

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

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## Abstract

Governments in many developing countries offer social welfare schemes that few take up or even know of. We undertake a randomized-controlled evaluation of agents trained in the facilitation of 16 government social welfare schemes in south India. The agents were incentivized 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 application submission or scheme obtention. Among members of government-sponsored community groups, agents have no effect. Scheme-specific analysis suggests that the program may have had an impact on two schemes that were relatively new, subject to special focus under the program and administered by the implementing department. Despite a modest effect on awareness, 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 centers, one-stop centers, intra-government coordination, informal sector, randomized-controlled trial, India

# 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 programs in developing countries since the mid-1990s (Barrientos, 2013), we know very little about welfare take-up in these regions.

Fragmentation of services across multiple government departments is also a known problem, in rich and poor countries alike. This paper evaluates the impact of a program introducing single-window service centers intended to increase awareness and take-up of welfare schemes provided by a range of government departments. The program recruited, trained, incentivized and monitored 250 full-time facilitators, based at the centers and tasked with seeking out households eligible for welfare schemes, providing them with information about the schemes and helping them apply. The social protection schemes facilitated included pensions for the old, widows and the differently abled, health- and disability insurance, education-related benefits as well as programs targeting the unemployed and workers in specific sectors.

This paper is possibly the first robust evaluation of a demand-side intervention aimed at strengthening awareness of and the capacity to apply for social protection schemes in a developing country, without concomitant changes on the supply side. We analyze independently collected household survey data from a randomized-controlled trial involving 150 gram panchayats (village council areas) in the south Indian state of Karnataka. Due to imperfect compliance with the experimental protocol, we use assigned treatment as an instrumental variable for actually receiving the program. We find that while the program increased awareness of social welfare schemes in the eligible population, it did not significantly increase the number of schemes applied for, obtained or utilized. For households linked to Self-Help Groups (SHGs), a network of savings-and-loans clubs with positively selected membership, we find no aggregate effects. Possibly, SHG members are more likely to be already aware of relevant schemes, making it harder to increase knowledge further. The positive effects seem to be driven

by two schemes that were both new and also promoted over and above the general program in targeted enrollment ‘drives’.

While the impact on awareness and uptake was modest, people in treated areas were 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 the potential for ‘pushing welfare’ was limited and largely linked to schemes that are relatively new and do not require intra-government coordination.

While the literature on the targeting of anti-poverty programs 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) emphasize 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. (2019) find that social distance acts as a barrier to the flow of information about India’s National Health Insurance Scheme (RSBY), but that these barriers can be overcome by the introduction of a small incentive. They also find that improved knowledge about the program among eligible households has a causal effect on take-up.

Carneiro, Galasso and Ginja (2019) look at a Chilean program of frequent home visits and guaranteed access to social services. They employ a regression discontinuity design to identify a significant effect of the program on the take-up of other social services.

Abramovsky et al. (2016) evaluate a Colombian pilot program which aimed, in part, to

stimulate demand for existing social programs. 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 programs, 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. Our paper is, therefore, possibly the first to isolate the effects of a program focused exclusively on increasing the knowledge and take-up of a broad range of existing welfare schemes using a randomized-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 developing 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 further compound process costs. In contrast, stigma is probably less of a concern in contexts where ‘everyone’ is poor. Our paper contributes to this 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: First, a large majority of working-age Indians are in the informal labor market. This market is in constant flux. In the rural areas, small farmers leave and re-join the casual labor 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 located 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, in some cases leading to a dependence on exploitative middlemen.

The program under study attempted to overcome these barriers by seeking out and informing eligible households of available government schemes, and helping them to obtain the necessary documentation and apply. The centers formed under the evaluated program were conceived of as ‘single-window service centers’ (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 and legitimacy.

## **2 The Program**

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 Centers across the state as a pilot program. 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 program was formally launched in 2009, and the first center was opened in October 2010.<sup>2</sup>

The program was implemented in six districts of Karnataka: Bangalore Rural, Bangalore Urban, Bellary, Dakshina Kannada, Gulbarga and Mysore. The districts were 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 Centers were opened in 250 gram panchayats across these ten taluks.

## 2.1 Centers and facilitators

Centers were set up at the gram panchayat (GP), the village councils that constitute the lowest level of elected government in rural India. Each center was hosted within the GP administration building and equipped with an internet-connected computer, a printer and basic office furniture.

Each center was staffed by a single, full-time Community Facilitator appointed by the district government. The facilitators were typically young (the average age varied between 26 and 28 across districts) university graduates.<sup>4</sup> Local candidates were

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<sup>1</sup>Of all employed Indians, 88.2% are in the informal sector (International Labour Organization, 2018). The term used for these in India is 'unorganized workers'.

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

<sup>3</sup>In each participating district, the district headquarters taluk was selected because a location near an urban center 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 and construction workers.

<sup>4</sup>The minimum qualification for facilitators was a university degree, and most were recruited soon after graduation. A few also had postgraduate qualifications. Only a small number of facilitators had

preferred as this was expected to contribute to their productivity and acceptance within the community, and this was achieved in most cases. The facilitators were provided with and encouraged to wear a uniform specific to the program, and to use the program's logo actively to promote visibility.

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 training on 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 more detailed 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 program 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 villages in the jurisdiction of their GP. Rather than waiting for potential beneficiaries to come to them, facilitators were primarily expected to go from door to door.<sup>5</sup> Using a structured questionnaire, they were to collect data on the background and social security needs of each household with the purpose of matching benefits with their social security needs. Facilitators provided information on relevant social security schemes and assisted the households in obtaining necessary documentation and submitting applications.

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prior work experience, and none had experience with data collection or facilitation.

<sup>5</sup>Facilitators were asked to contact every household, working street by street, and screen for program eligibility based on whether at least one household member was working in the informal sector.

There was a system of regular quality checks of the facilitators' work. 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 of 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. Where serious problems were found, facilitators received a warning from the project administration or, in a small number of cases, were dismissed and replaced.

## **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>6</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 of submitted

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<sup>6</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.

applications. For most schemes, this took the form of an acknowledgement from the government office receiving 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 for the project (where 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 real prospects of progression to other jobs within the government sector or GIZ during or after the project.

### **2.3 Targeted schemes**

A review identified approximately 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 program. 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 politicized selection process, because they were only provided at irregular intervals, or because they were community-based rather than individual or household schemes.

The list of rewarded schemes changed as the program progressed. Three initially targeted schemes were dropped early on due to either 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 program 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 program. Finally, applications for two schemes were continuously rewarded but the reward rates were reduced during the program, as they were perceived to be easier to facilitate. An overview of the 16 schemes ever targeted by the program is provided in Table 1. Further details on the schemes, including eligibility criteria, benefits and application procedures, are provided in the Appendix, Table 13, and in Government of Karnataka and GTZ (2010).

The value of benefits provided by the targeted schemes varied. The social pension schemes (for the elderly, the differently abled and widows) provided a recurring monthly pension of 400 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. Nevertheless, pensions can enhance the self-respect of pensioners and improve their intra-household bargaining position. 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 whether claims are made.

In term of their benefits, the schemes can be categorized into four types:

i) those providing immediate benefits. These include pensions, scholarships, a nutritional program for adolescent girls, a self-employment scheme for differently abled and government schools. All pensions provided regular annual benefit of 4800 rupees, the self-employment scheme for the differently abled provided a one-time benefit (loan amount) of 20,000 rupees, the nutritional program provided provisions in kind (equivalent to annual benefit of 1000 rupees) and scholarship amounts varied between 100 and 400 rupees per annum.

ii) those addressing specific contingency of the households and involving monetary

contributions from beneficiaries. These include the National Health Insurance Scheme (RSBY), Vajpayee Arogyasree, Insurance for Drivers of Commercial Vehicles, National Pension Scheme - Lite, Janashree Bima Yojana, Aam Admi Yojana and the National Rural Employment Guarantee (NREG). They provided benefits to households only when claims are made. These schemes tend to be attractive but the benefits are hard to quantify in expected terms.

iii) those providing future benefits. The Bhayalakshmi scheme provides a substantial lump-sum amount to girls on attaining 18 years of age, subject to certain conditions.

iv) one of the schemes, the Construction Workers Welfare Fund, has several components including insurance, immediate and long-term benefits. The scheme is generally considered attractive, but the value of the bundle to individual households is hard to quantify.

Towards the end of the project, facilitation 'drives' were organized to promote two particular schemes: the Construction Workers' Welfare Fund and Insurance for Drivers of Commercial Vehicles. 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 organize the drives, and shared the rewards.

The pilot program ended on 31 March 2014, and applications facilitated after this date were not rewarded. According to the project administration, the total number of households screened over the course of the pilot was 278,000. Of these, 260,000 were found to be eligible for at least one scheme. Applications were submitted for 47% of eligible households during the program period, thus indicating a considerable gap between the number of eligible beneficiaries and the number of applications submitted. However, 72% of applications submitted were approved by the end of the project. (Some of the remaining 28% would be approved over the following weeks or months.)

## 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 evaluation 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>7</sup>

Of the 150 selected GPs, 100 were randomly assigned to the treatment group, and the project administration agreed to open centers in these. The remaining 50 GPs were assigned to the control group, and the project administration agreed not to open centers 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 also opened an additional 150 centers in locations of their choice, which were not part of the evaluation. In all, 250 centers were opened. The identity of treatment and control GPs was not assigned at the time of the baseline survey, and not explicitly revealed to the survey enumerators at follow-up. The enumerators were employed directly by the researchers and not affiliated with the project administration.

The statistical power of the main test was estimated ex ante, assuming significance level 0.05, a minimum detectable effect size of 0.2 standard deviations, 50 control-group clusters (GPs), 100 treatment-group clusters and cluster sizes of 20 in both treatment and control groups. Although we generally drew 10 households from each of 2 villages within each GP, considering GPs rather than villages as clusters is more conservative. Treatment was also randomized at the GP level.

The intra-cluster correlation ( $\rho$ ) was not known at the design stage, but given the above, statistical power would be 0.96 with  $\rho = 0.05$ , 0.85 with  $\rho = 0.10$  and 0.65 with

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<sup>7</sup>The three GPs with ‘pre-pilot’ centers were excluded from the draw.

$\rho = 0.20$ . Ex-post, intra-cluster correlations in the representative sample control group, for the main outcome variables, were 0.120, 0.032 and 0.050 for the number of programs aware of, applied for and obtained, respectively. Using the larger of these numbers and assumptions as above gives a statistical power of 0.81.

Compliance with the experimental protocol was imperfect. At the time of the follow-up survey, centers had been opened in only 71 out of the 100 GPs in the treatment arm. Centers 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 program effects if non-compliance was non-random. This could happen if, for example, there was some tendency that the 71 treatment GPs in which centers 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 program. We also use the original assignments to compute intention-to-treat (reduced-form) estimates.

A referee suggested that we should check whether centers were preferentially opened in villages where the pre-existing level of awareness was higher. To investigate this, we regressed actual treatment on baseline awareness, both for the complete set of villages, and also within the intended treatment group only. In both cases, the estimated coefficients (not reported) were small and not statistically significant, suggesting that actual treatment did not happen preferentially in villages with higher pre-existing awareness levels. We also ran a village-level regression (not reported) of actual treatment on intended treatment and a number of village characteristics. The only village characteristic that predicts actual treatment over and above intended treatment is the proportion of informal-sector workers in the village. Given that the program targeted informal-sector workers (Section 2), this is perhaps not surprising.

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 centers the original facilitator had left. The average center had been open for 19 months when the main survey was fielded.

## **3.2 The Household Surveys**

The analysis is based entirely on data from independently fielded household surveys, rather than administrative data.

While treatment was at the level of the GP, not all villages in the jurisdiction of 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 a single 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 program 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 subsidized bank loans. The SHGs studied here are government-sponsored, but SHGs linked to NGOs also exist. The decision by the program implementers (GIZ and the 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 in November 2010, it was decided that the facilitators would target all informal-sector worker households in their GP, not only those affiliated with SHGs. As a consequence, 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 this 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 program. Informal-sector workers constitute a large majority of India's labor force. The survey was conducted in June–September 2013, that is, some 32–35 months after the opening of the first centers. 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>8</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.

In Table 4, the first three columns of numbers present household summary statistics for the representative sample, by intended treatment group. There 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.

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<sup>8</sup>Nearly all eligible households agreed to be interviewed.

### 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 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>9</sup> In all, 1198 households from the SHG-member sample were revisited for the follow-up survey.<sup>10</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.

The last three columns of Table 4 present baseline summary statistics for the SHG sample. The variables appear largely balanced across the treatment arms. Comparing the two samples, there appears to be some positive selection into SHG membership: the most important difference is perhaps that heads of SHG households are somewhat less likely to be agricultural laborers. However, the differences are not statistically

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<sup>9</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>10</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.

significant.

### 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.

The questionnaire asked about awareness, application and obtention for each of the 16 schemes targeted by the program at any point (Table 1). But 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. (For example, some schemes have income- or asset-based criteria, and it is hard to replicate exactly how these are measured by the authorities.) 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 schemes for the differently abled were not asked of households in which no such member was present. 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.

Following the suggestion of a referee we have also used as alternative outcome variables awareness, obtention, applications and access as *proportions* of the number of schemes that each household is eligible for. As mentioned, we are unable to establish precise eligibility of a household for every scheme, but we can approximate it. The results based on proportional outcome variables (not reported) are qualitatively similar to those presented below. This is perhaps not surprising, as it is hard to imagine an

intervention affecting scheme *counts* without also affecting *proportions*, or vice versa.

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

## 4 Results

### 4.1 Difference Estimates: The Representative Sample

#### 4.1.1 Main results

The main research question is whether the program increased awareness, applications, obtention or claims for the 16 targeted social schemes. Table 5 presents the main results. In Panel A, 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>11</sup>

Column 1 presents the OLS estimate. The result suggests that households in villages with a center 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

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<sup>11</sup>The household-level controls are household size, caste group (whether the household belongs 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 (population in number of households) and whether the village is a GP headquarters.

variables results. The local-average-treatment-effect (LATE) interpretation is that households living in villages in which centers were opened as a result of random assignment are aware of 0.28 more schemes on average, with a 95% confidence interval of (.06, .49). This compares to a control-group mean of 2.32. 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 informal-sector households aware of one extra scheme each (or 160 households aware of two extra schemes each, et cetera).

It is worth checking the assumptions of the LATE theorem here, since these must hold in order for our second-stage coefficients to be consistent estimators of causal effects. The first assumption is that the instrument should be statistically independent of baseline characteristics. This assumption is satisfied because the instrument (intended treatment) was randomly allocated. The balance checks in Tables 2 and 4 confirm that randomization was successful as far as observables are concerned. Under certain conditions the second assumption, the exclusion restriction, is also satisfied under randomization: since intended treatment was randomly allocated, it could not have affected the outcomes other than through actual treatment.<sup>12</sup> The third requirement, that there is a first stage, is satisfied since intended treatment status clearly made actual treatment more likely (column 3 of Table 5). Fourth, the monotonicity assumption requires that the number of ‘defiers’ is negligible. In our setting, any GP that would not be treated if assigned to the treatment group should also not be treated if assigned to the control group. While compliance was imperfect, 71 out of 100 (71%) intended treatment GPs were actually treated, and only 4 out of 50 (8%) intended control GPs were actually treated. While there is no way to rule out the possibility that the 29 intended-treatment GPs that were not actually treated would have received treatment if they had been in the intended control group, or that the 4 actually-treated villages in the intended control group would not have received a center if assigned to treatment, it seems unlikely.

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<sup>12</sup>This is true in the absence of general equilibrium or spillover effects. However, we believe that the intervention was too small to have non-negligible general equilibrium effects. On spillovers, see below.

Under these assumption, the estimated second-stage coefficients are consistent estimators of LATE. These estimates are valid for *compliers*: in our case, GPs that were actually treated and in the intended treatment group, but that would not have been treated if not assigned to the intended treatment group.

To be clear: we estimate the effect of a village-level intervention (the establishment of a Worker Facilitation Center in a village) on average household outcomes in that village. As with most other community-level programs, this includes any spillover effects arising from, for example, households talking to other households of schemes that they have newly become aware of. This contrasts with the effect one might get if individual households were treated in the absence of spillover effects. However, we believe that the effect *with* spillovers is likely to be the most relevant to policy.

Though the IV estimate of the coefficient of interest is larger than the OLS estimate, the OLS point estimate (0.084) is still within the 95% confidence interval of the IV estimate (.06, .49). To the extent that the estimates really are different, this suggests that non-compliance was not strategic: if anything, centers were opened in more challenging, rather than easier, GPs.<sup>13</sup>

As the outcome is a non-negative count variable, columns 5 and 6 of Table 5 present Poisson regressions. The outcome variables are over-dispersed and therefore unlikely to be Poisson-distributed in reality, but Poisson regression is robust to mis-specification as long as the logarithm of the expectation is linear.<sup>14</sup> There is, however, still the issue of endogenous treatment. Column 5 in Panel A presents a Poisson regression of the awareness variable on the intended treatment group, corresponding to the linear intention-to-treat specification in column 2. The estimated coefficient is 0.068. Since

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<sup>13</sup>As mentioned in Section 3, there is some evidence that centers were preferentially opened in locations with a higher proportion of informal-sector workers. But since targeting villages with a higher proportion of households eligible for the program might be expected to make facilitation easier, and thereby increase the OLS estimate relative to the IV rather than reduce it, this is unlikely to be the main cause of the divergence.

<sup>14</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 approach is less appealing than it may seem, because the upper bound, at least for obtention, is not fixed at 16 but rather varies according to the number of schemes that each household is eligible for.

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 modeled as a switching/threshold variable with a normally-distributed latent variable, akin to the probit estimator.<sup>15</sup> The estimated coefficient is 0.066, again corresponding to a marginal treatment effect of 0.16 schemes per household.

Overall, Panel A of Table 5 suggests that the program had a positive and statistically significant effect on awareness.

Panels B and C present equivalent 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 Panel A, but repeated for clarity. None of the specifications identify a significant effect of the program on either applications for or obtention of schemes. The 95% confidence interval for the 2SLS estimate of the effect on applications is (-.09, .08), so the true effect is likely to be small. For the effect on obtention, the 2SLS confidence interval is (-.06, .32), so that we are much less certain about the size of the true effect.

Beyond obtention, for a handful of schemes it is also meaningful to study *utilization*, that is, whether a claim has been made. In the case of insurance, obtaining the scheme (being covered) is clearly distinct from utilizing it (making a claim). Similarly, utilizing the employment guarantee scheme (applying for work) is distinct from joining it (obtaining a job card). The schemes for which we tracked utilization are the National Rural Employment Guarantee, the Construction Workers' Welfare Fund, the National Health Insurance Scheme, Janashree Bima Yojana and Vajpayee Arogyasri. Table 5, Panel D, presents regressions with identical specifications to the panels above, except

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<sup>15</sup>Instrumental variables Poisson regression implemented with the Generalized Method of Moments (GMM) does not allow discrete endogenous covariates.

that the outcome variable is scheme utilization. The effect of the program on utilization is not statistically significant for any specification. Here, the 95% confidence interval is  $(-.02, .03)$ , so the true effect size is likely to be very small.

As we have four main outcome variables, the standard issue of multiple hypotheses testing arises: the 0.05 significance level for the four individual tests, reported in Table 5, may be too generous. However, the 2SLS effect of the program on awareness (with a raw  $p$ -value of 0.011) remains statistically significant with a family-wise error rate of 0.05 when correcting for multiple-hypotheses testing using the Romano and Wolf (2005) method (run with 1000 iterations). The three other outcome variables remain not significant.

#### **4.1.2 Further analysis based on the representative sample**

Constructing alternative, *binary* outcome variables permits analysis of whether the effects are on the extensive or intensive margin. That is, did the program mainly affect those who were already aware of at least one scheme, or rather those who were aware of no scheme previously? Regression results, not reported, suggest that awareness was increased mainly among those who were already aware of at least one scheme. The analysis also confirms that the intervention had no effect on applications, obtention or utilization.

There was considerable variation in the age of the centers at the time of the survey. In the appendix, analysis is presented in which the duration of exposure to the program, in months, is used as an alternative treatment variable. The results are qualitatively similar to the above.

Table 6 presents estimates of treatment effects for various (partly overlapping) categorizations 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 utilization is only tracked for a handful of variables, so further sub-categorization is not meaningful.) The first row presents results for all targeted schemes, and thus reproduces the main coefficients from column 4 of Table 5, Panels A–C. The second row presents results for the five schemes that were consistently rewarded throughout the program, 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 had a stronger effect. There is also a marginally significant, positive effect on applications for schemes administered by the Department of Labour. As the facilitator program was also hosted by the Department of Labour, this may indicate that inter-departmental co-operation was lacking.

The last row presents a 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.

It is of interest to look at the effects on awareness, application, obtention and utilization of individual schemes. Table 7 presents two-stage least-square estimates of treatment effects for each of the targeted welfare schemes, each corresponding in regression specification to column 4 in Table 5, Panels A–D. The outcome variables are binary awareness, application and treatment indicators for each scheme, and actual treatment is instrumented by intended treatment as above. Blank cells indicate regressions that were not identified or, in column 4, schemes for which utilization (claims made) is not meaningful or tracked in the surveys.

The regressions seem to indicate that the program had the strongest effects on the Construction Workers' Welfare Fund (awareness, applications and obtention) and Insurance for Drivers of Commercial Vehicles (applications). There is also a negative and possibly spurious effect on the obtention of pensions for the differently abled. However, correcting for multiple hypotheses testing using the Romano and Wolf (2005) procedure, the only individual scheme effect that remains statistically significant at the 0.05 level is that on awareness of the Construction Workers' Welfare Fund.

An Associate Editor pointed out that it is of policy interest to know how big the increase in awareness is *among those households who are eligible for the scheme*. To look at this, we repeated the individual-scheme analysis in Table 7, but included in the regressions only those households who are, to the best of our knowledge, eligible for each scheme. The results (not reported) yield only two individually significant effects: those on awareness of the Construction Workers' Welfare Fund and of the Nutritional program for adolescent girls. The coefficient on the Construction Worker's Welfare Scheme is 0.38. We interpret this as the program increasing the proportion of households aware of the Construction Worker's Welfare Fund, among those who are eligible for it, by 38 %-points. Compared to a control-group mean awareness of 14% among the eligible, this is clearly a very large effect.

The effect on awareness of the nutritional program for adolescent girls, among those eligible for this scheme, is also individually estimated to be 11 %-points and significant at the 5% level. However, the significance of this effect did not survive Romano–Wolf correction.

Rather than analyzing 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 over-sampled. Analysis of the combined data (not reported) yield qualitatively similar results to the above: there is a significant effect of the program on awareness, but not on applications, obtention or utilization.

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

Longitudinal data are available for 1198 SHG-member households who were observed both at baseline and follow-up. Table 8 presents two-stage least-squares difference-in-difference regressions where the potentially endogenous interaction term *actual treatment*  $\times$  *after* is instrumented by *intended treatment*  $\times$  *after*. (The un-interacted treatment variables drop out due to the inclusion of household fixed effects.) The results suggest no significant effects of the program on awareness, applications, obtention or utilization. (However, the effect on scheme obtention is positive and 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.<sup>16</sup>

An Associate Editor pointed out that effects in the Self-Help Group sample may be muted if there are spillovers between these groups, caused by the sharing of information between groups. However, government-sponsored SHGs in this area are village-specific, and our understanding is that there is hardly any communication between SHGs in different GPs. For these reasons, and also because the sample GPs are generally quite far from each other, we believe that spillovers between different GPs do not play a big

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<sup>16</sup>An Associate Editor pointed out that the knowledge levels can be checked empirically. We do not have baseline data for the SHG sample, but we can compare knowledge levels at endline in the control group between the SHG and representative samples. Regressing awareness (count) on SHG-sample status in the control group at endline shows that the typical SHG-sample household is aware of 0.2 more schemes, from a base of 2.3. The p-value of this difference is 0.07 when standard errors are clustered at the GP level. The estimated difference between the groups is small but similar in magnitude to the estimated effect of the program on awareness. While this does not constitute conclusive evidence that pre-existing knowledge levels explain why there was no significant effect among SHG households, but it seems consistent with that story.

role.

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

Individual scheme effects for the Self-Help Group sample are reported in the appendix. After correcting for multiple-hypotheses testing using the Romano and Wolf (2005) procedure (with 1000 iterations), the only effects that remain significant at the 0.05 level are awareness and obtention of the Construction Workers' Welfare Fund, and awareness of Insurance for Drivers of Commercial Vehicles.

### **4.3 Subjective Welfare Outcomes**

The household survey included questions intended to capture any effect of the program on three 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 9, 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 program on agreement with each statement, where actual treatment is instrumented by intended treatment. 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 program 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 program is not associated with a statistically significant change in the proportion of households agreeing with statement 3. These findings are perhaps surprising, given the lack of effects on applications and obtention 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 that people are happy or grateful that the schemes exist and are being marketed and facilitated, even if they themselves have not benefited.

One could perhaps imagine 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 program. However, this seems unlikely. Field anecdotes suggest that most people do not hesitate to criticize the government or existing benefits. It should also be emphasized that the enumerators were directly employed by the researchers and had no affiliation with the government or the implementing agency.

## 5 Discussion

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

We believe we have followed best practice in relying on independently collected survey data rather than administrative records (which in any case did not exist for control areas). However, as pointed out by an Associate Editor, the fact that the main outcome variables are based on self-reported data could mean that they are subject to measurement error. We have no reason to believe that the distribution of measurement error would be different in treatment versus control areas. But even so, the presence of measurement error would tend to dampen the estimated effect of the program. It is possible, therefore, that we would have found larger and more significant effects if we had more objectively reported outcome measures.

Analysis of individual programs (Tables 7 and 12), after correcting for multiple-hypotheses testing, provide suggestive evidence that the awareness effect may be driven primarily by two individual schemes, the Construction Workers' Welfare Fund and Insurance for Drivers of Commercial Vehicles. Taken at face value, the individual-scheme regressions for the SHG sample suggest that the program may also have been linked to increased obtention of the Construction Workers' Welfare Fund.

These two schemes have several important features which, individually or in combination, could help explain why the program may have had some success in pushing them. First, they 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, these two schemes were less than five years old at the time the program started. (Insurance for Drivers of Commercial Vehicles was launched two years *after* the program 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, they 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 were both administered by the Department of Labour. While the intention of the centers was to act as ‘one-stop shops’ for schemes run by several departments, in practice co-ordination proved difficult in many cases. Feedback on design features constraining access to these schemes was regularly communicated to the top officials of the Department of Labour, and corrective measures were taken. It is, therefore, possible that the program, which was itself a project of the Department of Labour, was more successful in facilitating schemes offered by the Department of Labour.

While we do not identify positive effects of the program on overall scheme applications and obtention, we know from the project administration that more than 100,000 scheme applications were 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 or awareness. 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 program of this kind may be worthwhile even if the overall number of applications is not affected. The benefits of assistance (time saved, convenience) are difficult to quantify, but should be compared to the cost of running the program.

A referee suggested interacting the treatment with pre-existing awareness levels or education, in order to look for heterogeneous treatment effects. When we split the SHG sample by baseline awareness of schemes, we find a positive and significant effect on scheme obtention for households with low baseline awareness—but no other significant effects (not reported). It is possible that awareness is no longer significant because of a loss of power, and that the effect on obtention is significant for low-awareness

households because the facilitator submitted applications on their behalf without informing them. When we interact the treatment with a binary variable indicating literacy of the household head in the representative sample, we find no significant coefficients at all (not reported), again, possibly due a loss of statistical power.

One may ask how the program could have had an effect on awareness but not on applications or obtention. In general, awareness may be regarded as a prerequisite to applying for a scheme, and application must in turn precede obtention. It is, therefore, not implausible that an intervention may have an impact on the first link of this chain, but not the subsequent ones.

Other factors, specific to the context studied, may also have hindered application facilitation. Discussions with facilitators, respondent households and the project administration suggest the following.

First, providing information was significantly less costly than submitting applications. Awareness could be created at low cost by addressing pre-existing community groups, and by handing out printed material including leaflets and brochures. In contrast, facilitating applications required meeting individuals and households in their homes, in many cases more than once. Obtaining supporting documents, for example caste certificates and documentation relating to income, residence or disability, also required time and effort. There were also significant transport and opportunity costs associated with submitting applications to the relevant government offices.

Second, while our results suggest an improvement in scheme awareness, it does not follow that households obtained the level of knowledge required to make an application, or even to make a decision about whether to apply. In fact, 8% of the total sample did not apply for any schemes despite awareness of the centers and schemes they were eligible for. Some of them attributed this to ‘lack of in-depth information on schemes’, ‘facilitator not providing adequate information’, ‘facilitator not following up’ and so

on.<sup>17</sup>

Third, applications for three pension schemes included in this study (for the elderly, widows and the differently abled) were not accepted for a period of 18 months during the project.<sup>18</sup>

Finally, a feature of the bonus system may have limited the effect on the number of applications submitted. Recall that facilitator remuneration included 6000 rupees of fixed monthly payment plus a bonus per facilitation of 67 rupees, but only for up to 30 facilitations per month. This may have led some facilitators to limit themselves to 30 applications per month, even if they could have done more. Anecdotal evidence suggests that this did happen, but without analyzing administrative data we cannot know how widespread it was. In any case, this may have limited the number of applications submitted per month in some cases, but would not have stopped applications altogether.

While the idea of single-window service centers is to increase beneficiary convenience and reduce costs, it became apparent that individual government departments saw reasons to resist. In part, they perceived the initiative, which originated from the Department of Labour, as a threat to their own influence and prestige within government and also their visibility to the public. One government department explicitly asked the program to stop facilitating their schemes. This intra-government resistance would have made the facilitation of applications more difficult, while not affecting their effort in spreading awareness of schemes.

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<sup>17</sup>This should not, however, be taken as evidence that the facilitators were not doing their job. First, facilitators were required to present written receipts from the relevant government departments acknowledging each submitted application. This was the basis for calculating their bonus pay, and we know that a large majority of facilitators received at least some bonus pay every month, and many received the maximum bonus month after month. Second, there was a system of random spot checks of facilitators in the field. Where problems were found, the facilitators were asked to improve their performance or, in a few rare cases, dismissed and replaced. Third, during the initial screening exercise, high-level information on several schemes would have been provided, but no applications submitted. After the initial screening, facilitators would need to prioritize their work and may not have had the time to return to all households before the project ended.

<sup>18</sup>The implementing agency, the Directorate of Social Security and Pensions, decided to focus on weeding out bogus beneficiaries in this period. This hiatus contributed to a sharp decline in the number of beneficiaries for these schemes between 2010–11 and 2011–12, and in expenditure on the schemes (Government of Karnataka, 2018).

The program was successful in establishing a significant number of operational centers. It also succeeded in improving awareness of schemes. But there is suggestive evidence that the schemes that were most successfully promoted were 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 program to push them. Ultimately, the scope for one-stop centers was perhaps not as large as it may have appeared. We conclude that, in a context of fragmented welfare provision, the potential for 'pushing welfare' was limited and largely linked to schemes that are relatively new and do not require intra-government coordination.

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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 dis-continued
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 Pan-chayat 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
Department of Women and Child Development	Pension for the differently abled	Disability	1977	No	Yes, but applications suspended by govt.
	Old age pension	Pension	1964	No	Yes, but applications suspended by govt.
	Widow pension	Pension	1984	No	Yes, but applications suspended by govt.
Department for Empowerment of Differently Abled and Senior Citizens	Nutritional program for adolescent girls	Health	2005-06	No	Only initially
	Bhagyalakshmi	Mixed	2006	No	Only initially
	Adhara self-employment scheme for the differently abled	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 (pension for the differently abled, 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. Insurance 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 informal-sector 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 center opening	1.33 (0.53)
Female facilitators in post since center 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 center in months	19.4 (4.91)
Observations	75

*Notes:* Summary statistics for facilitators and centers. The statistics pertain to the 75 actually treated communities; 71 in the intended treatment group and 4 in the intended control group. Standard deviations in parentheses.

Table 4: Household Summary Statistics

	Cross-sectional sample (follow-up only)			Longitudinal (Self-Help Group) Sample, at Baseline		
	Control	Treatment	Difference	Control	Treatment	Difference
<b>Household characteristics</b>						
Scheduled caste / tribe	0.38 (0.49)	0.41 (0.49)	0.027 (0.021)	0.31 (0.46)	0.35 (0.48)	0.046 (0.029)
Pucca house	0.15 (0.36)	0.15 (0.36)	0.00018 (0.015)	0.13 (0.33)	0.13 (0.33)	-0.00062 (0.020)
Household size	5.52 (2.58)	5.4 (2.52)	-0.12 (0.11)	5.67 (2.57)	5.64 (2.58)	-0.026 (0.16)
Age of household head	49.1 (12.1)	49.5 (12.2)	0.4 (0.52)	48.9 (12.4)	48.7 (12.2)	-0.21 (0.75)
Household head is married	0.85 (0.36)	0.82 (0.38)	-0.024 (0.016)	0.85 (0.36)	0.83 (0.38)	-0.020 (0.023)
Female household head	0.15 (0.36)	0.16 (0.36)	0.0074 (0.016)	0.14 (0.35)	0.17 (0.37)	0.023 (0.022)
Illiterate household head	0.53 (0.5)	0.5 (0.5)	-0.026 (0.021)	0.45 (0.5)	0.48 (0.5)	0.027 (0.030)
Household head is agricultural wage laborer	0.45 (0.5)	0.46 (0.5)	0.011 (0.021)	0.31 (0.46)	0.35 (0.48)	0.037 (0.029)
At least one household member is differently abled	0.067 (0.25)	0.074 (0.26)	0.0072 (0.011)	0.057 (0.23)	0.073 (0.26)	0.017 (0.015)
Self-Help Group member	0.47 (0.5)	0.5 (0.5)	0.034 (0.021)	0.99 (0.11)	0.99 (0.087)	0.0047 (0.0058)
<b>Outcome variables</b>						
Number of schemes aware of	2.32 (1.44)	2.5 (1.57)	0.17*** (0.066)	2.02 (1.26)	2.01 (1.37)	-0.0095 (0.081)
Number of schemes applied for but not (yet) obtained	0.26 (0.51)	0.24 (0.52)	-0.014 (0.022)	0.2 (0.47)	0.25 (0.54)	0.047 (0.032)
Number of schemes obtained	1.27 (1.1)	1.32 (1.17)	0.049 (0.049)	1.06 (0.94)	0.98 (0.97)	-0.078 (0.059)
Number of schemes utilized	0.015 (0.13)	0.016 (0.13)	0.0014 (0.0056)	0.15 (0.36)	0.13 (0.34)	-0.024 (0.021)
Observations	805	1655	.	406	792	.

*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 columns, the numbers in parentheses are the standard errors of the difference  $t$ -tests. The last four rows present outcome variables. The first three columns of numbers present statistics for the representative sample, while the last three columns present baseline summary statistics for households in the Self-Help Group panel. The latter is not representative of all informal-sector worker households and therefore not preferred for the main analysis. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 5: Main Results

	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A:</b>	Number	Number	Actual	Number	Number	Number
<b>Awareness</b>	of schemes	of schemes	treatment	of schemes	of schemes	of schemes
Control-group mean: 2.32	aware of	aware of		aware of	aware of	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)	
<b>Panel B:</b>	Number	Number	Actual	Number	Number	Number
<b>Applications</b>	of schemes	of schemes	treatment	of schemes	of schemes	of schemes
Control-group mean: 0.26	applied	applied		applied	applied	applied
	for but	for but		for but	for but	for but
	not (yet)	not (yet)		not (yet)	not (yet)	not (yet)
	obtained	obtained		obtained	obtained	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)	
<b>Panel C:</b>	Number	Number	Actual	Number	Number	Number
<b>Obtention</b>	of schemes	of schemes	treatment	of schemes	of schemes	of schemes
Control-group mean: 1.27	obtained	obtained		obtained	obtained	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)	
<b>Panel D:</b>	Number	Number	Actual	Number	Number	Number
<b>Utilization</b>	of schemes	of schemes	treatment	of schemes	of schemes	of schemes
Control-group mean: 0.015	utilized	utilized		utilized	utilized	utilized
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)	
<b>All panels</b>						
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 (Panel A), has applied for but not (yet) obtained (Panel B), has obtained (Panel C) or has utilized, that is, has made a claim on (Panel D). 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 6: Effects by Scheme Category

	(1) Heard of	(2) Applied for but not (yet) obtained	(3) Obtained
All targeted schemes	0.28** (0.11) <i>2.32</i>	-0.0051 (0.044) <i>0.26</i>	0.13 (0.097) <i>1.27</i>
Consistently rewarded schemes	0.099* (0.060) <i>0.51</i>	0.0028 (0.0094) <i>0.01</i>	0.046 (0.043) <i>0.26</i>
New schemes (launched in 2005 or later)	0.17** (0.083) <i>1.33</i>	0.017 (0.022) <i>0.08</i>	0.056 (0.072) <i>0.85</i>
Contributory schemes (financial outlay required)	0.087** (0.040) <i>0.22</i>	0.015** (0.0072) <i>0.01</i>	0.050 (0.032) <i>0.13</i>
Labour Department schemes	0.080** (0.038) <i>0.20</i>	0.012* (0.0070) <i>0.01</i>	0.048 (0.032) <i>0.13</i>
Non-targeted schemes	0.095 (0.086) <i>1.59</i>	0.018 (0.014) <i>0.03</i>	0.014 (0.060) <i>0.78</i>

*Notes:* Two-stage least-squares regressions estimates of treatment effects for categories of schemes. Control-group means of dependent variables are shown in italics. The dependent 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 program, 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 program. 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: Individual Scheme Effects

	(1) Heard of	(2) Applied for but not (yet) obtained	(3) Obtained	(4) Utilized
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 (NREG)	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)	
Pension for the differently abled	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 program 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 differently abled	-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 utilization 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 utilization 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 8: 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 utilized
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
Control-group means of dependent variables at baseline		2.02	0.20	1.06	0.15
	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 9: 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
Control-group means of dependent variables	0.273	0.555	0.288

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

## Appendix: Further analysis and robustness checks

This appendix contains additional robustness checks and an analysis of individual scheme effects for the Self-Help Group sample.

There was considerable variation in the age of the centers at the time of the survey. Duration of exposure to the program 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 program, in months, at the time of the survey. Exposure is set equal to the age of the center in months in locations with a center, and zero elsewhere. As above, intended treatment (binary) 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 program, and the coefficient is highly significant.<sup>19</sup> The second-stage regressions in columns 2–4 corroborate what was found above: the program had a statistically significant positive impact on the number of welfare schemes a household is aware of (at a rate of 0.014 more schemes per month of exposure, relative to a control-group mean of 2.32), but no statistically significant impact on the number of schemes applied for, obtained or utilized. The confidence intervals around the latter three coefficients are tight, giving some confidence that the true effect sizes are very small.

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 to what extent this affects 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 excluded. The findings from above are confirmed: facilitators seem to affect awareness, but have no significant impact on applications, obtention or utilization.

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<sup>19</sup>Note that the first-stage coefficient in Table 10 is different from the ones in Table 5, column 3, because the treatment variable is different.

Table 12 presents individual scheme effects for the Self-Help Group sample. While not representative of the target population as a whole, we have panel data for this sample, so that we can compute difference-in-difference estimates while instrumenting for treatment with intended treatment. Apart from studying individual scheme effects, the specifications correspond to the regressions presented in Table 8. There are positive and significant effects (at the 5% level) of the program for the Construction Workers' Welfare Fund (awareness, applications and obtention), the National Health Insurance Scheme (utilization), Insurance for Drivers of Commercial Vehicles (awareness). There is also a negative and possibly spurious effect on obtention of Government residential schools. However, after correcting for multiple-hypotheses testing using the Romano and Wolf (2005) procedure (with 1000 iterations), the only effects that remain significant at the 0.05 level are awareness and obtention of the Construction Workers' Welfare Fund, and awareness of Insurance for Drivers of Commercial Vehicles.

Table 13 presents further details on the eligibility criteria, benefits, documentation requirements and application procedures of the targeted schemes.

Table 10: Exposure Duration as Alternative Treatment Variable

	(1) Exposure to program 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 utilized
Intended treatment	12.8*** (1.13)				
Exposure to program 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
Control-group means of dependent variables	1.00	2.32	0.26	1.27	0.015
	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 Subsample with No Turnover in Facilitators

	(1)	(2)	(3)	(4)	(5)
	Actual treatment	Number of schemes heard of	Number of schemes applied for but not (yet) obtained	Number of schemes obtained	Number of schemes utilized
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
Control-group means of dependent variables	0.047	2.33	0.26	1.28	0.015
	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: Individual Scheme Effects for the Self-Help Group Sample

	(1) Heard of	(2) Applied for but not (yet) obtained	(3) Obtained	(4) Utilized
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 (NREG)	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)	
Pension for the differently abled	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 program 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 utilization indicators, and the interaction of actual treatment and 'after' is instrumented by the interaction of intended treatment and 'after', as in Table 8. 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 utilization 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 utilization 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 13: Further Information on Welfare Schemes

Scheme	Eligibility	Key benefits	Documents required	Application procedure / fee
Construction Workers' Welfare Fund	<ul style="list-style-type: none"> <li>Construction workers in the age group of 18 – 60</li> </ul>	<ul style="list-style-type: none"> <li>Maternity benefit</li> <li>Death benefit and funeral expenses</li> <li>Reimbursement of medical expenses</li> <li>Loan for purchase of construction equipment and for construction of house</li> <li>Support for marriage expenses of up to two children</li> <li>Pension</li> </ul>	<ul style="list-style-type: none"> <li>Age proof and certificate from contractor and labor inspector</li> </ul>	<ul style="list-style-type: none"> <li>Workers should approach a trade union representative or labor inspector in the taluk to enroll.</li> <li>Registration/renewal fee of Rs 25 per annum</li> <li>Rs 10 monthly subscription</li> </ul>
National Health Insurance Scheme (RSBY)	<ul style="list-style-type: none"> <li>Up to five persons from the Below-the-Poverty-Line (BPL) households.</li> </ul>	<ul style="list-style-type: none"> <li>Cashless health insurance, including for pre-existing conditions, but for in-patient treatment only</li> <li>Cover of up to Rs 30,000 per year per household</li> </ul>		<ul style="list-style-type: none"> <li>Beneficiaries are enrolled in their village by the insurance company, and a smart card is issued.</li> <li>Payment of Rs 30 annual registration fee</li> </ul>
National Pension Scheme - Lite	<ul style="list-style-type: none"> <li>Informal-sector workers aged 18–60</li> </ul>	<ul style="list-style-type: none"> <li>End benefit not defined</li> </ul>	<ul style="list-style-type: none"> <li>Proof of identification and age</li> <li>Photo</li> <li>Bank account details</li> </ul>	<ul style="list-style-type: none"> <li>The application is to be submitted to an 'aggregator', an agency to which collecting applications and issuing cards is outsourced.</li> <li>Rs 1000 per annum contribution from the beneficiary, with a matched contribution from the government</li> </ul>

Table 13: Further Information on Welfare Schemes

Scheme	Eligibility	Key benefits	Documents required	Application procedure / fee
Insurance for Drivers of Commercial Vehicles	<ul style="list-style-type: none"> <li>Private vehicle drivers with badge for commercial vehicles</li> <li>Aged 20–70</li> </ul>	Up to Rs 200,000 in the event of death or disability	<ul style="list-style-type: none"> <li>Driver's license</li> <li>Badge number</li> <li>Photo</li> </ul>	<ul style="list-style-type: none"> <li>Applications to submitted either at the District Office of an outsourced agency or at the Jana Snehi centers at the taluk level.</li> </ul>
Vajpayee Arogyasri	BPL ration card holders	<ul style="list-style-type: none"> <li>Up to Rs 150,000 of tertiary care per household through network hospitals for catastrophic illness with an additional buffer of Rs 50,000 for a family of five on a case-to-case basis.</li> <li>Pre-existing diseases are included.</li> </ul>	BPL ration card	<ul style="list-style-type: none"> <li>BPL households are automatically enrolled. For utilization, patients are referred to network hospitals and health camps conducted in the each taluk or district headquarters</li> <li>Rs 300 premium borne by the government</li> </ul>
Government residential schools	<ul style="list-style-type: none"> <li>Students from 1st to 5th standard from Scheduled Castes (SC), Scheduled Tribes (ST) and Other Backward Castes (OBC)</li> </ul>	<ul style="list-style-type: none"> <li>Free tuition, boarding and lodging facilities</li> <li>Books and uniforms</li> <li>Medical expenses</li> </ul>	<ul style="list-style-type: none"> <li>Caste certificate</li> <li>Income certificate (if sought)</li> </ul>	<ul style="list-style-type: none"> <li>Applicants should contact the Social Welfare Officer in their taluk with their caste certificates</li> </ul>

Table 13: Further Information on Welfare Schemes

Scheme	Eligibility	Key benefits	Documents required	Application procedure / fee
Janshree Bima Yojana (JBY)	<ul style="list-style-type: none"> <li>Men and women aged 18–59 and working in 44 specified occupations</li> <li>Minimum group size 25</li> <li>Barring those physically handicapped by birth, persons becoming differently abled after accidents are eligible for disability benefit</li> </ul>	<ul style="list-style-type: none"> <li>Life insurance benefit of Rs 30,000–75,000</li> <li>Accident leading to disability Rs 37,500–75,000</li> <li>Scholarship of Rs 1200 per annum per child for up to two school-going children in 9th – 12th standard (including vocational courses)</li> </ul>	<ul style="list-style-type: none"> <li>Proof of age</li> <li>BPL ration card / income certificate</li> <li>Separate application for scholarship along with mark sheet</li> <li>Bank details</li> </ul>	<p>NGOs, as nodal agencies, should submit a proposal to LIC along with the list of members to be covered under JBY. The policy will be in the name of the NGO. JBY members whose children are eligible for scholarship should fill in an application form and submit every year for renewal.</p>
Pre-/post-matric scholarship	<ul style="list-style-type: none"> <li>School/ college-going boys and girls of Backward Castes or SC or ST, and not staying in government hostels.</li> <li>Post-matric scholarships are open for from 11th standard up to post-graduate university studies.</li> <li>Household income must be below Rs 15,000 per annum.</li> </ul>	<ul style="list-style-type: none"> <li>Rs 75–100 for pre-matric students, Rs 300–400 for post-matric students</li> </ul>	<ul style="list-style-type: none"> <li>Statement from head of educational institution</li> <li>Mark sheet of previous class</li> <li>Income certificate issued by the tehsildar</li> <li>Caste certificate</li> </ul>	<p>Students apply through their school or college. A plain statement from the head of school that the student is currently enrolled and attending regularly is sufficient. In addition to this, a mark sheet is also required. Along with the application, the income and caste certificates of the students should be provided. The school forwards the applications to the Taluk Panchayat, where they are screened and the selection is made.</p>

Table 13: Further Information on Welfare Schemes

Scheme	Eligibility	Key benefits	Documents required	Application procedure / fee
National Rural Employment Guarantee	Anyone aged 18 or above and residing in rural areas, and willing to undertake manual work	<ul style="list-style-type: none"> <li>Up to 100 days of work per household per year</li> <li>Government minimum wages paid, which should be equal for men and women</li> <li>Unemployment allowance of up to 50 percent of the prevailing daily wage if employment not provided within 15 days of demand</li> </ul>	<ul style="list-style-type: none"> <li>To register and obtain a 'job card', applicants need to provide details of a bank or post office account for payment, along with photos of household members.</li> <li>Registered beneficiaries need only their job card to apply for work.</li> </ul>	The GP issues the job cards. Job card holders can apply for work by specifying the number of days of work they need, in writing or orally.
Aam Admi Bima Yojana (AABY)	BPL and landless	<ul style="list-style-type: none"> <li>Life benefit: Rs 30,000-75,000</li> <li>Disability: Rs 37,500-75,000</li> <li>Scholarship of Rs 100 per month for up to two children studying in 9th to 12th standard</li> </ul>	<ul style="list-style-type: none"> <li>BPL ration card</li> </ul>	The application is sent to the tehsildhar who assesses it.
Disability Pension	<ul style="list-style-type: none"> <li>Differently abled with at least 40 percent disability, and belonging to BPL category.</li> <li>Annual income of no more than Rs 6000</li> </ul>	<ul style="list-style-type: none"> <li>Rs 400 per month if the degree of disability 40-75 percent. Rs 1000 per month if the degree of disability is more than 75 percent.</li> </ul>	<ul style="list-style-type: none"> <li>Disability ID / medical certificate</li> <li>Income certificate</li> </ul>	<p>Applications are to be submitted at the tehsildar's office along with medical certificate or disability identity card and income certificate.</p> <p>The tehsildar's office makes the final selection.</p>

Table 13: Further Information on Welfare Schemes

Scheme	Eligibility	Key benefits	Documents required	Application procedure / fee
Old age pension	<ul style="list-style-type: none"> <li>• For State scheme, 65 years and above of age, and for National scheme it is 60 or above</li> <li>• Below specified annual household income limit</li> <li>• Not in receipt of other pensions</li> </ul>	Rs 400 per month	<ul style="list-style-type: none"> <li>• Occupation Certificate</li> <li>• Identification Card</li> <li>• Passport-sized photo</li> <li>• Below-the-Poverty-Line card / income certificate</li> <li>• Proof of age</li> </ul>	The application is sent to the tehsildar's office.
Destitute Widow pension	Widows aged 18–60 whose income is less than Rs 6000 per annum.	Rs 400 per month.	<ul style="list-style-type: none"> <li>• BPL ration card or income certificate</li> <li>• Death certificate of husband</li> </ul>	Application to be submitted to tehsildar.
Nutritional program for adolescent girls	Girls aged 11–18 weighing less than 35 kg	<ul style="list-style-type: none"> <li>• 6 kilograms of rice per month at a subsidized rate of Rs 6.16 per kilo for 6–9 months depending on whether weight of 35 kg is attained.</li> </ul>	<ul style="list-style-type: none"> <li>• Certificate of weight from anganwadi teacher</li> </ul>	All adolescent girls are periodically weighed at the anganwadi centers, and those weighing less than 35 kg are identified as beneficiaries.

Table 13: Further Information on Welfare Schemes

Scheme	Eligibility	Key benefits	Documents required	Application procedure / fee
Bhagyalakshmi	<ul style="list-style-type: none"> <li>Girls born after 31 March 2006</li> <li>Parents must have undergone terminal family planning methods so that the total number of children does not exceed three</li> <li>Up to two girls per household covered</li> <li>Annual household income limit of Rs 12,000 (rural) and Rs 17,000 (urban)</li> </ul>	<ul style="list-style-type: none"> <li>Lump sum amount paid out when girl turns 18</li> <li>Annual scholarships of Rs 300–1000 for girls attending school regularly</li> <li>Medical insurance up to Rs 25,000 per annum</li> <li>For the parents, life insurance of Rs 42,500–100,000 sum assured</li> <li>Disability insurance for parents, Rs 37,500–75,000 for total disability</li> </ul>	<ul style="list-style-type: none"> <li>BPL ration card or income certificate</li> <li>Birth certificate of the child</li> <li>Declaration by parents that they do not have more than three children, or that they will undergo compulsory sterilization after the third child</li> <li>Declaration by parents that they will not send the girl to work or get her married before age 18, and that they will educate her to at least 8th standard and will bring her up without gender bias</li> </ul>	<p>Application is to be submitted to the staff of the department at the village level. The district officer selects based on the recommendation of sub-district staff and forwards to the Life Insurance Company of India, where it is further assessed before being accepted.</p>
Adhara self-employment scheme for the differently abled	<ul style="list-style-type: none"> <li>40 percent disability or more</li> <li>Annual household income of no more than Rs 11,500</li> <li>Unemployed and above 18 years of age</li> </ul>	<ul style="list-style-type: none"> <li>Box type kiosk worth Rs 15,000</li> <li>Up to Rs 20,000 interest free loan.</li> </ul>	<ul style="list-style-type: none"> <li>Certificates on degree of disability, income, age and residential status of applicants</li> </ul>	<p>The application is to be manually submitted either at the district office or to Multipurpose Rehabilitation Workers at taluk level or to Village Rehabilitation Workers at the village.</p>

*Notes:* Scheme details as of 2010. Further information in Government of Karnataka and GTZ (2010).