

# Pushing Welfare: Encouraging Awareness and Uptake of Social Benefits in South India\*

Erlend Berg (University of Bristol)<sup>†</sup>

D Rajasekhar (Institute for Social and Economic Change)

R Manjula (Institute for Social and Economic Change)

26 October 2018

## Abstract

Governments in many developing countries offer social welfare schemes that few take up or even know of. We undertake a randomised-controlled evaluation of agents trained in the facilitation of 16 government welfare schemes in south India. The agents were incentivised to seek out eligible households, provide them with information about the schemes and help them apply. We find that the agents increase scheme awareness, but not submitted applications or scheme obtention. Among Self-Help Group members, a positively selected sub-population, agents have no effect. Scheme-by-scheme analysis suggests that the programme may have had an impact on awareness, applications and enrolment for two schemes that were subject to special focus under the programme. We conclude that unmet demand for the existing range of social services is not as large as it may appear. However, despite the limited impact on applications, the intervention led to a significant increase in satisfaction with government services.

**JEL Codes:** I38, O17, D04

**Key words:** Social security, welfare schemes, take-up, awareness, single-window service centres, one-stop centres, informal sector, randomised-controlled trial, India

---

\*We are grateful to Mr S V Ranganath, IAS, former Chief Secretary, Government of Karnataka (GoK); to former Labour Commissioners, GoK, Mr K S Manjunath, IAS; Mr Ravi Shankar, IAS; Mr Niranjana, IAS; Mr M B Dyaberi, IAS, former Secretary, Department of Labour, GoK; Mr Hans-Christoph Ammon, Team Leader, German International Cooperation (GIZ); and Ms Namerta, Senior Technical Expert, GIZ, for extending their full cooperation and support. We would also like to thank our team of enumerators, led by Bheema, Veena, Murali and Sangeetha, for their assistance, and Zahra Siddique and Christine Valente for helpful discussions. We gratefully acknowledge funding from GIZ and from Improving Institutions for Pro-Poor Growth (iiG), a research programme consortium funded by DFID.

<sup>†</sup>Correspondence: Erlend Berg, Department of Economics, University of Bristol, Priory Road Complex, Bristol BS8 1TU, United Kingdom. Email: erlend.berg@bristol.ac.uk

# 1 Introduction

The problem of low take-up of welfare benefits is well documented in rich countries (Hernanz, Malherbet and Pellizzari, 2004). However, despite rapid growth in the number of social protection programmes in developing countries since the mid-1990s (Barrientos, 2013), we know very little about welfare take-up in these regions. This paper evaluates the impact of single-window service centres intended to increase awareness and take-up welfare schemes provided by a range of government departments. The programme recruited, trained, incentivised and monitored 250 full-time facilitators tasked with seeking out households eligible for welfare schemes, providing them with information about the schemes and helping them apply.

We analyse household survey data from a randomised-controlled trial involving 150 gram panchayats (village council areas) in the southern Indian state of Karnataka. Due to imperfect compliance with the experimental protocol, we use treatment-arm allocation as an instrumental variable for actually receiving the programme. We find that while the programme increased awareness of social welfare schemes in the eligible population, it did not significantly increase the number of schemes applied for, obtained or utilised. For households linked to Self-Help Groups, a network of savings-and-loans clubs with positively selected membership, we find no aggregate effects. Possibly, Self-Help Group members are more likely to already be aware of relevant schemes, making it harder to increase knowledge further. The positive effects seem to be driven by a small number of individual schemes that were promoted over and above the general programme in targeted enrolment ‘drives’.

While the impact is modest, people in treated areas are significantly more satisfied with government provision of social protection. This could be because of a greater awareness of schemes they might be able to draw on in times of need, because they are satisfied that the government has seemingly intensified its effort in promoting such schemes, or, perhaps, because at the time of the follow-up survey they had started working on one or more scheme applications but not yet submitted them.

We conclude that unmet demand for the existing range of social services is not as large as it may appear, and that the potential for ‘pushing welfare’ may, in the context, be largely confined to schemes that are relatively new and not already ‘owned’ by local stakeholders.

While the literature on the targeting of anti-poverty programmes is large, only a small number of papers specifically study the determinants of the take-up of welfare schemes, conditional on existing eligibility rules, in developing countries. O’Brien and Pellerano (2015) compare the characteristics of participants and non-participants in a cash-transfer scheme in Kazakhstan. Xu, Guan and Yao (2011) do the same for social insurance schemes among Chinese rural-to-urban migrants. Bertrand, Mullainathan and

Shafir (2004) emphasise the potentially deterring effect of seemingly small ‘hassle’ costs as well as the possible importance of procrastination in depressing applications for welfare schemes. Berg et al. (2018) find that social distance acts as a barrier to the flow of information about an Indian social health insurance programme, but that these barriers can be overcome by the introduction of a small incentive. They also find that improved knowledge about the programme among eligible households has a causal effect on take-up.

Two studies of programmes in Latin America are perhaps closest in spirit to our paper. Carneiro, Galasso and Ginja (2015) look at a Chilean programme of frequent home visits and guaranteed access to social services. They employ a regression discontinuity design to identify a significant effect of the programme on the take-up of other social services. Abramovsky et al. (2016) evaluate a Colombian pilot programme which aimed, in part, to stimulate demand for existing social programmes. However, the intervention they study is bundled, as it also aimed to help participating households escape poverty by identifying areas of vulnerability and developing household-specific action plans. There was also a simultaneous effort to expand and improve the supply of social programmes, to which participating households were given preferential access. In contrast, the intervention studied here was solely aimed at increasing awareness of and access to existing social benefits, without any concurrent changes on the supply side. This paper is, therefore, possibly the first to isolate the effects of a programme focused exclusively on increasing the knowledge and take-up of a broad range of existing welfare schemes using a randomised-controlled trial.

The literature offers three standard reasons for the low take-up of welfare schemes: a lack of information, process costs and stigma (e.g. Currie 2006). The former two are likely to be important in poor countries. People tend to be less educated and less literate than in developed countries, making the transmission of information about schemes more difficult, and the cost of completing an application form higher. Inefficient bureaucracies and greater social distance between officialdom and ‘common people’ may compound process costs further. In contrast, stigma is probably less of a concern in contexts where ‘everyone’ is poor. This paper contributes to the literature by experimentally identifying the importance of demand-side barriers to the take-up of existing welfare schemes, and finding it to be modest. However, Kleven and Kopczuk (2011) argue that there is a trade-off between reducing application costs and improving targeting, so that full uptake is not necessarily optimal from the social planner’s point of view.

Several features of the local context may also contribute to low take-up, and informed the design of the programme studied here: First, a large majority of working-age Indians are in the informal labour market. This market is in constant flux. In the rural areas, small farmers leave and re-join the casual labour market, and rural workers commute to work in the growing urban informal sector. This makes it difficult to identify potential beneficiaries and target social security benefits.

Second, social security in India is not provided via a coherent, universal system. Rather, it takes the form of a large number of individual schemes that have been introduced piecemeal, sometimes with one particular contingency or target group in mind. Schemes have been formulated to meet both promotional and protective social security needs such as housing, food and nutrition, education, and cover for illness, old age, disability and death. Typically there is also a legal and operational division of authority over each scheme between the national, state and local government tiers. In some cases, delivery is entrusted to private or quasi-governmental agencies. In Karnataka, more than 50 social security schemes are offered by ten government departments.

Third, officialdom tends to be physically quite far from beneficiaries. In Karnataka, the state and national governments design schemes and provide funding, the district administration screens applications and sub-district-level (taluk) officials deliver benefits. Given that the average distance between a village and the taluk headquarters is 15 km, opportunity costs associated with accessing benefits (obtaining application forms and necessary documents, submitting the application and following it up) are high, leading in some cases to a dependence on exploitative middlemen.

The programme under study attempted to overcome these barriers by seeking out and informing eligible households of available schemes, and helping them to obtain the necessary documentation and apply. The centres formed under the evaluated programme were conceived of as ‘single-window service centres’ (Turner, 2012; Bent, Kernaghan and Marson, 1999) in response to the fragmentation of existing services. They were based at the gram panchayat (village council) level in order to reduce transaction costs and improve targeting.

## 2 The Programme

In collaboration with the Government of Karnataka’s Department of Labour, the German Agency for International Cooperation (GIZ) set up a number of Worker Facilitation Centres across the state as a pilot programme. The aims were to build awareness among informal-sector workers<sup>1</sup> of welfare schemes and entitlements, identify eligible households and workers, help them obtain relevant documents, and submit and follow up on applications for schemes. The programme was formally launched in 2009, and the first centre opened in October 2010.<sup>2</sup>

The programme was implemented in six districts of Karnataka: Bangalore Rural, Bangalore Urban, Bellary, Dakshina Kannada, Gulbarga and Mysore. The districts were

---

<sup>1</sup>Of all employed Indians, 88.2% are in the informal sector (International Labour Organization, 2018).

<sup>2</sup>Three ‘pre-pilot’ centres were opened from January 2010. These centres were subject to intense administrative attention and numerous changes in procedures and are, therefore, excluded from the evaluation presented here.

chosen to represent Karnataka’s diversity in climate and geography. Two participating taluks (sub-district administrative units) were selected from each district,<sup>3</sup> except in Bangalore Rural and Bangalore Urban, each of which had only one participating taluk. The number of participating taluks was, therefore, ten. During the project, Worker Facilitation Centres were opened in 250 gram panchayats across these ten taluks.

## 2.1 Centres and facilitators

Centres were set up at the level of the gram panchayat (GP), the village councils that constitute the lowest level of democracy and administration in rural India. Each centre was co-located with the local GP administration and equipped with an internet-connected computer, a printer and basic office furniture.

Each centre was staffed by a single, full-time Community Facilitator appointed by the district government. The facilitators were typically young university graduates. Local candidates were preferred as this was expected to contribute to their productivity and acceptance within the community. The facilitators were provided with and encouraged to wear a uniform specific to the programme, and to use the programme’s logo actively for improving visibility of the programme.

Facilitators recruited at the outset were provided with several modules of training: First, a four-day induction introduced the project, the concepts of social security and informal-sector workers, how to interact with the community and collect basic information, roles and responsibilities and details on welfare schemes. This module also included detailed training about each of the targeted schemes. Second, a three-day module on effective communication and active listening. Third, a four-day module on self-assessment and conflict management. Finally, a further one-day orientation module provided further information on a few key welfare schemes.

Training was provided by an NGO, the staff of which had themselves received ‘teacher training’ from GIZ. The NGO trained all facilitators in a district, together, as soon as recruitment in that district was complete. However, there was no structured training programme for those recruited to replace facilitators leaving the project mid-way. Instead, District Coordinators were asked to provide some orientation for newly recruited facilitators in their district.

Facilitators were asked to cover all areas of their GP. Rather than waiting for potential beneficiaries to come to them, facilitators were expected to go from door to door. Using a structured questionnaire, they were to collect data on the background and social security needs of each household. Facilitators provided information on relevant social

---

<sup>3</sup>In each participating district, the district headquarters taluk was selected because a location near an urban centre would enable the project to work with diverse types of informal-sector workers who typically commute into towns. The second taluk from each district was chosen from those adjacent to the district headquarters taluk, with the idea of covering primarily informal-sector agricultural workers.

security schemes and assisted the households in obtaining necessary documentation and submitting applications. On average, each facilitator was expected to cover up to 900 households over the project period of about three years.

A system of regular quality checks of the facilitators' work was introduced. The quality checks had three components; a) physical spot-checks of randomly selected questionnaires completed by all the facilitators; b) back-checks of the information in randomly selected questionnaires by visiting the concerned households; and c) checking the quality of the facilitation by shadowing the facilitator. The quality on each of these was rated, and the feedback was provided in one-to-one meetings with each facilitator, followed by feedback to all facilitators covering common points.

## 2.2 Facilitator remuneration

All facilitators had the same remuneration package. Their monthly pay consisted of a fixed base salary plus a performance-based component. Initially, performance pay was linked mainly to interviewing households and conducting information meetings as part of a 'scoping exercise'.<sup>4</sup>

In April 2011, the scoping exercise was largely complete and the facilitator reward system was simplified and linked exclusively to the number of welfare applications facilitated. From then on, facilitators received a fixed monthly payment of 6000 rupees (composed of 3800 rupees of 'basic pay' plus a travel allowance of 2200 rupees, which everyone received). In addition, the first 30 applications facilitated each month were associated with a bonus. The maximum total bonus was 2000 rupees per month, so that each facilitation was worth about 67 rupees. There was no further reward for facilitating more than 30 applications per month.

In order to receive the bonus, facilitators were required to provide evidence for submitted applications. For most schemes, this took the form of an acknowledgement from the government office accepting the application. While the facilitators were rewarded for applications *submitted* rather than *approved*, the acceptance rate was expected to be high because the government offices in question tend not to accept applications that are incomplete or from ineligible applicants. Effectively, there was a form of pre-screening of applications at the time of submission.

In two cases, the best-performing facilitators were promoted to taluk coordinators (wherever the post of taluk coordinators had become vacant). There were no other non-financial incentives such as diplomas, 'employee of the month' schemes or similar, and no

---

<sup>4</sup>Performance pay was initially based on five criteria: (i) holding information meetings; (ii) conducting Executive Committee meetings; (iii) data collection from the households; (iv) entry and analysis of the data collected; and (v) facilitation of the benefits to the eligible households. In this period, the fixed component of the salary was 4100 rupees per month (including a 500-rupee travel allowance, which everyone received), and the maximum level of incentive pay was 2400 rupees per month.

real prospects of progression to other jobs within the government sector.

## 2.3 Targeted schemes

An initial scoping exercise identified about 50 government social security schemes potentially suitable for informal-sector workers in Karnataka (Government of Karnataka and GTZ, 2010). From these, a subset of schemes was chosen to be targeted by the programme. The targeted schemes were selected on the basis of there being a substantial eligible population and the scheme being financially or otherwise substantial enough to make a difference to beneficiaries. Some schemes were excluded because of unclear application procedures, a politicised selection process or because they were only provided at irregular intervals.

The list of rewarded schemes changed as the programme progressed. Three initially targeted schemes were dropped early on due to either a perceived low demand or because another actor from the respective department objected to outsiders facilitating ‘their’ scheme. A further three schemes were continuously rewarded in principle, but in practice were not facilitated for an extended period because the government stopped accepting applications (for reasons unrelated to the programme studied here). Two schemes that were not initially rewarded were later included, and one scheme was discontinued by the government during the course of the programme. Finally, applications for two schemes were continuously rewarded but the reward rates were reduced during the programme, as they were perceived to be easier to facilitate. An overview of the 16 schemes ever targeted by the programme is provided in Table 1.

The value of benefits provided by the targeted schemes varied. The social pension schemes (for the elderly, the disabled and widows) provide an ongoing monthly pension of 500 rupees. This is likely to be a significant proportion of income for pensioners living on their own, whereas for households where one or more members are engaged in paid work, the proportion of the pension to overall income is likely to be more modest. The educational scholarships vary in size, but generally represent a modest proportion of typical educational expenditure. For schemes with an element of insurance, the contribution to income depends on the extent to which the scheme is utilised (claims made).

Towards the end of the project, facilitation ‘drives’ were organised to promote two particular schemes: the construction workers’ welfare fund and life insurance for commercial vehicle drivers. In this phase, facilitators were offered an additional reward of 150 rupees per submitted application for these two schemes. This was, however, a group reward: groups of about five facilitators came together to organise the drives, and shared the rewards.

The pilot programme ended on 31 March 2014, and applications facilitated after this date were not rewarded.

## 3 Experimental Design and Data

### 3.1 Experimental Design

We collected data from nine of the ten project taluks. (The tenth taluk, Bangalore Rural, was excluded since it was not certain at the research design stage that it would be included in the intervention.) From the 341 GPs in these taluks, 150 were randomly selected to take part in the evaluation.<sup>5</sup> The draw was stratified so that the nine taluks were approximately proportionately represented in the sample of GPs.

Of the 150 selected GPs, 100 were randomly assigned to the treatment group, and the project administration agreed to open centres in all of these. The remaining 50 GPs were assigned to the control group, and the project administration agreed not to open centres in these during the evaluation. The draw was also stratified by taluk, so that each taluk was approximately proportionally represented in both treatment and control groups. The project management were free to choose the locations of the remaining 150 centres (they opened 250 in all over the course of the programme), but they were asked to open centres in the treatment GPs first. The identity of treatment and control GPs was not revealed to the survey enumerators, who were employed directly by the authors.

However, compliance with the experimental protocol was imperfect. By the time of the follow-up survey, centres had been opened in only 71 out of the 100 GPs in the treatment arm. Centres had also been opened in 4 out of 50 control GPs. Hence, simple comparison across the treatment arms would lead to potentially biased estimates of programme effects if non-compliance was non-random. This could happen if, for example, there was some tendency that the 71 treatment GPs in which centres were actually opened were ‘easier’ or more amenable than the 29 in which they were not. Instead, by using assigned treatment arm as an instrumental variable for actual treatment, we estimate local average treatment effects of the programme. We also use the original assignments to compute intention-to-treat (reduced-form) estimates.

Table 2 presents village summary statistics by intended treatment group. The arms are fairly well balanced. In spite of imperfect compliance, there is a large and highly significant difference in actual treatment between the two groups, suggesting that experimental assignment is a strong instrument for treatment.

Table 3 presents summary statistics for the facilitators, including turnover. At the time of the main survey, described below, 38% of facilitators were female, and in 31% of centres the original facilitator had left. The average centre had been open for 19 months when the main survey was fielded.

---

<sup>5</sup>The three GPs with ‘pre-pilot’ centres were excluded from the draw.



## 3.2 The Household Surveys

While treatment was at the level of the GP, not all villages in the participating GPs were included in the household surveys. Two villages were drawn at random (simple random sampling) from each sample GP. Some GPs consist of only one village, in which case that village was selected. The final village sample consisted of 254 villages drawn from the 150 GPs.

The initial focus of the programme was on households with at least one informal-sector worker and also at least one member of a Self-Help Group (SHG). SHGs are savings and loans clubs that also benefit from subsidised bank loans. The SHGs studied here are government-sponsored, but SHGs linked to NGOs also exist. The decision by programme implementers (GiZ and Department of Labour) to focus the facilitators' efforts on SHG member households was on the basis that these meet regularly and are, therefore, easy to address as groups. They are also accustomed to receiving various kinds of information from visitors to their meetings.

However, soon after completion of the baseline survey, it was decided that the facilitators would target all informal-sector worker households in their GP, not only those affiliated with SHGs. As a response, it was decided that a fresh sample, representative of all informal-sector worker households, would be interviewed for the follow-up survey. In order to fund the representative survey, only a subset of the SHG member households visited at baseline were re-surveyed at follow-up. We therefore have two household samples, as described below.

### 3.2.1 The Representative Sample

The main focus of the analysis will be on data from the cross-sectional survey designed to be representative of informal-sector worker households, the target population of the programme. The survey was conducted in June–September 2013, that is, some 32–35 months after the opening of the first centres. We do not have baseline ('before') data for this sample.

The representative sample was constructed as follows. A list of all households in the selected villages was obtained from the GP office. The lists did not contain information on whether each household had an informal-sector worker among its members. Therefore, households were visited in random order and, if contactable, asked whether any of its members were informal-sector workers (after explaining the meaning of the term). If so, the household would be included in the sample and interviewed.<sup>6</sup> This process continued until the target of ten interviewed households per village was reached. In all, 2460 households were interviewed as part of the representative sample.

Table 4 presents household summary statistics by intended treatment group. There

---

<sup>6</sup>Nearly all eligible households agreed to be interviewed.

is no significant difference in any of the household characteristics, but our main result is foreshadowed by the fact that treated households are, on average, aware of 0.17 more schemes than those not treated at follow-up.

### 3.2.2 The Self-Help Group Sample

Survey data were also collected from a sample of Self-Help Group (SHG) member households. The SHG member households were drawn from the same villages as the representative sample. The baseline interviews took place between August and November 2010, before the project’s ground operations started. From each sample village, one government-sponsored SHG was drawn at random, and from each selected SHG half of the members were randomly selected for baseline interviews. In all, 2062 SHG-member households with informal-sector workers were interviewed from 300 SHGs in 263 villages across 150 GPs.

The follow-up survey was conducted at the same time as the representative survey discussed above, in the period June–September 2013. Due to the shift in focus from SHG member households to all informal-sector worker households, only a subset of the baseline SHG sample was revisited for the follow-up survey.<sup>7</sup> In all, 1198 households from the SHG-member sample were revisited for the follow-up survey.<sup>8</sup>

SHG members tend to be positively selected and therefore not necessarily representative of the general population. However, the SHG movement is large and its membership forms a population worthy of study in its own right. Moreover, for this population, baseline as well as follow-up data were collected. This allows the inclusion of household fixed effects to control for unobserved, time-invariant heterogeneity across households.

Table 5 presents baseline summary statistics for the SHG sample. The variables appear largely balanced across the treatment arms. Comparing Tables 4 and 5, there appears to be only weak positive selection into SHG membership: heads of SHG households are somewhat less likely to be agricultural labourers.

### 3.2.3 Outcome Variables

The main outcome variables are simple counts of the number of schemes a household is aware of, the number of schemes the household has applied for but not (yet) obtained, and the number of schemes it has obtained. For a subset of the schemes (primarily insurance schemes) households were also asked whether a claim had been made against the scheme.

---

<sup>7</sup>For sample SHGs with six or fewer members, all member households were included. For sample SHGs with more than six members, half the members were randomly selected to take part in the end survey.

<sup>8</sup>There was also a mid-term survey of 466 households, conducted in the period May–June 2012, in a subset of 30 treatment GPs and 30 control GPs. Analysis of the mid-term data, not presented here, yields qualitatively similar results.

The questionnaire had questions on awareness, application and obtention for each of the 16 schemes targeted by the programme at any point (Table 1). However, a few if any households are eligible for all 16 schemes. Also, for some schemes, establishing eligibility is straightforward, while for others it is more complex. While the survey did not establish precise eligibility for every scheme, the surveyors were asked to skip questions about schemes for which the households were obviously not eligible. For example, questions about awareness and uptake of schemes for children were not asked when interviewing households without children, and questions related to disability schemes were not asked of households in which no member was disabled. Since most households were not asked about all of the 16 schemes, the theoretical maximum for each outcome variable varies between households but is never greater than 16.

The questionnaires also tracked awareness of and access to 13 schemes that were never targeted by the programme.

## 4 Results

### 4.1 Impact on Awareness, Applications, Obtention and Claims

#### 4.1.1 Difference Estimates: The Representative Sample

The main research question is whether the programme increased awareness, applications, obtention or claims for the 16 targeted social schemes. Table 6 presents the main results for awareness. The dependent variable, except in column 3, is the count of schemes that the household is aware of. Here and elsewhere, standard errors are robust and clustered at the GP level, and household- and village-level controls are included in addition to taluk (regional) and interviewer fixed effects.<sup>9</sup> Column 1 presents the OLS estimates. The result suggests that households in villages with a centre are aware of 0.08 more schemes on average. However, this is a potentially biased estimate of the treatment effect due to imperfect compliance with the experimental protocol. Column 2 presents the linear intention-to-treat (reduced form) results and suggests that households in the intended treatment group were aware of 0.17 more schemes than households in the intended control group. Column 3 presents a regression of actual on intended treatment status, and confirms that the first stage of the two-stage least-squares regression is strong.

Column 4, our preferred specification, presents the two-stage least-squares instrumental variables results. The local-average-treatment-effect (LATE) interpretation is that households living in villages in which centres were opened as a result of random

---

<sup>9</sup>The household-level controls are household size, caste group (whether belonging to a scheduled caste/tribe or not), the age of the household head and binaries for whether the household head is married, female or illiterate. The regressions also control for village size (number of households) and whether the village is a GP headquarters.

assignment are aware of 0.28 more schemes on average. Given that the average GP in the sample has 1140 households with informal-sector workers, the effect of the average facilitator is equivalent to making 319 households aware of one extra scheme each (or 160 households aware of two extra schemes each, etcetera).

It is interesting that the IV estimate is so much larger than the OLS estimate. This suggests that non-compliance was not strategic: if anything, centres were opened in more challenging, rather than easier, villages.

As the outcome is a non-negative count variable, columns 5 and 6 present Poisson regressions. The outcome variables are over-dispersed and therefore unlikely to be Poisson-distributed, but Poisson regression is robust to misspecification as long as the logarithm of the expectation is linear.<sup>10</sup> There is however, still the issue of endogenous treatment. Column 5 presents a Poisson regression of the awareness variable on the intended treatment group, corresponding to the linear specification in column 2. The estimated coefficient is 0.068. Since the mean of the outcome variable in the control group is 2.32, the coefficient suggests that intended treatment increased awareness by 0.16 schemes — very close to the coefficient from the equivalent linear regression.

Column 6 presents the result of a Poisson regression for endogenous treatment variable (Terza, 1998). The endogenous treatment variable is modelled as a switching/threshold variable with a normally-distributed latent variable, akin to the probit estimator.<sup>11</sup> The estimated coefficient is 0.066, again corresponding to a marginal treatment effect of 0.16 schemes per household.

All specifications in Table 6 suggest that the programme had a positive and statistically significant effect on awareness of about 0.16 additional schemes per household.

Tables 7 and 8 present similar regressions for, respectively, the counts of schemes that the household has applied for, but not (yet) obtained; and obtained. Note that column 3 in both tables, the first stage of the 2SLS specification, is identical to the one in Table 6, but repeated for convenience. None of the specifications identify a significant effect of the programme on either applications for or obtention of schemes.

Beyond obtention, for a handful of schemes it also meaningful to study *utilisation*, that is, whether a claim has been made. In the case of insurance, obtaining the scheme (being covered) is clearly distinct from utilising it (making a claim). Similarly, utilising the employment guarantee scheme (applying for work) is distinct from joining it (obtaining a job card). The schemes for which we tracked utilisation are NREG, the Construction Workers' Welfare Scheme, the National Health Insurance Scheme, Janashree Bima Yojana

---

<sup>10</sup>Since there is a fixed upper bound to the number of schemes a household can be aware of or access, one could alternatively assume the (positive) binomial distribution. However, this assumption is less appealing than it may seem initially, because the upper bound, at least for obtention, is not fixed at 16 but rather at the number of schemes for which the household is eligible. This number varies between households and is also unknown since we only have a rough idea of eligibility.

<sup>11</sup>Instrumental variables Poisson regression implemented with the Generalized Method of Moments (GMM) does not allow discrete endogenous covariates.

and Vajpayee Arogyasri. Table 9 presents regressions with identical specifications to the preceding tables, except that the outcome variable is scheme utilisation. The effect of the programme on utilisation is not statistically significant for any specification.

There was considerable variation in the age of the centres at the time of the survey. Duration of exposure to the programme could therefore be considered an alternative treatment variable. Table 10 presents the results of two-stage least-squares regressions where the treatment variable is the household's exposure to the programme, in months, at the time of the survey. Exposure is set equal to the age of the centre in months in locations with a centre, and zero elsewhere. As before, intended treatment is the instrumental variable. Column 1 shows that, as expected, the first stage is strong. Households living in the intended treatment areas had on average had 12.8 months longer exposure to the programme, and the coefficient is highly significant. The second-stage regressions in columns 2–4 corroborate what was found above: the programme had a statistically significant positive impact on the number of welfare schemes a household has heard of, but no statistically significant impact on the number of schemes applied for, obtained or utilised.

Several of the original facilitators left their post between their appointment and the post-intervention survey (see Table 3). It is of interest to see that what extent this could affect the results. Table 11 presents two-stage least-squares regressions where treatment localities in which the original facilitator left before the survey was conducted, were deleted. The findings from above are confirmed: facilitators seem to affect awareness, but have no significant impact on applications, obtention or utilisation.

Table 12 presents estimates of treatment effects for various (partly overlapping) categorisations of the schemes. The groups were defined with reference to the information in Table 1. In each case, the presented coefficient is the two-stage least-squares estimate (see table notes for full regression specification). Results for awareness, applications and obtention are presented in columns 1, 2 and 3, respectively. (Scheme utilisation is only tracked for a handful of variables, so further sub-categorisation is not meaningful.) The first row presents results for all targeted scheme, and thus reproduces the main coefficients from column 4 of Tables 6, 7 and 8. The second row presents results for the five schemes that were consistently rewarded throughout the programme, that is, those for which the last column of Table 1 says 'Yes'. The results are similar, albeit with some loss of significance. The results are also similar for relatively new schemes, that is, those launched in 2005 or later.

Contributory schemes are those in which a financial outlay is required on the part of the beneficiary. For these schemes there is a positive and significant effect on applications as well as awareness. One may speculate that contributory schemes are less immediately appealing to potential beneficiaries, and therefore the active 'selling' of the schemes by the facilitators may have a stronger effect. There is also a marginally significant, positive

effect on applications for schemes administered by the Department of Labour. As the facilitator programme was also hosted by the Department of Labour, this may indicate that inter-departmental co-operation was lacking.

The last row presents a form of placebo test. While the scheme targeted 16 schemes at one point or another, the household survey tracks 29 schemes. For the 13 welfare schemes that were never targeted by the scheme, we would not expect an effect on any outcome variable, and this is confirmed by the regressions.

Table 13 presents two-stage least-square estimates of treatment effects for each of the targeted welfare schemes, corresponding in regression specification to column 4 in Tables 6–9. The outcome variables are binary awareness, application and treatment indicators for each scheme, and actual treatment is instrumented by intended treatment. Blank cells indicate regressions that were not identified or, in column 4, schemes for which utilisation (claims made) is not meaningful or tracked in the surveys.

One should be careful not to over-interpret the significance of any individual test due to issues of multiple hypothesis testing, but in conjunction with the category analysis in Table 12 and the difference-in-difference estimates in Table 14, these regressions seem to indicate that the programme had the strongest effects on the Construction workers' welfare fund (awareness, applications and obtention) and insurance for drivers of commercial vehicles (applications). The negative and significant coefficient for the obtention of disability pension is counter-intuitive, but with this number of individual tests, a few cases of spurious results are to be expected.

Rather than analysing the representative sample separately, it can also be combined with the post-intervention observations from the Self-Help Group sample. This combined data set is equivalent to a post-intervention cross-sectional survey in which Self-Help Group member households were oversampled. Analysis of the combined data (not reported) yield qualitatively similar results to the above: there is a significant effect of the programme on awareness, but not on applications, obtention or utilisation.

#### **4.1.2 Difference-in-Differences Estimates: The Self-Help Group Sample**

Longitudinal data is available for 1198 SHG-member households who were observed both at baseline and follow-up. Table 14 presents two-stage least-squares difference-in-difference regressions where the potentially endogenous interaction term *actual treated*  $\times$  *after* is instrumented by *intended treatment*  $\times$  *after*. (The un-interacted treatment variable drops out due to the inclusion of household fixed effects.) The results suggest no significant effects of the programme on awareness, applications, obtention or utilisation. (The effect on scheme obtention is significant at the 10% level.)

It may seem surprising that there is no effect on awareness for this specification when a positive and significant effect on awareness was found in the main results above. However,

this could be because SHG members are better informed of the most relevant schemes to begin with. As mentioned, SHG members tend to be positively selected, and it is also the case that SHG meetings are often used by the government and NGOs to spread information about various initiatives.

We present difference-in-differences rather than analysis-of-covariance (ANCOVA) regressions because the autocorrelations in the outcome variables are relatively high (McKenzie, 2012), but the ANCOVA regressions yield qualitatively similar results: no significant effects.

At the level of individual schemes (Table 15), there are positive and significant effects (at the 5% level) of the programme for the Construction workers' welfare fund (awareness, applications and obtentions), the national health insurance scheme (utilisation), insurance for drivers of commercial vehicles (awareness).

## 4.2 What Predicts Application Submission?

Households in GPs with centres were asked whether the facilitator had helped them submit welfare scheme applications. While it was found above that the scheme had no significant effect on the number of applications submitted overall in the representative household sample, a number of interviewed households in treated villages indicate that the facilitator did in fact help them apply for one or more welfare schemes. It is of interest to look at which factors predict facilitated submission of applications.

For the treated subsample, we create a binary indicator for whether the household reports being helped by the facilitator to submit one or more applications. Table 16 regresses this indicator on range of characteristics pertaining to the household, centre, facilitator and village.

A few key significant effects stand out. First, submissions are negatively associated with facilitator turnover, as measured by the number of facilitators in post since the centre opened conditional on centre age. This has several possible interpretations: It could take time for new facilitators to find their feet in the job. It could be that some villages, which are inherently difficult to work in, led facilitators to quit with a higher rate. It is also possible that replacement facilitators, and/or replaced facilitators, were less effective. In the case of replacement facilitators, this might be because they received less training than those originally appointed.

Second, the further away from the GP headquarters village a household resides, the worse the outcome in terms of applications submitted. There is a negative association between applications submitted and the distance in kilometres of the village from the GP headquarters. This may be because facilitating applications outside the headquarters village is costlier, presumably because the facilitator is based in the headquarters and would incur travel costs if going elsewhere. There may be other costs associated with

remoteness, such as, possibly, a lower level of trust in outsiders. Whatever their origin, the costs may represent a permanent disadvantage of living in a more remote village, but it could also be that the facilitator tend cover the headquarters village initially and outlying villages later. In the latter case, one would expect the distance effects to weaken over time.

Third, there is a marginal (10% significance) and negative effect associated with female facilitators. One should be careful not to over-interpret this finding, but possibly female facilitators are at a disadvantage in a job that requires going from door to door and talking to household heads about issues that may traditionally be seen (for at least some of the schemes) as falling within the male domain.

No other variable is associated with a significantly higher or lower probability of facilitated application submissions.

### 4.3 Subjective Welfare Outcomes

While the household survey did not capture overall welfare in a rigorous manner, it did include some items intended to capture any effect of the programme on some basic subjective welfare outcomes. Respondents were presented with the following statements in turn:

1. ‘The government takes good care of poor people in the delivery of social security schemes.’
2. ‘All things considered, these days I am satisfied with my life as a whole.’
3. ‘I feel secure about the future for myself and my family.’

For each statement, respondents were asked to what extent they agreed. The answer options were ‘disagree completely’, ‘disagree somewhat’, ‘neutral, neither agree nor disagree’, ‘agree somewhat’ and ‘agree completely’.

In table 17, the outcome variables are binary indicators for whether a respondent agreed ‘somewhat’ or ‘completely’ with a given statement. The presented coefficients are two-stage least-squares estimates of the effect of the programme on agreement with each statement, where actual treatment is instrumented by intended outcome. As above, the regressions include taluk and interviewer fixed effects and household control variables, and the standard errors are clustered at the GP level.

The programme is associated with an 8 percentage point increase in the proportion of households agreeing with statements 1 and 2. These coefficients are statistically significant. The programme is not associated with a statistically significant change in the proportion of households agreeing with statement 3. These findings are perhaps surprising, given the relatively weak overall effects on applications and obtentions of welfare



schemes. Perhaps the higher levels of satisfaction are due to applications that have been started, but not yet submitted. Alternatively, in line with the increased levels of awareness, it could be people that are happy or grateful that the schemes exist and are being marketed and facilitated, even if they themselves have not benefited. It is also possible that these results reflect strategic responses: respondents might want to express satisfaction if they believe that this can make them more likely to obtain some future benefit from the programme. It should, however, be emphasised that the enumerators were directly employed by the researchers and had no affiliation with the government or GIZ.

## 5 Discussion

Overall, in the representative sample, we identify modest, positive effects of the programme on scheme awareness, but no significant effect on aggregate applications, obtention or utilisation. No significant effects are identified in the sample of Self-Help Group member households.

Analysis of individual programmes (Tables 13 and 15) provide suggestive evidence that the awareness effect may be driven primarily by two individual schemes, the Construction workers' welfare package and Insurance for drivers of commercial vehicles. Taken at face value, the coefficients also suggest that the programme may have been linked to increased applications and obtention of these two schemes, as well as increased utilisation of the National Health Insurance Scheme (RSBY). However, these effects should be treated with caution since the analysis of individual programmes is subject to the problems of multiple hypothesis testing.

The three schemes mentioned above have several important features which, individually or in combination, could help explain why the programme may have had some success in pushing them. First, two of them were the subject of special 'drives' as described in Section 2. These drives may have been successful because each facilitation was associated with a higher level of reward, or because groups of facilitators working together may have been more effective than each of them working on their own.

Second, all three schemes were less than five years old at the time the programme started. (Insurance for drivers of commercial vehicles was launched two years *after* the programme launch.) It may be that there is more scope for increasing awareness and take-up of schemes that are relatively new, whereas the demand for older schemes may be largely saturated. However, when all new schemes are considered together as a category, the effects on applications and obtention are not significant.

Third, all three are contributory schemes, that is, they require a financial outlay from the beneficiary. It is possible that contributory schemes require a more active 'sell', in this case by the facilitator, than 'free' schemes that are more obviously desirable. This, too, could explain why demand for these schemes was not already saturated.

Fourth, they are all administered by the Department of Labour. While part of the intention of the centres were to act as ‘one-stop shops’ for schemes run by several departments, in practice co-ordination proved to be difficult in many cases. It is therefore possible that the programme, which was itself a project of the Department of Labour, was more successful in facilitating schemes offered by the Department of Labour.

While we find only modest effects of the programme on overall scheme applications and obtention, we know from the project’s administrative data that a significant number of scheme applications were in fact submitted by the facilitators. It is possible that our statistical tests are not powerful enough to detect the aggregate effect. But it is also possible that the facilitator assisted households with applications that would have been submitted even without their help. If so, the facilitator may have reduced the cost to the household of submitting the applications, even though the rate of applications did not increase. In this respect, a programme of this kind may be worthwhile even if the number of applications are not affected. The benefits of assistance (time saved, convenience) are difficult to quantify, but should be compared to the cost of running the programme.

We conclude that the programme was successful in establishing a significant number of operational centres. They also succeeded in providing awareness of and access to a wide range of schemes. But, possibly due to a combination of bureaucratic inertia and possibly lower latent demand than expected, the schemes that were most successfully promoted were all relatively new, they may have required a more active ‘sell’ because they involved a financial outlay on the part of the beneficiaries, and they were all based in the Department of Labour, the department that was also behind the programme to push them. Ultimately, unmet demand for the existing range of social services is perhaps not as large as it may appear, and the potential for ‘pushing welfare’ may be largely confined to schemes that are relatively new and not already ‘owned’ by local stakeholders.

## References

- Abramovsky, Laura, Orazio Attanasio, Kai Barron, Pedro Carneiro, and George Stoye.** 2016. “Challenges to Promoting Social Inclusion of the Extreme Poor: Evidence from a Large-Scale Experiment in Colombia.” *Economía*, 16(2): 89–141.
- Barrientos, Armando.** 2013. *Social assistance in developing countries*. Cambridge University Press.
- Bent, Stephen, Kenneth Kernaghan, and D. Brian Marson.** 1999. *Innovations and good practices in single-window service*. Canadian Centre for Management Development.

- Berg, Erlend, Maitreesh Ghatak, R Manjula, D Rajasekhar, and Sanchari Roy.** 2018. “Motivating knowledge agents: Can incentive pay overcome social distance?” *Economic Journal*. Forthcoming.
- Bertrand, Marianne, Sendhil Mullainathan, and Eldar Shafir.** 2004. “A Behavioral-Economics View of Poverty.” *The American Economic Review*, 94(2): 419–423.
- Carneiro, Pedro Manuel, Emanuela Galasso, and Rita Ginja.** 2015. “Tackling social exclusion: Evidence from Chile.” World Bank Policy Research Working Paper 7180.
- Currie, Janet.** 2006. “The Take-Up of Social Benefits.” In *Poverty, the Distribution of Income, and Public Policy*, ed. David Card Alan Auerbach and John Quigley, 80–148. New York:Russell Sage.
- Government of Karnataka, Department of Labour, and German Technical Cooperation GTZ.** 2010. “Government Social Security Schemes for Unorganised Workers in Karnataka.”
- Hernanz, Virginia, Franck Malherbet, and Michele Pellizzari.** 2004. “Take-Up of welfare benefits in OECD countries: A review of the evidence.” OECD Social, Employment and Migration Working Paper 17.
- International Labour Organization.** 2018. “Women and Men in the Informal Economy: A Statistical Picture.” 3rd Edition.
- Kleven, Henrik Jacobsen, and Wojciech Kopczuk.** 2011. “Transfer program complexity and the take-up of social benefits.” *American Economic Journal: Economic Policy*, 3(1): 54–90.
- McKenzie, David.** 2012. “Beyond baseline and follow-up: The case for more T in experiments.” *Journal of development Economics*, 99(2): 210–221.
- O’Brien, Clare, and Luca Pellerano.** 2015. “Determinants of take-up of welfare programmes: Evidence from a conditional cash transfer in Kazakhstan.” *Journal of Poverty and Social Justice*, 23(2): 89–104.
- Terza, Joseph V.** 1998. “Estimating count data models with endogenous switching: Sample selection and endogenous treatment effects.” *Journal of Econometrics*, 84(1): 129–154.
- Turner, Mark.** 2012. “Decentralization, politics and service delivery: the introduction of one-stop shops in Mongolia.” *Public Management Review*, 14(2): 197–215.

**Xu, Qingwen, Xinping Guan, and Fangfang Yao.** 2011. "Welfare program participation among rural-to-urban migrant workers in China." *International Journal of Social Welfare*, 20(1): 10–21.

Table 1: Targeted Welfare Schemes

Responsible Government Unit	Name of Scheme	Type	Introduced	Contri- butory?	Facilitation rewarded?
Department of Labour	Construction workers' welfare fund	Mixed	2006	Yes	Yes
	National health insurance scheme (RSBY)	Health	2008	Yes	Yes
	National Pension Scheme - Lite	Pension	2010	Yes	Yes
Department of Health and Family Welfare	Insurance for drivers of commercial vehicles	Insurance	2012-13	Yes	Yes, from introduction
	Vajpayee arogyasri	Health	2010	No	Yes
	Government residential school	Education	Late 1990s	No	Yes
	Janshree Bima Yojana (JBY)	Mixed	2000	Yes	Yes, until scheme discontinued
Directorate of Minorities, Department of Social Welfare, Department of Backward Classes	Pre- and post-matric scholarship	Education	Early 1990s	No	Yes, but reward reduced
	Department of Rural Development and Panchayat Raj	Employment	2006	No	Yes, but reward reduced
Department of Revenue (Directorate of Social Security and Pensions)	National Rural Employment Guarantee (NREG)	Employment	2006	No	Yes, but reward reduced
	Aam Admi Bima Yojana (AABY)	Mixed	2007	No	Yes, but only towards the end
	Disability pension	Disability	1977	No	Yes, but applications suspended by govt.
Department of Women and Child Development	Old age pension	Pension	1964	No	Yes, but applications suspended by govt.
	Widow pension	Pension	1984	No	Yes, but applications suspended by govt.
	Nutritional programme for adolescent girls	Health	2005-06	No	Only initially
Department for Empowerment of Differently Abled and Senior Citizens	Bhagyalakshmi	Mixed	2006	No	Only initially
	Adhara self-employment scheme for the disabled	Disability	1994-95	No	Only initially

*Notes:* This table provides an overview of the 16 welfare schemes targeted by the project. Column 1 provides the government unit responsible for the scheme; column 2, the name of the scheme; column 3, a crude classification of the type of welfare scheme; column 4, the year (or period) the scheme was introduced; column 5, whether the scheme requires a financial outlay from the beneficiary; and column 6, the status of the scheme in the bonus pay system for facilitators. At the outset, the facilitator was rewarded equally for applications for all schemes, up to a limit of 30 applications per month. However, three schemes were dropped from the project and the reward system early on (rewarded only initially). A further two schemes were rewarded throughout the period, but the bonus per facilitated application was reduced during the project. Applications for three schemes (disability pension, old age pension and widow pension) were in principle rewarded throughout, but for a substantial period applications for these schemes were not accepted by the government, for reasons unrelated to the project. The insurance scheme for drivers of commercial vehicles was introduced mid-project and rewarded from then on. Janshree Bima Yojana was rewarded from the start, but the scheme was discontinued before the project ended. Finally, Aam Admi Bima Yojana was rewarded only in the final months of the project.

Table 2: Village Summary Statistics

	Control	Treatment	Difference
Village is GP headquarters	0.52 (0.50)	0.43 (0.50)	-0.094 (0.067)
Distance from GP headquarters	1.80 (2.45)	2.46 (3.11)	0.65* (0.39)
Village size (households)	801.8 (642.7)	794.1 (756.0)	-7.66 (96.8)
Proportion of SC/ST households in village	0.30 (0.24)	0.30 (0.26)	0.0034 (0.034)
Proportion of unorganised-worker households in village	0.60 (0.17)	0.58 (0.15)	-0.017 (0.021)
Proportion of village agricultural land that is irrigated	0.40 (0.34)	0.43 (0.34)	0.031 (0.046)
Actual treatment	0.073 (0.26)	0.70 (0.46)	0.62*** (0.055)
Observations	82	172	.

*Notes:* ‘Treatment’ and ‘Control’ refer to the intended treatment groups. In the Control and Treatment columns, the numbers in parentheses are standard deviations. In the Difference column, the numbers in parentheses are the standard errors of the difference  $t$ -tests. The last row presents the actual treatment variable. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 3: Facilitator Summary Statistics

Facilitators in post since centre opening	1.33 (0.53)
Female facilitators in post since centre opening	0.52 (0.72)
Facilitator tenure in months (mean)	16.2 (5.92)
Incumbent facilitator tenure in months	15.4 (6.70)
Incumbent facilitator is female	0.38 (0.49)
Original facilitator has left	0.31 (0.46)
Age of centre in months	19.4 (4.91)
Observations	75

*Notes:* Summary statistics for facilitators and centres. Standard deviations in parentheses.

Table 4: Household Summary Statistics: Cross-Sectional Sample

	Control	Treatment	Difference
Scheduled caste / tribe	0.38 (0.49)	0.41 (0.49)	0.027 (0.021)
Pucca house	0.15 (0.36)	0.15 (0.36)	0.00018 (0.015)
Household size	5.52 (2.58)	5.40 (2.52)	-0.12 (0.11)
Age of household head	49.1 (12.1)	49.5 (12.2)	0.40 (0.52)
Household head is married	0.85 (0.36)	0.82 (0.38)	-0.024 (0.016)
Female household head	0.15 (0.36)	0.16 (0.36)	0.0074 (0.016)
Illiterate household head	0.53 (0.50)	0.50 (0.50)	-0.026 (0.021)
Household head is agricultural wage labourer	0.45 (0.50)	0.46 (0.50)	0.011 (0.021)
At least one household member is disabled	0.067 (0.25)	0.074 (0.26)	0.0072 (0.011)
Self-Help Group member	0.47 (0.50)	0.50 (0.50)	0.034 (0.021)
Number of schemes aware of	2.32 (1.44)	2.50 (1.57)	0.17*** (0.066)
Number of schemes applied for but not (yet) obtained	0.26 (0.51)	0.24 (0.52)	-0.014 (0.022)
Number of schemes obtained	1.27 (1.10)	1.32 (1.17)	0.049 (0.049)
Number of schemes utilised	0.015 (0.13)	0.016 (0.13)	0.0014 (0.0056)
Observations	805	1655	.

*Notes:* ‘Treatment’ and ‘Control’ refer to the intended treatment groups. In the Control and Treatment columns, the numbers in parentheses are standard deviations. In the Difference column, the numbers in parentheses are the standard errors of the difference  $t$ -tests. The last four rows present outcome variables. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



Table 5: Household Summary Statistics: Longitudinal (Self-Help Group) Sample, at Baseline

	Control	Treatment	Difference
Scheduled caste / tribe	0.31 (0.46)	0.35 (0.48)	0.046 (0.029)
Pucca house	0.13 (0.33)	0.13 (0.33)	-0.00062 (0.020)
Household size	5.67 (2.57)	5.64 (2.58)	-0.026 (0.16)
Age of household head	48.9 (12.4)	48.7 (12.2)	-0.21 (0.75)
Household head is married	0.85 (0.36)	0.83 (0.38)	-0.020 (0.023)
Female household head	0.14 (0.35)	0.17 (0.37)	0.023 (0.022)
Illiterate household head	0.45 (0.50)	0.48 (0.50)	0.027 (0.030)
Household head is agricultural wage labourer	0.31 (0.46)	0.35 (0.48)	0.037 (0.029)
At least one household member is disabled	0.057 (0.23)	0.073 (0.26)	0.017 (0.015)
Self-Help Group member	0.99 (0.11)	0.99 (0.087)	0.0047 (0.0058)
Number of schemes aware of	2.02 (1.26)	2.01 (1.37)	-0.0095 (0.081)
Number of schemes applied for but not (yet) obtained	0.20 (0.47)	0.25 (0.54)	0.047 (0.032)
Number of schemes obtained	1.06 (0.94)	0.98 (0.97)	-0.078 (0.059)
Number of schemes utilised	0.15 (0.36)	0.13 (0.34)	-0.024 (0.021)
Observations	406	792	.

*Notes:* Baseline summary statistics for households in the Self-Help Group panel. This sample is not representative of all informal-sector worker households and therefore not preferred for the main analysis. ‘Treatment’ and ‘Control’ refer to the intended treatment groups. In the Control and Treatment columns, the numbers in parentheses are standard deviations. In the Difference column, the numbers in parentheses are the standard errors of the difference  $t$ -tests. The last four rows present outcome variables. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 6: Aggregate Awareness of Schemes

	(1) Number of schemes aware of	(2) Number of schemes aware of	(3) Actual treatment	(4) Number of schemes aware of	(5) Number of schemes aware of	(6) Number of schemes aware of
Actual treatment	0.084 (0.064)			0.28** (0.11)		0.066** (0.030)
Intended treatment		0.17** (0.068)	0.63*** (0.059)		0.068** (0.029)	
Taluk fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Interviewer fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2460	2460	2460	2460	2460	2460
Specification	OLS	ITT	2SLS 1st stage	2SLS 2nd stage	Poisson ITT	Poisson IV

*Notes:* The dependent variable, except in column 3, is the count of welfare schemes that the household is aware of. As well as fixed effects for taluk (region) and interviewer, the regressions control for whether the household belongs to a scheduled caste or tribe, whether the household head is married, female and illiterate, the age of the household head, the household size, whether the village is a GP headquarters and the size of the village in number of households. Standard errors, in parentheses, are robust and clustered at the GP level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 7: Aggregate Applications for Schemes

	(1) Number of schemes applied for but not (yet) obtained	(2) Number of schemes applied for but not (yet) obtained	(3) Actual treatment	(4) Number of schemes applied for but not (yet) obtained	(5) Number of schemes applied for but not (yet) obtained	(6) Number of schemes applied for but not (yet) obtained
Actual treatment	0.012 (0.027)			-0.0051 (0.044)		0.069 (0.16)
Intended treatment		-0.0032 (0.028)	0.63*** (0.059)		-0.015 (0.11)	
Taluk fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Interviewer fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2460	2460	2460	2460	2460	2460
Specification	OLS	ITT	2SLS 1st stage	2SLS 2nd stage	Poisson ITT	Poisson IV

*Notes:* The dependent variable, except in column 3, is the count of welfare schemes that the household has applied for but not (yet) obtained. As well as fixed effects for taluk (region) and interviewer, the regressions control for whether the household belongs to a scheduled caste or tribe, whether the household head is married, female and illiterate, the age of the household head, the household size, whether the village is a GP headquarters and the size of the village in number of households. Standard errors, in parentheses, are robust and clustered at the GP level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 8: Aggregate Obtention of Schemes

	(1) Number of schemes obtained	(2) Number of schemes obtained	(3) Actual treatment	(4) Number of schemes obtained	(5) Number of schemes obtained	(6) Number of schemes obtained
Actual treatment	0.016 (0.052)			0.13 (0.097)		0.025 (0.046)
Intended treatment		0.082 (0.061)	0.63*** (0.059)		0.049 (0.047)	
Taluk fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Interviewer fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2460	2460	2460	2460	2460	2460
Specification	OLS	ITT	2SLS 1st stage	2SLS 2nd stage	Poisson ITT	Poisson IV

*Notes:* The dependent variable, except in column 3, is the count of welfare schemes that the household has obtained. As well as fixed effects for taluk (region) and interviewer, the regressions control for whether the household belongs to a scheduled caste or tribe, whether the household head is married, female and illiterate, the age of the household head, the household size, whether the village is a GP headquarters and the size of the village in number of households. Standard errors, in parentheses, are robust and clustered at the GP level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 9: Aggregate Schemes Utilised (Made a Claim On)

	(1) Number of schemes utilised	(2) Number of schemes utilised	(3) Actual treatment	(4) Number of schemes utilised	(5) Number of schemes utilised	(6) Number of schemes utilised
Actual treatment	-0.0035 (0.0073)			0.0043 (0.013)		0.62 (1.28)
Intended treatment		0.0027 (0.0080)	0.63*** (0.059)		0.22 (0.47)	
Taluk fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Interviewer fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2460	2460	2460	2460	2460	2460
Specification	OLS	ITT	2SLS 1st stage	2SLS 2nd stage	Poisson ITT	Poisson IV

*Notes:* The dependent variable, except in column 3, is the count of welfare schemes that the household has utilised, that is, made a claim on. Utilisation only applies to a handful of schemes, as detailed in Table 13. As well as fixed effects for taluk (region) and interviewer, the regressions control for whether the household belongs to a scheduled caste or tribe, whether the household head is married, female and illiterate, the age of the household head, the household size, whether the village is a GP headquarters and the size of the village in number of households. Standard errors, in parentheses, are robust and clustered at the GP level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 10: Exposure Duration as Alternative Treatment Variable

	(1)	(2)	(3)	(4)	(5)
	Exposure to programme in months	Number of schemes heard of	Number of schemes applied for but not (yet) obtained	Number of schemes obtained	Number of schemes utilised
Intended treatment	12.8*** (1.13)				
Exposure to programme in months		0.014** (0.0054)	-0.00025 (0.0022)	0.0064 (0.0048)	0.00021 (0.00062)
Taluk fixed effects	Yes	Yes	Yes	Yes	Yes
Interviewer fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	2460	2460	2460	2460	2460
	1st stage	2nd stage	2nd stage	2nd stage	2nd stage

*Notes:* Two-stage least-squares regressions. The first stage is presented in column 1, and the second-stage regressions in columns 2-4. As well as fixed effects for taluk (region) and interviewer, the regressions control for whether the household belongs to a scheduled caste or tribe, whether the household head is married, female and illiterate, the age of the household head, the household size, whether the village is a GP headquarters and the size of the village in number of households. Standard errors, in parentheses, are robust and clustered at the GP level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 11: Results for Treatment with No Turnover in Facilitators

	(1) Exposure to programme in months	(2) Number of schemes heard of	(3) Number of schemes applied for but not (yet) obtained	(4) Number of schemes obtained	(5) Number of schemes utilised
Intended treatment	0.56*** (0.062)				
Actual treatment		0.31** (0.14)	-0.031 (0.055)	0.17 (0.12)	0.0063 (0.015)
Taluk fixed effects	Yes	Yes	Yes	Yes	Yes
Interviewer fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	1979	1979	1979	1979	1979
	1st stage	2nd stage	2nd stage	2nd stage	2nd stage

*Notes:* Two-stage least-squares regressions. Only locations where the original facilitator remained in place until the survey are included in the treatment group. The first stage is presented in column 1, and the second-stage regressions in columns 2-4. As well as fixed effects for taluk (region) and interviewer, the regressions control for whether the household belongs to a scheduled caste or tribe, whether the household head is married, female and illiterate, the age of the household head, the household size, whether the village is a GP headquarters and the size of the village in number of households. Standard errors, in parentheses, are robust and clustered at the GP level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 12: Effects by Scheme Category

	(1) Heard of	(2) Applied for but not (yet) obtained	(3) Obtained
All targeted schemes	0.28** (0.11)	-0.0051 (0.044)	0.13 (0.097)
Consistently rewarded schemes	0.099* (0.060)	0.0028 (0.0094)	0.046 (0.043)
New schemes (launched in 2005 or later)	0.17** (0.083)	0.017 (0.022)	0.056 (0.072)
Contributory schemes (financial outlay required)	0.087** (0.040)	0.015** (0.0072)	0.050 (0.032)
Labor Department schemes	0.080** (0.038)	0.012* (0.0070)	0.048 (0.032)
Non-targeted schemes	0.095 (0.086)	0.018 (0.014)	0.014 (0.060)

*Notes:* Two-stage least-squares regressions estimates of treatment effects for categories of schemes. The outcome variables are counts of binary awareness, application and obtention indicators, and actual treatment is instrumented by intended treatment. Eight schemes were consistently rewarded at the full rate throughout the programme, and applications were not suspended by the government (refer to Table 1). ‘Non-targeted schemes’ are a group of 13 schemes that were tracked in the surveys but not targeted by the programme. As well as fixed effects for taluk (region) and interviewer, the regressions control for whether the household belongs to a scheduled caste or tribe, whether the household head is married, female and illiterate, the age of the household head, the household size, whether the village is a GP headquarters and the size of the village in number of households. Standard errors, in parentheses, are robust and clustered at the GP level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



Table 13: Individual Scheme Effects

	(1) Heard of	(2) Applied for but not (yet) obtained	(3) Obtained	(4) Utilised
Construction workers' welfare fund	0.16*** (0.051)	0.044** (0.021)	0.064* (0.035)	0.0010 (0.0011)
National health insurance scheme (RSBY)	0.037 (0.035)	-0.0033 (0.0032)	0.038 (0.031)	0.0080* (0.0041)
National Pension Scheme - Lite	0.00053 (0.0070)	0.0010 (0.0010)	-0.0050 (0.0047)	
Insurance for drivers of commercial vehicles	0.086 (0.088)	0.058** (0.028)	-0.028 (0.058)	
Vajpayee arogyasri	0.0045 (0.038)	-0.0040 (0.0038)	-0.026 (0.032)	0.0021 (0.0029)
Government residential school	0.031 (0.048)	-0.0021 (0.0096)	0.044* (0.026)	
Janshree bima yojana (JBY)	0.0068 (0.0074)	0.0037* (0.0019)	0.0016 (0.0041)	
Pre-/post-matric scholarship	-0.044 (0.033)	-0.13* (0.074)	-0.0036 (0.085)	
National Rural Employment Guarantee (NREGA)	0.023 (0.044)	0.0022 (0.013)	0.0088 (0.043)	-0.0068 (0.011)
Aam admi bima yojana	0.0054 (0.0079)	0.0036* (0.0022)	-0.00064 (0.0051)	
Disability pension	0.11 (0.069)	0.069 (0.084)	-0.24** (0.11)	
Old-age pension	0.011 (0.010)	-0.018 (0.055)	0.12* (0.066)	
Widow pension	0.011 (0.014)	-0.072 (0.059)	-0.066 (0.080)	
Nutritional programme for adolescent girls	0.097** (0.049)	-0.0045 (0.0041)	0.0040 (0.061)	
Bhagyalakshmi	-0.018 (0.014)	0.019 (0.048)	0.015 (0.062)	
Adhara self-employment scheme for the disabled	-0.075 (0.14)	-0.060 (0.055)		

*Notes:* Two-stage least-squares regressions estimates of treatment effects for individual schemes. The outcome variables are binary awareness, application, obtention and utilisation indicators, and actual treatment is instrumented by intended treatment. As well as fixed effects for taluk (region) and interviewer, the regressions control for whether the household belongs to a scheduled caste or tribe, whether the household head is married, female and illiterate, the age of the household head, the household size, whether the village is a GP headquarters and the size of the village in number of households. A blank cell indicates that there was not enough variation in the data to identify the regression or, for column (4), that utilisation is not meaningfully defined. Standard errors, in parentheses, are robust and clustered at the GP level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 14: Two-Stage Least-Squares Difference-in-Difference Results for the Self-Help Group Sample

	(1) Actual treatment × after	(2) Number of schemes aware of	(3) Number of schemes applied for but not (yet) obtained	(4) Number of schemes obtained	(5) Number of schemes utilised
After	0.46*** (0.11)	0.29 (0.42)	0.0098 (0.20)	0.15 (0.29)	-0.31** (0.14)
Intended treatment × after	0.63*** (0.062)				
Actual treatment × after		0.21 (0.18)	-0.053 (0.068)	0.21* (0.12)	0.050 (0.051)
Hosuehold fixed effects	Yes	Yes	Yes	Yes	Yes
Interviewer fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	2396	2396	2396	2396	2396
Households	1198	1198	1198	1198	1198
	1st stage	2nd stage	2nd stage	2nd stage	2nd stage

*Notes:* Column 1 presents the first stage, while columns 2–4 present second-stage regressions for awareness, applications and obtention, respectively. The un-interacted treatment variables drop out because of the household fixed effects. The regressions include fixed effects for household and interviewer. Standard errors, in parentheses, are robust and clustered at the GP level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 15: Individual Scheme Effects for the Self-Help Group Sample

	(1) Heard of	(2) Applied for but not (yet) obtained	(3) Obtained	(4) Utilised
Construction workers' welfare fund	0.42*** (0.13)	0.14** (0.056)	0.25** (0.12)	0.0052 (0.0036)
National health insurance scheme (RSBY)	-0.0050 (0.15)	0.0042 (0.023)	0.17 (0.13)	0.014** (0.0066)
National Pension Scheme - Lite	0.019 (0.016)	0.0013 (0.0015)	0.0091 (0.0077)	
Insurance for drivers of commercial vehicles	0.40*** (0.11)	0.11* (0.059)	-0.0016 (0.064)	
Vajpayee arogyasri	0.044 (0.100)	-0.0015 (0.016)	-0.0066 (0.078)	-0.011 (0.0076)
Government residential school	-0.078 (0.089)	-0.0015 (0.014)	-0.069** (0.032)	
Janshree bima yojana (JBY)	-0.015 (0.028)	0.0025 (0.0032)	-0.017 (0.021)	-0.011* (0.0058)
Pre-/post-matric scholarship	0.060 (0.33)	-0.46* (0.28)	-0.25 (0.39)	
National Rural Employment Guarantee (NREGA)	0.098* (0.055)	-0.047 (0.033)	0.053 (0.055)	0.053 (0.049)
Aam admi bima yojana	-0.047 (0.036)	0.010 (0.012)	-0.024 (0.017)	
Disability pension	0.30 (0.38)	0.85 (0.60)	-0.15 (0.40)	
Old-age pension	0.010 (0.021)	0.0014 (0.094)	0.16* (0.094)	
Widow pension	0.026 (0.026)	0.028 (0.12)	-0.16 (0.13)	
Nutritional programme for adolescent girls	0.068 (0.10)	0.013 (0.013)	-0.023 (0.11)	
Bhagyalakshmi	0.062 (0.099)	0.14 (0.10)	-0.14 (0.13)	

*Notes:* Two-stage least-squares regressions estimates of difference-in-difference treatment effects for individual schemes for the SHG sample. The outcome variables are binary awareness, application, obtention and utilisation indicators, and the interaction of actual treatment and 'after' is instrumented by the interaction of intended treatment and 'after', as in Table 14. All regressions include interviewer and household fixed effects. A blank cell or row indicates that there was not enough variation in the data to identify the regression or, for column (4), that utilisation is not meaningfully defined. Two schemes, insurance for drivers of commercial vehicles and National Pension Scheme - lite, were launched after the baseline survey. For these two schemes, baseline awareness, applications, obtention and utilisation were set to zero. Standard errors, in parentheses, are robust and clustered at the GP level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 16: What Predicts Facilitated Applications?

	Has submitted application with facilitator help
Scheduled caste / tribe	0.0082 (0.014)
Age of household head	0.00052 (0.00060)
Household head is married	0.014 (0.034)
Female household head	0.038 (0.035)
Illiterate household head	-0.024 (0.014)
Household head is agricultural wage labourer	0.0017 (0.015)
Household size	0.0026 (0.0028)
Exposure to programme in months	0.0018 (0.0024)
Facilitators in post since centre opening	-0.042*** (0.016)
Incumbent facilitator is female	-0.037* (0.022)
Distance from GP headquarters	-0.0073*** (0.0019)
Village size (households)	0.0000022 (0.000025)
Proportion of SC/ST households in village	-0.0077 (0.041)
Proportion of unorganised-worker households in village	-0.078 (0.056)
Proportion of village agricultural land that is irrigated	0.021 (0.040)
Taluk fixed effects	Yes
Interviewer fixed effects	Yes
Observations	1038

*Notes:* Ordinary least-squares regression. Only households in villages with facilitators are included. Standard errors, in parentheses, are robust and clustered at the GP level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 17: Subjective Welfare Outcomes

	(1) Social security satisfaction	(2) Life satisfaction	(3) Feel secure
Actual treatment	0.0775** (0.0326)	0.0813*** (0.0290)	0.0304 (0.0343)
Taluk fixed effects	Yes	Yes	Yes
Interviewer fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	2460	2460	2460

*Notes:* Two-stage least-squares regressions. Actual treatment is instrumented by intended treatment. In each column, the outcome variable is a binary indicator for whether the respondent agreed ‘somewhat’ or ‘completely’ with a given statement. The statements were as follows. Column 1: ‘The government takes good care of poor people in the delivery of social security schemes.’ Column 2: ‘All things considered, these days I am satisfied with my life as a whole.’ Column 3: ‘I feel secure about the future for myself and my family.’ As well as fixed effects for taluk (region) and interviewer, the regressions control for whether the household belongs to a scheduled caste or tribe, whether the household head is married, female and illiterate, the age of the household head, the household size, whether the village is a GP headquarters and the size of the village in number of households. Standard errors, in parentheses, are robust and clustered at the GP level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .