clarifies and simplifies the rules, while also creating a mechanism to hold local agents to account (Ravallion et al. (2013); Muralidharan et al. (2023); Das et al. (2023)). Importantly, Muralidharan et al. (2023) find that a more efficiently implemented MGNREGA increases wages and/or employment in the local private sector. Muralidharan et al. (2023) attribute 86 % of income gains observed to non-program earnings. These general equilibrium effects are likely to further increase the benefits of the intervention observed in this paper.

The remaining part of this paper is structured as follows. Section 2 provides an overview of MGNREGA in India, its implementation in AP, and the accountability intervention. We provide a short conceptual framework in section 3. This is followed by

Section 4, with an overview of the data and summary statistics. Section 5.1 lays out the identification strategy, and Section 5.2 presents the empirical specification used to study the impact of the accountability intervention on MGNREGA outcomes. The findings are presented in Section 6. Section 7 concludes.

2 The role of accountability in guaranteeing employment in India

In 2005, the Indian Government passed the National Rural Employment Act, under which any rural household could demand up to 100 days of employment per year. The Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA), as it was later called, was designed as a demand-driven Act with universal access across rural India.[3] Through this Act, the Government provides the rural poor with a type of self-targeting social safety net underpinned by guaranteeing work for a set number of days.

Yet, glaring problems in the implementation of the Act remain, almost 20 years after it began. For instance, participation remains uneven across India. For states such as Haryana and Maharashtra, the participation rate (extensive margin) for MGNREGA is between 9 and 12 percent for those classified as poor (Dutta et al. 2014). On the intensive margin, the total number of workdays obtained in 2018 through the scheme is also lower than the 100-day threshold for all states (Reddy et al. 2021)

Even with implementation gaps, the impact of MGNREGA on poverty is substantial. Zimmermann (2013) finds that MGNREGA acts as a safety net for households in the face of a negative economic shock. In the short run, Deininger and Liu (2013) find that participants increased their protein and energy intake, while in the medium term they accumulate more non-financial assets. Imbert and Papp (2015) find that in early implementation districts, private sector wages increased, which resulted in large welfare gains for the poor even beyond the direct program effects of MGNREGA. This impact of MGNREGA is not just limited to the current generation. MGNREGA increases the

[3]In 2006, MGNREGA was rolled out in the 200 poorest districts in India, 130 more districts were added in 2007 - 2008, while the remaining districts came under MGNREGA in 2008-2009 (Azam 2012).

educational outcomes of children whose mothers participated in the scheme (Afridi et al. 2012). Consistent with this, Klonner and Oldiges (2022) find that MGNREGA led to large increases in seasonal consumption, as well as increases in adolescent schooling, in states where the program was implemented intensely.

Given that the existing research points to important welfare impacts of MGNREGA on households, a key question is why households who want to work do not obtain this work, whether they obtain the work when they need it, and what can be done to translate demand into employment. Making MGNREGA function more efficiently can have a significant impact on poverty. Ravallion et al. (2013) argue that poverty could be reduced by up to 12 percentage points as a result of income gains from MGNREGA in Bihar.

2.1 Implementation of MGNREGA in Andhra Pradesh and Telangana

AP and Telangana in South India are two of the stronger MGNREGA performers (Dutta et al. 2014). With both states having between 61 and 65 percent of their population living in rural areas (Office of the Registrar General & Census Commissioner, India 2011, Government of Telangana 2022), MGNREGA has important implications for rural households and poverty levels.

Since the introduction of MGNREGA, AP, which at that point included Telangana, was active in preventing corruption. First, it took a transparent approach by making all program information accessible and traceable online for the public as early as 2007 (Deininger and Liu 2013; Kumari et al. 2008). Second, to minimize the potential for corruption in payments, the state streamlined the payment system (Deininger and Liu 2013). Third, AP implemented social audits to uncover any fraud or other problems in the delivery of the Act from 2008. Both states maintained these initiatives after their 2014 separation.

Yet, even with these steps, Masiero and Maiorano (2017) argue that the power structure of MGNREGA at the village level left scope for local capture. To apply for work, until 2010, a household had to submit a written application to the field assistant, the key

government appointed official at the village level.[4] A duplicate of the submitted application is created, which the field assistant signs and then submits to the mandal level official. At this point, the application is computerized and entered into the MGNREGA monitoring system. Within 15 days of the application, work must be sanctioned, if not the household has the right to apply for (less attractive) unemployment benefits.[5]

Therefore, the field assistant has a certain degree of discretionary power over what information is inputted into the computer system (Masiero and Maiorano 2017). Masiero and Maiorano (2017) argue that the state's system enables the field assistant to retain their power at the village level and undermine the empowerment of households who want to work. This then has important implications for the number of workdays supplied to these job seekers and the timing of that work.

The Accountability Intervention: Information Awareness and Monitoring Campaign

In this paper, we focus on the bottom-up accountability mechanism that the AP government agreed to in 2010 and implemented between 2010 and 2016. Figure 1 provides a timeline of the different steps of the intervention. The intervention reformed how beneficiaries could access work under MGNREGA and provided information to beneficiaries on MGNREGA, including their entitlements and how to address their MGNREGA-related grievances. The state established a civil society–government collaboration called the AP NGO Alliance (APNA) to deliver this intervention, aiming to "mobilize the rural poor and empower them to fully use the entitlements provided by the Act" (Government of Andhra Pradesh 2010b).

This intervention had significant potential to change the power structure of MGNREGA implementation by changing how informed job seekers were, how they applied for

[4]The field assistant is the main government official who manages the scheme at the village level and is the point of contact for the households (Masiero and Maiorano 2017). The roles and responsibilities of the field assistant: "Assists the Panchayat Secretary, supervises the works, maintains the muster rolls, gives mark outs at work sites, maintains the register of material procured, maintains the village information boards." (Government of Andhra Pradesh 2013).

[5]Unemployment payments are only a fraction of MGNREGA wages per day and count towards the workday limit (Babu et al. 2014).

work, and how field assistants were monitored. First, as part of this intervention, the structure of how work was applied for was reformed. As outlined in a government order, starting in mid-2010, all workers who had worked at least 20 days would be formed into fixed labor groups (FLGs), consisting of up to 20 members. The government order further specifies that in areas where APNA NGOs were working, they would facilitate the formation of these groups, while in other areas the field assistant would. Once a household was part of an FLG, they could only apply for work together, not individually. This step addresses a supply-side constraint, where sanctioned work needed a minimum number of households to apply to make the work feasible. By having a group fulfill the minimum threshold, it makes it easier to supply work. This step of the intervention was done in all areas of AP. While the formation of FLGs in different areas may have been facilitated by different entities, the implementation protocols are uniformly applicable throughout the state and detail steps for group formation and the ability to migrate to another group (Government of Andhra Pradesh (2010c)).

The next component of the intervention was to increase knowledge of the Act. Between May 2011 and August 2012, APNA NGOs carried out an information awareness campaign for the FLGs. The campaign's purpose was to educate workers on their rights and entitlements under the Act and on how they could access their entitlements under the Act (Government of Andhra Pradesh 2010b).[6] Each FLG selected four representatives to attend these sessions. A total of five FLG groups attended each session, so the NGO trained 20 people per session.[7] The NGO would send a staff member (who was not from the village) to the village to provide the training. Guidelines from the government were provided to each NGO on the materials to be covered, but how the NGO delivered the information was up to them. At the end of the training, the NGO had to submit a completion report to the government. As for the FLGs representatives, their role was to share the information with those not at the training and then apply the information for the benefit of their FLG members.

[6]Information in the paragraph is based on GO 80 (Government of Andhra Pradesh 2010b).

[7]Around 50 to 65 households would be covered by each training that was given as part of the information campaign.

The final component of the intervention was a monitoring and grievance mechanism, where NGOs had monthly meetings with FLGs to monitor implementation of the Act in the village. In an internal presentation from the AP government, they described the role of APNA NGOs as providing feedback on implementation and corruption, and conducting fact-finding missions in cases of suspected misappropriation and/or complaints (Government of Andhra Pradesh (2012)). Furthermore, NGOs conducted their work through a cascading approach: they first held monthly meetings in treated areas, during which the NGO would note down grievances and observe irregular behavior. They would then carry out fact-finding missions on any complaints brought up about MGNREGA implementation within the village. As part of this cascading approach, any issues from the village would be brought up next at a monthly district meeting, and finally at the monthly state-level meeting, both of which were held with relevant government officials (Government of Andhra Pradesh (2012)). This component of the intervention was initiated in September 2013 and continued until September 2016.[8]

As an example of the work done by NGOs in the field, *Watershed Support Services And Activities Network* (WASSAN)'s description of its activities under APNA helps clarify both what the intervention entailed as well as why such an intervention had the potential to change the power structures at the village level, particularly the field assistant's level of discretionary power. According to WASSAN, first, they trained households on how to maintain and update records at the village level through their FLGs. Second, they conducted regular meetings at the village level. Third, they supported FLGs in identifying what works could be done on their land. Fourth, they encouraged job card holders to use the toll-free complaint number to voice their grievances. Finally, they mobilized wage seekers to follow up with the mandal administration for the implementation of identified works (National Consortium on MGNREGA, 2016).

[8]In AP and Telgana by APNA and TSNA NGOs respectively.

3 Conceptual Framework

We interpret the implementation of MGNREGA in AP in a principal-agent-client setting: government, local agent, and beneficiaries. The government-designated agent (the field assistant), who implements the program at the local level, can take self-motivated decisions that result in the misallocation or loss of resources for beneficiaries (Burguet et al. 2016). This local agent can collude with local elites or engage in favoritism toward beneficiaries (Dutta 2015). As a result, a local agent may shirk their duty (Reinikka and Svensson 2011), take bribes (Mauro 1995) or misuse their role for private gains (Treisman 2000). Depending on the type of decision made by the local agent, the outcome may also benefit some beneficiaries (Burguet et al. 2016). Jha et al. (2015) find significant program capture of the Rural Public Works and Food for Work Programs in India, reflecting a degree of collusion between program agents and the village elite. From the principal's perspective, any deviation from the program design is nonoptimal (Burguet et al. 2016).

Discussing an MGNREGA information campaign in Bihar, Ravallion et al. (2013) argue that increasing demand alone is not enough to improve individual outcomes if individuals cannot exercise their rights. An effective intervention, therefore, needs "credible changes on the supply side, including more effective implementation of public disclosure and grievance processes, and more rapid responses to demand" (Ravallion et al., 2013). Increasing the expected cost of deviation for local implementers can ensure that the demand for work is met. The discretionary power of local agents, along with the clarity and complexity of the program, are factors that influence their expected cost of deviating. Therefore, an accountability initiative that makes the rules of a program clearer and simpler, while also creating a mechanism to hold local agents to account when they do not follow the rules can decrease the discretionary power of a local agent and increase their expected cost of deviating (Ryvkin and Serra 2012).

The accountability campaign studied in this paper addresses several concerns about the distribution of MGNREGA work and on the effectiveness of enforcement measures. First, information is provided publicly to eligible households, informing them of their specific rights to demand work. We argue that as the information campaign directly

tackles information constraints, the expected cost of deviating should increase. This empowers households to demand their entitlements from a local agent, who in turn is aware of the new information symmetry.

Second, if households feel their rights have not been respected, they are given the opportunity to complain in monthly meetings with NGOs in their own village. As these NGOs operate on a supra-village level, the discretionary power of the local agent and the risk of collusion between agents and village elites is lowered. In addition, through regular monitoring by an external actor (NGO) of the agent's behavior, any irregularities could be highlighted to the principal through the established grievance mechanism, thereby increasing the agent's expected costs of deviation.

As a third leg of the accountability campaign, these two mechanisms were preceded by the grouping of individual households to facilitate the supply of labor. Although arguably an important element, this component is discussed but not empirically evaluated in this paper, as it was implemented in all areas. Importantly, all accountability measures had the political support of the state government.

We argue that as a result of this increase in knowledge and monitoring in treatment areas, the expected cost of deviation for the local agent (the field assistant) should increase there. Under the assumption of effective supply-side constraints, where workers wanted to work more days and spread the days more evenly over the year before the intervention, we hypothesize the following changes in work patterns.

- *Hypothesis 1:* An increase in the expected cost of the agent's deviation increases the total days worked in treatment areas, within the entitled limit (100 days).

- *Hypothesis 2:* An increase in the expected cost of deviation increases the number of days worked in the agricultural season in treatment areas.

4 Data Description

In this paper, we use a panel of 1,225 households from the Young Lives Rounds 3–5 (2009–2016) in our main estimation.[9] These three rounds collected information on MGNREGA, including the total number of workdays obtained under MGNREGA for each individual in the household, as well as total days worked by lean and agricultural seasons, from 15 rural mandals in 7 districts of AP and Telangana, India. In our dataset, the lean season is defined as March to June, while the agricultural season spans the kharif (July–October) and rabi (October–February) cropping seasons.[10] This enables us to look at total days worked under MGNREGA and by season across all areas. For our main regressions, our sample consists of all rural households who did not move between Round 3, 4 and 5.

Table A1 (appendix) shows that by the end of 2009 (Round 3), 85 percent of households in our sample had job cards. This increased to 93 percent by the end of 2013 (Round 4) and to 95 percent by 2016 (Round 5). However, the number of households engaging in MGNREGA work decreased from 70 to 67 percent between 2009 (Round 3) and 2016 (Round 5). For those who did work, the total number of days worked in the prior 12 months increased from 41.6 to 46.3 days between 2009 and 2013, before decreasing to 40.6 in 2016.[11] Table A1 also shows that the total days worked in the agricultural season (July–February) decreased between the rounds, while the days worked during the lean season increased overall from 2009.

We merge the survey information with data obtained from government orders and monitoring reports on which mandals received the accountability intervention, and by which NGO.[12] A significant number of NGOs applied to be part of APNA, and 330

[9]The survey is representative of the geographical regions and poverty distribution within the two states (Galab et al. 2011), and has a low attrition rate (0%–2%) between rounds (Galab et al. 2014). Limiting the analysis to the younger cohort households (children were born in 2001–2002) decreased the likelihood of the survey switching to a new household when the child set up their own household, as was the case with the older cohort.

[10]While the 2006/2007 (Round 2) survey also collected MGNREGA data, MGNREGA had not been rolled out in all survey areas. In 2006, Anantapur, Kadapa, Mahbubnagar and Karimnagar were brought under MGNREGA; in 2007 Srikakulam was added; and West Godavari was included by 2008 (Berg et al. 2018).

[11]Round 3 was collected in late 2009–early 2010, Round 4 in late 2013–early 2014, and Round 5 in late 2015–early 2016.

[12]NGOs had to deliver the information training to all villages in a mandal from June 2011 to May 2012, while the monitoring and grievance component only started in 2013 and ran until 2016.

NGOs were accepted into the initiative (Government of Andhra Pradesh 2010a). A total of 703 out of the 1,098 mandals in Andhra Pradesh were included in this initiative (Government of Andhra Pradesh 2010a). However, not all included mandals implemented the intervention. Based on MIS reports from the government, we are able to determine which mandals received the treatment and by which NGOs.[13] Of the 15 rural mandals in the Young Lives survey, a total of 6 areas received the accountability intervention, while the other 9 did not.

5 Empirical Strategy

5.1 Identification - allocation of treatment

We use spatial variation in the implementation of the accountability campaign to identify the impact of the intervention on MGNREGA outcomes. The variation in treatment arises from over-commitment of NGOs that were allocated more areas than they were able to deliver, or from non-allocation of areas by the state government.

Generally, we do not know why an area did not get treatment. Following up with NGOs, one explanation NGOs received for non-allocated areas was that if a mandal bordered another state, it was not allocated. From mandals that were not treated, we can see that Gudibanda and Dhahur border Karnataka, and Mandasa borders Orissa. This accounts for 3 of the 9 non-treated mandals. Importantly, these mandals are not less accessible or remote than other mandals per se. For instance, Gudibanda is near Bengaluru in neighboring Karnataka. We find no significant differences between the allocated versus non-allocated areas across the points raised below.

Other than this, we are not aware of a conceptual connection between the failure to provide treatment to an area and MGNREGA outcomes expected in the absence of treatment. We test and find no factors that statistically explain treatment allocation before treatment. Given the considerations below, we conclude that our estimates reflect

[13]Main reports used to obtain this information were R12.11.4 Grievance Report, R7.2 APNO NGO–Wise Performance Report, and MIS reports of state and district-level meetings, which list participating NGOs and the mandals they covered in each state.

treatment effects and not diverging trends between areas.

In the following, we check our main concerns about potential endogeneity between MGNREGA outcomes and the probability of treatment. First, we test for parallel trends on household wealth, land, and animal ownership pre-intervention.[14] Second, we compare the implementation of MGNREGA across treatment and non-treatment areas before treatment. If there are large differences in MGNREGA outcomes between these areas, particularly on the intensive margin, areas might be structurally different in the implementation of MGNREGA and parallel trend assumptions are less likely to hold. Other areas of concern include whether treatment is determined by the reach of NGOs, where NGOs are more likely to treat areas if their presence in the area is high. This could also correlate with opportunity costs of taking up MGNREGA work if other options are available. In addition, access could determine treatment, and harder to reach areas are less likely to be treated. We address these points below by comparing pre-treatment summary statistics, as well as through our robustness checks.

We first address whether treatment and control areas varied by wealth, as well as land and livestock ownership, prior to the intervention, by checking for parallel trends in Table A4 (appendix). We would expect that if there are diverging differences in wealth, this may help explain any differences in days worked under MGNREGA. We find that parallel trends hold prior to the intervention for these outcomes.

Next, we address the performance of MGNREGA in treatment versus non-treatment areas. Table 1 tells us that treatment areas are not more likely to have job cards pre-intervention, compared to non-treatment areas. We do not find any significant differences in having done NREGA work in the prior 12 months for treated versus non-treated households, pre-intervention. Similarly, for the intensive margin, there is no significant difference in the annual lean or agricultural season days worked between treatment versus non-treatment areas.

Another concern one may have is that NGOs are more likely treat a mandal if their reach is high in the area. In the alliance setup, government criteria for APNA participation

[14] As MGNREGA was not implemented in all areas prior to Round 2 (2006/2007), we are unable to test for parallel trends for our sample of treated and non-treated households.

was that the NGO needed to have experience working on rights-based issues. In 2002, the Young Lives survey collected information on whether NGOs worked in a locality, and in 2006, information on NGO type. Table A2 (appendix) shows that the difference between treated and non-treated areas on whether an NGO is working in the area and by what type of NGO it is, including if it is a rights-based NGO, is not statistically significant.

Another issue could be the opportunity cost of taking up work under MGNREGA. One outside option for households is work in factories. We look at the difference between treatment versus non-treatment households in their access to a factory, as well as the difference in households with waged salary. Factories provide an option for longer term work for households. If a higher percentage of households work in a factory, there is potentially less demand for MGNREGA. What we find is that while non-treatment households are significantly more likely to have a factory within 5 km of their village, non-treatment households are not more likely to have waged non-agriculture employment nor salaried employment. In our estimations, we control for having a factory within 5 km of their village.

Finally, it could be an issue of access. Treatment areas may be physically different from non-treated areas, which also could impact the outcome variables. Accessibility is key because areas that were less accessible may be harder to treat but also harder to implement MGNREGA in. To see if this is determining the delivery of the treatment, we look at access to these areas. When we look at accessibility, the key takeaway from Table A3 (appendix) is that all these areas are accessible and we find no significant difference between areas with respect to motor accessibility. This is critical, as an NGO would have to visit all parts of the mandal over the intervention years.

5.2 Estimation Specification

The accountability intervention should affect the expected cost of deviating for field assistants who supply work to the beneficiaries. As motivated in the conceptual framework, a successful accountability intervention should affect the total days supplied to households who want to work. If one observes an increase in the number of days worked (Hypothesis

1), or an increase in the total days provided during the agricultural season (Hypothesis 2), this would signal an increase in the field assistant's expected cost of deviating. As we cannot measure these costs directly, we use MGNREGA outcomes to estimate their impact.

We estimate a linear difference-in-differences model as our main specification, while providing alternative specifications as robustness checks, including individual household or mandal fixed effects. For each of the dependent variables we use the empirical specification laid out in Equation 1.

$$y_{imt} = \beta_t Tr_{im} \times R_t + \delta Tr_{im} + R_t\rho + X_{imt}\gamma + \alpha_m + \epsilon_{imt} \tag{1}$$

where y_{imt} is our outcome of interest for household i, living in mandal m, at time t. Tr_{im} is a dummy variable that indicates whether household i is located in a treated mandal, i.e. the information campaign and grievance mechanism were implemented in mandal m from round 4 and round 5 respectively.

R_t is a dummy for survey round or time period t. α_m is a fixed effect for the region, that is an aggregation of mandals. X_{imt} contains further household and village control variables. This includes the education level, age and gender of household head, the total size of the household, the wealth index of the household, and whether the household experienced a natural disaster. These variables help control for any changes in the household's circumstances, separate from the impact of the accountability intervention, that affect MGNREGA take up of work. On the village level, we include whether there is a factory within 5 km of the village and whether the village is accessible to a motorized vehicle. This helps address whether there are any time varying factors at the village level that could affect the likelihood of work alternatives and therefore the take-up of work under MGNREGA. ϵ_{imt} is the individual error term, and standard errors are clustered at the mandal level.[15]

The β_t coefficients in Equation 1, are our main estimates of interest. For round 4, β_4

[15]Standard errors are clustered at the mandal level using wild cluster bootstrap with 500 replications as we only have a small number of mandals, 15.

tells us to what extent there is a differential change in the work obtained by households in treatment areas after the information campaign, while β_5 captures the impact of implementing the NGO monitoring and grievance mechanism, in addition to long-term effects of the information campaign.

Next, we also estimate the impact of the campaign on the extensive margin, that is the likelihood of the household having a job card and whether the intervention affects their likelihood of working in the prior 12 months. As the information was provided to already participating groups of MGNREGA households, we do not expect the intervention to affect our extensive measures of MGNREGA.

Finally, we run a number of robustness checks to first test our choice of estimation method, including running a household fixed effects regression, second to test the sensitivity of our results to our sample choice, and third to see whether our results vary based on the controls we use.

6 Results

6.1 Main results

Across all areas, we see in Table 2 an increase in the number of days worked during the lean season from 2009 to 2016 (survey round 3 to 5), while also seeing a decrease in total days worked during the agricultural season. In non-treatment areas, this shift between the lean and agricultural season results in no substantial overall change in total days worked in either round, that is we find insignificant point estimates of 2.3 and -4.1 days in 2013 and 2016 compared to 2009. By contrast, in the treatment area, by 2013 (round 4) total days worked in the prior year increased by 8.5 days and by 6.2 in 2016 compared to 2009. This difference of 10.3 days between treated and non-treated areas by 2016 is statistically significant at the 5% level.

By contrast, in 2013, a year after the information campaign was rolled out, we find no significant treatment effects. This might suggest that information by itself was not enough to enable households to access work. This result is in line with findings from Ravallion

et al. (2013), that increased awareness by itself is not enough to increase accountability and delivery of a program.

On the other hand, by 2016, we see the combined impact of the information campaign and of the NGO monitoring and grievance mechanism. We find that by 2016 (Round 5), households in treatment areas worked over 10 more days relative to the non-treated area. We interpret the statistically significant and large increase as a success of the accountability measures in lowering corruption in the implementation of MGNREGA at the village level. While not significant on its own, patterns in treatment areas seemed to differ in two ways compared to non-treatment areas. First, days worked in the lean season were higher than in non-treated areas. The point estimate for the impact during the lean season is relatively high at 7.2 days, rising to a significant difference of 11.6 days once we restrict our sample to households who hold a job card in all rounds (see robustness specifications below). Second, treatment areas saw a lower decrease in days worked during the agricultural season compared to non-treatment areas. This combination results in an overall significant increase in total days worked.

Last, we explore the extent to which the accountability intervention changes the likelihood to participate in MGNREGA on the extensive margin. From column (1) in Table A5 (appendix), we see that households in treatment areas experienced no significant increase in the likelihood of having a job card. As discussed previously, given how the accountability intervention targeted households already working, this result is not surprising. We expect the accountability intervention to influence MGNREGA outcomes on the intensive margin. Similarly, in column (2) of Table A5 we find that treatment does not affect the likelihood of a household working under MGNREGA in the prior 12 months.

6.2 Gender, Alternative Estimation Specifications, Potential Mechanisms

In our sample, women work almost 96,000 days within the MGNREGA program, while men work around 66,000 days. Men have a slightly lower ratio of lean to agricultural season days, that is 2.5 as opposed to 2.7 for women. Women are expected to demand

more MGNREGA work, as the wage differential relative to the regular labor market is larger. However, it is not clear from this whether easier access to guaranteed work affects the female-male ratio of days worked. If supply is low pre-intervention, the limited days might have been filled by women within households. An increased supply would then lead to a catch-up effect by men working more days. However, other factors could have led to female days being relatively more restricted despite higher demand.

The estimates in Table A7 (appendix) are not clear-cut. The estimated treatment effect is higher for men, but the difference between men and women is not statistically significant. However, since women work more days pre-intervention, we may conclude that men have a larger relative increase in days worked.

To test how the choice of functional form and estimation method may be driving our results, we first run a linear time and household fixed effects model and find that our positive impact on total days worked by 2016 (Round 5) holds for treated households, and the magnitude of our impact is similar (Table A8 (appendix)).

Next, we re-estimate our results with a difference-in-difference Poisson for total days worked, and during the lean and agricultural season. As this is testing the proportional effect, by 2016 (Round 5), we find that the total number of days is estimated to be about 30 ($e^{0.259} - 1$) percent higher in the last round, after the introduction of the information and grievance mechanism. Again, this result is in line with our main specification. The smaller increase in the previous round is not statistically significant, in line with the linear specification shown before.

We also run three robustness tests to gauge how sensitive our results are to changes in the sample in Table A9 (appendix). We first restrict our sample to households that had a job card in at least one round (Column 1-3), and next we restrict our sample to households that had job cards in all the three survey rounds (Column 4, 5, and 6). The main findings on total days worked hold for all specifications (Column 1 and 4). However, we observe a larger total effect on days worked when only including households that have a job card in all three rounds (Column 4). This is driven by a substantial and significant increase in days worked during the lean season of 11.6 days (Column 5).

As another robustness check, we vary the controls in our regressions. First, as three of the nine mandals that were not treated border another state, but no treatment mandal does, there may be something different about these areas that is affecting our results. To check this, we rerun our regressions, using a dummy variable to control for border mandals. In Column 1, Table A11 (appendix), we see that our main result on increased days worked under MGNREGA by 2016 (Round 5) holds. Next, instead of access to a motorized road, we control for time taken to get to the district capital on the fastest mode of transport. We find our main result holds (Column 3, Table A11). Finally, we remove our region controls, and our main result in Column 6 (Table A11) still holds.

For the final part of our analysis, we explore what mechanisms may be driving our results. We test whether the intervention increased the likelihood of households addressing non-MGNREGA community issues (Table A12 (appendix)). If the intervention created a general sense of collective action capacity as a result of the impact of the intervention, we might expect spillovers to other accountability issues. We examine whether treated households are more likely to (a) discuss a serious problem in the community with others and (b) take action on a serious problem in the community with others, finding no impact on either measure.

This null finding suggests the intervention worked through program-specific channels rather than generally enhancing treated households' capacity to address other community issues through collective action. Drawing on the social accountability literature, this result helps illuminate the underlying mechanisms. Kosec and Wantchekon (2020) argue that for information to be impactful, it must be deemed relevant, and individuals must have both the power and incentives to act on it. Fox (2015) similarly distinguishes between "tactical" approaches, which are primarily focused on information provision, and "strategic" approaches that coordinate mechanisms for enhancing citizen voice with institutional responsiveness. Evidence demonstrates that citizens face constraints in influencing public services through information alone (Banerjee et al., 2010b; Bettinger et al., 2012), unless complemented by mechanisms that facilitate institutional responsiveness (Devarajan et al., 2011; Ringold et al., 2012b).

The intervention studied in this paper went beyond information provision by combining training with the existing MGNREGA grievance redress mechanism, providing both knowledge and a concrete institutional tool that gave households the power to act. Notably, impacts emerge only after the grievance mechanism component is added, though we cannot definitively rule out that the information simply required more time to affect behavior. The evidence is most consistent with a supply-side mechanism: MGNREGA implementing agents recognize that treated households possess both knowledge of entitlements and access to a credible, program-specific grievance mechanism, creating accountability pressure specifically within MGNREGA. These findings align with the literature demonstrating that social accountability interventions are more likely to succeed when information is relevant and actionable, and when paired with institutional mechanisms that enable responsiveness (Kosec and Wantchekon, 2020; Fox, 2015), though even strategic interventions may not generate broader accountability gains beyond their specific programmatic context (Devarajan et al., 2011).

7 Conclusion

The findings from this paper contribute to the empirical literature by showing that an information campaign combined with a grievance mechanism can affect the delivery of a program when individuals are organized to use the information. We show that while there is no significant impact from the roll-out of an information campaign, once an NGO monitoring and grievance mechanism is introduced, total days worked over 12 months increase for treated households overall. Prior to the roll out of the intervention, all rural households in these areas accessing MGNREGA work were reorganized into working groups. It is likely that the structure of the working groups makes these households more mobilized to benefit from the intervention.

Our findings indicate that interventions which go beyond simply providing information, such as independent monitoring of public agents and access to grievance redress mechanisms, can enhance service delivery. This aligns with Ravallion et al. (2015), who argue that lack of information is not the main constraint to MGNREGA performance

and that complementary measures are essential. Specifically, monitoring public agents appears to be a key mechanism for improvement, consistent with Nagavarapu and Sekhri (2016). Additionally, enabling MGNREGA workers to access grievance redress through NGOs may be another channel through which one can enhance program outcomes by supporting collective action, as highlighted by both Ravallion et al. (2015) and Buntaine et al. (2018). The absence of spillover effects on broader collective action, where treated areas are no more likely to engage in community problem-solving, suggests that the primary mechanism of this impact may be through the supply side, with increased beneficiary awareness and access to a credible grievance system enhancing accountability and program delivery by the agent.

These findings have important implications for the implementation of MGNREGA in India as it provides evidence that the combination of informing workers and providing them with access to a monitoring and grievance mechanism affects the ability of households to access work when they want it. This also has implications for other anti-poverty programs in developing countries, where the provision of bottom-up accountability mechanisms could impact the effectiveness and reach of these programs. In addition, this has implications for the role civil society organizations (CSOs) can play in helping strengthen government accountability in program implementation. Further research on how combining anti-poverty programs with bottom-up accountability interventions would help provide greater insight into how to address corruption and increase beneficiary participation in such programs.

One limitation of our analysis is that we are unable to directly test the parallel trends assumption for the primary outcome variable due to data constraints. However, we test for parallel trends on other related variables and examine pre-intervention differences in the main outcome, finding no statistically significant differences between treatment and control groups prior to the intervention. Nonetheless, it is important to interpret our findings with this limitation in mind.

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Figure for inclusion in main text

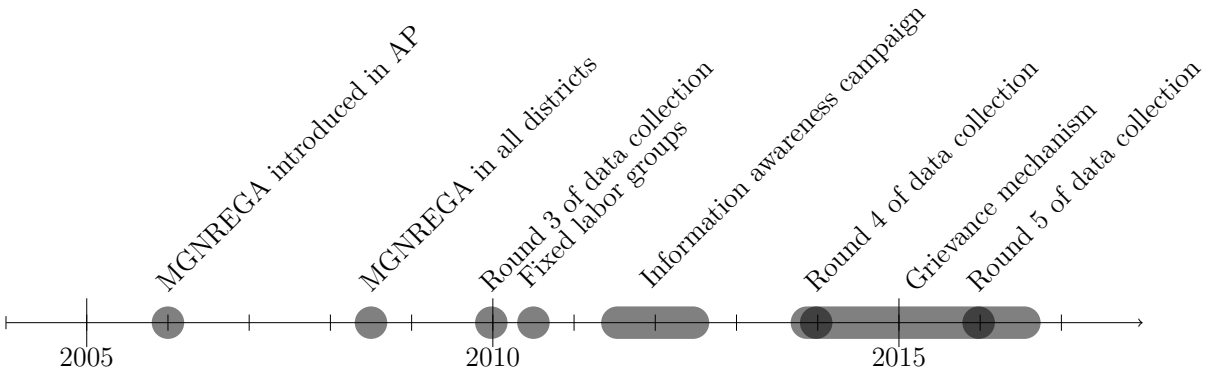

Figure 1: Timeline of MGNREGA, Intervention, and Surveys in AP

Tables for inclusion in main text

Table 1: Balance Table on MGNREGA, Non-treatment versus treatment, Round 3, 2010

	Full Sample Mean	Non-Treated Mean	Treated Mean	p-value
Did NREGA work	0.70	0.68	0.72	0.68
Got NREGA unemployment benefit	0.04	0.05	0.03	0.53
Total NREGA days worked	41.56	41.45	41.72	0.97
Total NREGA agri. season days	17.51	16.40	19.11	0.53
Total NREGA lean days	24.08	25.10	22.61	0.65
NREGA days by females	24.39	25.00	23.50	0.77
NREGA lean days by females	14.15	15.26	12.55	0.35
NREGA agri. season days by females	10.24	9.74	10.95	0.63
NREGA days by Males	17.19	16.50	18.19	0.70
NREGA lean days by Males	9.93	9.84	10.05	0.94
NREGA agri. season days by Males	7.26	6.66	8.13	0.51
Observations	1,225	724	501	

Standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications

Table 2: MGNREGA Outcomes

	(1)	(2)	(3)
	Ttl days Worked	Ttl days lean	Ttl days agri. season
Treated households	3.491	2.437	1.004
	(9.97)	(6.29)	(4.72)
Treated households × Round=4	6.227	0.690	5.595
	(11.28)	(10.11)	(4.86)
Treated households × Round=5	10.262**	7.195	3.123
	(4.77)	(6.02)	(5.08)
Round=4	2.271	11.811*	-9.596***
	(5.64)	(6.22)	(3.57)
Round=5	-4.100	5.649	-9.807***
	(4.41)	(5.45)	(3.04)
Observations	3,675	3,675	3,675
Regional Dummies	Yes	Yes	Yes
Household and Village Controls	Yes	Yes	Yes
Treatment4+Treatment5	16.49	7.884	8.718
P value	0.267	0.582	0.315
Mean Control R4	44.16	37.49	6.663
SD Control R4	47.94	44.12	17.88
Mean Control R5	36.35	30.28	6.066
SD Control R5	39.08	33.79	16.83

*** p<0.01, ** p<0.05, * p<0.1

Notes: Table shows OLS estimates based on Equation 1. Unit of observation is households in three rounds of the Young Lives Survey, collected in 2009, 2012-13 and 2015-16. Standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications.

Appendix: Tables

Table A1: Descriptive Statistics on MGNREGA, Round 3, 4 & 5, Young Lives Survey Data

	Full Sample	Round 3 2009-2010	Round 4 2013-2014	Round 5 2015-2016
	Mean	Mean	Mean	Mean
Did NREGA work	0.68	0.70	0.68	0.67
Got NREGA unemployment benefit	0.07	0.04	0.10	0.05
Total NREGA days worked	42.81	41.56	46.31	40.55
Total NREGA agri. season days	11.90	17.51	9.91	8.26
Total NREGA lean days	30.92	24.08	36.39	32.29
NREGA days by females	25.32	24.39	26.88	24.70
NREGA lean days by females	18.44	14.15	21.46	19.70
NREGA agri. season days by females	6.89	10.24	5.42	5.00
NREGA days by males	17.59	17.19	19.44	16.14
NREGA lean days by males	12.58	9.93	14.95	12.87
NREGA agri. season days by males	5.01	7.26	4.49	3.27
Observations	3,675	1,225	1,225	1,225

Table A2: NGO Figures, Non-treatment versus treatment, Round 3, 2010

	Full Sample Mean	Non-Treated Mean	Treated Mean	p-value
NGO in area, 2002	0.48	0.40	0.59	0.31
NGO in area, 2006	0.48	0.43	0.55	0.55
Rights based NGO, 2006	0.13	0.12	0.15	0.80
Child related NGO, 2006	0.17	0.11	0.25	0.41
Observations	1,225	724	501	

Standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications

Table A3: Household & Village Characteristics, Non-treatment versus treatment, Round 3, 2010

	Full Sample Mean	Non-Treated Mean	Treated Mean	p-value
Household characteristics				
Female household head	0.05	0.06	0.04	0.17
Household size	5.63	5.74	5.48	0.38
Household head years of education	5.96	5.33	6.87	0.09
Household head age	38.68	38.87	38.42	0.58
Wealth index of household	0.45	0.46	0.44	0.51
Household time (minutes) to district capital	145.51	161.65	121.99	0.22
Household was non-agri wage employment	0.23	0.24	0.21	0.55
Household was salaried employment	0.17	0.16	0.17	0.81
Village characteristics				
Experienced natural disaster prior yr	0.39	0.40	0.39	0.97
Village is motor accessible	0.94	0.91	0.97	0.21
Village has factory	0.25	0.41	0.02	0.00
Observations	1,225	724	501	

Standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications

Table A4: Parallel Trends for wealth, total land and animal ownership

	(1)	(2)
	Wealth index	Total Land
Treated households	-0.023	0.771*
	(0.08)	(0.44)
Treated households × Round=3	-0.022	-0.203
	(0.01)	(0.25)
Round = 3	0.076***	0.308***
	(0.01)	(0.10)
Observations	2,726	2,729
Additional Household and Village	Yes	Yes
Treatment+Interaction	-0.0445	0.568
P value	0.609	0.255
Mean Control	4734	2.335
SD Control	3656	3.973

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Table shows OLS estimates based on Equation 1. Unit of observation is households in three rounds of the Young Lives Survey, collected in 2009, 2012-13 and 2015-16. Standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications.

Table A5: Extensive Margin: Having a Job Card & Working in the last 12 Months

	(1)	(2)
	JCard (0/1)	NREGA Work (0/1)
Treated households	-0.036	0.061
	(0.04)	(0.07)
Treated households × Round = 4	-0.021	0.042
	(0.03)	(0.08)
Treated households × Round = 5	-0.029	0.002
	(0.03)	(0.07)
Round = 4	0.094***	-0.052
	(0.02)	(0.07)
Round = 5	0.116***	-0.038
	(0.03)	(0.07)
Observations	3,675	3,675
Regional Dummies	Yes	Yes
Household and Village Controls	Yes	Yes
Treatment4+Treatment5	-0.0498	0.0440
P value	0.423	0.767
Mean Control R4	0.936	0.646
SD Control R4	0.244	0.478
Mean Control R5	0.959	0.657
SD Control R5	0.199	0.475

*** p<0.01, ** p<0.05, * p<0.1

Notes: Table shows OLS estimates based on Equation 1. Unit of observation is households in three rounds of the Young Lives Survey, collected in 2009, 2012-13 and 2015-16. Standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications.

Table A6: Impact of Intervention on Wealth, Land and Animal Ownership

	(1)	(2)	(3)
	wealth_indx	animal	TotalLand
Treated households	-0.041	0.048	0.678
	(0.09)	(0.05)	(0.44)
Treated households × Round = 4	0.025	-0.011	-0.219
	(0.02)	(0.03)	(0.46)
Treated households × Round = 5	0.011	-0.030	0.120
	(0.02)	(0.04)	(0.56)
Round = 4	0.077***	0.057**	0.850**
	(0.01)	(0.02)	(0.35)
Round = 5	0.143***	0.025	0.745**
	(0.01)	(0.04)	(0.34)
Observations	3,673	3,675	3,675
Regional Dummies	Yes	Yes	Yes
Additional Household and Village	Yes	Yes	Yes
Treatment4+Treatment5	0.0357	-0.0411	-0.0994
P value	0.288	0.522	0.918
Mean Control R4	0.527	0.622	3.097
SD Control R4	0.137	0.485	3.763
Mean Control R5	0.586	0.580	3.024
SD Control R5	0.140	0.494	3.787

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Table shows OLS estimates based on Equation 1. Unit of observation is households in three rounds of the Young Lives Survey, collected in 2009, 2012-13 and 2015-16. Standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications.

Table A7: MGNREGA Outcomes by Gender

	(1)	(2)	(3)	(4)	(5)	(6)
	Female total days	Female days lean	Fem. days agri. season	Male total days	Male days lean	Male days agri. season
Treated households	2.456	1.492	0.965	0.915	0.901	0.014
	(5.27)	(3.46)	(2.53)	(5.40)	(3.27)	(2.58)
Treated households × Round = 4	2.749	0.571	2.178	3.597	0.161	3.436
	(6.36)	(5.41)	(2.94)	(5.18)	(4.79)	(2.22)
Treated households × Round = 5	4.000	2.610	1.391	5.847**	4.095	1.752
	(2.93)	(3.32)	(2.90)	(2.65)	(2.99)	(2.39)
Round = 4	1.334	7.040**	-5.706***	0.932	4.818*	-3.885**
	(3.01)	(3.37)	(2.21)	(2.82)	(2.90)	(1.55)
Round = 5	-0.507	5.068*	-5.575***	-3.064	1.161	-4.225***
	(2.60)	(3.03)	(1.69)	(2.18)	(2.66)	(1.44)
Constant	14.306	7.701	6.605	-3.263	-3.585	0.322
	(11.71)	(8.04)	(4.81)	(9.97)	(6.74)	(4.87)
Observations	3,675	3,675	3,675	3,675	3,675	3,675
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Household and Village Controls	Yes	Yes	Yes	Yes	Yes	Yes
Treatment4+Treatment5	6.750	3.180	3.569	9.443	4.256	5.188
P value	0.435	0.682	0.498	0.177	0.534	0.177
Mean Control R4	26.82	22.67	4.148	17.34	14.82	2.515
SD Control R4	31.64	27.91	13.28	25.20	23.01	7.897
Mean Control R5	23.84	19.78	4.064	12.99	10.99	2.003
SD Control R5	29	24.34	11.88	23.43	21.27	7.943

*** p<0.01, ** p<0.05, * p<0.1

Notes: Table shows OLS estimates based on Equation 1. Unit of observation is households in three rounds of the Young Lives Survey, collected in 2009, 2012-13 and 2015-16. Standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications.

Table A8: Robustness checks with Estimation Variations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Fixed Effects OLS			Poisson					
	Ttl days worked	Ttl days lean	Ttl days agri	Ttl days worked	Ttl days IRR	Ttl days lean	Lean days IRR	Ttl days agri	Agri days IRR
Treated households				0.137	1.147	0.061	1.063	0.183	1.201
				(0.22)	(0.256)	(0.26)	(0.274)	(0.23)	(0.280)
Treated households × Round=4	5.997	0.857	5.199	0.166	1.181	0.088	1.092	0.696	2.005
	(11.457)	(10.370)	(4.951)	(0.28)	(0.330)	(0.37)	(0.405)	(0.50)	(1.009)
Treated households × Round=5	10.682**	8.065	2.671	0.254*	1.289*	0.252	1.287	0.524	1.689
	(5.076)	(6.488)	(5.145)	(0.14)	(0.182)	(0.22)	(0.278)	(0.47)	(0.802)
Round = 4	3.333	12.224*	-8.934**	0.149	1.160	0.468**	1.596**	-0.766*	0.465*
	(6.305)	(6.437)	(3.740)	(0.14)	(0.158)	(0.19)	(0.297)	(0.42)	(0.197)
Round = 5	-3.087	5.980	-9.106***	0.076	1.079	0.368*	1.445*	-0.724**	0.485**
	(5.683)	(5.709)	(3.221)	(0.14)	(0.148)	(0.19)	(0.277)	(0.36)	(0.174)
Observations	3,673	3,673	3,673	3,673	3,673	3,673	3,673	3,673	3,673
R-squared	0.017	0.043	0.051						
Number of childid	1,225	1,225	1,225	1,225		1,225		1,225	
Regional Dummies	No	No	No	Yes		Yes		Yes	
Household and Village Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*** p<0.01, ** p<0.05, * p<0.1

Notes: Table shows OLS estimates with household fixed effects based on Equation 1 (Column 1-3) and quasi-Poisson estimates (Columns 4-9) . Unit of observation is households in three rounds of the Young Lives Survey, collected in 2009, 2012-13 and 2015-16. Standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications.

Table A9: Robustness checks with Sample Variations

	(1)	(2)	(3)	(4)	(5)	(6)
	JC one round min			JC all rounds		
	Ttl days worked	Ttl days lean	Ttl days agri.	Ttl days worked	Ttl days lean	Ttl days agri.
Treated households	6.510	2.931	3.515	5.140	1.664	3.405
	(9.36)	(6.08)	(4.39)	(9.86)	(6.27)	(4.83)
Treated households × Round = 4	7.818	1.690	6.186	10.735	3.920	6.881
	(12.46)	(10.87)	(5.42)	(12.66)	(11.10)	(6.12)
Treated households × Round = 5	11.217**	7.996	3.278	15.406***	11.620*	3.854
	(5.16)	(6.40)	(5.56)	(4.62)	(6.23)	(6.13)
Round = 4	5.291	14.033**	-8.801**	0.006	10.553*	-10.615**
	(6.06)	(6.45)	(3.98)	(5.25)	(6.22)	(4.54)
Round = 5	1.483	9.378	-7.960**	-5.397	4.587	-10.060**
	(5.55)	(5.78)	(3.67)	(4.84)	(5.34)	(4.14)
Observations	3,490	3,490	3,490	3,130	3,130	3,130
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Household and Village Controls	Yes	Yes	Yes	Yes	Yes	Yes
Treatment4+Treatment5	19.04	9.687	9.465	26.14	15.54	10.73
P value	0.252	0.530	0.325	0.109	0.311	0.325
Mean Control R4	46.06	39.11	6.951	46.79	39.29	7.505
SD Control R4	48.06	44.35	18.21	48.69	44.93	18.99
Mean Control R5	37.92	31.59	6.329	37.46	30.77	6.685
SD Control R5	39.16	33.91	17.14	37.02	31.22	17.56

*** p<0.01, ** p<0.05, * p<0.1

Notes: Table shows OLS estimates based on Equation 1. Sample restrictions vary between households who hold a job card in at least one round (Columns 1-3) and households who hold a job card in all rounds (Columns 4-6). Unit of observation is households in three rounds of the Young Lives Survey, collected in 2009, 2012-13 and 2015-16. Standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications.

Table A10: Robustness checks with Variation in Control Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Border Mandal			Time to Dist Capital			W/o Region Controls		
	Ttl days worked	Ttl days lean	Ttl days agri	Ttl days worked	Ttl days lean	Ttl days agri	Ttl days worked	Ttl days lean	Ttl days agri
Treated households	6.673	5.635	1.037	-2.022	-2.858	0.791	5.597	2.477	3.059
	(10.32)	(7.11)	(5.20)	(9.55)	(6.06)	(4.97)	(8.88)	(5.77)	(4.16)
Treated households × Round=4	6.314	0.777	5.596	7.381	2.083	5.352	7.620	1.577	6.098
	(11.24)	(10.00)	(4.88)	(12.28)	(10.71)	(5.28)	(11.85)	(10.30)	(5.18)
Treated households × Round=5	10.274**	7.206	3.123	12.537**	10.027	2.563	10.711**	7.470	3.298
	(4.57)	(5.91)	(5.04)	(5.53)	(6.34)	(5.53)	(5.02)	(5.92)	(5.26)
Round = 4	2.219	11.759**	-9.596***	6.832	14.231**	-7.456*	5.865	13.996**	-8.188**
	(5.38)	(5.97)	(3.52)	(6.12)	(6.32)	(3.80)	(5.91)	(6.18)	(3.83)
Round = 5	-3.995	5.755	-9.806***	2.473	8.817	-6.408*	2.951	10.025*	-7.137**
	(4.25)	(5.27)	(2.97)	(6.13)	(6.02)	(3.64)	(5.60)	(5.62)	(3.58)
Observations	3,675	3,675	3,675	3,610	3,610	3,610	3,673	3,673	3,673
Border Dummy	Yes	Yes	Yes	No	No	No	No	No	No
Regional Dummies	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Household and Village Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment4+Treatment5	16.59	7.983	8.719	19.92	12.11	7.914	18.33	9.047	9.395
P value	0.257	0.570	0.313	0.228	0.422	0.408	0.249	0.533	0.307
Mean Control R4	44.16	37.49	6.663	44.08	37.35	6.728	44.16	37.49	6.663
SD Control R4	47.94	44.12	17.88	47.69	43.79	17.96	47.94	44.12	17.88
Mean Control R5	36.35	30.28	6.066	36.35	30.28	6.066	36.35	30.28	6.066
SD Control R5	39.08	33.79	16.83	39.08	33.79	16.83	39.08	33.79	16.83

*** p<0.01, ** p<0.05, * p<0.1

Notes: Table shows OLS estimates based on Equation 1. Empirical specifications differ by inclusion of control variables, that is border mandal dummy in columns 1-3, time to district capital (4-6) and the removal of the main specification regional controls (7-10). Unit of observation is households in three rounds of the Young Lives Survey, collected in 2009, 2012-13 and 2015-16. Standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications.

Table A11: Robustness checks with Mandal Fixed Effects

	(1) Fixed Effects Ttl days worked	(2) Fixed Effects Ttl days lean	(3) Fixed Effects Ttl days agri
Treated households × Round=4	7.154	1.230	5.983
	(12.038)	(10.450)	(5.129)
Treated households × Round=5	11.197**	8.009	3.245
	(5.152)	(5.833)	(5.271)
Round=4	6.673	14.893**	-8.274**
	(6.010)	(6.108)	(3.879)
Round=5	3.012	10.345*	-7.386**
	(5.457)	(5.426)	(3.620)
Observations	3,673	3,673	3,673
R-squared	0.042	0.055	0.039
Number of Mandals	15	15	15
Regional Dummies	No	No	No
Household and Village Controls	Yes	Yes	Yes

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Table shows OLS estimates based on Equation 1. Empirical specifications differ by inclusion of mandal fixed effects, standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications.

Table A12: Intervention Impact on Collective Action - Discussing and Acting on a Serious Community Problem

	(1)	(2)
	Talk Problem	Act Problem
Treated households	0.134**	0.119***
	(0.06)	(0.04)
Treated households × Round=4	-0.144	-0.092
	(0.09)	(0.10)
Treated households × Round=5	0.067	-0.040
	(0.08)	(0.07)
Round = 4	0.022	0.050
	(0.07)	(0.07)
Round = 5	0.063	0.167***
	(0.05)	(0.03)
Observations	3,675	3,674
Regional Dummies	Yes	Yes
Additional Household and Village	Yes	Yes
Treatment4+Treatment5	-0.0768	-0.132
P value	0.544	0.287
Mean Control R4	0.133	0.251
SD Control R4	0.339	0.434
Mean Control R5	0.180	0.373
SD Control R5	0.384	0.484

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Standard errors in parentheses, clustered at mandal level using wild cluster bootstrap with 500 replications.