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# Barriers to Labor Migration for the Rural Poor

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# Barriers to Labor Migration for the Rural Poor*

## Abstract

Migration into stable urban manufacturing jobs offers large returns, yet rural-to-urban migration remains limited in developing countries. Using a field experiment in Bangladesh, we sequentially relax constraints on information, skills, liquidity, and migration risk for poor rural youth seeking urban apparel-sector jobs. Information and training alone have limited effects. By contrast, combining training with stipend, paid internship, and transition support increases migration, employment, and earnings, reduces poverty, and produces intra-household spillovers. We find that reducing job-search uncertainty and migration risk is central to labor-market entry, and skills-training programs may require complementary financing, migration assistance, and job-search support for scalable impacts.

## JEL classification

J61, O15, R23

## Keywords

field experiment, internal migration, vocational training, internship, Bangladesh, apparel sector

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## 1 Introduction

Extreme poverty remains overwhelmingly rural: more than three-quarters of the global extreme poor reside in rural areas, where income opportunities are limited (World Bank, 2024). Migration offers a potential pathway out of poverty by allowing workers to move from low-productivity rural areas to higher-productivity urban labor markets. Large and persistent rural–urban wage gaps in low-income economies imply substantial private returns to internal migration, particularly in expanding manufacturing sectors. Despite these apparent gains, poor rural households migrate at rates far lower than economic models would predict (Lewis, 1954; Harris and Todaro, 1970).

This gap between high returns and low migration presents a fundamental puzzle. Why do poor households fail to take advantage of profitable urban employment opportunities? More specifically, what frictions prevent rural youth from converting urban labor demand into stable wage employment, and can relaxing these frictions meaningfully increase migration and reduce poverty? We study these questions in the context of urban apparel-sector jobs in Bangladesh, where labor demand is concentrated in cities, while poor rural youth remain weakly connected to these opportunities. Our focus is on *stable* structural migration into manufacturing employment—a pathway that requires both physical relocation and sector-specific skill acquisition—rather than temporary or seasonal migration as a risk-diversification strategy (Bryan et al., 2014).

A growing body of work highlights several barriers that may limit stable rural-to-urban migration. First, information frictions increase job-search costs because rural workers often lack reliable information about job requirements, vacancy channels, and working conditions in urban labor markets (Beaman and Magruder, 2012). In standard job-search frameworks, such frictions reduce expected returns to migration and raise reservation thresholds, leading to under-migration. Second, skill constraints limit access to formal-sector employment. Many urban jobs in manufacturing require sector-specific skills that are difficult to acquire in rural locations. While access to credit can facilitate investment in human capital (Becker, 1962; Schultz, 1961), poor households often face binding credit and savings constraints that impede such investments. Even when training programs are fully subsidized, the opportunity cost of participation and limited liquidity make it difficult to undertake skill acquisition. Missing or incomplete credit markets for both training and migration further restrict mobility for the poor.

Third, migration itself is inherently risky. Uncertainty about job-search success, working conditions, and urban living environments can deter migration, particularly for risk-averse individuals. These concerns are amplified by the importance of informal networks in hiring processes, especially in sectors such as apparel, where referrals play a central role (Heath, 2018). For individuals lacking networks or financial buffers, the risk of a failed job search may outweigh even large expected wage gains. Crucially, these frictions interact. Information about vacancies is of limited value to a worker who lacks the skills to apply. Skills are of limited value to a worker who cannot finance migration. Similarly, both skills and liquidity matter little if the residual risk of job-search failure is high enough to deter the attempt entirely. These interactions imply that interventions that address only one barrier in isolation may have limited effects on labor reallocation.

This paper studies these frictions in rural Bangladesh through a unique field experiment that se-

quentially adds support for migration and employment. We implement a set of interventions that combines job information, vocational training, financial support, and paid internship experience for economically marginalized rural youth. This layered design identifies the incremental effect of adding each program component to the preceding package and allows us to assess which increasingly comprehensive packages are sufficient to generate migration and employment. We conduct this study in northern Bangladesh, one of the poorest regions of the country, where migration to the rapidly expanding apparel sector remains limited despite strong labor demand. While the sector has generated millions of jobs and is concentrated in urban centers such as Dhaka and Chittagong, participation from poorer regions remains low. This setting provides a natural context to examine why substantial wage differentials do not translate into large-scale spatial labor reallocation.

We recruit 2,215 youth, aged 18–35, from moderate- and ultra-poor households in Gaibandha, a highly disadvantaged district in northern Bangladesh, targeting individuals interested in urban apparel-sector employment. Despite substantial wage differentials between rural and urban labor markets,[1] baseline mobility among our sample is extremely limited: 98.8 percent of participants report never having migrated outside their district. Although many respondents are broadly aware that urban manufacturing jobs offer higher earnings, they face severe barriers to accessing those opportunities. Knowledge of formal skill-acquisition pathways is particularly limited: only 16.5 percent report having heard of vocational training programs related to apparel-sector employment, and fewer than 1 percent had ever enrolled in such programs. Migration also appears highly risky for this population. Among the small share of participants with prior migration experience, employment outcomes were often unstable and temporary, while migration itself was commonly financed through personal savings or informal borrowing. Participants widely recognize that apparel-sector jobs require technical skills, referrals, and familiarity with urban labor markets, yet most lack credible pathways to acquire these prerequisites.

We randomly assign participants to one of four treatment arms—along with a control group—using a sequentially layered, multi-arm RCT design. The first treatment provides an in-person job and training-related information session in the local vicinity (*Information treatment*), which primarily addresses information frictions related to job opportunities and requirements. The second treatment adds a one-month fully subsidized residential skills training program in Gaibandha (*Training treatment*), addressing constraints in sector-specific human capital. The third treatment combines information and training with a cash stipend to cover migration costs and forgone income (*Stipend treatment*), thereby addressing short-term liquidity constraints. The fourth and most intensive treatment adds a paid factory internship in Dhaka (*Internship treatment*) together with transition support. This design identifies the incremental and cumulative effects of increasingly comprehensive intervention packages.

Our randomized intervention achieved strong overall take-up, though take-up declines with intervention intensity: 66.5 percent of participants enrolled in their assigned treatment, and 86.8 percent

[1]Baseline annual rural wage income averages USD 380 (29,706 BDT; exchange rate: 78 BDT per USD). At six months, employed participants earn approximately USD 804 annually, implying a gross earnings gain of USD 424. Against conservative annual migration costs of USD 138–254 (Microfinance Opportunities, 2018, 2019; Khan and Anker, 2016; Khan and Rashid, 2023), the implied short-run net return is 44-75 percent of baseline rural income, rising to 50 to more than 100 percent under common overtime arrangements.

of enrollees completed their assigned intervention. Using the first participant follow-up, conducted approximately six months after each phase-specific information or training activity, our intent-to-treat estimates show that the more comprehensive packages lead to substantial improvements in employment outcomes. The *Stipend treatment* increases employment in the apparel sector by 7.3 percentage points relative to the pure control group. In contrast, providing information alone, or in combination with training, does not yield statistically or economically meaningful effects. Adding the internship and transition-support package increases employment by a further 20.4 percentage points. As a result, nearly one-quarter of participants in the internship group are employed in the manufacturing sector at the six-month follow-up, compared with only 4.8 percent in the pure control group. We observe similarly large impacts on job retention, months worked, working hours, earnings, remittances, and saving behavior, alongside reductions in borrowing and family-related stress. The employment effect persists at the 18-month follow-up, especially for the internship group, indicating that temporary workplace exposure can generate durable labor-market attachment.

We show that rural-to-urban migration is a key intermediate channel linking the intervention to improved labor-market outcomes. Relative to the pure control group's 6 percent migration rate, the *Stipend treatment* and *Internship treatment* packages increase migration by 18 and 36 percentage points, respectively. These gains are accompanied by substantial improvements in employment, earnings, and remittances, particularly for the *Internship* group. At six months, employed participants earn an average of BDT 5,268 (USD 67) per month and remit approximately 36 percent of their income to their origin households. We find no adverse effects on physical or mental well-being for the stipend and internship groups compared with the pure control group. These individual gains extend to the household level through higher remittance inflows, reductions in income poverty and greater labor-force participation among other household members.

Beyond the direct participant effects, we document positive job-support outcomes and intra-household spillovers. Participants assigned to the internship package are more likely to inspire others in their communities to seek urban employment and to help these contacts secure jobs. These outcomes suggest that workplace placement may facilitate employer matching and credible referrals. We also find substantially larger treatment effects among participants classified as risk-averse at baseline. This pattern is consistent with the *stipend* and *internship* treatments reducing uncertainty and the downside risk associated with migration and job search. However, *internship* treatment also provides job exposure, employer access, work experience, transportation and accommodation assistance, and other transition support. Therefore, *internship* treatment could alter the returns to migration in addition to the risk that the participants face.

The strong employment outcomes among participants assigned to the internship package are consistent with meaningful demand for trained workers alongside labor-market frictions that leave unemployed or underemployed workers disconnected from vacancies (Breza and Kaur, 2025). A natural question is why firms do not invest in training rural unemployed youth to fill this gap. Our focus group discussions with factory owners suggest that retention concerns are important: firms fear that training investments will be appropriated by competing firms if workers switch jobs, leading to high turnover. This poaching risk may discourage firm-sponsored training and contribute to persistent skill

shortages, a common feature of labor markets in LMICs (Donovan et al., 2023).

Our paper contributes to the literature on internal migration and spatial frictions in developing countries. Existing work shows that poor households often under-migrate despite large rural-urban income gaps because mobility is constrained by liquidity, migration risk, incomplete insurance, and, in some settings, social norms (Bryan et al., 2014; Lagakos, 2020; Amirapu et al., 2022). We extend this literature by studying a pathway into stable urban wage employment that requires both migration and sector-specific skill acquisition, linking the migration decision to labor-market entry.

We speak to the growing literature on labor-market frictions in low-income settings. Prior work emphasizes the role of information, social networks, and matching frictions in shaping job access and retention (Beaman and Magruder, 2012; Heath, 2018; Banerjee and Chiplunkar, 2024; Chakravorty et al., 2024). In particular, social networks can transmit valuable information through referrals, firms often rely on network-based hiring, and imperfect information about worker preferences and job conditions can generate substantial matching inefficiencies. Our findings show that interventions that jointly reduce search costs and uncertainty—especially through paid internships—can substantially improve job matching and retention. In our setting, where referrals are important and new migrants have limited information about urban work conditions, internships serve not only as work experience but also as a mechanism that provides credible information, facilitates employer connections, and reduces the downside risk of a failed migration attempt. By tracing impacts on take-up, migration, employment, remittances, household poverty, and medium-run persistence, we show that relaxing migration-related frictions can amplify the returns to skill acquisition and extend benefits beyond the individual worker to the origin household.

Third, we also contribute to the literature on vocational training and active labor market programs. Existing reviews and meta-analyses show that average impacts of training programs are often modest and highly dependent on design (McKenzie, 2017; Card et al., 2018; Kluve et al., 2019). A growing body of work argues that these limited effects may arise because training programs often fail to address complementary constraints—such as search frictions, imperfect information, and weak links to labor demand—rather than reflecting low returns to skills (Carranza and McKenzie, 2024). Related evidence from South Asia suggests that training programs can improve labor-market outcomes in some settings, but effects vary by design, target population, and the extent to which programs connect trainees to employment opportunities. In India, Maitra and Mani (2017) find positive medium-run effects of a subsidized stitching and tailoring program for low-income women. In Nepal, Chakravarty et al. (2019) report positive effects of a large skills-training and placement program, though the evidence comes from a quasi-experimental evaluation. In Bangladesh, Das (2021) shows that BRAC's apprenticeship-based on-the-job training program improved short-run labor-market participation and earnings among disadvantaged youth, with more mixed evidence on persistence and the incremental value of classroom training. More recent evidence from India further suggests that job-information interventions can improve self-selection into training and placement outcomes (Chakravorty et al., 2024).

Our study embeds training within the broader migration decision. In our setting, successful labor-market entry requires not only acquiring skills but also overcoming liquidity constraints and reducing the risks associated with moving to an urban labor market. We show that training alone has limited

effects, but becomes highly effective when bundled with interventions that relax financial and risk constraints. This suggests that the modest impacts documented in much of the prior literature may reflect incomplete program design rather than low returns to skills.

This paper is organized as follows. Section 2 discusses the study background, while Section 3 discusses the sample selection and RCT design. Section 4 describes the data sources and the empirical method. Sections 5 and 6 analyze the program uptake and impact, respectively, and Section 7 explores the mechanism, followed by the conclusion in Section 8.

## 2 Background

### 2.1 Economic Transformation and the Apparel Sector

Bangladesh has sustained remarkable economic growth over the past two decades, averaging approximately 6 percent annually,[2] accompanied by substantial reductions in poverty. The national poverty rate fell from 48.9 percent in 2000 to 18.7 percent by 2022 (World Bank, 2023). Central to this transformation has been the rapid expansion of the apparel sector, which has emerged as the country's dominant export industry, currently accounting for more than 80 percent of export earnings and employing over four million workers (Kathuria and Malouche, 2016; Impact Investment Exchange (IIX), 2025).[3] Beyond its export contribution, the sector has generated large numbers of formal wage-paying manufacturing jobs, making it one of the most important engines of non-agricultural employment growth in Bangladesh. The industry remains geographically concentrated in the urban centers of Dhaka and Chittagong, and approximately 85 percent of its workforce consists of internal migrants (World Bank, 2011).

Despite this structural transformation, labor-market outcomes in Bangladesh remain highly informal and precarious. Although the official unemployment rate stood at only around 3.5 percent in 2022, approximately 85 percent of workers were employed informally (LFS, 2023). This stark gap between low open unemployment and pervasive informality reflects the reality that many individuals classified as "employed" continue to rely on irregular, low-productivity, and subsistence-oriented activities. A key factor sustaining this vulnerability is the limited availability of marketable skills: participation in vocational training remains extremely low, with only 1.46 percent of individuals aged 15 and above reporting any formal vocational training in the latest Labor Force Survey of Bangladesh (LFS, 2023).

### 2.2 The Migration Paradox in Northern Bangladesh

These constraints are especially pronounced in northern Bangladesh, one of the country's poorest and most disaster-prone regions. Households here depend heavily on seasonal agricultural labor and are acutely exposed to flooding, riverbank erosion, and agricultural seasonality (Khandker and Mahmud, 2012). Poverty rates in the Rangpur region consequently remain well above the national average, exceeding the rest of the country's average by approximately 8 percentage points as of 2022 (World Bank, 2023). Given these conditions, migration to urban manufacturing centers would appear to be a rational strategy for income diversification and poverty reduction, particularly in light of the rapid expansion of the apparel sector and the positive net savings it appears to generate. Financial

[2] World Development Indicators, accessed July 24, 2025.

[3] Export Promotion Bureau (2024), "Pocket Export Statistics 2023–24" Export Promotion Bureau (2024).

diary evidence from Bangladeshi garment workers reports mean monthly earnings of approximately USD 110 against mean monthly expenditures of approximately USD 66 (Microfinance Opportunities, 2018, 2019),[4] suggesting that urban apparel-sector employment generates economically meaningful net welfare gains relative to the highly seasonal and volatile income streams of rural labor markets, even after accounting for elevated urban living costs.

Yet migration rates from northern Bangladesh remain strikingly low (Alam and Mamun, 2022). While the national internal migration rate stands at approximately 9.8 percent, Bangladesh Bureau of Statistics records consistently show lower out-migration from the northern region relative to the rest of the country (BBS, 2015, p. 53). Drawing on nationally representative Household Income and Expenditure Survey data spanning 2000 to 2022, we document that migration into the apparel sector among the poor in the Rangpur region remained below 4 percent throughout this period—approximately 1 percent in 2000, rising to 3.76 percent in 2010, and declining modestly to 3.43 percent thereafter (see Figure 1). The persistence of this gap between the apparent economic returns to migration and actual migration rates constitutes the central empirical puzzle motivating this study.

### 2.3 Labor-Market Frictions and the Returns to Migration

Understanding why this paradox persists requires examining the specific frictions that characterize both the demand and supply sides of the apparel labor market. On the demand side, hiring was largely informal during the study period and remains heavily relationship- and referral-based despite the sector's scale and continued growth. Most vacancies are filled through gate-side notices, walk-in interviews, referrals, and short practical assessments rather than formal recruitment channels.[5] Non-management positions are typically filled within 7–8 days, and managerial roles within 12–15 days, reflecting both strong labor demand and high turnover. Workers without prior experience generally enter as "helpers" at the lowest wage tier across cutting, sewing, and finishing lines, while those with relevant skills or training can access higher-paying junior machine operator or machine operator positions directly (Jalil et al., 2016). Prior skills and experience thus allow workers to bypass one to two years of on-the-job learning otherwise required for entry-level advancement, generating a meaningful skill premium at the point of entry. Although Bangladesh's apparel sector is often characterized as a major source of formal employment for women, it is not female-exclusive. Women remain highly represented in lower-grade production roles, particularly sewing and helper positions, but men are also employed across various production roles. Consistent with this broader occupational structure, recent LFS-based estimates document a growing male share of RMG employment, reaching 61 percent in 2022 (World Bank, 2025). The more recent minimum wage structure, effective since December 2023, further illustrates the skill gradient in the sector, setting monthly wages of BDT 7,500 for helper positions and at least BDT 12,500 for machine operators (Sikder, 2025).

On the supply side, frictions are equally significant. Firms invest little in training rural workers, partly because of concerns over turnover and poaching, leaving prospective migrants with limited employer-sponsored pathways into the sector. Awareness of formal training opportunities also remains

[4] Using the study-period exchange rate of BDT 78 per USD in mid-2015. This applies to the rest of the paper.

[5] Factories rarely circulate recruitment information through websites or newspaper advertisements for entry-level production positions.

limited among potential workers, even as many recognize that skills, education, and referrals are important for securing employment (Jalil et al., 2016). Among the small share of respondents in our sample with prior migration experience, moves were typically financed through personal savings or informal borrowing and frequently resulted in unstable or unsuccessful employment outcomes. Once in the city, migrant workers often rely on adaptive consumption strategies—including shared accommodation, communal meals, and overtime work—to manage urban living costs. The Bangladesh Institute of Development Studies estimates that the sector will require an additional 1.5 million skilled workers over the coming decade (Rahman and Hossain, 2017), yet the market has so far failed to generate the coordinated mechanisms needed to connect this latent demand with large pools of underemployed rural workers in regions such as Rangpur. It is precisely this market failure—the interaction of information, skill, liquidity, migration-risk, and matching frictions—that our intervention is designed to address.

### 2.4 Program Origins and Institutional Context

Our field experiment builds directly on an earlier skills program implemented by our NGO partner, Gana Unnayan Kendra (GUK), in Gaibandha district between December 2010 and July 2013, with funding from DFID's SHIREE initiative.[6] That program trained poor, unskilled youth from northern Bangladesh for sewing-machine operator roles in the urban apparel sector. Using program funds, GUK established a dedicated residential training institute in Gaibandha, staffed by industry experts, which served as the institutional foundation for our experimental intervention. This prior engagement enabled GUK to accumulate operational expertise in residential skills training for jobs in the apparel sector, lending credibility and quality of implementation to our intervention.

## 3 Participant Selection and Experimental Design

### 3.1 Sample Selection

To avoid contamination and minimize spillovers from the past GUK-DFID program, we collected sample participants from all sub-districts of Gaibandha, except Gaibandha Sadar, from which the previous program participants were selected. Our sample was selected using community-based targeting (CBT) combined with proxy means testing (PMT). We first conducted participatory rural appraisals (PRAs) in each village with our implementing partner, GUK, to compile a CBT roster, then administered a short survey (SS) to generate PMT scores and screen candidates by demographics and poverty status.[7] With this, GUK targeted 6 upazilas (sub-districts) and 50 unions (sub-sub-districts) for our study,

[6] SHIREE (Stimulating Household Improvements Resulting in Economic Empowerment) was a DFID-funded program supporting economic empowerment interventions for ultra-poor households in Bangladesh. The GUK program described here was funded under Phase 3 of the Innovation Round; see `https://www.shiree.org/innovation-round-3/`.

[7] Eligibility was based on the following criteria: completed primary education (industry requirement for minimum education), irregular income sources and regularly unemployed, aged between 18 and 35 (this was the industry requirement for the desired hiring age), from moderate-poor or ultra-poor (extreme-poor) households, and willing to change their current occupation. Poverty status was measured based on household conditions, assets, livestock, income-generating activities, and marital status (including widowed or female-headed households). This criterion for targeting ultra-poor individuals was developed in collaboration with our implementing partner, using their poverty classification definition. GUK considers a household to be ultra-poor [moderately poor] if the household matches four [two] of the following six criteria: (1) have less than 10 decimal of land (including agricultural, homestead and fallow), (2) do not have any functional productive asset (like livestock), (3) the house is mainly built with straw and mud with only one room, with no separate arrangement such as bedroom, living room and dining room, (4) does not possess any basic sanitary, water or hygiene amenities (like tube-well and sanitary latrine), (5) does not have any regular source of income and remains regularly unemployed, and (6) the household head is widowed, elderly or a disabled person.

which covered 231 villages.[8] We then verified poverty markers through household visits and finalized the eligible sample. By design, only one participant was selected per household.[9] With this process, a total of 2,215 households participated in our study.

### 3.2 RCT Design

The experimental design follows directly from the four barriers emphasized above: information frictions, skill constraints, liquidity constraints, and uncertainty surrounding migration and entry into urban manufacturing employment. We therefore designed a sequentially layered, multi-arm randomized controlled trial in which each successive treatment arm adds another form of support. This design identifies the incremental effect of adding each program component to the preceding package and allows us to assess which increasingly comprehensive packages are sufficient to support pathways to stable urban labor market entry.

For the experiment, eligible participants were enrolled on a rolling basis across four implementation phases. Upon recruitment, all eligible participants completed a baseline survey, which was administered on a rolling basis from September 2013 to August 2014. Because GUK could accommodate only a limited number of trainees at a time,[10] the intervention was implemented through four sequential phases following the program rollout schedule. Within each phase, participants were randomly allocated to equal proportions across the four treatment arms, ensuring comparable treatment-group sizes throughout the phased rollout. Under this design, 191 individuals were assigned to a pure control group and received no intervention, while the remaining 2,024 participants were distributed equally across the four treatment arms, yielding 506 participants per treatment arm. The relatively small control group reflects an implementation constraint. Given GUK's previous skill program implementation in the study area, the management was reluctant to exclude a large share of eligible applicants from program support due to community expectations and reputational concerns. Consequently, maintaining a large pure control group was not operationally feasible, and a smaller no-intervention comparison group was negotiated at the design stage. The details of our four treatment arms were as follows.

**i) Information (T1).** Participants assigned to T1 received a half-day, union-level information session on urban apparel-sector employment. The session covered factory working conditions, working hours, wages and overtime, urban living conditions, hiring procedures, required skills, and available training opportunities. It also included a moderated question-and-answer session conducted by GUK industry experts. This treatment arm was designed to relax information frictions about job opportunities, workplace conditions, and the process of obtaining factory employment. The total cost was approximately BDT 160 (about USD 2) per participant, including venue, moderator, administrative, logistical, and lunch costs.

**ii) Information + Training (T2).** Participants assigned to T2 received the information intervention plus a full-board, one-month residential training program in Gaibandha lasting 22 working days. The training covered sewing-machine and overlock-machine operation, the fundamentals of the apparel production process, and occupational safety and health. A typical training day combined

[8] These sub-districts are: Fulchari, Gobendagonj, Polashbari, Sadullahpur, Saghata and Sundargonj.

[9] In cases where there was more than one participant eligible for the program from the same household, our survey team randomly selected one participant by lottery.

[10] GUK could accommodate approximately 100 trainees in a single training batch.

instructor-led technical instruction with extensive hands-on practice designed to develop the speed, precision, and consistency required to meet industry-standard stitching quality. This emphasis on practical skills reflects the hiring practices of the apparel sector, where workers are typically evaluated through walk-in practical tests rather than formal credentials (Rahman and Hossain, 2017). Participants who successfully completed the program received a training certificate issued by GUK.[11] This treatment arm was designed to relax both information frictions and sector-specific skill constraints. The total cost of the intervention (including direct and administrative costs) was approximately BDT 10,500 (about USD 125) per participant.

**iii) Information + Training + Stipend (T3).** Participants assigned to T3 received the information and training components, along with a stipend of 3,500 BDT (approximately USD 45). The stipend was intended to help cover the opportunity cost of full-time training, forgone income, and initial migration-related expenses. The amount was calibrated using the local daily wage rate of approximately 160 BDT (about 2 USD) per day. The stipend was paid in two installments: one at the beginning of training and the other upon completion. Participants who discontinued after receiving the first installment were not eligible for the second installment but were not required to repay the first. This arm was designed to relax information, skill, and liquidity constraints. The total cost was approximately BDT 15,225 (about USD 195) per participant (including direct and indirect costs).

**iv) Information + Training + Stipend + Paid Internship (T4).** Participants assigned to T4 received all components of T3 plus a one-month paid internship in an apparel factory in Dhaka. GUK arranged internship placements through agreements with 14 garment factories. These placements were feasible because of GUK's prior apparel-sector training experience and its existing relationships with factories from earlier programs. Internship openings varied across factories depending on availability at the time of placement. In response to participants' logistical needs, GUK also facilitated group transportation to Dhaka and helped participants identify dormitory accommodation through its factory networks. This internship and transition-support package provided direct exposure to factory work, urban living conditions, employer networks, and potential pathways to stable employment, while also reducing uncertainty and downside risk. The total cost (including direct and administrative costs) was BDT 19,425 (about USD 249) per participant. Figure 2 summarizes the experimental design.

The layered structure of the RCT permits identification of both incremental and cumulative treatment effects. The comparison $T1 - C$ estimates the effect of information alone. The comparison $T2 - T1$ identifies the marginal contribution of vocational skills training. The comparison $T3 - T2$ captures the incremental effect of adding the stipend, while the comparison $T4 - T3$ identifies the additional effect of the internship and transition-support package. Comparisons between each treatment arm and the control group capture the cumulative effects of the increasingly comprehensive packages.

Participants assigned to a treatment arm were informed of their treatment status and implementation schedule prior to the start of their assigned activities, allowing sufficient time to make household, employment, and travel arrangements. Control-group participants did not receive any program-related notification. GUK conducted training in twelve batches across the four implementation phases. Details

[11] Completion certificates can, in principle, reduce information asymmetries between workers and employers. However, because apparel-sector recruitment relies primarily on practical assessments rather than formal credentials (Rahman and Hossain, 2017), certification carries limited independent weight at the hiring stage in our context.

of treatment delivery and follow-up survey timing are reported in Table 1. Throughout the analysis, we include phase fixed effects to account for the phased implementation of the intervention and village fixed effects to absorb local labor-market and network heterogeneity.[12]

## 4 Data and Estimation

### 4.1 Data

We administered a detailed baseline survey, collecting rich individual- and household-level information, including demographics, education, employment history, assets, migration experience, and unincentivized measures of risk preferences.[13] We subsequently conducted two phone-based participant follow-up surveys, scheduled approximately 6 and 18 months after each phase-specific information session or Gaibandha-based residential training activity. In addition, we conducted an in-person survey in Gaibandha approximately 12 months after the intervention by revisiting participants' origin households.

Employment-duration and earnings outcomes at the first follow-up include both point-in-time or last-month measures and retrospective measures over the preceding six months. Because the paid internship for T4 treatment arm was part of the treatment package and occurred after the Gaibandha-based training component, retrospective six-month outcomes for T4 could include internship-related work and compensation. We therefore interpret these cumulative six-month outcomes as short-run effects of the full training-to-work transition package, while current employment, last-month outcomes, and the 18-month follow-up provide clearer evidence on post-internship persistence.

Baseline summary statistics for the control group and results of the balance test are reported in Table 2, the latter of which indicates that the treatment groups are mostly balanced in observable characteristics. Among the 76 treatment-control comparisons, only two coefficients are statistically significant at the 10 percent level, and none is significant at the 5 percent level. Moreover, the joint balance tests fail to reject equality across treatment assignments.

The baseline characteristics underscore the severe labor-market constraints faced by our sample. Thirty percent of participants are female, and the average age is approximately 22 years. None completed education beyond the higher secondary level, roughly one-third are currently married, and approximately half belong to ultra-poor households. Professional skills are virtually absent, asset ownership is minimal, and most participants rely on irregular agricultural wage labor or are unemployed.

To facilitate participant tracking after the intervention, all respondents received a mobile phone SIM card at baseline and airtime top-up incentives for participating in follow-up phone surveys. These efforts contributed to low attrition rates: the overall attrition rates were 2.93 percent at six months, 2.62 percent for the 12-month household survey, and 6.19 percent at 18 months. There is also no evidence that our results are driven by differential attrition. Across treatment groups, attrition rates are similar in each survey round (Appendix Table A1), and the attrition regression also shows no

[12] Some villages contained more eligible participants than could be accommodated in a given phase, given the capacity constraints; hence, some participants from the same village appeared in multiple phases.

[13] The baseline survey lasted approximately 90 minutes. Data collection was conducted by the South Asian Network on Economic Modeling (SANEM).

significant differences across treatment arms (Appendix Table A2).

### 4.2 Estimation

Let $Y_{ijp}^t$ denote an outcome measured for individual $i$, residing in village $j$ and assigned to implementation phase $p$, in survey round $t$. We estimate intention-to-treat (ITT) effects using the following specification:

$$Y_{ijp}^t = \alpha + \sum_{k=1}^{4} \beta_k^t \, 1\{T_i = k\} + \mathbf{X}_i'\theta + \mu_j + \phi_p + \varepsilon_{ijp}^t, \tag{1}$$

where $\beta_k^t$ captures the ITT effect of assignment to treatment arm $k \in \{1, 2, 3, 4\}$ relative to the pure control group. The vector $\mathbf{X}_i$ contains baseline covariates,[14] while $\mu_j$ and $\phi_p$ denote village and implementation-phase fixed effects, respectively. Village fixed effects absorb local labor-market and network heterogeneity, while phase fixed effects account for the phased rollout of the intervention. Standard errors are clustered at the village level.

For most main tables, we pool the pure control and information-only arms into a common base group, termed "Information Pooled with Control" estimation. This choice is motivated by both design and empirical considerations. Since treatment was randomized at the individual level within villages, information provided to T1 participants could in principle diffuse to untreated participants through local social networks. Such information spillovers would make the pure control versus information comparison most vulnerable to interference (Hudgens and Halloran, 2008; Aronow and Samii, 2017). This approach is similar in spirit to Bryan et al. (2014), who show that their information-only migration treatment had no independent effect relative to control and verify that their main incentive effects are robust to alternative comparison groups. The appendix reports specifications that estimate information as a separate treatment arm, with the pure control group as the omitted category. The substantive conclusions are unchanged under both specifications.

Our survey instruments measure a broad set of outcomes related to migration, employment, earnings, remittances, household welfare, aspirations, and social networks. To address concerns arising from multiple hypothesis testing, we group related outcomes into migration, employment, employment duration and hours, earnings and remittances, participant well-being and financial behavior, 18-month employment, and origin-household welfare families. We construct summary indices following Kling et al. (2007) and Anderson (2008). Specifically, each outcome is standardized using the base-category group distribution and transformed into a z-score, after which family-level indices are calculated as equally weighted averages of the standardized outcomes. We additionally report false-discovery-rate-adjusted sharpened $q$-values following Benjamini et al. (2006) and Anderson (2008).

For household outcomes observed both at baseline and follow-up, we estimate treatment effects using an Analysis of Covariance (ANCOVA) specification:

$$Y_{ijp} = \alpha + \sum_{k=1}^{4} \beta_k \, 1\{T_i = k\} + \mathbf{X}_i'\theta + \gamma Y_{ijp}^0 + \mu_j + \phi_p + \varepsilon_{ijp}, \tag{2}$$

[14] These include age, age squared, gender, educational achievement, marital status, ultra-poor status, household size, number of children, household-head status, food insecurity measures, landholdings, occupation categories, and religion.

where $Y_{ijp}^0$ denotes the baseline value of the outcome variable. This specification improves statistical power and is the preferred approach when baseline measures are available.

In addition to the ITT estimates, we report IV estimates of the effects of treatment take-up. Take-up means beginning or participating in the assigned treatment and does not require completion. For the specification that estimates information as a separate treatment arm, let $D_{ki}$ denote take-up of treatment $k \in \{1, 2, 3, 4\}$, corresponding to T1 through T4, and let $Z_{ki} = 1\{T_i = k\}$ denote randomized assignment to treatment arm $k$. We estimate the following second-stage equation:

$$Y_{ijp}^t = \delta_0 + \sum_{k=1}^{4} \delta_k^t D_{kijp} + \mathbf{X}_i'\theta + \mu_j^S + \phi_p^S + \varepsilon_{ijp}^t, \tag{3}$$

where actual treatment take-up is instrumented using the randomized treatment assignment indicators. The corresponding first-stage equations are:

$$D_{kijp} = \pi_{k0} + \sum_{\ell=1}^{4} \pi_{k\ell} Z_{\ell i} + \mathbf{X}_i'\rho_k + \mu_{kj}^F + \phi_{kp}^F + \nu_{kijp}, \qquad k = 1, \ldots, 4. \tag{4}$$

In the pooled specification, the information-only arm is combined with the pure control group, and the model includes take-up and assignment indicators for T2–T4 only. Randomized assignment to the treatment arms provides instruments for treatment take-up. Under the standard IV assumptions of relevance, exclusion, and monotonicity, the coefficients identify local average effects of treatment take-up for participants whose take-up responds to randomized assignment. Because take-up means beginning or participating in the assigned treatment and does not require completion, these estimates should not be interpreted as effects of completing the full assigned package. As such, we retain the ITT estimates as our primary results and report the IV estimates in the appendix. All specifications include baseline covariates and village and implementation-phase fixed effects, with standard errors clustered at the village level.

## 5 Intervention Uptake

We first analyze participants' intervention uptake. Excluding controls, program uptake—enrollment among those assigned—was 67 percent, and completion among enrollees was 87 percent. For the training-related arms (T2–T4), the uptake and completion rates were 64 and 82 percent, respectively. Uptake varied across treatment arms (Figure 3): uptake rates were about 75 percent for both T1 and T3, 60 percent for T4, with 32 percent completing both training and the internship. The lower T4 completion rate reflects the dual demands of residential training and a paid internship that required temporary relocation. Considering only the residential training component, dropout was about 5 percent in T2, 2 percent in T3, and 9 percent in T4.

Gender patterns largely mirror the overall results across arms, except for T4. Women's uptake in T4 is 46 percent, compared with 69 percent for men, and women's completion is 16 percent, compared with 42 percent for men (Figure 4). We asked participants to report the main reason for not taking up the program or discontinuing training (Figure 5). The leading reason was "already managed an income activity" (38 percent), followed by "not interested/afraid to go to Dhaka" (18 percent) and spousal

resistance (15 percent). The descriptive pattern differs by gender (Figure 6): men primarily reported economic factors (57 percent), whereas women more often cited spousal opposition (31 percent).

Table 3 estimates uptake determinants separately by arm. For T4, uptake is lower among women, older participants, and those facing lean-season food insecurity, but higher among those with more than 12 years of education and among Muslims. For T3, uptake is lower among married and less-educated participants and those with lean-season food insecurity, but higher among women, individuals with professional status, those regularly unemployed, and those with year-round food insecurity. Risk aversion is positively associated with uptake in T1 and T4.

Table 4 reports uptake regressions for the full sample and gender-specific subsamples. Relative to information only (T1), training (T2), and the internship package (T4) exhibit lower uptake. In the full sample, T2 uptake is 18 percentage points below the T1 rate ($p < 0.001$). Adding the stipend increases uptake by 19 percentage points relative to T2 ($p < 0.001$), whereas adding the internship package reduces uptake by 14 percentage points overall, with a larger decline among women.

We repeat the analysis for the subsample offered residential training to examine completion across T2, T3, and T4 (Appendix Table A3). Relative to the T2 completion rate of 52 percent, T3 raises completion by about 20 percentage points, while the T4 completion rate is not statistically different from T2. Internship completion for women is approximately 18 percentage points lower, a pattern also reflected in the gender-specific estimates. This difference is consistent with additional mobility and participation constraints for women as reported in World Bank (2025). As the focus of the current paper is employment-related outcomes, we examine the impacts on both men and women combined. Gender-specific impacts and mechanisms will be explored in a separate paper and are therefore beyond the scope of the current paper.

## 6 Impacts

### 6.1 Short-Run Impacts on Participants: Primary Outcomes

#### Employment, Job Continuation, and Earnings

Our primary outcome of interest is employment. Table 5 presents the ITT specification that pools the information treatment arm with the pure control group as the base. The outcomes are any employment (column (1)), employment in the apparel or related sector (column (2)), employment in manufacturing (column (3)), full-time employment (column (4)), and an employment index that combines these four outcomes (column (5)).[15] These outcomes are measured over the six-month recall window preceding the first participant follow-up. For participants assigned to T+S+I, the paid internship was part of the treatment package and may fall within this recall window. Respondents may therefore report internship-related work as employment. Accordingly, the six-month employment outcomes for T+S+I should be interpreted as cumulative short-run effects of the full training-to-work transition package on entry into urban employment, rather than as effects solely on post-internship regular employment.

With this interpretation in mind, the table shows that skill training alone (T) has a small and statistically insignificant effect of 0.118 SD on the employment index. Adding the stipend (T+S) leads

[15] The apparel or related sector includes ready-made garments (RMG), knitting, and textile factories, but it is labeled as "RMG" for ease of presentation.

to an economically and statistically significant increase of 0.474 SD. Further adding the internship-and-transition-support package (T+S+I) generates considerably larger gains of 1.194 SD. The estimated incremental effects are 0.356 SD for adding the stipend to training and 0.720 SD for adding the internship-and-transition-support package to T+S, as shown at the bottom of the table.

The streamlined presentation is intended to focus the discussion on the incremental effects of the training, stipend, and internship-and-transition-support packages. Appendix Table A4 reports ITT estimates that treat information as a separate treatment arm. The table shows that Information alone (T1) does not produce statistically or economically meaningful gains in any of the employment outcomes. Further, other implications remain similar in both quantitative and qualitative terms. For example, the effects of adding a stipend and an internship component on the employment index are estimated at 0.394 SD and 0.794 SD, respectively, and remain highly significant. Not surprisingly, the corresponding IV estimates are much larger at around 0.6 SD and 2.0 SD regardless of pooling information and pure control together or keeping it separate (Appendix Tables A5 and A6). As this pattern holds across the outcomes, we focus on the streamlined presentation by pooling pure control and information arms into a common base group in the main text and report the fully specified RCT estimates in the appendix.

Table 6 examines employment duration and hours. The outcomes are months of employment during the preceding six months (column (1)), weekly wage-work hours in the previous month (column (2)), any wage-related work during the preceding six months (column (3)), weekly wage-related work hours during the preceding six months (column (4)), and a job-continuation index that combines these four outcomes (column (5)). As with the employment outcomes above, retrospective six-month measures for T+S+I may include the internship period and should therefore be interpreted as cumulative short-run work attachment generated by the full transition package. By contrast, last-month-based outcomes, as reported in column (2), provide clearer evidence on post-internship labor-market engagement. Participants assigned to the training-plus-stipend package (T+S) work approximately 0.355 additional months, while those assigned to the internship-and-transition-support package (T+S+I) work approximately 1.075 additional months relative to the pooled base-group mean of 0.641 months. The same ordering appears for wage-work hours and the job-continuation index. When the information treatment is treated as a separate arm, information has a null effect on employment duration and hours, and the remaining conclusions are similar (Appendix Table A7). The corresponding IV estimates are reported in Appendix Tables A8 and A9.

Earnings outcomes show a similar pattern. Table 7 reports effects on any income during the preceding six months (column (1)), any income in the previous month (column (2)), total income during the preceding six months (column (3)), income in the previous month (column (4)), and an income index that combines these four outcomes (column (5)). Appendix Table A10 reports the corresponding fully specified ITT estimates with information treated as a separate arm. In the pooled ITT specification, adding the stipend to training (T+S) and adding the internship-and-transition-support package to T+S both increase earnings outcomes, with substantially larger effects for the latter. The adjacent-arm contrasts attribute BDT 2,210 (USD 28.3) to adding the stipend and BDT 5,112 (USD 65.5) to adding the internship-and-transition-support package. Because some six-month

earnings measures may overlap with the internship period for T+S+I, particularly for columns (1), (3) and (5), these estimates should be interpreted as cumulative short-run earnings effects of the full transition package. Appendix Tables A11 and A12 report the corresponding IV estimates.

**Putting the Earnings Gains in Cost Perspective**

To put the earnings gain in cost perspective, we consider the undiscounted payback period. Unlike the net present value or internal rate of return criteria, the undiscounted payback period does not require us to specify the flow of wage over the course of participants' entire life. Using the per-participant costs of BDT 15,225 [19,425] and the six-month ITT earnings gains of BDT 3,691 [8,800] for T3 [T4] reported in Appendix Table A10, the average earnings gain observed over those six months is about 24.2 [45.3] percent of the program cost. Assuming that these gains persist, the corresponding undiscounted payback periods would be approximately 25 [13] months for T3 [T4]. On an incremental basis, the additional cost of the stipend relative to T2 would be recovered in 13 months, while the additional cost of the internship package (inclusive of transition support) would be recovered in 5 months. Because the earnings outcome covers all work rather than apparel employment alone, these calculations should be interpreted as program-cost payback from total earnings gains, not as an apparel-sector wage return. Therefore, even with a modest decline in gains and discounting, the stipend and internship components still yield a reasonably short payback period.

**Remittances**

Table 8 reports remittance outcomes measured over the six-month recall window preceding the first participant follow-up. The outcomes include any remittance (column (1)), the total amount remitted (column (2)), remittance frequency (column (3)), remitting in the previous month (column (4)), and a standardized remittance index (column (5)). As with the employment and earnings outcomes, retrospective six-month remittance measures for participants assigned to T+S+I may include remittances financed by income earned during the paid internship period.

Relative to the pooled base-group mean, participants assigned to T+S remit approximately BDT 1,024 (USD 13) more over the six-month recall window, while participants assigned to T+S+I remit approximately BDT 2,243 (USD 29) more. The remittance index increases by 0.431 standard deviations for T+S and by 1.137 standard deviations for T+S+I. The previous-month remittance outcome, reported in column (4), provides a cleaner measure of ongoing remittance behavior closer to the time of the first follow-up. ITT estimates with information treated as a separate arm are reported in Appendix Table A13, while the corresponding IV estimates are reported in Appendix Tables A14 and A15.

### 6.2 Secondary Outcomes: Aspirations, Health, Financial Outcomes, and Job Support

We next evaluate participants' aspirations, health, financial outcomes, and job support. Since the interventions were designed to facilitate employment in manufacturing, particularly in the apparel sector, a natural concern is whether exposure to factory work and migration-related adjustment adversely affected participants' physical or psychological well-being (Blattman and Dercon, 2016). Table 9 reports standardized measures of aspirations (column (1)), health (column (2)), work-related physical stress (column (3)), work-related mental stress (column (4)), family-related stress (column (5)),

and non-work-related stress (column (6)).[16]

The training-plus-stipend (T+S) and internship-and-transition-support packages (T+S+I) both increase aspirations. We find no evidence that either package worsens self-reported health or increases physical or psychological stress. If anything, both packages reduce family-related stress in the short run, although these effects do not survive adjustment for multiple-hypothesis testing. Appendix Tables A17–A19 report the ITT estimates with information treated as a separate arm and the corresponding IV take-up specifications.

Table 10 reports financial outcomes, including borrowing (column (1)) and savings (column (2)). The T+S+I package reduces the borrowing index by 0.266 SD, while the T+S and T+S+I packages increase the savings index by 0.214 SD and 0.161 SD, respectively.[17] These results suggest that the more intensive packages improved participants' short-run financial position, although six-month financial outcomes for T+S+I may partly reflect income and liquidity generated during the internship period.

The table also reports whether participants inspired others to seek apparel-sector employment (column (3)), whether those contacts obtained jobs (column (4)), and the number of people inspired or placed (columns (5)–(6)). The T+S+I package has positive effects across these job-support measures. These outcomes suggest that treated participants helped transmit information, encouragement, and job-search support to others in their networks. Such effects are substantively important because they indicate the potential for social spillovers beyond directly treated participants. If similar spillovers reached individuals in the pooled base group, our estimated treatment effects would likely understate the total effect of the intervention. Appendix Tables A20–A22 report the corresponding ITT estimates without pooling and the two IV take-up specifications, respectively.

### 6.3 Longer-Term Impacts on Participants

The 18-month follow-up allows us to assess whether the short-run employment gains persist beyond the initial transition period. This is important because some workers may return to their villages after initial migration because of demanding working conditions, homesickness, difficulty adjusting to urban life, or family obligations. Table 11 reports the same employment outcomes as in the six-month analysis, measured at the 18-month follow-up.

The effects remain positive but attenuate over time. The T+S+I package increases the employment index by approximately 0.407 SD, and the effect remains statistically significant after adjustment for multiple-hypothesis testing. The estimate for T+S is approximately 0.153 SD and is less precisely estimated. The comparison between T+S+I and T+S indicates that adding the paid internship and transition-support package continues to contribute to employment 18 months after the intervention.

The attenuation partly reflects convergence in employment outcomes among participants in the pooled base group. For example, the base-group mean for any employment increases from 7 percent

[16] Aspirations were elicited using the question: "Do you think your life will be better in the coming six months compared with now?" The self-reported health index is created from two questions: "My overall health has been good over the last seven days" and "My overall health has improved over the last six months." The stress measures were elicited on a Likert scale, converted into indicators, and standardized. Appendix Table A16 describes the index components.

[17] The savings index combines cash and in-kind savings and indicators for formal bank and savings accounts. The borrowing index combines total borrowing and indicators for borrowing from microfinance institutions, moneylenders, social networks, employers, and shopkeepers. See Appendix Table A16.

at the six-month follow-up to 31.6 percent at the 18-month follow-up. This catch-up may reflect delayed labor-market entry among base-group participants, broader improvements in local or urban employment opportunities, or information and job-search spillovers from treated participants to others in their networks. To the extent that such spillovers could reach the base group, the estimated treatment effects at 18 months would understate the total longer-run effect of the intervention. At the same time, the persistence of larger effects for T+S+I suggests that the paid internship and transition-support package generated employment gains beyond the initial placement period.

The ITT estimates with information treated as a separate treatment arm and the two IV take-up specifications are reported in Appendix Tables A23–A25. Figure 7 combines the six- and 18-month survey rounds to display the dynamics of the employment index. The figure shows that the effects of T+S and T+S+I persist over time, with larger estimates for T+S+I, although the magnitude declines between the first and second participant follow-ups.

### 6.4 Impacts on Origin Households

#### Household Socioeconomic Status

The 12-month household survey collected information on health, income, expenditure, food consumption, savings, borrowing, housing conditions, productive and non-productive assets, and livestock. We group related outcomes and construct standardized indices described in Appendix Table A16. Table 12 reports the pooled-control ITT-ANCOVA estimates. T+S+I reduces the illness index by 0.156 SD (column (1)) and increases the household asset index by 0.155 SD (column (2)), although these estimates are insignificant if adjusted for multiple testing. The coefficients for livestock, housing, savings, and borrowing are not statistically distinguishable from zero once adjusted for multiple hypothesis testing (columns (3)–(6)). Appendix Tables A26–A28 report the ITT estimates with information as a separate treatment arm and the two IV take-up specifications.

#### Remittances and Intra-Household Spillovers

Table 13 reports remittance flows received by origin households. Relative to the pooled base group, households of participants assigned to T+S and T+S+I receive remittances for approximately 0.432 and 0.974 additional months, in the last 12 months, respectively (column (1)). The adjacent-arm comparisons show that adding the stipend increases remittance frequency by approximately 0.220 months, while adding the internship-and-transition-support package increases it by approximately 0.588 months (column (2)). Total remittances received in the last 12 months are also higher for households of T+S+I participants, by approximately BDT 2,972 (USD 38).

The same table examines labor income among other household members. Assignment to T+S+I increases the probability that another household member earns urban labor income by 4.9 percentage points relative to a pooled base-group mean of 9.4 percent (column (4)). It also increases total urban labor income for other household members by approximately BDT 4,901 (USD 63) as reproted in column (5). These estimates indicate intra-household spillovers from assignment to the most intensive package, consistent with treated participants lowering information, accommodation, or job-search barriers for relatives. The corresponding ITT-ANCOVA estimates with information treated as a separate arm and the two IV take-up specifications are reported in Appendix Tables A29–A31.

### Poverty Status

Table 14 reports daily per capita household income and consumption, along with indicators for income and consumption poverty.[18] Assignment to T+S and T+S+I increases daily per capita income by BDT 8.81 and BDT 9.82, respectively, relative to a pooled base-group mean of BDT 66.44 (column (1)). These gains correspond to economically meaningful increases of approximately 13–15 percent relative to the base-group mean. The probability of income poverty falls by 8.1 percentage points for T+S and 12.1 percentage points for T+S+I, relative to a base-group rate of 53.7 percent (column (5)). We find no corresponding effect on total, food, or non-food consumption or on consumption poverty (columns (2)–(4) and (6)). Appendix Tables A32–A34 report the ITT estimates with information as a separate treatment arm and the two IV take-up specifications.

## 7 Anatomy of the Impacts

### Rural-to-Urban Migration

One important aspect of our intervention is its impact on rural-to-urban migration, a key intermediate outcome. Table 15 reports the treatment effects on migration at the six-month follow-up. The outcomes are ever migrated after the intervention (column (1)), currently migrant (column (2)), migration to Dhaka (column (3)), migration to Chittagong (column (4)), and migration outside Rangpur other than Dhaka or Chittagong (column (5)). The table demonstrates that providing information alone (T1) produces little change in migration outcomes. Adding skill training (T2) increases the probability of ever migrating by 6.9 percentage points relative to a control mean of 6.4 percent, although this estimate does not remain statistically significant after adjustment for multiple-hypothesis testing. The incremental comparison between T2 and T1 suggests that training raises ever migration by 5.3 percentage points, but it has no detectable effect on current migration or destination-specific migration.

The effects become substantially larger when the treatment package relaxes liquidity and transition-related constraints. The training-plus-stipend package (T3) increases ever migration by 18.2 percentage points and migration to Dhaka by 6.9 percentage points. The effect on current migration is positive, at 4.2 percentage points, but is only marginally significant before multiple-hypothesis adjustment. Adding the paid internship and transition-support package (T4) generates the largest effects: ever migration increases by 36.6 percentage points, current migrant status by 13.4 percentage points, and migration to Dhaka by 18.9 percentage points. These effects remain statistically significant after multiple-hypothesis adjustment.

The destination-specific results are informative. The large increase in migration is concentrated in Dhaka, the destination directly connected to the apparel-sector employment opportunities facilitated by the treatment intervention. By contrast, we find no meaningful increase in migration to Chittagong, and only a small and statistically fragile increase in migration to other destinations outside Rangpur. This pattern suggests that the intervention did not simply raise general migration propensity. Instead, it increased migration to the labor market targeted by the program, consistent with the internship and

[18] We use an international poverty line of USD 1.90 per person per day, converted to Bangladeshi taka using the relevant purchasing-power-parity conversion factor and expressed in survey-year taka. This produces a threshold of BDT 53.39 per person per day for the 12-month household survey.

transition-support package reducing destination-specific job-search, matching, and relocation barriers.

The adjacent-arm comparisons reinforce this interpretation. Adding the stipend to training increases ever migration by 11.3 percentage points, current migration by 4.9 percentage points, and migration to Dhaka by 6.0 percentage points. Adding the paid internship and transition-support package to T3 generates an additional increase of 18.4 percentage points in ever migration, 9.2 percentage points in current migration, and 12.0 percentage points in migration to Dhaka. There is no corresponding statistically significant increase in migration to Chittagong or to other destinations outside Rangpur. The T4–T3 comparison therefore identifies the incremental effect of the complete paid internship and transition-support package on migration to the program-linked urban labor market, rather than a broad increase in migration to any destination.

**Job-Search Costs and Employer Access**

Table 16 presents descriptive statistics on employment-sector placement at the first follow-up. The table reports, for each treatment arm, the share of participants observed in different employment sectors during the six-month follow-up. The pattern is strongly consistent with the main employment results. Only 3.14 percent of control participants and 4.55 percent of information-only participants are observed in ready-made garment sector employment, compared with 7.31 percent in T2, 12.85 percent in T3, and 25.49 percent in T4. A similar gradient appears for textile employment, where the corresponding shares rise from 1.57 percent in the control group to 6.32 percent in T4. Combining RMG and textile employment, the share increases from 4.71 percent in the control group to 31.81 percent among T4 participants.

The table also shows a sharp decline in the share of participants with no formal employment, from 94.24 percent in the control group to 65.02 percent in T4. While these statistics are descriptive and should not be interpreted as treatment effects, the pattern is consistent with the internship-and-transition-support package helping rural participants enter the program-linked apparel and textile labor market. Separate placement data further show that 56 percent of currently employed T4 participants continue working at their original internship factory, suggesting that the internship may have facilitated not only initial workplace access but also job continuation.

**Heterogeneity by Baseline Risk Aversion**

Table 17 presents treatment heterogeneity by baseline risk aversion, measured using a series of stated-preference questions in the baseline survey. The estimated interactions are small for information and training alone but larger and statistically significant for T3 and T4. For T3, the risk-aversion interaction is 21.1 percentage points for ever migrating (column (1)) and 9.1 percentage points for RMG employment (column (3)). The corresponding interactions for T4 are 42.4 and 27.8 percentage points. While this analysis is exploratory in nature, the larger effects among initially risk-averse participants are consistent with the stipend and internship package reducing uncertainty or limiting the downside consequences of an unsuccessful move. Note, however, that risk aversion may also influence responses to direct placement, employer access, transportation and accommodation assistance, and the expected returns to migration.

## 8 Conclusion

This paper studies why poor rural youth often fail to move into higher-return urban manufacturing jobs despite strong labor demand. The intervention is motivated by a sequence of potentially interacting barriers: limited information, lack of sector-specific skills, liquidity constraints, and uncertainty associated with migration, job search, and retention.

Using a randomized experiment in northern Bangladesh, we show that information alone and information combined with training have limited effects on migration and employment. Training becomes more effective when paired with a stipend that offsets forgone income and migration-related costs. The largest and most persistent impacts arise when the package also includes a paid factory internship. The internship package substantially increases migration, apparel-sector employment, job continuation, earnings, remittances, and savings, with employment effects persisting 18 months after the intervention.

These findings suggest that the modest impacts of many vocational training programs may reflect incomplete program design rather than low returns to skills. In our setting, skills alone are insufficient because workers must also finance migration and enter an unfamiliar urban labor market. The stipend provides financial support, while the internship and transition-support package provides direct exposure to factory work, urban living conditions, employer networks, and pathways to stable employment. The sharp increase in impacts under internship treatment arm, together with the larger effects among risk-averse participants, suggests that reducing job-search uncertainty and downside risk appears to be an important mechanism.

The benefits extend beyond treated individuals. Increased migration and employment generate remittances to origin households, reduce income poverty, and encourage labor-market participation among other household members. We also find positive job-support outcomes: participants assigned to the internship package are more likely to help others in their communities seek and secure urban jobs.

The earnings gains are also large relative to the listed intervention costs. A simple undiscounted benchmark, assuming the persistence of gains, implies that the total internship package (along with residential training and stipend) cost is approximately 13 months of the observed earnings gain, while the incremental cost of adding the internship and transition-support package is approximately 5 months. These figures are based on the assumption that the gains persist and thus provide only short-run payback benchmarks, rather than long-horizon benefit-cost estimates. At the same time, women are substantially less likely than men to take up and complete the internship package, especially when participation requires relocation to Dhaka. This pattern motivates a separate analysis of gender-specific participation and mobility constraints, which we leave for future research.

The external validity of these findings depends on two important conditions. First, the intervention is most relevant in settings where expanding urban or peri-urban labor markets coexist with geographically concentrated pockets of rural poverty and limited autonomous mobility. In such settings, the central market failure is not simply a shortage of skills, but a weak bridge between poor workers and available jobs. Bangladesh's apparel sector provides one such environment: urban factories have demand for trainable workers, while poor rural youth in regions such as Rangpur face information, liq-

uidity, and migration-risk constraints that prevent them from accessing these jobs. Similar conditions may arise in other South Asian economies, including parts of India and Pakistan, where large rural populations, regional labor-market disparities, and efforts to expand labor-intensive manufacturing remain central development challenges (World Bank, 2012; López-Acevedo et al., 2016; World Bank, 2026). They may also arise in some African settings, such as Ethiopia, where industrial parks and light manufacturing have been promoted as channels for job creation (Abreha et al., 2021; World Bank, 2018). However, the results may travel less well to settings where labor demand is weak, manufacturing jobs are not expanding, urban jobs do not offer meaningful returns over rural opportunities, or migration already occurs without substantial frictions.

Second, the internship component requires an institutional arrangement capable of matching trained rural workers to firms. In our setting, the implementing NGO had prior experience and employer relationships that allowed it to negotiate factory placements, arrange transition support, and make the internship credible to both workers and firms. Scaling such a model therefore requires more than adding a short internship to a training curriculum. It requires a functioning skill-to-work transition system: training providers must identify suitable firms, firms must be willing to host and screen trainees, and intermediaries must provide the coordination, trust, and logistical support needed for first-time migrants to enter unfamiliar urban labor markets. Without such institutional capacity, the internship component may not replicate the workplace access, employer connections, and risk reduction observed in this experiment.

Overall, the results show that effective training policy must support the transition from rural origin to urban employment. For disadvantaged youth, connecting skills to jobs requires not only classroom training, but also financial support, employer engagement, job-search assistance, and support for the initial move into an unfamiliar urban labor market. The broader policy lesson is therefore conditional but important: where growing sectors demand labor and poor workers remain spatially and socially disconnected from those opportunities, training programs may be far more effective when embedded in a credible transition system that reduces both the cost and risk of first labor-market entry.

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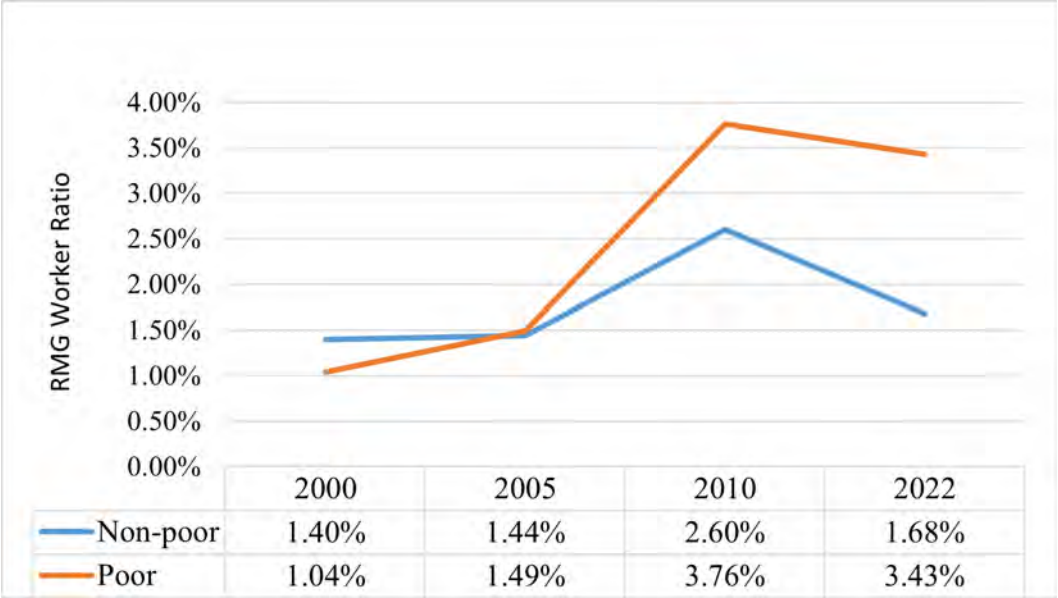


Figure 1: Share of Poor Rangpur Residents Employed in the RMG Sector

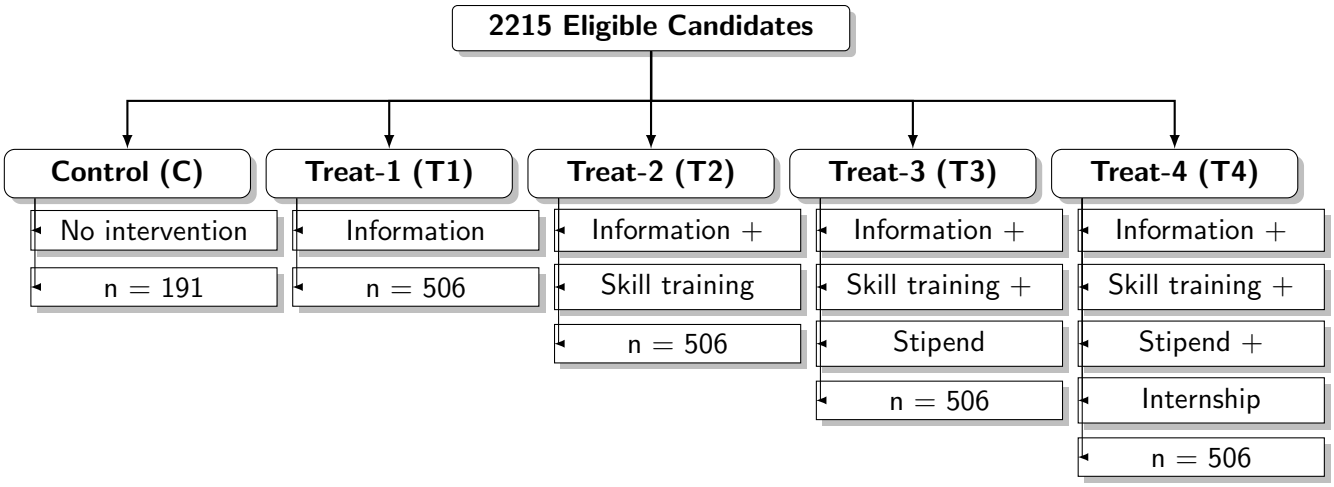


Figure 2: Trial Design

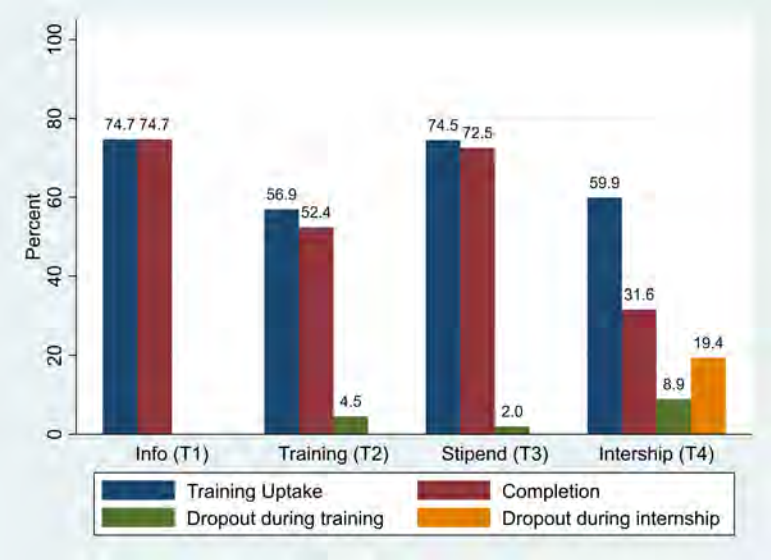


Figure 3: Intervention Uptake and Completion

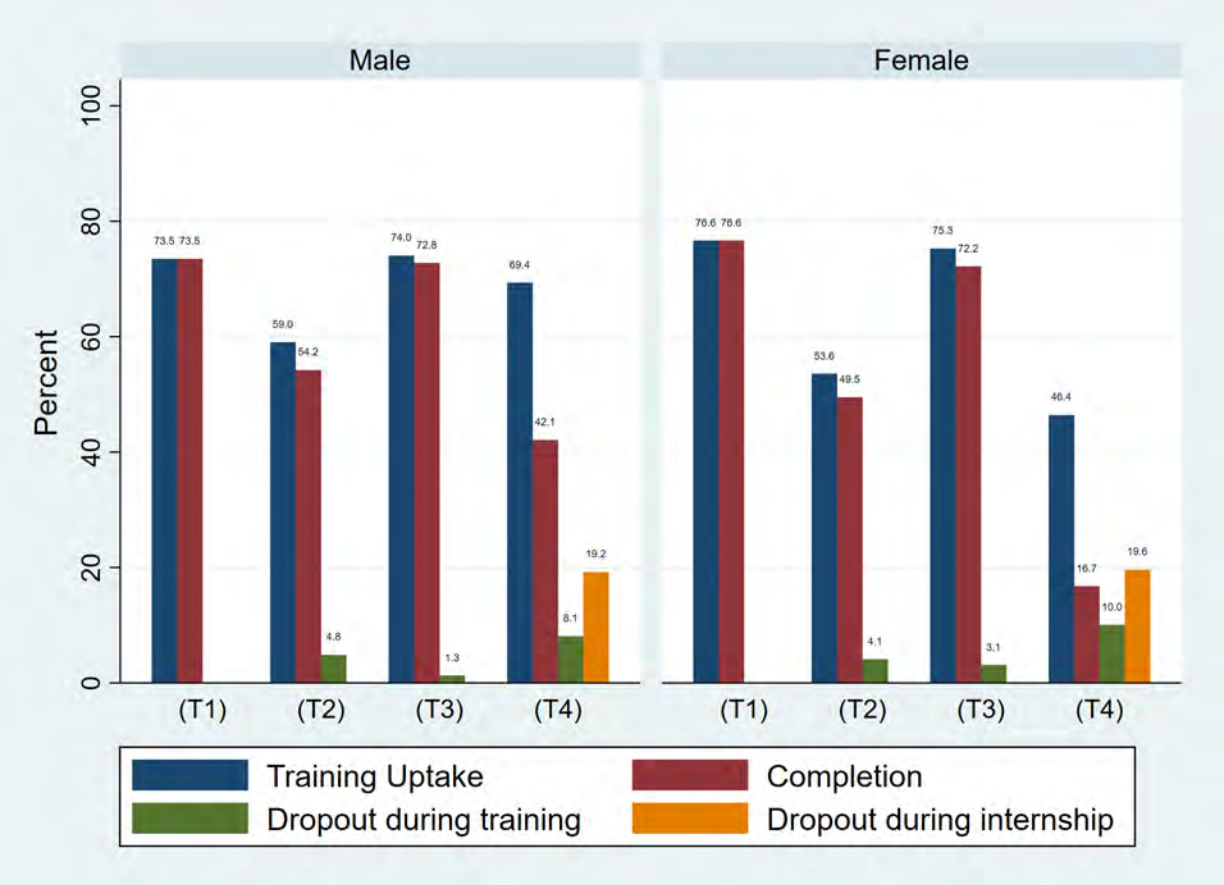


Figure 4: Intervention Uptake and Completion by Gender

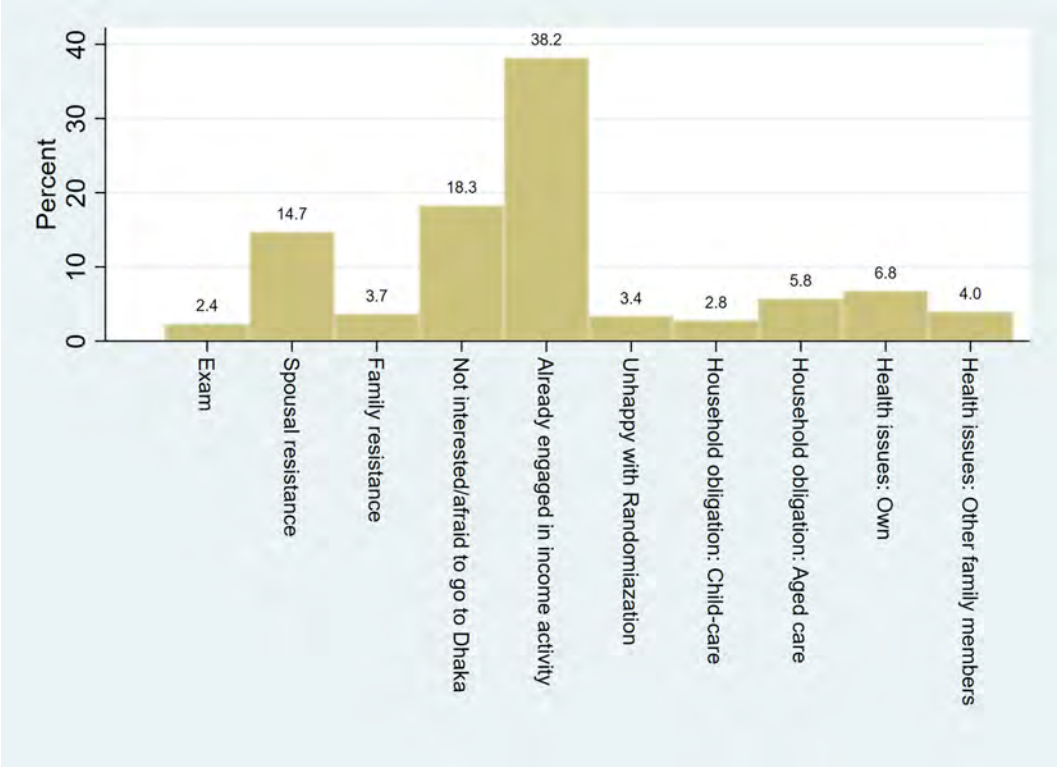


Figure 5: Reported Reasons for Not Taking Up the Assigned Intervention

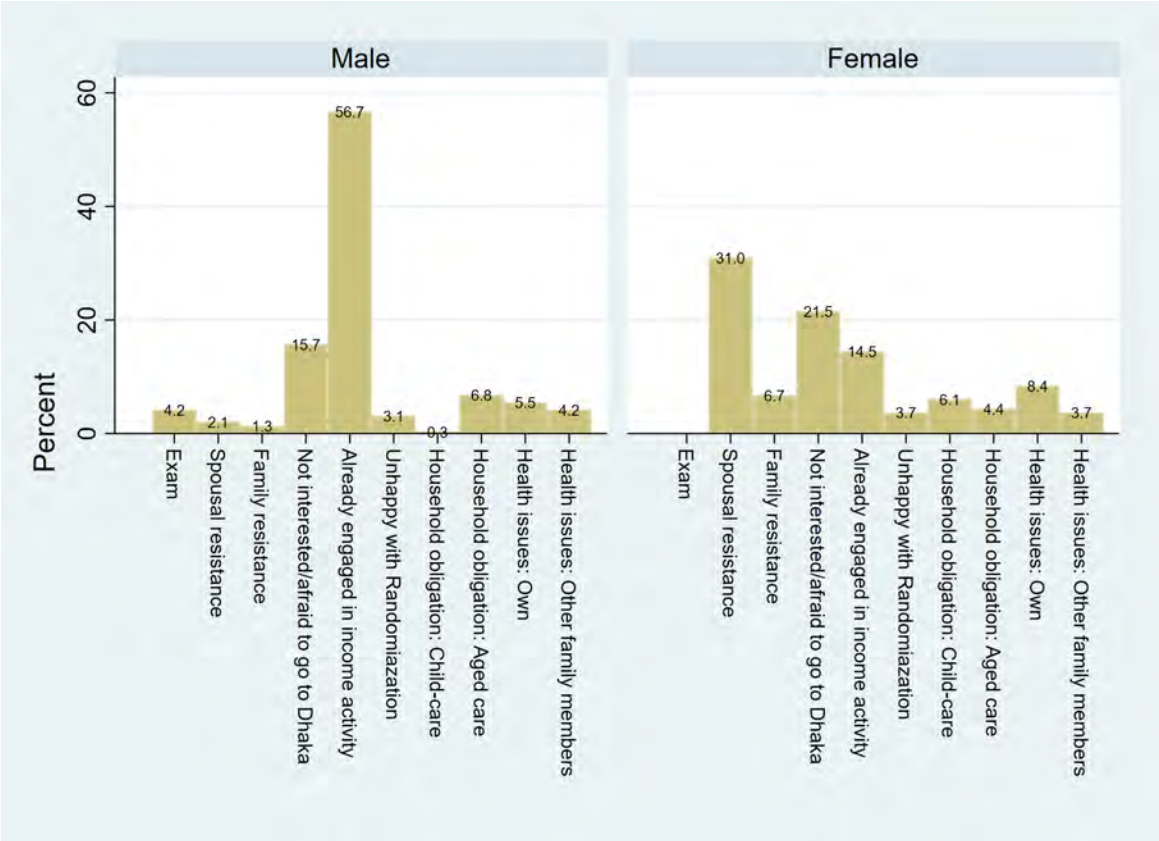


Figure 6: Reported Reasons for Non-Take-Up by Gender

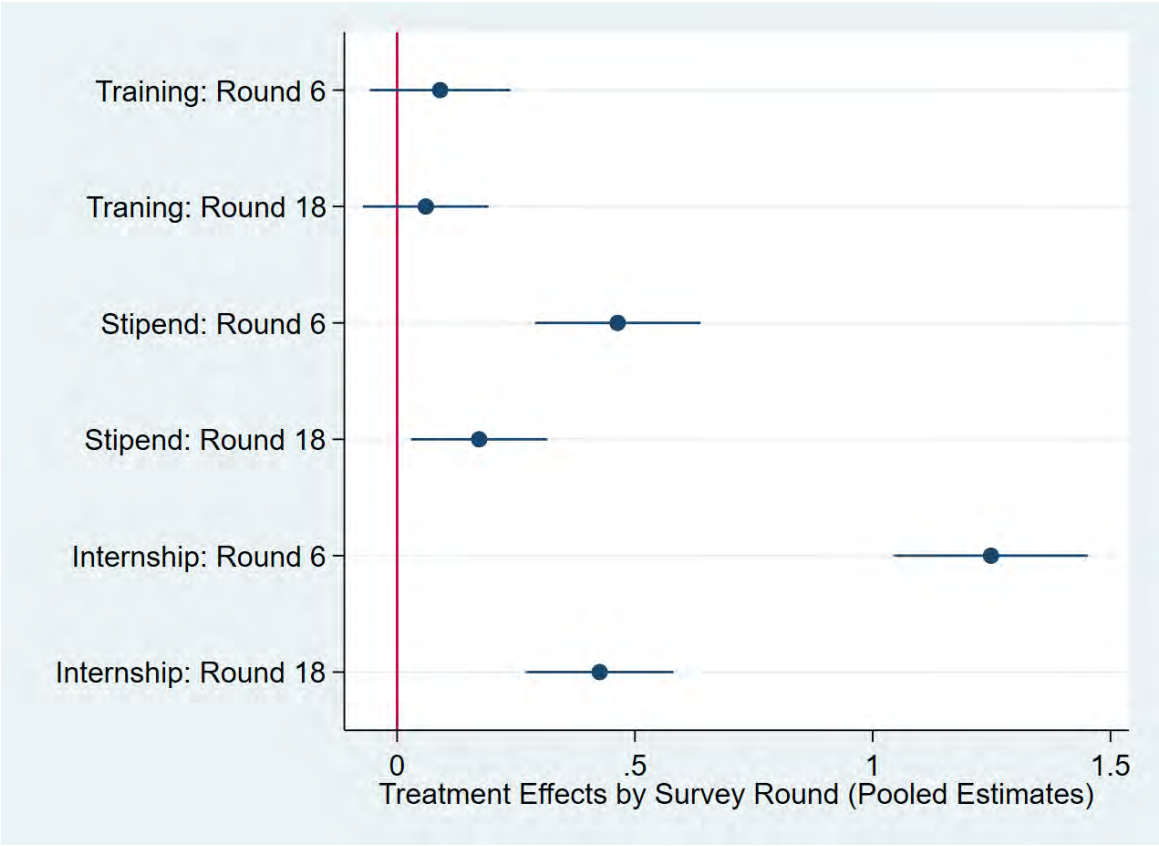


Figure 7: Employment Index at the 6- and 18-Month Follow-Ups

Table 1: Project Timeline and Phased Experimental Rollout

| **Baseline**: September 2013 to August 2014 | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| **Phase** | **Training Batch** | **Treatment Name** | **Start of residential training** | **End of residential training** | **Total sample allotted** | **Treatment assigned** | **Uptake** | **6 months participant follow-up** | **12 months household panel** | **18 months participant follow-up** |
| 1 | 0 | Information (T1) | 15 Dec, 2013 | 25 Dec, 2013 | 518 | 114 | 100 | Jun-14 | Dec-14 | Jun-15 |
| | 1 | Training (T2) | 04 Nov, 2013 | 25 Nov, 2013 | | 114 | 70 | May-14 | Nov-14 | May-15 |
| | 2 | Stipend (T3) | 01 Dec, 2013 | 22 Dec, 2013 | | 114 | 87 | Jun-14 | Dec-14 | Jun-15 |
| | 3 | Internship (T4) | 01 Jan, 2014 | 30 Jan, 2014 | | 114 | 62 | Jul-14 | Jan-15 | Jul-15 |
| 2 | 0 | Information (T1) | 15 Mar, 2014 | 25 Mar, 2014 | 579 | 130 | 100 | Sep-14 | Mar-15 | Sep-15 |
| | 4 | Training (T2) | 08 Feb, 2014 | 01 Mar, 2014 | | 130 | 74 | Aug-14 | Feb-15 | Aug-15 |
| | 5 | Stipend (T3) | 06 Mar, 2014 | 31 Mar, 2014 | | 130 | 100 | Sep-14 | Mar-15 | Sep-15 |
| | 6 | Internship (T4) | 03 Apr, 2014 | 30 Apr, 2014 | | 130 | 78 | Oct-14 | Apr-15 | Oct-15 |
| 3 | 0 | Information (T1) | 15 Jun, 2014 | 25 Jun, 2014 | 570 | 135 | 85 | Dec-14 | Jun-15 | Dec-15 |
| | 7 | Training (T2) | 11 May, 2014 | 01 Jun, 2014 | | 135 | 61 | Nov-14 | May-15 | Nov-15 |
| | 8 | Stipend (T3) | 07 Jun, 2014 | 28 Jun, 2014 | | 135 | 100 | Dec-14 | Jun-15 | Dec-15 |
| | 9 | Internship (T4) | 05 July, 2014 | 08 Aug, 2014 | | 135 | 62 | Jan-15 | Jul-15 | Jan-16 |
| 4 | 0 | Information (T1) | 18 Oct, 2014 | 25 Oct, 2014 | 548 | 127 | 93 | Mar-15 | Sep-15 | Mar-16 |
| | 10 | Training (T2) | 12 Aug, 2014 | 02 Sep, 2014 | | 127 | 60 | Feb-15 | Aug-15 | Feb-16 |
| | 11 | Stipend (T3) | 11 Sep, 2014 | 08 Oct, 2014 | | 127 | 80 | Mar-15 | Sep-15 | Mar-16 |
| | 12 | Internship (T4) | 16 Oct, 2014 | 06 Nov, 2014 | | 127 | 56 | Apr-15 | Oct-15 | Apr-16 |

Table 2: Baseline Summary Statistics by Treatment Assignment

| Variables | Pure-Control Mean | Beta coefficient of the balance test (OLS) | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | (T1 - C) | S.E. | (T2 - C) | S.E. | (T3 - C) | S.E. | (T4 - C) | S.E. |
| Age | 22.27 | 0.471 | (0.643) | 0.197 | (0.559) | -0.232 | (0.689) | -0.102 | (0.605) |
| Female (=1) | 0.30 | 0.117 | (0.110) | 0.021 | (0.093) | 0.038 | (0.106) | 0.054 | (0.098) |
| Education: 5-8 years of schooling | 0.10 | 0.034 | (0.062) | -0.025 | (0.054) | 0.039 | (0.053) | 0.054 | (0.066) |
| Education: 9-12 years of schooling | 0.47 | -0.076 | (0.090) | -0.039 | (0.085) | -0.081 | (0.093) | -0.063 | (0.097) |
| Education: 12+ years of schooling | 0.00 | 0.000 | (0.001) | 0.000 | (0.001) | 0.011 | (0.010) | -0.001 | (0.001) |
| Married (currently) | 0.32 | 0.071 | (0.085) | 0.020 | (0.062) | -0.043 | (0.080) | -0.041 | (0.064) |
| Belongs to an ultra-poor household | 0.52 | -0.095 | (0.087) | -0.062 | (0.084) | -0.014 | (0.072) | -0.084 | (0.083) |
| Number of children in the household | 0.43 | 0.182 | (0.161) | 0.152 | (0.117) | 0.057 | (0.134) | 0.041 | (0.111) |
| Size of the household | 3.99 | 0.041 | (0.235) | -0.029 | (0.212) | 0.238 | (0.212) | 0.132 | (0.177) |
| Participant is the head of household | 0.21 | -0.037 | (0.064) | 0.020 | (0.059) | -0.070 | (0.067) | -0.061 | (0.053) |
| Food-shortage in Monga | 0.37 | 0.093 | (0.067) | 0.024 | (0.060) | 0.097 | (0.060) | 0.089* | (0.053) |
| Size of Landholdings (in decimals) | 2.26 | -0.240 | (0.195) | -0.291 | (0.197) | -0.173 | (0.224) | -0.152 | (0.174) |
| Profession: Wage Employment | 0.17 | 0.023 | (0.087) | 0.044 | (0.080) | -0.037 | (0.065) | -0.036 | (0.057) |
| Profession: Self-employment | 0.04 | -0.011 | (0.027) | -0.010 | (0.022) | 0.004 | (0.032) | 0.023 | (0.029) |
| Profession: Farming | 0.04 | -0.022 | (0.041) | -0.024 | (0.045) | 0.016 | (0.046) | -0.037 | (0.044) |
| Profession: Small business | 0.03 | 0.014 | (0.020) | 0.016 | (0.017) | 0.012 | (0.016) | -0.016 | (0.023) |
| Unemployed (dummy) | 0.71 | 0.022 | (0.097) | 0.004 | (0.083) | 0.033 | (0.086) | 0.074 | (0.068) |
| Religion is Muslim (dummy) | 0.93 | -0.029 | (0.032) | -0.015 | (0.019) | -0.055 | (0.043) | -0.013 | (0.023) |
| Risk Averse (dummy) | 0.73 | 0.099* | (0.059) | 0.050 | (0.083) | 0.009 | (0.085) | 0.061 | (0.072) |
| $p$-value of F-test of joint significance | | 0.61 | | 0.34 | | 0.34 | | 0.74 | |
| Observations | | 697 | | 697 | | 697 | | 697 | |

*Notes* $*, **$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively. All regressions controlled for village and phase fixed effects with clustered standard errors at the village level. Regressions were done comparing each treatment with the control sample. The F-test was conducted, with a particular treatment variable regressed against all the covariates. "C" denotes "Control" and T1-T4 denote treatment 1-4, respectively. Here, T1 is information, T2 is T1 plus skill training, T3 is T2 plus stipend, and T4 is T3 plus paid-internship component.

Table 3: Baseline Predictors of Intervention Uptake

| Dependent Variable: Treatment Uptake | (1) T1 sample (Information) | (2) T2 sample (T1 + Training) | (3) T3 sample (T2 + Stipend) | (4) T4 sample (T3 + Internship) |
|---|---|---|---|---|
| Age | 0.089 | -0.000 | -0.028 | -0.157* |
| | (0.069) | (0.080) | (0.077) | (0.082) |
| Age Squared | -0.002 | 0.000 | 0.001 | 0.003* |
| | (0.001) | (0.002) | (0.002) | (0.002) |
| Female (=1) | -0.061 | -0.123 | 0.116* | -0.284*** |
| | (0.075) | (0.083) | (0.067) | (0.090) |
| Education: 5-8 years of schooling | -0.036 | -0.106 | -0.131* | 0.101 |
| | (0.062) | (0.109) | (0.071) | (0.113) |
| Education: 9-12 years of schooling | 0.079 | 0.077 | -0.028 | -0.066 |
| | (0.077) | (0.082) | (0.059) | (0.061) |
| Education: 12+ years of schooling | -0.129 | 0.248 | -0.279 | 1.049*** |
| | (0.397) | (0.183) | (0.228) | (0.143) |
| Married (currently) | 0.014 | -0.110 | -0.149* | 0.017 |
| | (0.099) | (0.097) | (0.080) | (0.098) |
| Belongs to an ultra-poor household | 0.069 | -0.087 | -0.024 | -0.034 |
| | (0.054) | (0.077) | (0.060) | (0.069) |
| Number of children in the household | -0.040 | -0.048 | -0.000 | 0.038 |
| | (0.047) | (0.063) | (0.054) | (0.068) |
| Size of the household | -0.003 | 0.012 | 0.003 | -0.007 |
| | (0.020) | (0.022) | (0.018) | (0.022) |
| Participant is the head of household | -0.042 | -0.061 | -0.013 | -0.147 |
| | (0.106) | (0.121) | (0.107) | (0.137) |
| Food-insecurity (all year) | 0.081 | -0.038 | 0.541*** | 0.183 |
| | (0.100) | (0.214) | (0.199) | (0.115) |
| Food-insecurity (Lean Season) | -0.059 | 0.091 | -0.543*** | -0.295** |
| | (0.088) | (0.204) | (0.195) | (0.132) |
| Size of Landholdings (in decimals) | 0.010 | -0.041 | 0.020 | 0.001 |
| | (0.025) | (0.027) | (0.028) | (0.035) |
| Profession: Wage Employment | 0.068 | -0.436* | 1.110*** | -0.046 |
| | (0.296) | (0.263) | (0.130) | (0.306) |
| Profession: Self-employment | 0.033 | -0.107 | 1.142*** | -0.066 |
| | (0.341) | (0.292) | (0.212) | (0.311) |
| Profession: Farming | 0.054 | -0.234 | 0.895*** | 0.162 |
| | (0.328) | (0.307) | (0.179) | (0.322) |
| Profession: Small business | -0.033 | -0.028 | 1.183*** | -0.152 |
| | (0.361) | (0.343) | (0.271) | (0.422) |
| Regularly Unemployed (dummy) | 0.141 | -0.248 | 1.169*** | -0.092 |
| | (0.284) | (0.259) | (0.101) | (0.316) |
| Religion is Muslim (dummy) | -0.006 | -0.038 | -0.078 | 0.248** |
| | (0.144) | (0.212) | (0.099) | (0.107) |
| Risk-averse (dummy) | 0.134** | 0.084 | 0.110* | -0.008 |
| | (0.067) | (0.082) | (0.065) | (0.106) |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes |
| Observations | 506 | 506 | 506 | 506 |
| $R^2$ | 0.488 | 0.482 | 0.508 | 0.485 |

*Notes:* The table reports associations between baseline characteristics and take-up within each assigned treatment arm. Village-clustered standard errors are in parentheses. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 4: Intervention Uptake by Treatment Arm and Gender

| **Dependent Variable:** **Treatment Uptake (dummy)** | (1) All Sample | (2) Men | (3) Women |
|---|---|---|---|
| (T1) [Information] | 0.759*** | 0.758*** | 0.725*** |
| | (0.034) | (0.046) | (0.069) |
| (T2) [T1 + Skill training] | 0.580*** | 0.597*** | 0.508*** |
| | (0.034) | (0.042) | (0.074) |
| (T3) [T2 + Stipend] | 0.765*** | 0.771*** | 0.754*** |
| | (0.037) | (0.047) | (0.072) |
| (T4) [T3 + Internship] | 0.619*** | 0.709*** | 0.443*** |
| | (0.036) | (0.042) | (0.068) |
| Observations | 2,215 | 1,361 | 854 |
| $R^2$ | 0.322 | 0.384 | 0.395 |
| Mean of the control group | 0.000 | 0.000 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 |
| Skill-Training effect: T2 - T1 (beta coef.) | -0.180 | -0.161 | -0.217 |
| Skill Training effect: T2 - T1 ($p$-value) | 0.000 | 0.003 | 0.000 |
| Stipend effect: T3 - T2 (beta coef.) | 0.186 | 0.175 | 0.247 |
| Stipend effect: T3 - T2 ($p$-value) | 0.000 | 0.001 | 0.000 |
| Internship effect: T4 - T3 (beta coef.) | -0.146 | -0.062 | -0.311 |
| Internship effect: T4 - T3 ($p$-value) | 0.000 | 0.133 | 0.000 |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes |

*Notes:* The table reports differences in intervention uptake across assigned treatment packages. The sample excludes the pure control group. Village-clustered standard errors are in parentheses. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 5: Employment Outcomes at the Six-Month Follow-Up: ITT Estimates (Information Pooled with Control)

| | **At any point within six months after intervention** | | | | |
|---|---|---|---|---|---|
| | (1) Any Job | (2) Jobs at RMG | (3) Job at Manufacturing | (4) Any full-time job | (5) Employment Index |
| (T) [Skill training] | 0.021 | 0.027 | 0.024 | 0.032 | 0.118 |
| | (0.021) | (0.016) | (0.020) | (0.018) | (0.078) |
| | [0.324] | [0.100] | [0.232] | [0.072]* | [0.132] |
| | {0.740} | {0.503} | {0.662} | {0.503} | {0.503} |
| (T+S) [Skill training with Stipend] | 0.110 | 0.093 | 0.109 | 0.110 | 0.474 |
| | (0.024) | (0.019) | (0.023) | (0.021) | (0.093) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** |
| | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** |
| (T+S+I) [Skill training, stipend plus internship] | 0.289 | 0.224 | 0.280 | 0.272 | 1.194 |
| | (0.025) | (0.024) | (0.025) | (0.024) | (0.101) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** |
| | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| Base-group mean | 0.077 | 0.043 | 0.067 | 0.061 | 0.000 |
| $R^2$ | 0.231 | 0.209 | 0.228 | 0.226 | 0.234 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 0.089 | 0.066 | 0.085 | 0.078 | 0.356 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.001 | 0.005 | 0.001 | 0.001 | 0.001 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | 0.179 | 0.131 | 0.171 | 0.162 | 0.720 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates comparing T2–T4 with the base group, which combines the pure control and information-only arms. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 6: Employment Duration and Hours at the Six-Month Follow-Up: ITT Estimates (Information Pooled with Control)

| | (1) Months of Employment | (2) Weekly wage-work Hours last month | (3) Any wage-work hours: last 6 months | (4) Weekly wage-work hours: last 6 months | (5) Job Continuation Index |
|---|---|---|---|---|---|
| (T) [Skill training] | 0.101 | 0.038 | 0.008 | 1.237 | 0.046 |
| | (0.136) | (0.217) | (0.026) | (1.190) | (0.077) |
| | [0.456] | [0.860] | [0.759] | [0.300] | [0.547] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T+S) [Skill training with Stipend] | 0.355 | 0.380 | 0.113 | 3.369 | 0.230 |
| | (0.117) | (0.221) | (0.028) | (1.077) | (0.072) |
| | [0.003]*** | [0.086]* | [0.000]*** | [0.002]*** | [0.002]*** |
| | {0.008}*** | {0.197} | {0.001}*** | {0.008}*** | {0.008}*** |
| (T+S+I) [Skill training, stipend plus internship] | 1.075 | 1.674 | 0.263 | 10.805 | 0.695 |
| | (0.138) | (0.268) | (0.028) | (1.344) | (0.083) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** |
| | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| Base-group mean | 0.641 | 1.062 | 0.148 | 5.510 | 0.000 |
| $R^2$ | 0.218 | 0.203 | 0.222 | 0.222 | 0.225 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 0.254 | 0.342 | 0.105 | 2.132 | 0.184 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.045 | 0.123 | 0.000 | 0.060 | 0.011 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | 0.720 | 1.294 | 0.150 | 7.436 | 0.465 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates comparing T2–T4 with the base group, which combines the pure control and information-only arms. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 7: Earnings Outcomes at the Six-Month Follow-Up: ITT Estimates (Information Pooled with Control)

| | (1) Any income | (2) Any income last month | (3) Total income | (4) Income last month | (5) Income index |
|---|---|---|---|---|---|
| (T) [Skill training] | 0.009 | -0.010 | 970.584 | -59.796 | 0.013 |
| | (0.026) | (0.024) | (899.447) | (151.358) | (0.074) |
| | [0.727] | [0.680] | [0.282] | [0.693] | [0.858] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T+S) [Skill training with Stipend] | 0.114 | 0.035 | 3,180.180 | 427.937 | 0.245 |
| | (0.028) | (0.023) | (835.302) | (213.687) | (0.078) |
| | [0.000]*** | [0.127] | [0.000]*** | [0.046]** | [0.002]*** |
| | {0.001}*** | {0.290} | {0.001}*** | {0.132} | {0.007}*** |
| (T+S+I) [Skill training, stipend plus internship] | 0.263 | 0.138 | 8,291.764 | 1,089.792 | 0.649 |
| | (0.029) | (0.027) | (1,028.132) | (208.215) | (0.087) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** |
| | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| Base-group mean | 0.146 | 0.111 | 3,852.121 | 688.685 | 0.000 |
| $R^2$ | 0.222 | 0.188 | 0.237 | 0.189 | 0.222 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 0.104 | 0.045 | 2,209.597 | 487.732 | 0.231 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.000 | 0.047 | 0.018 | 0.025 | 0.003 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | 0.149 | 0.103 | 5,111.584 | 661.855 | 0.404 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.000 | 0.000 | 0.000 | 0.014 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates comparing T2–T4 with the base group, which combines the pure control and information-only arms. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 8: Remittance Outcomes at the Six-Month Follow-Up: ITT Estimates (Information Pooled with Control)

| | **At any point within six months after intervention** | | | | |
|---|---|---|---|---|---|
| | (1) Any remittance | (2) Total remittances | (3) Remittance frequency | (4) Remittances last month | (5) Remittance index |
| (T) [Skill training] | 0.030 | 547.661 | 0.140 | 0.028 | 0.209 |
| | (0.016) | (217.942) | (0.061) | (0.015) | (0.089) |
| | [0.066]* | [0.013]** | [0.022]** | [0.058]* | [0.020]** |
| | {0.150} | {0.085}* | {0.085}* | {0.150} | {0.085}* |
| (T+S) [Skill training with Stipend] | 0.084 | 1,024.237 | 0.238 | 0.062 | 0.431 |
| | (0.019) | (250.694) | (0.059) | (0.016) | (0.095) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** |
| | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** |
| (T+S+I) [Skill training, stipend plus internship] | 0.229 | 2,242.984 | 0.731 | 0.167 | 1.137 |
| | (0.023) | (261.665) | (0.084) | (0.023) | (0.123) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** |
| | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| Base-group mean | 0.049 | 340.976 | 0.129 | 0.034 | 0.000 |
| $R^2$ | 0.228 | 0.204 | 0.217 | 0.212 | 0.226 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 0.054 | 476.576 | 0.098 | 0.034 | 0.222 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.012 | 0.165 | 0.166 | 0.078 | 0.061 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | 0.145 | 1,218.747 | 0.493 | 0.105 | 0.706 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.000 | 0.001 | 0.000 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates comparing T2–T4 with the base group, which combines the pure control and information-only arms. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 9: Aspirations and Health at the Six-Month Follow-Up: ITT Estimates (Information Pooled with Control)

| | (1) Aspiration Index | (2) Health Index | (3) Work-related physical stress Index | (4) Work-related mental stress Index | (5) Family-related stress Index | (6) Non-work related stress Index |
|---|---|---|---|---|---|---|
| (T) [Skill training] | 0.012 | 0.091 | -0.103 | 0.064 | -0.043 | 0.105 |
| | (0.076) | (0.067) | (0.081) | (0.071) | (0.069) | (0.077) |
| | [0.877] | [0.178] | [0.204] | [0.371] | [0.532] | [0.175] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T+S) [Skill training with Stipend] | 0.243 | -0.041 | -0.096 | -0.157 | -0.135 | 0.124 |
| | (0.074) | (0.071) | (0.059) | (0.062) | (0.060) | (0.079) |
| | [0.001]*** | [0.563] | [0.102] | [0.012]** | [0.026]** | [0.117] |
| | {0.017}** | {1.000} | {0.345} | {0.087}* | {0.125} | {0.345} |
| (T+S+I) [Skill training, stipend plus internship] | 0.249 | 0.073 | -0.022 | 0.078 | -0.127 | 0.076 |
| | (0.077) | (0.070) | (0.075) | (0.071) | (0.071) | (0.080) |
| | [0.001]*** | [0.295] | [0.770] | [0.274] | [0.075]* | [0.340] |
| | {0.021}** | {1.000} | {1.000} | {1.000} | {0.549} | {1.000} |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 | 2142 |
| Base-group mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| $R^2$ | 0.211 | 0.190 | 0.193 | 0.175 | 0.208 | 0.199 |
| $p$-value for joint significance | 0.000 | 0.104 | 0.185 | 0.003 | 0.109 | 0.370 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 0.231 | -0.132 | 0.007 | -0.221 | -0.092 | 0.019 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.002 | 0.032 | 0.926 | 0.002 | 0.148 | 0.824 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | 0.006 | 0.114 | 0.074 | 0.236 | 0.008 | -0.048 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.935 | 0.076 | 0.240 | 0.002 | 0.890 | 0.529 |
| Outcome at baseline | No | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates comparing T2–T4 with the base group, which combines the pure control and information-only arms. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 10: Financial Behavior and Job Support, 6-month follow-up: ITT (Information Pooled with Control)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Borrowing | Savings | Job Support: | | | |
| | Index | Index | Inspired to seek a job | Helped obtain a job | No. inspired to seek jobs | No. helped obtain jobs |
| (T) [Skill training] | -0.146 | 0.019 | 0.000 | 0.002 | 0.009 | 0.011 |
| | (0.074) | (0.067) | (0.008) | (0.007) | (0.014) | (0.013) |
| | [0.050]** | [0.779] | [0.962] | [0.760] | [0.531] | [0.414] |
| | {0.732} | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T+S) [Skill training with Stipend] | -0.041 | 0.214 | 0.017 | 0.012 | 0.024 | 0.017 |
| | (0.069) | (0.074) | (0.009) | (0.008) | (0.013) | (0.012) |
| | [0.559] | [0.004]*** | [0.071]* | [0.131] | [0.075]* | [0.154] |
| | {1.000} | {0.059}* | {0.369} | {0.454} | {0.369} | {0.454} |
| (T+S+I) [Skill training, stipend plus internship] | -0.266 | 0.161 | 0.023 | 0.017 | 0.032 | 0.026 |
| | (0.073) | (0.069) | (0.009) | (0.008) | (0.013) | (0.012) |
| | [0.000]*** | [0.020]** | [0.017]** | [0.034]** | [0.016]** | [0.028]** |
| | {0.005}*** | {0.072}* | {0.072}* | {0.084}* | {0.072}* | {0.083}* |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 | 2142 |
| Base-group mean | 0.000 | 0.000 | 0.009 | 0.006 | 0.010 | 0.007 |
| $R^2$ | 0.170 | 0.199 | 0.176 | 0.178 | 0.285 | 0.301 |
| $p$-value for joint significance | 0.002 | 0.015 | 0.027 | 0.119 | 0.099 | 0.142 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 0.106 | 0.196 | 0.016 | 0.010 | 0.015 | 0.007 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.202 | 0.013 | 0.111 | 0.249 | 0.378 | 0.663 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | -0.225 | -0.053 | 0.006 | 0.004 | 0.008 | 0.008 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.002 | 0.441 | 0.589 | 0.649 | 0.558 | 0.555 |
| Outcome at baseline | No | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates comparing T2–T4 with the base group, which combines the pure control and information-only arms. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 11: Employment Outcomes, 18-Month Follow-Up: ITT (Information Pooled with Control)

| | **During the 18-months follow-up** | | | | |
|---|---|---|---|---|---|
| | (1) Any Job | (2) Jobs at RMG | (3) Job at Manufacturing | (4) Any full-time job | (5) Employment Index |
| (T) [Skill training] | 0.002 | 0.008 | 0.011 | 0.007 | 0.040 |
| | (0.034) | (0.013) | (0.016) | (0.034) | (0.081) |
| | [0.951] | [0.556] | [0.470] | [0.844] | [0.626] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T+S) [Skill training with Stipend] | 0.016 | 0.031 | 0.050 | 0.003 | 0.153 |
| | (0.030) | (0.015) | (0.019) | (0.029) | (0.078) |
| | [0.605] | [0.045]** | [0.010]*** | [0.928] | [0.051]* |
| | {1.000} | {0.193} | {0.112} | {1.000} | {0.193} |
| (T+S+I) [Skill training, stipend plus internship] | 0.097 | 0.062 | 0.091 | 0.106 | 0.407 |
| | (0.029) | (0.016) | (0.019) | (0.029) | (0.078) |
| | [0.001]*** | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** |
| | {0.002}*** | {0.000}*** | {0.000}*** | {0.001}*** | {0.000}*** |
| Observations | 2085 | 2085 | 2085 | 2085 | 2085 |
| Base-group mean | 0.316 | 0.028 | 0.041 | 0.303 | 0.000 |
| $R^2$ | 0.339 | 0.154 | 0.185 | 0.331 | 0.245 |
| $p$-value for joint significance | 0.002 | 0.001 | 0.000 | 0.000 | 0.000 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 0.013 | 0.023 | 0.038 | -0.004 | 0.113 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.646 | 0.203 | 0.041 | 0.885 | 0.178 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | 0.081 | 0.031 | 0.041 | 0.103 | 0.254 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.007 | 0.087 | 0.092 | 0.000 | 0.005 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates comparing T2–T4 with the base group, which combines the pure control and information-only arms. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 12: Household Socioeconomic Indices, 12 Months: ITT-ANCOVA (Info Pooled with Control)

| | (1) Illness Index | (2) Asset Index | (3) Livestock Index | (4) Housing Index | (5) Savings Index | (6) Borrowing index |
|---|---|---|---|---|---|---|
| (T) [Skill training] | -0.071 | 0.090 | 0.079 | 0.063 | 0.050 | 0.022 |
| | (0.060) | (0.066) | (0.103) | (0.057) | (0.059) | (0.051) |
| | [0.241] | [0.173] | [0.443] | [0.270] | [0.397] | [0.658] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T+S) [Skill training with Stipend] | -0.039 | 0.188 | 0.067 | 0.096 | -0.046 | 0.016 |
| | (0.061) | (0.104) | (0.061) | (0.051) | (0.061) | (0.069) |
| | [0.519] | [0.073]* | [0.270] | [0.062]* | [0.455] | [0.820] |
| | {1.000} | {0.533} | {1.000} | {0.533} | {1.000} | {1.000} |
| (T+S+I) [Skill training, stipend plus internship] | -0.156 | 0.155 | 0.098 | 0.076 | -0.005 | -0.059 |
| | (0.079) | (0.067) | (0.070) | (0.061) | (0.057) | (0.060) |
| | [0.049]** | [0.022]** | [0.161] | [0.217] | [0.932] | [0.325] |
| | {0.364} | {0.329} | {0.787} | {0.796} | {1.000} | {0.954} |
| Observations | 2120 | 2120 | 2120 | 2120 | 2120 | 2120 |
| Base-group mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| $R^2$ | 0.182 | 0.431 | 0.335 | 0.329 | 0.274 | 0.257 |
| $p$-value for joint significance | 0.263 | 0.048 | 0.506 | 0.298 | 0.579 | 0.558 |
| Stipend effect: (T+S)-(T) (beta coef.) | 0.031 | 0.098 | -0.012 | 0.034 | -0.095 | -0.007 |
| Stipend effect: (T+S)-(T) ($p$-value) | 0.660 | 0.372 | 0.883 | 0.563 | 0.163 | 0.923 |
| Internship effect: (T+S+I)-(T+S) (beta coef.) | -0.117 | -0.034 | 0.031 | -0.020 | 0.041 | -0.075 |
| Internship: (T+S+I)-(T+S) ($p$-value) | 0.187 | 0.778 | 0.650 | 0.726 | 0.519 | 0.254 |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 13: Remittance and Labor Income, 12-months: ITT-ANCOVA (Info Pooled with Control)

| | (1) Months of remittances | (2) Frequency of remittances | (3) Total remittances | (4) Any urban labor income | (5) Total urban labor income |
|---|---|---|---|---|---|
| (T) [Skill training] | 0.208 | 0.217 | 799.526 | 0.025 | 2,497.313 |
| | (0.127) | (0.128) | (671.841) | (0.021) | (2,128.911) |
| | [0.101] | [0.092]* | [0.235] | [0.233] | [0.242] |
| | {0.553} | {0.553} | {0.553} | {0.553} | {0.553} |
| (T+S) [Skill training with Stipend] | 0.432 | 0.437 | 2,768.919 | 0.032 | 2,797.057 |
| | (0.132) | (0.133) | (1,024.247) | (0.017) | (1,856.570) |
| | [0.001]*** | [0.001]*** | [0.007]*** | [0.059]* | [0.133] |
| | {0.007}*** | {0.007}*** | {0.028}** | {0.169} | {0.304} |
| (T+S+I) [Skill training, stipend plus internship] | 0.974 | 1.025 | 2,971.970 | 0.049 | 4,901.315 |
| | (0.145) | (0.150) | (773.231) | (0.020) | (2,177.326) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.014]** | [0.025]** |
| | {0.000}*** | {0.000}*** | {0.001}*** | {0.040}** | {0.058}* |
| Observations | 2120 | 2120 | 2120 | 2120 | 2120 |
| Base-group mean | 0.428 | 0.433 | 2,200.363 | 0.094 | 10,123.440 |
| $R^2$ | 0.227 | 0.226 | 0.174 | 0.186 | 0.196 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.051 | 0.131 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 0.224 | 0.220 | 1,969.394 | 0.006 | 299.743 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.093 | 0.096 | 0.026 | 0.785 | 0.899 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | 0.542 | 0.588 | 203.051 | 0.018 | 2,104.258 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.001 | 0.001 | 0.810 | 0.434 | 0.368 |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 14: Household Income and Poverty, 12-months: ITT-ANCOVA (Info Pooled with Control)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Daily household per capita measures (BDT) | | | | | |
| | Total income | Total consumption | Food consumption | Non-food consumption | Income poverty | Consumption poverty |
| (T) [Skill training] | 2.718 | -1.621 | -0.669 | -0.099 | -0.044 | 0.010 |
| | (4.586) | (2.037) | (1.003) | (0.316) | (0.031) | (0.012) |
| | [0.554] | [0.427] | [0.506] | [0.754] | [0.155] | [0.379] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T+S) [Skill training with Stipend] | 8.811 | 1.814 | 0.646 | 0.203 | -0.081 | -0.005 |
| | (4.258) | (2.049) | (1.031) | (0.346) | (0.032) | (0.013) |
| | [0.040]** | [0.377] | [0.532] | [0.559] | [0.011]** | [0.698] |
| | {0.292} | {1.000} | {1.000} | {1.000} | {0.161} | {1.000} |
| (T+S+I) [Skill training, stipend plus internship] | 9.821 | -1.255 | -0.686 | -0.024 | -0.121 | -0.004 |
| | (4.440) | (2.028) | (1.010) | (0.315) | (0.035) | (0.014) |
| | [0.028]** | [0.537] | [0.497] | [0.940] | [0.001]*** | [0.772] |
| | {0.206} | {1.000} | {1.000} | {1.000} | {0.008}*** | {1.000} |
| Observations | 2120 | 2120 | 2120 | 2120 | 2120 | 2120 |
| Base-group mean | 66.436 | 66.615 | 27.452 | 8.182 | 0.537 | 0.964 |
| $R^2$ | 0.269 | 0.343 | 0.330 | 0.302 | 0.301 | 0.171 |
| $p$-value for joint significance | 0.030 | 0.160 | 0.330 | 0.712 | 0.004 | 0.456 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 6.094 | 3.436 | 1.315 | 0.302 | -0.036 | -0.015 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.087 | 0.034 | 0.087 | 0.264 | 0.283 | 0.220 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | 1.010 | -3.069 | -1.332 | -0.226 | -0.040 | 0.001 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.770 | 0.069 | 0.110 | 0.405 | 0.249 | 0.943 |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* ITT-ANCOVA estimates comparing T2–T4 with the base group, which combines the pure control and information-only arms. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Income data are winsorized at the 1st and 99th percentile levels. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 15: Migration Outcomes at the Six-Month Follow-Up: ITT Estimates (Information as a Separate Treatment Arm)

| | (1) Ever Migrated | (2) Currently Migrant | (3) Migration: Dhaka | (4) Migration: Chittagong | (5) Migration: Outside Rangpur (except Dhaka/Chittagong) |
|---|---|---|---|---|---|
| (T1) [Information] | 0.016 | -0.028 | -0.009 | -0.009 | 0.001 |
| | (0.034) | (0.026) | (0.026) | (0.008) | (0.003) |
| | [0.647] | [0.291] | [0.739] | [0.308] | [0.660] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T2) [T1 + Skill training] | 0.069 | -0.006 | 0.009 | -0.009 | 0.000 |
| | (0.034) | (0.024) | (0.024) | (0.007) | (0.003) |
| | [0.042]** | [0.798] | [0.718] | [0.213] | [0.960] |
| | {0.480} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T3) [T2 + Stipend] | 0.182 | 0.042 | 0.069 | -0.003 | 0.001 |
| | (0.034) | (0.024) | (0.024) | (0.008) | (0.003) |
| | [0.000]*** | [0.080]* | [0.005]*** | [0.716] | [0.681] |
| | {0.000}*** | {0.303} | {0.029}** | {1.000} | {1.000} |
| (T4) [T3 + Internship] | 0.366 | 0.134 | 0.189 | -0.001 | 0.008 |
| | (0.039) | (0.030) | (0.029) | (0.008) | (0.004) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.902] | [0.065]* |
| | {0.000}*** | {0.000}*** | {0.000}*** | {1.000} | {0.185} |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| Pure-control mean | 0.064 | 0.053 | 0.053 | 0.011 | 0.005 |
| $R^2$ | 0.276 | 0.172 | 0.189 | 0.127 | 0.206 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.625 | 0.419 |
| Training effect: T2 - T1 (beta coef.) | 0.053 | 0.022 | 0.017 | -0.001 | -0.001 |
| Training effect: T2 - T1 ($p$-value) | 0.038 | 0.208 | 0.368 | 0.888 | 0.788 |
| Stipend effect: T3 - T2 (beta coef.) | 0.113 | 0.049 | 0.060 | 0.006 | 0.001 |
| Stipend effect: T3 - T2 ($p$-value) | 0.000 | 0.014 | 0.013 | 0.209 | 0.804 |
| Internship effect: T4 - T3 (beta coef.) | 0.184 | 0.092 | 0.120 | 0.002 | 0.007 |
| Internship effect: T4 - T3 ($p$-value) | 0.000 | 0.000 | 0.000 | 0.754 | 0.236 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates with the information-only arm estimated separately and the pure control group as the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses and $p$-values are in square brackets. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table 16: Employment Sector by Treatment Assignment at the Six-Month Follow-up

| Treatment arm | Employment sector (row percentages) | | | | | |
|---|---|---|---|---|---|---|
| | **RMG** | **Textile** | **Other Factory** | **Service** | **Other services** | **No formal employment** |
| Control (C) | 3.14 | 1.57 | 0.00 | 1.05 | 0.00 | 94.24 |
| Information (T1) | 4.55 | 1.98 | 0.59 | 0.40 | 0.59 | 91.90 |
| Training (T2) | 7.31 | 1.78 | 0.59 | 0.59 | 0.00 | 89.72 |
| Stipend (T3) | 12.85 | 2.96 | 0.99 | 0.59 | 0.59 | 82.02 |
| Internship (T4) | 25.49 | 6.32 | 1.38 | 1.58 | 0.20 | 65.02 |

*Notes:* Each row reports the distribution of participants within a treatment arm across employment sectors and sums to 100 percent. The table is descriptive and should not be interpreted as an ITT estimate.

Table 17: Heterogeneity Analysis by Baseline Risk Aversion

| | (1) Ever migrated after intervention | (2) Currently migrant | (3) Employed in RMG sector |
|---|---|---|---|
| Control × Risk-averse | -0.00579 | 0.0430 | 0.00163 |
| | (0.0729) | (0.0490) | (0.0549) |
| Information (T1) | 0.00983 | -0.0228 | -0.0204 |
| | (0.0362) | (0.0236) | (0.0230) |
| Information (T1) × Risk-averse | 0.0240 | 0.00813 | -0.0136 |
| | (0.0443) | (0.0347) | (0.0306) |
| Training (T2) | 0.0815** | 0.00202 | -0.000152 |
| | (0.0385) | (0.0222) | (0.0240) |
| Training (T2) × Risk-averse | 0.0416 | 0.00929 | 0.0279 |
| | (0.0427) | (0.0334) | (0.0322) |
| Stipend (T3) | 0.171*** | 0.0529* | 0.0616** |
| | (0.0387) | (0.0270) | (0.0254) |
| Stipend (T3) × Risk-averse | 0.211*** | 0.0603* | 0.0912** |
| | (0.0492) | (0.0361) | (0.0387) |
| Internship (T4) | 0.347*** | 0.123*** | 0.168*** |
| | (0.0476) | (0.0315) | (0.0307) |
| Internship (T4) × Risk-averse | 0.424*** | 0.214*** | 0.278*** |
| | (0.0583) | (0.0499) | (0.0591) |
| Observations | 2142 | 2142 | 2142 |
| Other Controls | Yes | Yes | Yes |

*Notes:* Village-clustered standard errors are in parentheses. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

## A Appendix: Supplementary Tables [For online publication Only]

Table A1 reports attrition rates separately for the 6-month participant survey, the 12-month origin-household survey, and the 18-month participant survey, allowing readers to compare follow-up coverage across survey rounds. Table A2 tests whether attrition differs systematically across the original randomized treatment assignments and therefore addresses whether selective follow-up threatens the experimental comparisons. Table A3 describes completion of the assigned program components and selected heterogeneity in completion,.

Table A4 reports ITT estimates for employment at six months with information as a separate treatment arm, including any employment, apparel-related and manufacturing employment, full-time work, and the employment index. Table A5 reports IV estimates of treatment take-up with information pooled with control. Table A6 reports the corresponding IV estimates with information as a separate treatment arm.

Table A7 presents ITT effects on months worked, wage-work hours, and job continuation with information as a separate treatment arm, excluding the assigned internship period. Table A8 reports IV estimates of treatment take-up with information pooled with control. Table A9 reports the IV estimates with information as a separate treatment arm.

Table A10 reports ITT estimates with information as a separate treatment arm for total earnings over six months, earnings in the preceding month, and the earnings index; these estimates provide the inputs for the short-run payback discussion in the main text. Table A11 gives IV estimates of treatment take-up with information pooled with control. Table A12 reports the IV estimates with information as a separate treatment arm.

Table A13 reports ITT estimates with information as a separate treatment arm for whether participants remit, the amount and frequency of remittances, and the remittance index during the first six months. Table A14 presents IV estimates of treatment take-up with information pooled with control. Table A15 reports the IV estimates with information as a separate treatment arm.

Table A16 defines the component outcomes used to construct the standardized indices for participant stress and financial behavior and for origin-household housing, assets, livestock, health, and vulnerability.

Table A17 reports ITT estimates with information as a separate treatment arm for aspirations and physical, family-related, and non-work-related stress at six months. Table A18 provides IV estimates of treatment take-up with information pooled with control. Table A19 reports the corresponding IV estimates with information as a separate treatment arm.

Table A20 reports ITT estimates with information as a separate treatment arm for borrowing, savings, and direct job-support outcomes, including whether participants inspired others to seek apparel work or helped contacts obtain jobs. Table A21 gives IV estimates of treatment take-up with information pooled with control. Table A22 reports the IV estimates with information as a separate treatment arm.

Table A23 reports ITT estimates with information as a separate treatment arm for employment at 18 months. Table A24 presents IV estimates of treatment take-up with information pooled with control. Table A25 reports the IV estimates with information as a separate treatment arm.

Table A26 reports ITT-ANCOVA estimates with information as a separate treatment arm for household livestock, housing, assets, savings, borrowing, illness, and vulnerability indices at 12 months. Table A27 presents IV estimates of treatment take-up with information pooled with control. Table A28 provides the corresponding IV estimates with information as a separate treatment arm.

Table A29 reports ITT-ANCOVA estimates with information as a separate treatment arm for remittances received by origin households and for migration and urban labor income among other household members. Table A30 gives IV estimates of treatment take-up with information pooled with control. Table A31 presents the IV estimates with information as a separate treatment arm and provides the fuller evidence behind the paper's discussion of remittance transmission and intra-household labor-market effects.

Table A32 reports ITT-ANCOVA estimates with information as a separate treatment arm for per capita income and consumption and for income and consumption poverty at 12 months. Table A33 reports IV estimates of treatment take-up with information pooled with control. Table A34 gives the corresponding IV estimates with information as a separate treatment arm.

Table A1: Attrition Rates by Survey Round

| **Treatments** | **6 months follow-up** | **12-month household panel** | **18 months follow-up** |
|---|---|---|---|
| Control (C) | 2.09 | 2.62 | 3.66 |
| Information (T1) | 3.36 | 2.76 | 5.73 |
| Training (T2) | 2.57 | 1.98 | 6.32 |
| Training + Stipend (T3) | 2.17 | 2.17 | 7.31 |
| Training + Stipend + Internship (T4) | 3.95 | 3.56 | 6.32 |
| **Overall** | **2.93** | **2.62** | **6.19** |

*Notes:* Entries are attrition rates in percentage points.

Table A2: Tests for Differential Attrition

| | (1) | (2) | (3) |
|---|---|---|---|
| VARIABLES | **Attrition during 6-month follow-up (participant)** | **Attrition during 12-month panel survey (HH)** | **Attrition during 18-month follow-up (participant)** |
| Information (T1) | 0.018 | -0.012 | 0.029 |
| | (0.017) | (0.013) | (0.032) |
| Training (T2) | 0.013 | -0.004 | 0.026 |
| | (0.015) | (0.014) | (0.031) |
| Stipend (T3) | 0.009 | -0.013 | 0.040 |
| | (0.016) | (0.014) | (0.034) |
| Internship (T4) | 0.025 | -0.001 | 0.030 |
| | (0.019) | (0.014) | (0.032) |
| Observations | 2,215 | 2,215 | 2,215 |
| $R^2$ | 0.147 | 0.185 | 0.163 |
| Mean of the pure control group | 0.0209 | 0.0262 | 0.0366 |
| $p$-value for joint significance | 0.63 | 0.33 | 0.78 |
| Baseline covariates, village and phase FE | Yes | Yes | Yes |

*Notes:* Coefficients, standard errors, and control means are expressed as proportions. All specifications include baseline covariates, village and phase fixed effects; standard errors are clustered by village.

Table A3: Program Completion Analysis

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Dependent Variable:** | **Sample excluding the control and information-only arms** | | | |
| **Training Completion (dummy)** | **Gender combined sample** | | **Male** | **Female** |
| Stipend (T3) | 0.204*** | 0.171*** | 0.191*** | 0.275*** |
| | (0.040) | (0.053) | (0.058) | (0.062) |
| Internship (T4) | -0.003 | 0.072 | 0.075 | -0.127** |
| | (0.034) | (0.047) | (0.051) | (0.052) |
| Female | -0.080* | -0.045 | | |
| | (0.045) | (0.059) | | |
| Risk-averse | 0.058 | 0.055 | 0.073 | 0.052 |
| | (0.043) | (0.042) | (0.053) | (0.067) |
| Stipend (T3) × Female | | 0.088 | | |
| | | (0.072) | | |
| Internship (T4) × Female | | -0.185*** | | |
| | | (0.071) | | |
| Observations | 1,518 | 1,518 | 919 | 599 |
| $R^2$ | 0.274 | 0.285 | 0.323 | 0.422 |
| Mean of the T2 group | 0.524 | 0.524 | 0.542 | 0.495 |
| $p$-value for joint significance | 0.00 | 0.005 | 0.004 | 0.000 |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes |

*Notes:* The sample excludes the pure control and information-only arms. Village-clustered standard errors are in parentheses. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A4: Six-Month Employment Outcomes: ITT Estimates (Information as a Separate Treatment Arm)

| | **At any point within six months after intervention** | | | | |
|---|---|---|---|---|---|
| | (1) Any Job | (2) Jobs at RMG | (3) Job at Manufacturing | (4) Any full-time job | (5) Employment Index |
| (T1) [Information] | -0.009 | -0.023 | -0.006 | -0.016 | -0.070 |
| | (0.031) | (0.027) | (0.029) | (0.029) | (0.140) |
| | [0.761] | [0.406] | [0.842] | [0.583] | [0.619] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T2) [T1 + Skill training] | 0.013 | 0.007 | 0.019 | 0.018 | 0.069 |
| | (0.027) | (0.025) | (0.025) | (0.024) | (0.120) |
| | [0.624] | [0.781] | [0.460] | [0.441] | [0.562] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T3) [T2 + Stipend] | 0.102 | 0.073 | 0.104 | 0.096 | 0.463 |
| | (0.030) | (0.027) | (0.028) | (0.027) | (0.134) |
| | [0.001]*** | [0.008]*** | [0.000]*** | [0.000]*** | [0.001]*** |
| | {0.002}*** | {0.017}** | {0.002}*** | {0.002}*** | {0.002}*** |
| (T4) [T3 + Internship] | 0.281 | 0.204 | 0.275 | 0.259 | 1.257 |
| | (0.031) | (0.034) | (0.032) | (0.031) | (0.153) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** |
| | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| Pure-control mean | 0.059 | 0.032 | 0.048 | 0.059 | 0.000 |
| $R^2$ | 0.231 | 0.210 | 0.228 | 0.226 | 0.234 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Training effect: T2 - T1 (beta coef.) | 0.023 | 0.030 | 0.024 | 0.034 | 0.139 |
| Training effect: T2 - T1 ($p$-value) | 0.338 | 0.088 | 0.258 | 0.084 | 0.146 |
| Stipend effect: T3 - T2 (beta coef.) | 0.089 | 0.066 | 0.085 | 0.078 | 0.394 |
| Stipend effect: T3 - T2 ($p$-value) | 0.001 | 0.005 | 0.001 | 0.001 | 0.001 |
| Internship effect: T4 - T3 (beta coef.) | 0.179 | 0.131 | 0.171 | 0.162 | 0.794 |
| Internship effect: T4 - T3 ($p$-value) | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates with the information-only arm estimated separately and the pure control group as the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A5: Effects of Treatment Take-Up on Six-Month Employment Outcomes (IV; Information Pooled with Control)

| | **At any point within six months after intervention** | | | | |
|---|---|---|---|---|---|
| | (1) Any Job | (2) Jobs at RMG | (3) Job at Manufacturing | (4) Any full-time job | (5) Employment Index |
| Take-up (T) [Skill training] | 0.035 | 0.046* | 0.040 | 0.055* | 0.200 |
| | (0.036) | (0.027) | (0.033) | (0.030) | (0.132) |
| Take-up (T+S) [Skill training with Stipend] | 0.148*** | 0.125*** | 0.146*** | 0.148*** | 0.637*** |
| | (0.028) | (0.023) | (0.028) | (0.025) | (0.111) |
| Take-up (T+S+I) [Skill training, stipend plus internship] | 0.481*** | 0.372*** | 0.465*** | 0.452*** | 1.984*** |
| | (0.037) | (0.033) | (0.038) | (0.035) | (0.146) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| $R^2$ | 0.299 | 0.257 | 0.293 | 0.285 | 0.301 |
| Base-group mean | 0.077 | 0.043 | 0.067 | 0.061 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Stipend effect: (T+S)-(T) (beta coef.) | 0.113 | 0.079 | 0.107 | 0.093 | 0.437 |
| Stipend effect: (T+S)-(T) ($p$-value) | 0.003 | 0.017 | 0.005 | 0.004 | 0.004 |
| Internship effect: (T+S+I)-(T+S) (beta coef.) | 0.333 | 0.247 | 0.319 | 0.304 | 1.346 |
| Internship: (T+S+I)-(T+S) ($p$-value) | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T2–T4 as instruments. The pure control and information-only arms are combined as the omitted base group. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A6: Effects of Treatment Take-Up on Six-Month Employment Outcomes (IV; Information as a Separate Treatment Arm)

| | **At any point within six months after intervention** | | | | |
|---|---|---|---|---|---|
| | (1) Any Job | (2) Jobs at RMG | (3) Job at Manufacturing | (4) Any full-time job | (5) Employment Index |
| Take-up (T1) [Information] | -0.021 | -0.037 | -0.016 | -0.029 | -0.131 |
| | (0.038) | (0.034) | (0.036) | (0.036) | (0.175) |
| Take-up (T2) [T1 + Skill training] | 0.011 | 0.004 | 0.021 | 0.022 | 0.070 |
| | (0.046) | (0.042) | (0.043) | (0.041) | (0.205) |
| Take-up (T3) [T2 + Stipend] | 0.130*** | 0.093*** | 0.133*** | 0.123*** | 0.591*** |
| | (0.037) | (0.034) | (0.036) | (0.034) | (0.169) |
| Take-up (T4) [T3 + Internship] | 0.459*** | 0.333*** | 0.448*** | 0.422*** | 2.050*** |
| | (0.049) | (0.050) | (0.050) | (0.047) | (0.231) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| $R^2$ | 0.299 | 0.257 | 0.293 | 0.285 | 0.300 |
| Pure-control mean | 0.059 | 0.032 | 0.048 | 0.059 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Training effect: T2 - T1 (beta coef.) | 0.032 | 0.040 | 0.038 | 0.051 | 0.201 |
| Training effect: T2 - T1 ($p$-value) | 0.352 | 0.129 | 0.240 | 0.075 | 0.151 |
| Stipend effect: T3 - T2 (beta coef.) | 0.119 | 0.090 | 0.111 | 0.101 | 0.521 |
| Stipend effect: T3 - T2 ($p$-value) | 0.002 | 0.008 | 0.003 | 0.002 | 0.002 |
| Internship effect: T4 - T3 (beta coef.) | 0.328 | 0.240 | 0.316 | 0.299 | 1.459 |
| Internship effect: T4 - T3 ($p$-value) | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T1–T4 as instruments. The information-only arm is estimated separately and the pure control group is the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A7: Six-Month Employment Duration and Hours: ITT Estimates (Information as a Separate Treatment Arm)

| | (1) Months of Employment | (2) Weekly wage-working hours: last month | (3) Any wage-working hours: last 6 months | (4) Weekly wage-working hours: last 6 months | (5) Job Continuation Index |
|---|---|---|---|---|---|
| (T1) [Information] | 0.155 | -0.077 | 0.013 | 1.145 | 0.059 |
| | (0.179) | (0.353) | (0.040) | (1.605) | (0.134) |
| | [0.386] | [0.828] | [0.753] | [0.476] | [0.660] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T2) [T1 + Skill training] | 0.235 | -0.028 | 0.019 | 2.219 | 0.109 |
| | (0.154) | (0.321) | (0.036) | (1.364) | (0.117) |
| | [0.128] | [0.931] | [0.597] | [0.105] | [0.352] |
| | {0.733} | {1.000} | {1.000} | {0.733} | {1.000} |
| (T3) [T2 + Stipend] | 0.489 | 0.314 | 0.124 | 4.351 | 0.328 |
| | (0.149) | (0.291) | (0.036) | (1.313) | (0.111) |
| | [0.001]*** | [0.282] | [0.001]*** | [0.001]*** | [0.004]*** |
| | {0.005}*** | {0.643} | {0.005}*** | {0.005}*** | {0.010}** |
| (T4) [T3 + Internship] | 1.208 | 1.608 | 0.274 | 11.783 | 0.888 |
| | (0.163) | (0.347) | (0.037) | (1.552) | (0.125) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** |
| | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| Pure-control mean | 0.417 | 0.845 | 0.112 | 3.615 | 0.000 |
| $R^2$ | 0.218 | 0.203 | 0.222 | 0.222 | 0.225 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Training effect: T2 - T1 (beta coef.) | 0.079 | 0.049 | 0.006 | 1.074 | 0.050 |
| Training effect: T2 - T1 ($p$-value) | 0.593 | 0.835 | 0.829 | 0.411 | 0.624 |
| Stipend effect: T3 - T2 (beta coef.) | 0.254 | 0.342 | 0.105 | 2.132 | 0.219 |
| Stipend effect: T3 - T2 ($p$-value) | 0.045 | 0.123 | 0.000 | 0.059 | 0.012 |
| Internship effect: T4 - T3 (beta coef.) | 0.719 | 1.294 | 0.150 | 7.432 | 0.561 |
| Internship effect: T4 - T3 ($p$-value) | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates with the information-only arm estimated separately and the pure control group as the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A8: Effects of Treatment Take-Up on Six-Month Employment Duration and Hours (IV; Information Pooled with Control)

| | (1) Months of Employment | (2) Hours worked last month | (3) Any wage-work hours: last 6 months | (4) Weekly wage-work hours: last 6 months | (5) Work Index |
|---|---|---|---|---|---|
| Take-up (T) [Skill training] | 0.172 | 0.053 | 0.012 | 2.113 | 0.076 |
| | (0.229) | (0.366) | (0.044) | (2.013) | (0.130) |
| Take-up (T+S) [Skill training with Stipend] | 0.478*** | 0.513* | 0.152*** | 4.538*** | 0.309*** |
| | (0.144) | (0.272) | (0.034) | (1.318) | (0.087) |
| Take-up (T+S+I) [Skill training, stipend plus internship] | 1.786*** | 2.783*** | 0.437*** | 17.960*** | 1.154*** |
| | (0.211) | (0.408) | (0.042) | (2.042) | (0.126) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| $R^2$ | 0.220 | 0.213 | 0.246 | 0.231 | 0.237 |
| Base-group mean | 0.641 | 1.062 | 0.148 | 5.510 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Stipend effect: (T+S)-(T) (beta coef.) | 0.307 | 0.460 | 0.140 | 2.425 | 0.233 |
| Stipend effect: (T+S)-(T) ($p$-value) | 0.126 | 0.164 | 0.000 | 0.171 | 0.038 |
| Internship effect: (T+S+I)-(T+S) (beta coef.) | 1.308 | 2.270 | 0.286 | 13.423 | 0.845 |
| Internship: (T+S+I)-(T+S) ($p$-value) | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T2–T4 as instruments. The pure control and information-only arms are combined as the omitted base group. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A9: Effects of Treatment Take-Up on Six-Month Employment Duration and Hours (IV; Information as a Separate Treatment Arm)

| | (1) Months of Employment | (2) Weekly wage-work Hours last month | (3) Any wage-work hours: last 6 months | (4) Weekly wage-work hours: last 6 months | (5) Job Continuation Index |
|---|---|---|---|---|---|
| Take-up (T1) [Information] | 0.170 | -0.152 | 0.009 | 1.167 | 0.052 |
| | (0.230) | (0.446) | (0.053) | (2.057) | (0.173) |
| Take-up (T2) [T1 + Skill training] | 0.366 | -0.121 | 0.022 | 3.443 | 0.155 |
| | (0.272) | (0.549) | (0.064) | (2.404) | (0.206) |
| Take-up (T3) [T2 + Stipend] | 0.623*** | 0.383 | 0.159*** | 5.534*** | 0.417*** |
| | (0.196) | (0.375) | (0.048) | (1.711) | (0.147) |
| Take-up (T4) [T3 + Internship] | 1.965*** | 2.622*** | 0.446*** | 19.189*** | 1.447*** |
| | (0.258) | (0.545) | (0.061) | (2.442) | (0.200) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| $R^2$ | 0.213 | 0.215 | 0.245 | 0.227 | 0.235 |
| Pure-control mean | 0.417 | 0.845 | 0.112 | 3.615 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Training effect: T2 - T1 (beta coef.) | 0.196 | 0.031 | 0.013 | 2.276 | 0.104 |
| Training effect: T2 - T1 ($p$-value) | 0.374 | 0.929 | 0.755 | 0.237 | 0.491 |
| Stipend effect: T3 - T2 (beta coef.) | 0.258 | 0.504 | 0.137 | 2.091 | 0.261 |
| Stipend effect: T3 - T2 ($p$-value) | 0.198 | 0.161 | 0.001 | 0.243 | 0.062 |
| Internship effect: T4 - T3 (beta coef.) | 1.342 | 2.239 | 0.287 | 13.655 | 1.030 |
| Internship effect: T4 - T3 ($p$-value) | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T1–T4 as instruments. The information-only arm is estimated separately and the pure control group is the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A10: Six-Month Earnings Outcomes: ITT Estimates (Information as a Separate Treatment Arm)

| | (1) Any income | (2) Any income last month | (3) Total earnings (six months) | (4) Earnings last month | (5) Income index |
|---|---|---|---|---|---|
| (T1) [Information] | 0.020 | -0.001 | 595.112 | 37.741 | 0.036 |
| | (0.041) | (0.038) | (1,206.558) | (220.566) | (0.117) |
| | [0.632] | [0.989] | [0.622] | [0.864] | [0.759] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T2) [T1 + Skill training] | 0.026 | -0.010 | 1,481.102 | -27.420 | 0.043 |
| | (0.037) | (0.037) | (1,029.846) | (216.353) | (0.106) |
| | [0.479] | [0.780] | [0.152] | [0.899] | [0.685] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T3) [T2 + Stipend] | 0.130 | 0.035 | 3,690.834 | 460.321 | 0.298 |
| | (0.037) | (0.036) | (1,028.276) | (242.058) | (0.107) |
| | [0.001]*** | [0.336] | [0.000]*** | [0.058]* | [0.006]*** |
| | {0.003}*** | {0.767} | {0.003}*** | {0.167} | {0.022}** |
| (T4) [T3 + Internship] | 0.280 | 0.138 | 8,800.296 | 1,122.042 | 0.739 |
| | (0.037) | (0.037) | (1,182.361) | (252.049) | (0.114) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** |
| | {0.000}*** | {0.001}*** | {0.000}*** | {0.000}*** | {0.000}*** |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| Pure-control mean | 0.107 | 0.091 | 2,939.364 | 545.460 | 0.000 |
| $R^2$ | 0.222 | 0.188 | 0.237 | 0.189 | 0.222 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Training effect: T2 - T1 (beta coef.) | 0.006 | -0.010 | 885.990 | -65.161 | 0.007 |
| Training effect: T2 - T1 ($p$-value) | 0.824 | 0.701 | 0.370 | 0.686 | 0.934 |
| Stipend effect: T3 - T2 (beta coef.) | 0.104 | 0.045 | 2,209.732 | 487.741 | 0.255 |
| Stipend effect: T3 - T2 ($p$-value) | 0.000 | 0.047 | 0.018 | 0.025 | 0.003 |
| Internship effect: T4 - T3 (beta coef.) | 0.149 | 0.103 | 5,109.461 | 661.720 | 0.440 |
| Internship effect: T4 - T3 ($p$-value) | 0.000 | 0.000 | 0.000 | 0.014 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates with the information-only arm estimated separately and the pure control group as the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A11: Effects of Treatment Take-Up on Six-Month Earnings Outcomes (IV; Information Pooled with Control)

| | (1) Any income | (2) Any income last month | (3) Total earnings | (4) Earnings last month | (5) Earnings index |
|---|---|---|---|---|---|
| Take-up (T) [Skill training] | 0.014 | -0.019 | 1662.143 | -115.811 | 0.018 |
| | (0.044) | (0.040) | (1514.336) | (254.297) | (0.125) |
| Take-up (T+S) [Skill training with Stipend] | 0.152*** | 0.048* | 4276.147*** | 574.389** | 0.329*** |
| | (0.034) | (0.029) | (1020.856) | (263.225) | (0.095) |
| Take-up (T+S+I) [Skill training, stipend plus internship] | 0.437*** | 0.230*** | 13778.395*** | 1809.733*** | 1.078*** |
| | (0.043) | (0.042) | (1578.898) | (319.833) | (0.133) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| $R^2$ | 0.245 | 0.191 | 0.236 | 0.192 | 0.230 |
| Base-group mean | 0.146 | 0.111 | 3,852.121 | 688.685 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Stipend effect: (T+S)-(T) (beta coef.) | 0.138 | 0.066 | 2,614.004 | 690.199 | 0.311 |
| Stipend effect: (T+S)-(T) ($p$-value) | 0.000 | 0.053 | 0.069 | 0.020 | 0.007 |
| Internship effect: (T+S+I)-(T+S) (beta coef.) | 0.284 | 0.182 | 9,502.248 | 1,235.344 | 0.749 |
| Internship: (T+S+I)-(T+S) ($p$-value) | 0.000 | 0.000 | 0.000 | 0.001 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T2–T4 as instruments. The pure control and information-only arms are combined as the omitted base group. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A12: Effects of Treatment Take-Up on Six-Month Earnings Outcomes (IV; Information as a Separate Treatment Arm)

| | (1) Any income | (2) Any income last month | (3) Total earnings | (4) Earnings last month | (5) Earnings index |
|---|---|---|---|---|---|
| Take-up (T1) [Information] | 0.018 | -0.005 | 525.088 | 15.181 | 0.025 |
| | (0.054) | (0.048) | (1536.880) | (282.123) | (0.152) |
| Take-up (T2) [T1 + Skill training] | 0.034 | -0.024 | 2260.815 | -98.503 | 0.045 |
| | (0.066) | (0.063) | (1791.346) | (371.945) | (0.189) |
| Take-up (T3) [T2 + Stipend] | 0.168*** | 0.043 | 4724.619*** | 587.354* | 0.380*** |
| | (0.049) | (0.046) | (1323.223) | (309.925) | (0.141) |
| Take-up (T4) [T3 + Internship] | 0.455*** | 0.225*** | 14331.345*** | 1825.719*** | 1.202*** |
| | (0.062) | (0.058) | (1844.521) | (390.710) | (0.183) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| $R^2$ | 0.243 | 0.192 | 0.233 | 0.192 | 0.229 |
| Pure-control mean | 0.107 | 0.091 | 2,939.364 | 545.460 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Training effect: T2 - T1 (beta coef.) | 0.017 | -0.019 | 1,735.727 | -113.683 | 0.020 |
| Training effect: T2 - T1 ($p$-value) | 0.700 | 0.622 | 0.229 | 0.650 | 0.882 |
| Stipend effect: T3 - T2 (beta coef.) | 0.133 | 0.068 | 2,463.804 | 685.857 | 0.336 |
| Stipend effect: T3 - T2 ($p$-value) | 0.001 | 0.062 | 0.086 | 0.028 | 0.010 |
| Internship effect: T4 - T3 (beta coef.) | 0.288 | 0.181 | 9,606.726 | 1,238.365 | 0.822 |
| Internship effect: T4 - T3 ($p$-value) | 0.000 | 0.000 | 0.000 | 0.001 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T1–T4 as instruments. The information-only arm is estimated separately and the pure control group is the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A13: Six-Month Remittance Outcomes: ITT Estimates (Information as a Separate Treatment Arm)

| | (1) Any remittance | (2) Total remittances | (3) Remittance frequency | (4) Remittances last month | (5) Remittance index |
|---|---|---|---|---|---|
| (T1) [Information] | -0.022 | -308.680 | -0.084 | -0.020 | -0.129 |
| | (0.026) | (247.446) | (0.073) | (0.021) | (0.114) |
| | [0.386] | [0.214] | [0.252] | [0.344] | [0.258] |
| | {0.882} | {0.882} | {0.882} | {0.882} | {0.882} |
| (T2) [T1 + Skill training] | 0.011 | 282.941 | 0.068 | 0.010 | 0.088 |
| | (0.025) | (307.328) | (0.076) | (0.023) | (0.121) |
| | [0.667] | [0.358] | [0.373] | [0.652] | [0.468] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T3) [T2 + Stipend] | 0.064 | 759.835 | 0.166 | 0.045 | 0.305 |
| | (0.024) | (280.876) | (0.074) | (0.020) | (0.113) |
| | [0.007]*** | [0.007]*** | [0.026]** | [0.030]** | [0.008]*** |
| | {0.029}** | {0.029}** | {0.068}* | {0.068}* | {0.029}** |
| (T4) [T3 + Internship] | 0.210 | 1,978.050 | 0.659 | 0.150 | 1.003 |
| | (0.032) | (325.955) | (0.093) | (0.028) | (0.147) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** |
| | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| Pure-control mean | 0.043 | 397.861 | 0.118 | 0.027 | 0.000 |
| $R^2$ | 0.228 | 0.205 | 0.218 | 0.212 | 0.228 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Training effect: T2 - T1 (beta coef.) | 0.033 | 591.621 | 0.152 | 0.031 | 0.217 |
| Training effect: T2 - T1 ($p$-value) | 0.056 | 0.008 | 0.019 | 0.041 | 0.017 |
| Stipend effect: T3 - T2 (beta coef.) | 0.054 | 476.894 | 0.098 | 0.034 | 0.217 |
| Stipend effect: T3 - T2 ($p$-value) | 0.012 | 0.165 | 0.166 | 0.078 | 0.055 |
| Internship effect: T4 - T3 (beta coef.) | 0.145 | 1,218.215 | 0.493 | 0.105 | 0.698 |
| Internship effect: T4 - T3 ($p$-value) | 0.000 | 0.001 | 0.000 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates with the information-only arm estimated separately and the pure control group as the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A14: Effects of Treatment Take-Up on Six-Month Remittance Outcomes (IV; Information Pooled with Control)

| | (1) Any remittance | (2) Total remittances | (3) Remittance frequency | (4) Remittances last month | (5) Remittance index |
|---|---|---|---|---|---|
| Take-up (T) [Skill training] | 0.052* | 959.370*** | 0.244** | 0.048* | 0.364** |
| | (0.028) | (368.004) | (0.103) | (0.025) | (0.151) |
| Take-up (T+S) [Skill training with Stipend] | 0.113*** | 1377.539*** | 0.321*** | 0.083*** | 0.581*** |
| | (0.023) | (307.277) | (0.073) | (0.020) | (0.116) |
| Take-up (T+S+I) [Skill training, stipend plus internship] | 0.380*** | 3725.644*** | 1.215*** | 0.277*** | 1.890*** |
| | (0.035) | (414.666) | (0.126) | (0.035) | (0.187) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| $R^2$ | 0.259 | 0.204 | 0.231 | 0.230 | 0.242 |
| Base-group mean | 0.049 | 340.976 | 0.129 | 0.034 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 0.061 | 418.169 | 0.077 | 0.035 | 0.217 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.047 | 0.398 | 0.465 | 0.202 | 0.208 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | 0.268 | 2,348.105 | 0.895 | 0.194 | 1.309 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T2–T4 as instruments. The pure control and information-only arms are combined as the omitted base group. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A15: Effects of Treatment Take-Up on Six-Month Remittance Outcomes (IV; Information as a Separate Treatment Arm)

| | (1) Any remittance | (2) Total remittances | (3) Remittance frequency | (4) Remittances last month | (5) Remittance index |
|---|---|---|---|---|---|
| Take-up (T1) [Information] | -0.036 | -469.948 | -0.132 | -0.031 | -0.202 |
| | (0.032) | (291.140) | (0.092) | (0.026) | (0.139) |
| Take-up (T2) [T1 + Skill training] | 0.011 | 423.564 | 0.093 | 0.012 | 0.114 |
| | (0.042) | (495.880) | (0.128) | (0.038) | (0.198) |
| Take-up (T3) [T2 + Stipend] | 0.082*** | 976.161*** | 0.208** | 0.057** | 0.387*** |
| | (0.030) | (336.782) | (0.092) | (0.025) | (0.139) |
| Take-up (T4) [T3 + Internship] | 0.342*** | 3230.759*** | 1.076*** | 0.244*** | 1.638*** |
| | (0.047) | (500.654) | (0.142) | (0.042) | (0.222) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| $R^2$ | 0.261 | 0.210 | 0.238 | 0.233 | 0.251 |
| Pure-control mean | 0.043 | 397.861 | 0.118 | 0.027 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Training effect: T2 - T1 (beta coef.) | 0.047 | 893.512 | 0.225 | 0.044 | 0.317 |
| Training effect: T2 - T1 ($p$-value) | 0.085 | 0.015 | 0.024 | 0.077 | 0.027 |
| Stipend effect: T3 - T2 (beta coef.) | 0.071 | 552.597 | 0.115 | 0.044 | 0.272 |
| Stipend effect: T3 - T2 ($p$-value) | 0.030 | 0.282 | 0.281 | 0.139 | 0.109 |
| Internship effect: T4 - T3 (beta coef.) | 0.260 | 2,254.598 | 0.869 | 0.188 | 1.251 |
| Internship effect: T4 - T3 ($p$-value) | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T1–T4 as instruments. The information-only arm is estimated separately and the pure control group is the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A16: Index Definitions and Construction

| **Index** | **Variables included** |
|---|---|
| Work-related physical stress | Experienced physical stress during the previous seven days; physical stress worsened during the previous six months. |
| Family stress | Experienced mental stress connected with family; family-related mental stress due to work worsened during the previous six months. |
| Non-work-related stress | Experienced mental stress unrelated to work or family; non-work-related mental stress worsened during the previous six months. |
| Borrowing | Amounts borrowed from microfinance institutions, commercial banks, local grocery shops, relatives, and neighbors or friends. |
| Savings | Any positive savings; savings held at home, with an NGO or microfinance institution, or in a bank; jewelry and cash holdings; outstanding lending; and access to formal financial services. |
| Housing conditions | Durable wall and floor materials; more than one room; separate kitchen and toilet; sanitary latrine; and electricity from the national grid or solar power. |
| Household assets | Productive and consumption assets, livestock holdings, dwelling size and ownership, cultivated land, and total landholdings. |
| Household livestock | Numbers of cattle, buffalo, goats, sheep, chickens, ducks, and hens. |
| Illness | Number of ill household members; illness of the household head; and illness among children younger than five and adults older than 65. |

Table A17: Six-Month Aspirations and Stress: ITT Estimates (Information as a Separate Treatment Arm)

| | (1) Aspiration Index | (2) Health Index | (3) Work-related physical stress Index | (4) Work-related mental stress Index | (5) Family-related stress Index | (6) Non-work related stress Index |
|---|---|---|---|---|---|---|
| (T1) [Information] | 0.436** | -0.038 | -0.171 | -0.129 | -0.321** | 0.141 |
| | (0.177) | (0.113) | (0.128) | (0.129) | (0.149) | (0.155) |
| (T2) [T1 + Skill training] | 0.395** | 0.057 | -0.246* | -0.047 | -0.321** | 0.227 |
| | (0.173) | (0.107) | (0.141) | (0.132) | (0.157) | (0.147) |
| (T3) [T2 + Stipend] | 0.630*** | -0.074 | -0.239* | -0.268** | -0.415*** | 0.246 |
| | (0.168) | (0.116) | (0.131) | (0.129) | (0.151) | (0.150) |
| (T4) [T3 + Internship] | 0.630*** | 0.040 | -0.167 | -0.032 | -0.402*** | 0.198 |
| | (0.169) | (0.113) | (0.137) | (0.141) | (0.153) | (0.153) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 | 2142 |
| Pure-control mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| $R^2$ | 0.217 | 0.190 | 0.194 | 0.175 | 0.211 | 0.199 |
| $p$-value for joint significance | 0.000 | 0.184 | 0.205 | 0.004 | 0.048 | 0.362 |
| Training effect: T2 - T1 (beta coef.) | -0.041 | 0.096 | -0.075 | 0.081 | 0.001 | 0.086 |
| Training effect: T2 - T1 ($p$-value) | 0.607 | 0.183 | 0.343 | 0.266 | 0.992 | 0.300 |
| Stipend effect: T3 - T2 (beta coef.) | 0.235 | -0.131 | 0.007 | -0.221 | -0.094 | 0.019 |
| Stipend effect: T3 - T2 ($p$-value) | 0.002 | 0.032 | 0.923 | 0.002 | 0.144 | 0.825 |
| Internship effect: T4 - T3 (beta coef.) | 0.000 | 0.115 | 0.072 | 0.236 | 0.012 | -0.049 |
| Internship effect: T4 - T3 ($p$-value) | 0.999 | 0.073 | 0.241 | 0.002 | 0.837 | 0.526 |
| Outcome at baseline | No | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates with the information-only arm estimated separately and the pure control group as the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A18: Effects of Treatment Take-Up on Six-Month Aspirations and Stress (IV; Information Pooled with Control)

| | (1) Aspiration Index | (2) Health Index | (3) Work-related physical stress Index | (4) Work-related mental stress Index | (5) Family-related stress Index | (6) Non-work related stress Index |
|---|---|---|---|---|---|---|
| Take-up (T) [Skill training] | 0.020 | 0.161 | -0.184 | 0.112 | -0.076 | 0.187 |
| | (0.126) | (0.113) | (0.136) | (0.118) | (0.115) | (0.129) |
| Take-up (T+S) [Skill training with Stipend] | 0.325*** | -0.054 | -0.129* | -0.209*** | -0.181** | 0.167* |
| | (0.090) | (0.088) | (0.074) | (0.077) | (0.076) | (0.100) |
| Take-up (T+S+I) [Skill training, stipend plus internship] | 0.412*** | 0.123 | -0.036 | 0.132 | -0.211* | 0.126 |
| | (0.113) | (0.110) | (0.117) | (0.112) | (0.111) | (0.126) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 | 2142 |
| $R^2$ | 0.223 | 0.189 | 0.193 | 0.176 | 0.205 | 0.200 |
| Base-group mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.071 | 0.132 | 0.001 | 0.074 | 0.316 |
| Stipend effect: (T+S)-(T) (beta coef.) | 0.305 | -0.215 | 0.055 | -0.321 | -0.104 | -0.021 |
| Stipend effect: (T+S)-(T) ($p$-value) | 0.006 | 0.019 | 0.640 | 0.002 | 0.290 | 0.870 |
| Internship effect: (T+S+I)-(T+S) (beta coef.) | 0.087 | 0.177 | 0.093 | 0.341 | -0.030 | -0.040 |
| Internship: (T+S+I)-(T+S) ($p$-value) | 0.378 | 0.054 | 0.328 | 0.002 | 0.740 | 0.713 |
| Outcome at baseline | No | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T2–T4 as instruments. The pure control and information-only arms are combined as the omitted base group. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces.$*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A19: Effects of Treatment Take-Up on Six-Month Aspirations and Stress (IV; Information as a Separate Treatment Arm)

| | (1) Aspiration Index | (2) Health Index Index | (3) Work-related physical stress Index | (4) Work-related mental stress Index | (5) Family-related stress Index | (6) Non-work related stress Index |
|---|---|---|---|---|---|---|
| Take-up (T1) [Information] | 0.561** | -0.052 | -0.223 | -0.170 | -0.416** | 0.182 |
| | (0.220) | (0.138) | (0.155) | (0.159) | (0.184) | (0.190) |
| Take-up (T2) [T1 + Skill training] | 0.677** | 0.101 | -0.431* | -0.083 | -0.554** | 0.397 |
| | (0.280) | (0.175) | (0.229) | (0.215) | (0.259) | (0.242) |
| Take-up (T3) [T2 + Stipend] | 0.822*** | -0.099 | -0.314** | -0.355** | -0.542*** | 0.324* |
| | (0.209) | (0.143) | (0.158) | (0.157) | (0.187) | (0.186) |
| Take-up (T4) [T3 + Internship] | 1.018*** | 0.068 | -0.270 | -0.048 | -0.650*** | 0.320 |
| | (0.253) | (0.171) | (0.204) | (0.213) | (0.234) | (0.237) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 | 2142 |
| $R^2$ | 0.201 | 0.189 | 0.183 | 0.169 | 0.166 | 0.192 |
| Pure-control mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.138 | 0.138 | 0.001 | 0.028 | 0.306 |
| Training effect: T2 - T1 (beta coef.) | 0.115 | 0.153 | -0.208 | 0.087 | -0.138 | 0.215 |
| Training effect: T2 - T1 ($p$-value) | 0.377 | 0.168 | 0.125 | 0.465 | 0.282 | 0.097 |
| Stipend effect: T3 - T2 (beta coef.) | 0.145 | -0.199 | 0.117 | -0.272 | 0.012 | -0.073 |
| Stipend effect: T3 - T2 ($p$-value) | 0.261 | 0.033 | 0.349 | 0.016 | 0.918 | 0.597 |
| Internship effect: T4 - T3 (beta coef.) | 0.196 | 0.167 | 0.044 | 0.307 | -0.108 | -0.004 |
| Internship effect: T4 - T3 ($p$-value) | 0.079 | 0.074 | 0.653 | 0.011 | 0.287 | 0.976 |
| Outcome at baseline | No | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T1–T4 as instruments. The information-only arm is estimated separately and the pure control group is the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A20: Six-Month Financial Behavior and Job Support: ITT Estimates (Information as a Separate Treatment Arm)

| | (1) Borrowing index | (2) Savings index | (3) Spillover: Inspired to seek a job | (4) Spillover: Helped obtain a job | (5) Spillover: No. inspired to seek jobs | (6) Spillover: No. helped obtain jobs |
|---|---|---|---|---|---|---|
| (T1) [Information] | 0.104 | 0.192 | 0.025* | 0.019 | 0.032** | 0.026* |
| | (0.108) | (0.142) | (0.013) | (0.012) | (0.015) | (0.014) |
| (T2) [T1 + Skill training] | -0.068 | 0.171 | 0.022 | 0.018 | 0.037* | 0.033* |
| | (0.115) | (0.142) | (0.013) | (0.013) | (0.020) | (0.019) |
| (T3) [T2 + Stipend] | 0.055 | 0.410*** | 0.038*** | 0.029** | 0.052*** | 0.039** |
| | (0.111) | (0.154) | (0.014) | (0.013) | (0.018) | (0.016) |
| (T4) [T3 + Internship] | -0.188 | 0.339** | 0.044*** | 0.033** | 0.059*** | 0.047** |
| | (0.123) | (0.157) | (0.016) | (0.015) | (0.020) | (0.019) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 | 2142 |
| Pure-control mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| $R^2$ | 0.169 | 0.197 | 0.177 | 0.178 | 0.285 | 0.301 |
| $p$-value for joint significance | 0.002 | 0.020 | 0.023 | 0.154 | 0.046 | 0.119 |
| Training effect: T2 - T1 (beta coef.) | -0.172 | -0.021 | -0.003 | -0.001 | 0.004 | 0.007 |
| Training effect: T2 - T1 ($p$-value) | 0.036 | 0.781 | 0.681 | 0.920 | 0.759 | 0.580 |
| Stipend effect: T3 - T2 (beta coef.) | 0.123 | 0.239 | 0.016 | 0.010 | 0.015 | 0.007 |
| Stipend effect: T3 - T2 ($p$-value) | 0.161 | 0.006 | 0.112 | 0.250 | 0.379 | 0.663 |
| Internship effect: T4 - T3 (beta coef.) | -0.243 | -0.071 | 0.006 | 0.004 | 0.007 | 0.008 |
| Internship effect: T4 - T3 ($p$-value) | 0.001 | 0.350 | 0.599 | 0.657 | 0.570 | 0.564 |
| Outcome at baseline | No | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates with the information-only arm estimated separately and the pure control group as the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A21: Effects of Treatment Take-Up on Six-Month Financial Behavior and Job Support (IV; Information Pooled with Control)

| | (1) Borrowing index | (2) Savings index | (3) Spillover: Inspired to seek a job | (4) Spillover: Helped obtain a job | (5) Spillover: No. inspired to seek jobs |
|---|---|---|---|---|---|
| Take-up (T) [Skill training] | -0.277** | 0.011 | 0.000 | 0.003 | 0.016 |
| | (0.130) | (0.122) | (0.013) | (0.011) | (0.024) |
| Take-up (T+S) [Skill training with Stipend] | -0.047 | 0.328*** | 0.022* | 0.017 | 0.032* |
| | (0.091) | (0.105) | (0.012) | (0.010) | (0.017) |
| Take-up (T+S+I) [Skill training, stipend plus internship] | -0.461*** | 0.289** | 0.038*** | 0.028** | 0.052*** |
| | (0.119) | (0.121) | (0.015) | (0.012) | (0.020) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 |
| $R^2$ | 0.152 | 0.197 | 0.182 | 0.182 | 0.288 |
| Base-group mean | 0.085 | -0.031 | 0.009 | 0.006 | 0.010 |
| $p$-value for joint significance | 0.000 | 0.005 | 0.011 | 0.073 | 0.055 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 0.231 | 0.317 | 0.022 | 0.013 | 0.017 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.080 | 0.010 | 0.133 | 0.304 | 0.519 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | -0.414 | -0.039 | 0.015 | 0.011 | 0.020 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.000 | 0.720 | 0.339 | 0.406 | 0.282 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T2–T4 as instruments. The pure control and information-only arms are combined as the omitted base group. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A22: Effects of Treatment Take-Up on Six-Month Financial Behavior and Job Support (IV; Information as a Separate Treatment Arm)

| | (1) Borrowing index | (2) Savings index | (3) Spillover: Inspired to seek a job | (4) Spillover: Helped obtain a job | (5) Spillover: No. inspired to seek jobs | (6) Spillover: No. helped obtain jobs |
|---|---|---|---|---|---|---|
| Take-up (T1) [Information] | 0.143 | 0.246 | 0.032** | 0.024* | 0.041** | 0.033** |
| | (0.135) | (0.174) | (0.015) | (0.014) | (0.017) | (0.016) |
| Take-up (T2) [T1 + Skill training] | -0.114 | 0.291 | 0.037* | 0.031 | 0.063** | 0.056* |
| | (0.192) | (0.234) | (0.021) | (0.020) | (0.032) | (0.030) |
| Take-up (T3) [T2 + Stipend] | 0.076 | 0.538*** | 0.050*** | 0.037** | 0.068*** | 0.051*** |
| | (0.139) | (0.189) | (0.017) | (0.016) | (0.020) | (0.019) |
| Take-up (T4) [T3 + Internship] | -0.310 | 0.547** | 0.071*** | 0.053** | 0.096*** | 0.077*** |
| | (0.191) | (0.241) | (0.023) | (0.022) | (0.029) | (0.028) |
| Observations | 2142 | 2142 | 2142 | 2142 | 2142 | 2142 |
| $R^2$ | 0.164 | 0.187 | 0.175 | 0.175 | 0.284 | 0.299 |
| Pure-control mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.009 | 0.006 | 0.085 | 0.012 | 0.052 |
| Training effect: T2 - T1 (beta coef.) | -0.257 | 0.045 | 0.005 | 0.007 | 0.021 | 0.024 |
| Training effect: T2 - T1 ($p$-value) | 0.048 | 0.720 | 0.697 | 0.543 | 0.371 | 0.294 |
| Stipend effect: T3 - T2 (beta coef.) | 0.190 | 0.247 | 0.013 | 0.006 | 0.005 | -0.005 |
| Stipend effect: T3 - T2 ($p$-value) | 0.164 | 0.062 | 0.407 | 0.652 | 0.862 | 0.839 |
| Internship effect: T4 - T3 (beta coef.) | -0.386 | 0.010 | 0.022 | 0.016 | 0.028 | 0.025 |
| Internship effect: T4 - T3 ($p$-value) | 0.001 | 0.938 | 0.202 | 0.277 | 0.171 | 0.235 |
| Outcome at baseline | No | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T1–T4 as instruments. The information-only arm is estimated separately and the pure control group is the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A23: Employment Outcomes during 18-month follow-up: ITT Estimates (Information as a Separate Treatment Arm)

| | (1) Any Job | (2) Job at RMG | (3) Job at Manufacturing | (4) Any full-time job | (5) Employment Index |
|---|---|---|---|---|---|
| (T1) [Information] | 0.100 | -0.007 | 0.012 | 0.103 | 0.156 |
| | (0.057) | (0.023) | (0.026) | (0.057) | (0.151) |
| | [0.080]* | [0.763] | [0.661] | [0.070]* | [0.301] |
| | {0.456} | {1.000} | {1.000} | {0.456} | {1.000} |
| (T2) [T1 + Skill training] | 0.088 | 0.002 | 0.021 | 0.095 | 0.181 |
| | (0.062) | (0.026) | (0.030) | (0.063) | (0.173) |
| | [0.157] | [0.941] | [0.480] | [0.133] | [0.298] |
| | {0.897} | {1.000} | {1.000} | {0.897} | {1.000} |
| (T3) [T2 + Stipend] | 0.102 | 0.025 | 0.060 | 0.091 | 0.314 |
| | (0.059) | (0.026) | (0.032) | (0.060) | (0.168) |
| | [0.085]* | [0.342] | [0.063]* | [0.130] | [0.063]* |
| | {0.322} | {0.780} | {0.322} | {0.370} | {0.322} |
| (T4) [T3 + Internship] | 0.183 | 0.056 | 0.101 | 0.194 | 0.598 |
| | (0.060) | (0.026) | (0.030) | (0.061) | (0.163) |
| | [0.003]*** | [0.029]** | [0.001]*** | [0.002]*** | [0.000]*** |
| | {0.008}*** | {0.067}* | {0.006}*** | {0.006}*** | {0.004}*** |
| Observations | 2085 | 2085 | 2085 | 2085 | 2085 |
| Pure-control mean | 0.257 | 0.021 | 0.027 | 0.246 | 0.000 |
| $R^2$ | 0.340 | 0.154 | 0.185 | 0.332 | 0.240 |
| $p$-value for joint significance | 0.002 | 0.003 | 0.000 | 0.000 | 0.000 |
| Training effect: T2 - T1 (beta coef.) | -0.012 | 0.009 | 0.010 | -0.008 | 0.024 |
| Training effect: T2 - T1 ($p$-value) | 0.725 | 0.500 | 0.524 | 0.819 | 0.784 |
| Stipend effect: T3 - T2 (beta coef.) | 0.014 | 0.023 | 0.039 | -0.004 | 0.133 |
| Stipend effect: T3 - T2 ($p$-value) | 0.641 | 0.204 | 0.041 | 0.890 | 0.161 |
| Internship effect: T4 - T3 (beta coef.) | 0.081 | 0.031 | 0.041 | 0.103 | 0.284 |
| Internship effect: T4 - T3 ($p$-value) | 0.007 | 0.087 | 0.092 | 0.000 | 0.007 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates with the information-only arm estimated separately and the pure control group as the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A24: Effects of Treatment Take-Up on 18-Month Employment Outcomes (IV; Information Pooled with Control)

| | (1) Any Job | (2) Job at RMG | (3) Job at Manufacturing | (4) Any full-time job | (5) Employment Index |
|---|---|---|---|---|---|
| Take-up (T) [Skill training] | 0.003 | 0.014 | 0.020 | 0.011 | 0.068 |
| | (0.056) | (0.022) | (0.026) | (0.057) | (0.135) |
| | [0.959] | [0.537] | [0.448] | [0.847] | [0.615] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| Take-up (T+S) [Skill training with Stipend] | 0.022 | 0.042 | 0.068 | 0.004 | 0.208 |
| | (0.038) | (0.019) | (0.024) | (0.037) | (0.096) |
| | [0.566] | [0.028]** | [0.004]*** | [0.903] | [0.031]** |
| | {1.000} | {0.117} | {0.048}** | {1.000} | {0.117} |
| Take-up (T+S+I) [Skill training, stipend plus internship] | 0.160 | 0.103 | 0.150 | 0.174 | 0.671 |
| | (0.044) | (0.024) | (0.029) | (0.044) | (0.119) |
| | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** | [0.000]*** |
| | {0.001}*** | {0.000}*** | {0.000}*** | {0.000}*** | {0.000}*** |
| Observations | 2085 | 2085 | 2085 | 2085 | 2085 |
| Base-group mean | 0.315 | 0.029 | 0.042 | 0.301 | 0.00 |
| $R^2$ | 0.341 | 0.162 | 0.197 | 0.331 | 0.253 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Take-up (T) effect (beta coef.) | 0.003 | 0.014 | 0.020 | 0.011 | 0.068 |
| Take-up (T) effect ($p$-value) | 0.959 | 0.537 | 0.448 | 0.847 | 0.615 |
| Stipend effect: (T+S)-(T) (beta coef.) | 0.019 | 0.028 | 0.048 | -0.006 | 0.140 |
| Stipend effect: (T+S)-(T) ($p$-value) | 0.679 | 0.280 | 0.075 | 0.885 | 0.267 |
| Internship effect: (T+S+I)-(T+S) (beta coef.) | 0.138 | 0.061 | 0.083 | 0.170 | 0.463 |
| Internship effect: (T+S+I)-(T+S) ($p$-value) | 0.001 | 0.019 | 0.014 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T2–T4 as instruments. The pure control and information-only arms are combined as the omitted base group. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A25: Effects of Treatment Take-Up on Employment Outcomes, 18-month follow-up (IV; Information as a Separate Treatment Arm)

| | (1) Any job (at 18 months) | (2) Job at RMG (at 18 months) | (3) Job at Manufacturing (at 18 months) | (4) Any full-time job (at 18 months) | (5) Employment index (at 18 months) |
|---|---|---|---|---|---|
| Take-up (T1) [Information] | 0.128* | -0.011 | 0.012 | 0.132** | 0.188 |
| | (0.067) | (0.027) | (0.032) | (0.066) | (0.177) |
| Take-up (T2) [T1 + Skill training] | 0.148 | 0.001 | 0.033 | 0.161 | 0.293 |
| | (0.099) | (0.042) | (0.049) | (0.101) | (0.277) |
| Take-up (T3) [T2 + Stipend] | 0.132* | 0.032 | 0.078** | 0.118* | 0.407** |
| | (0.069) | (0.032) | (0.039) | (0.071) | (0.200) |
| Take-up (T4) [T3 + Internship] | 0.294*** | 0.091** | 0.163*** | 0.312*** | 0.963*** |
| | (0.087) | (0.039) | (0.046) | (0.088) | (0.242) |
| Observations | 2085 | 2085 | 2085 | 2085 | 2085 |
| $R^2$ | 0.325 | 0.162 | 0.196 | 0.314 | 0.243 |
| Pure-control mean | 0.257 | 0.021 | 0.027 | 0.246 | 0.000 |
| $p$-value for joint significance | 0.000 | 0.001 | 0.000 | 0.000 | 0.000 |
| Training effect: T2 - T1 (beta coef.) | 0.021 | 0.012 | 0.022 | 0.029 | 0.105 |
| Training effect: T2 - T1 ($p$-value) | 0.730 | 0.601 | 0.434 | 0.630 | 0.512 |
| Stipend effect: T3 - T2 (beta coef.) | -0.016 | 0.032 | 0.045 | -0.043 | 0.114 |
| Stipend effect: T3 - T2 ($p$-value) | 0.755 | 0.258 | 0.124 | 0.406 | 0.477 |
| Internship effect: T4 - T3 (beta coef.) | 0.162 | 0.059 | 0.085 | 0.194 | 0.556 |
| Internship effect: T4 - T3 ($p$-value) | 0.000 | 0.025 | 0.012 | 0.000 | 0.000 |
| Outcome at baseline | No | No | No | No | No |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T1–T4 as instruments. The information-only arm is estimated separately and the pure control group is the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A26: Origin-Household Socioeconomic Indices: ITT-ANCOVA Estimates (Information as a Separate Treatment Arm)

| | (1) Illness index | (2) Asset index | (3) Livestock index | (4) Housing index | (5) Savings index | (6) Borrowing index |
|---|---|---|---|---|---|---|
| (T1) [Information] | 0.106 | 0.142 | 0.030 | 0.081 | 0.081 | 0.104 |
| | (0.112) | (0.154) | (0.111) | (0.121) | (0.142) | (0.160) |
| | [0.343] | [0.357] | [0.790] | [0.502] | [0.566] | [0.515] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T2) [T1 + Skill training] | 0.020 | 0.259 | 0.113 | 0.135 | 0.120 | 0.104 |
| | (0.114) | (0.175) | (0.167) | (0.122) | (0.137) | (0.153) |
| | [0.859] | [0.140] | [0.497] | [0.269] | [0.382] | [0.496] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T3) [T2 + Stipend] | 0.053 | 0.441 | 0.100 | 0.166 | 0.025 | 0.094 |
| | (0.115) | (0.254) | (0.124) | (0.114) | (0.132) | (0.161) |
| | [0.647] | [0.083]* | [0.421] | [0.148] | [0.852] | [0.559] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T4) [T3 + Internship] | -0.065 | 0.374 | 0.135 | 0.145 | 0.066 | 0.015 |
| | (0.131) | (0.184) | (0.135) | (0.117) | (0.138) | (0.143) |
| | [0.621] | [0.043]** | [0.319] | [0.219] | [0.632] | [0.914] |
| | {1.000} | {0.629} | {1.000} | {1.000} | {1.000} | {1.000} |
| Observations | 2120 | 2120 | 2120 | 2120 | 2120 | 2120 |
| Pure-control mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| $R^2$ | 0.184 | 0.307 | 0.335 | 0.328 | 0.271 | 0.286 |
| $p$-value for joint significance | 0.252 | 0.149 | 0.673 | 0.363 | 0.706 | 0.775 |
| Training effect: T2 - T1 (beta coef.) | -0.086 | 0.118 | 0.084 | 0.054 | 0.039 | 0.000 |
| Training effect: T2 - T1 ($p$-value) | 0.171 | 0.311 | 0.452 | 0.347 | 0.530 | 0.996 |
| Stipend effect: T3 - T2 (beta coef.) | 0.032 | 0.182 | -0.013 | 0.031 | -0.096 | -0.010 |
| Stipend effect: T3 - T2 ($p$-value) | 0.649 | 0.434 | 0.883 | 0.583 | 0.164 | 0.905 |
| Internship effect: T4 - T3 (beta coef.) | -0.117 | -0.067 | 0.035 | -0.021 | 0.041 | -0.079 |
| Internship effect: T4 - T3 ($p$-value) | 0.181 | 0.784 | 0.651 | 0.713 | 0.513 | 0.324 |
| Outcome at baseline | Yes | Yes | Yes | Yes | Yes | Yes |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates with the information-only arm estimated separately and the pure control group as the omitted category. The estimates use ANCOVA specifications. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A27: Effects of Treatment Take-Up on Origin-Household Socioeconomic Indices (IV; Information Pooled with Control)

| | (1) Illness Index | (2) Asset index | (3) Livestock index | (4) Housing index | (5) Savings index | (6) Borrowing index |
|---|---|---|---|---|---|---|
| Take-up (T) [Skill training] | -0.125 | 0.162 | 0.141 | 0.112 | 0.089 | 0.041 |
| | (0.100) | (0.110) | (0.172) | (0.095) | (0.098) | (0.084) |
| Take-up (T+S) [Skill training with Stipend] | -0.054 | 0.253* | 0.090 | 0.129** | -0.060 | 0.021 |
| | (0.076) | (0.131) | (0.075) | (0.063) | (0.076) | (0.087) |
| Take-up (T+S+I) [Skill training, stipend plus internship] | -0.260** | 0.257** | 0.164 | 0.126 | -0.007 | -0.099 |
| | (0.127) | (0.105) | (0.108) | (0.097) | (0.088) | (0.093) |
| Observations | 2120 | 2120 | 2120 | 2120 | 2120 | 2120 |
| $R^2$ | 0.179 | 0.427 | 0.331 | 0.326 | 0.276 | 0.259 |
| Base-group mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| $p$-value for joint significance | 0.223 | 0.028 | 0.441 | 0.225 | 0.514 | 0.497 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 0.072 | 0.092 | -0.050 | 0.017 | -0.149 | -0.020 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.501 | 0.536 | 0.716 | 0.846 | 0.134 | 0.835 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | -0.206 | 0.004 | 0.073 | -0.003 | 0.053 | -0.120 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.119 | 0.981 | 0.461 | 0.969 | 0.548 | 0.188 |
| Outcome at baseline | Yes | Yes | Yes | Yes | Yes | Yes |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T2–T4 as instruments. The pure control and information-only arms are combined as the omitted base group. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A28: Effects of Treatment Take-Up on Origin-Household Socioeconomic Indices (IV; Information as a Separate Treatment Arm)

| | (1) Illness index | (2) Asset index | (3) Livestock index | (4) Housing index | (5) | (6) |
|---|---|---|---|---|---|---|
| Take-up (T1) [Information] | 0.144 | 0.182 | 0.036 | 0.105 | 0.107 | 0.139 |
| | (0.137) | (0.188) | (0.135) | (0.150) | (0.172) | (0.197) |
| Take-up (T2) [T1 + Skill training] | 0.039 | 0.455 | 0.197 | 0.236 | 0.213 | 0.186 |
| | (0.188) | (0.287) | (0.273) | (0.201) | (0.224) | (0.252) |
| Take-up (T3) [T2 + Stipend] | 0.070 | 0.585* | 0.131 | 0.219 | 0.032 | 0.125 |
| | (0.141) | (0.314) | (0.151) | (0.142) | (0.161) | (0.200) |
| Take-up (T4) [T3 + Internship] | -0.107 | 0.612** | 0.220 | 0.235 | 0.108 | 0.024 |
| | (0.201) | (0.276) | (0.203) | (0.183) | (0.207) | (0.217) |
| Observations | 2120 | 2120 | 2120 | 2120 | 2120 | 2120 |
| $R^2$ | 0.181 | 0.300 | 0.330 | 0.319 | 0.270 | 0.285 |
| Pure-control mean | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| $p$-value for joint significance | 0.188 | 0.097 | 0.609 | 0.276 | 0.646 | 0.724 |
| Training effect: T2 - T1 (beta coef.) | -0.105 | 0.273 | 0.162 | 0.131 | 0.105 | 0.048 |
| Training effect: T2 - T1 ($p$-value) | 0.313 | 0.157 | 0.411 | 0.180 | 0.305 | 0.671 |
| Stipend effect: T3 - T2 (beta coef.) | 0.031 | 0.130 | -0.066 | -0.017 | -0.181 | -0.061 |
| Stipend effect: T3 - T2 ($p$-value) | 0.779 | 0.677 | 0.692 | 0.867 | 0.121 | 0.646 |
| Internship effect: T4 - T3 (beta coef.) | -0.178 | 0.027 | 0.089 | 0.017 | 0.076 | -0.102 |
| Internship effect: T4 - T3 ($p$-value) | 0.186 | 0.933 | 0.446 | 0.854 | 0.442 | 0.358 |
| Outcome at baseline | Yes | Yes | Yes | Yes | Yes | Yes |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T1–T4 as instruments. The information-only arm is estimated separately and the pure control group is the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A29: Origin-Household Remittances and Urban Labor Income: ITT-ANCOVA Estimates (Information as a Separate Treatment Arm)

| | (1) Months of remittances | (2) Frequency of remittances | (3) Total remittances | (4) Any urban labor income | (5) Total urban labor income |
|---|---|---|---|---|---|
| (T1) [Information] | -0.026 | -0.034 | -297.509 | 0.025 | 6,012.698 |
| | (0.152) | (0.158) | (1,075.126) | (0.034) | (4,072.420) |
| | [0.863] | [0.830] | [0.782] | [0.470] | [0.141] |
| | {1.000} | {1.000} | {1.000} | {1.000} | {1.000} |
| (T2) [T1 + Skill training] | 0.186 | 0.188 | 528.799 | 0.046 | 7,659.422 |
| | (0.169) | (0.174) | (924.306) | (0.034) | (3,904.918) |
| | [0.272] | [0.282] | [0.568] | [0.177] | [0.051]* |
| | {0.803} | {0.803} | {1.000} | {0.803} | {0.583} |
| (T3) [T2 + Stipend] | 0.410 | 0.408 | 2,542.711 | 0.053 | 7,959.127 |
| | (0.163) | (0.170) | (1,067.154) | (0.034) | (4,152.738) |
| | [0.013]** | [0.017]** | [0.018]** | [0.123] | [0.057]* |
| | {0.069}* | {0.069}* | {0.069}* | {0.282} | {0.161} |
| (T4) [T3 + Internship] | 0.952 | 0.996 | 2,732.227 | 0.070 | 10,037.133 |
| | (0.164) | (0.170) | (878.950) | (0.037) | (4,295.862) |
| | [0.000]*** | [0.000]*** | [0.002]*** | [0.057]* | [0.020]** |
| | {0.000}*** | {0.000}*** | {0.008}*** | {0.131} | {0.058}* |
| Observations | 2120 | 2120 | 2120 | 2120 | 2120 |
| Pure-control mean | 0.358 | 0.364 | 2,440.642 | 0.096 | 10,716.578 |
| $R^2$ | 0.227 | 0.226 | 0.165 | 0.187 | 0.197 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.091 | 0.127 |
| Training effect: T2 - T1 (beta coef.) | 0.212 | 0.222 | 826.308 | 0.022 | 1,646.724 |
| Training effect: T2 - T1 ($p$-value) | 0.108 | 0.097 | 0.269 | 0.324 | 0.465 |
| Stipend effect: T3 - T2 (beta coef.) | 0.224 | 0.220 | 2,013.913 | 0.006 | 299.705 |
| Stipend effect: T3 - T2 ($p$-value) | 0.093 | 0.096 | 0.026 | 0.785 | 0.899 |
| Internship effect: T4 - T3 (beta coef.) | 0.542 | 0.588 | 189.516 | 0.018 | 2,078.006 |
| Internship effect: T4 - T3 ($p$-value) | 0.001 | 0.001 | 0.825 | 0.439 | 0.374 |
| Outcome at baseline | Yes | Yes | Yes | Yes | Yes |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates with the information-only arm estimated separately and the pure control group as the omitted category. The estimates use ANCOVA specifications. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A30: Effects of Treatment Take-Up on Origin-Household Remittances and Urban Labor Income (IV; Information Pooled with Control)

| | (1) Any urban labor income (=1) | (2) Total urban labor income (BDT) | (3) Months receiving remittances | (4) Remittance frequency | (5) Total remittances (BDT) |
|---|---|---|---|---|---|
| Take-up (T) [Skill training] | 0.045 | 4437.990 | 0.367* | 0.382* | 1419.854 |
| | (0.036) | (3569.186) | (0.214) | (0.217) | (1137.295) |
| Take-up (T+S) [Skill training with Stipend] | 0.043** | 3774.271 | 0.584*** | 0.591*** | 3717.424*** |
| | (0.021) | (2319.319) | (0.165) | (0.167) | (1285.510) |
| Take-up (T+S+I) [Skill training, stipend plus internship] | 0.082*** | 8157.181** | 1.621*** | 1.706*** | 4932.805*** |
| | (0.031) | (3456.350) | (0.229) | (0.238) | (1226.848) |
| Observations | 2120 | 2120 | 2120 | 2120 | 2120 |
| $R^2$ | 0.186 | 0.195 | 0.236 | 0.236 | 0.170 |
| Base-group mean | 0.094 | 10,123.440 | 0.428 | 0.433 | 2,200.363 |
| $p$-value for joint significance | 0.031 | 0.096 | 0.000 | 0.000 | 0.000 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | -0.002 | -663.719 | 0.217 | 0.209 | 2,297.570 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.947 | 0.856 | 0.279 | 0.295 | 0.049 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | 0.040 | 4,382.910 | 1.037 | 1.115 | 1,215.381 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.232 | 0.202 | 0.000 | 0.000 | 0.281 |
| Outcome at baseline | Yes | Yes | Yes | Yes | Yes |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T2–T4 as instruments. The pure control and information-only arms are combined as the omitted base group. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A31: Effects of Treatment Take-Up on Origin-Household Remittances and Urban Labor Income (IV; Information as a Separate Treatment Arm)

| | (1) Months of remittances | (2) Frequency of remittances | (3) Total remittances | (4) Any urban labor income | (5) Total urban labor income |
|---|---|---|---|---|---|
| Take-up (T1) [Information] | -0.063 | -0.074 | -473.108 | 0.031 | 7748.853 |
| | (0.181) | (0.188) | (1323.570) | (0.041) | (5017.824) |
| Take-up (T2) [T1 + Skill training] | 0.295 | 0.297 | 851.476 | 0.080 | 13309.016** |
| | (0.274) | (0.282) | (1504.816) | (0.057) | (6526.962) |
| Take-up (T3) [T2 + Stipend] | 0.530*** | 0.527** | 3350.020** | 0.069 | 10417.549** |
| | (0.196) | (0.205) | (1303.365) | (0.042) | (5156.514) |
| Take-up (T4) [T3 + Internship] | 1.555*** | 1.628*** | 4456.777*** | 0.115** | 16329.726** |
| | (0.240) | (0.251) | (1317.091) | (0.057) | (6703.946) |
| Observations | 2120 | 2120 | 2120 | 2120 | 2120 |
| $R^2$ | 0.238 | 0.238 | 0.163 | 0.182 | 0.181 |
| Pure-control mean | 0.358 | 0.364 | 2,440.642 | 0.096 | 10,716.578 |
| $p$-value for joint significance | 0.000 | 0.000 | 0.000 | 0.060 | 0.096 |
| Training effect: T2 - T1 (beta coef.) | 0.358 | 0.371 | 1,324.584 | 0.050 | 5,560.163 |
| Training effect: T2 - T1 ($p$-value) | 0.091 | 0.084 | 0.222 | 0.166 | 0.125 |
| Stipend effect: T3 - T2 (beta coef.) | 0.235 | 0.231 | 2,498.544 | -0.011 | -2,891.467 |
| Stipend effect: T3 - T2 ($p$-value) | 0.259 | 0.266 | 0.052 | 0.762 | 0.449 |
| Internship effect: T4 - T3 (beta coef.) | 1.025 | 1.101 | 1,106.756 | 0.045 | 5,912.177 |
| Internship effect: T4 - T3 ($p$-value) | 0.000 | 0.000 | 0.336 | 0.192 | 0.110 |
| Outcome at baseline | Yes | Yes | Yes | Yes | Yes |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T1–T4 as instruments. The information-only arm is estimated separately and the pure control group is the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A32: Origin-Household Income, Consumption, and Poverty: ITT-ANCOVA Estimates (Information as a Separate Treatment Arm)

| | (1) Total Income | (2) Total Consumption | (3) Food Consumption | (4) Non-Food Consumption | (5) Income Poverty | (6) Consumption Poverty |
|---|---|---|---|---|---|---|
| (T1) [Information] | 9.206 | 4.819 | 2.084 | 0.528 | -0.122 | -0.006 |
| | (5.797) | (2.528) | (1.010) | (0.453) | (0.060) | (0.023) |
| | [0.114] | [0.058]* | [0.040]** | [0.246] | [0.042]** | [0.790] |
| | {0.418} | {0.284} | {0.284} | {0.722} | {0.284} | {1.000} |
| (T2) [T1 + Skill training] | 11.253 | 2.599 | 1.150 | 0.358 | -0.157 | 0.005 |
| | (6.457) | (2.751) | (1.173) | (0.490) | (0.063) | (0.022) |
| | [0.083]* | [0.346] | [0.328] | [0.465] | [0.013]** | [0.832] |
| | {0.608} | {1.000} | {1.000} | {1.000} | {0.192} | {1.000} |
| (T3) [T2 + Stipend] | 16.469 | 5.875 | 2.423 | 0.630 | -0.185 | -0.011 |
| | (6.244) | (2.837) | (1.199) | (0.513) | (0.065) | (0.022) |
| | [0.009]*** | [0.040]** | [0.044]** | [0.220] | [0.005]*** | [0.627] |
| | {0.066}* | {0.163} | {0.163} | {0.647} | {0.066}* | {1.000} |
| (T4) [T3 + Internship] | 18.008 | 2.633 | 1.031 | 0.374 | -0.235 | -0.009 |
| | (6.625) | (2.831) | (1.205) | (0.530) | (0.065) | (0.023) |
| | [0.007]*** | [0.353] | [0.393] | [0.482] | [0.000]*** | [0.683] |
| | {0.052}* | {1.000} | {1.000} | {1.000} | {0.005}*** | {1.000} |
| Observations | 2120 | 2120 | 2120 | 2120 | 2120 | 2120 |
| Pure-control mean | 63.564 | 63.342 | 25.923 | 7.777 | 0.551 | 0.979 |
| $R^2$ | 0.230 | 0.340 | 0.325 | 0.297 | 0.258 | 0.170 |
| $p$-value for joint significance | 0.032 | 0.089 | 0.110 | 0.553 | 0.003 | 0.613 |
| Training effect: T2 - T1 (beta coef.) | 2.047 | -2.220 | -0.934 | -0.169 | -0.035 | 0.011 |
| Training effect: T2 - T1 ($p$-value) | 0.670 | 0.298 | 0.378 | 0.597 | 0.263 | 0.383 |
| Stipend effect: T3 - T2 (beta coef.) | 5.215 | 3.275 | 1.274 | 0.272 | -0.027 | -0.015 |
| Stipend effect: T3 - T2 ($p$-value) | 0.153 | 0.045 | 0.100 | 0.311 | 0.402 | 0.221 |
| Internship effect: T4 - T3 (beta coef.) | 1.539 | -3.242 | -1.393 | -0.257 | -0.051 | 0.001 |
| Internship effect: T4 - T3 ($p$-value) | 0.673 | 0.056 | 0.097 | 0.340 | 0.155 | 0.928 |
| Outcome at baseline | Yes | Yes | Yes | Yes | Yes | Yes |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table reports ITT estimates with the information-only arm estimated separately and the pure control group as the omitted category. The estimates use ANCOVA specifications. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A33: Effects of Treatment Take-Up on Origin-Household Income, Consumption, and Poverty (IV; Information Pooled with Control)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Daily household per capita measures (BDT) | | | | | |
| | Total income | Total consumption | Food consumption | Non-food consumption | Income poverty | Consumption poverty |
| Take-up (T) [Skill training] | 4.821 | -2.876 | -1.186 | -0.176 | -0.079 | 0.018 |
| | (7.690) | (3.415) | (1.680) | (0.529) | (0.053) | (0.020) |
| Take-up (T+S) [Skill training with Stipend] | 11.832** | 2.406 | 0.854 | 0.270 | -0.109*** | -0.007 |
| | (5.191) | (2.544) | (1.283) | (0.431) | (0.039) | (0.017) |
| Take-up (T+S+I) [Skill training, stipend plus internship] | 16.298** | -2.117 | -1.154 | -0.042 | -0.200*** | -0.007 |
| | (7.020) | (3.164) | (1.569) | (0.492) | (0.056) | (0.022) |
| Observations | 2120 | 2120 | 2120 | 2120 | 2120 | 2120 |
| $R^2$ | 0.266 | 0.341 | 0.329 | 0.302 | 0.288 | 0.170 |
| Base-group mean | 66.436 | 66.615 | 27.452 | 8.182 | 0.537 | 0.964 |
| $p$-value for joint significance | 0.014 | 0.112 | 0.263 | 0.666 | 0.002 | 0.391 |
| Stipend effect (S): (T+S)-(T) (beta coef.) | 7.011 | 5.283 | 2.040 | 0.445 | -0.030 | -0.025 |
| Stipend effect (S): (T+S)-(T) ($p$-value) | 0.221 | 0.032 | 0.079 | 0.263 | 0.565 | 0.162 |
| Internship effect (I): (T+S+I)-(T+S) (beta coef.) | 4.466 | -4.523 | -2.008 | -0.311 | -0.092 | 0.000 |
| Internship (I): (T+S+I)-(T+S) ($p$-value) | 0.397 | 0.063 | 0.091 | 0.414 | 0.074 | 0.995 |
| Outcome at baseline | Yes | Yes | Yes | Yes | Yes | Yes |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T2–T4 as instruments. The pure control and information-only arms are combined as the omitted base group. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.

Table A34: Effects of Treatment Take-Up on Origin-Household Income, Consumption, and Poverty (IV; Information as a Separate Treatment Arm)

| | (1) Total Income | (2) Total Consumption | (3) Food Consumption | (4) Non-Food Consumption | (5) Income Poverty | (6) Consumption Poverty |
|---|---|---|---|---|---|---|
| Take-up (T1) [Information] | 13.095** | 6.276* | 2.708** | 0.672 | -0.158** | -0.008 |
| | (6.520) | (3.288) | (1.333) | (0.561) | (0.072) | (0.028) |
| Take-up (T2) [T1 + Skill training] | 19.806** | 4.306 | 1.915 | 0.594 | -0.260** | 0.009 |
| | (9.778) | (4.624) | (1.962) | (0.817) | (0.102) | (0.036) |
| Take-up (T3) [T2 + Stipend] | 23.052*** | 7.800** | 3.182** | 0.848 | -0.244*** | -0.014 |
| | (6.331) | (3.622) | (1.528) | (0.629) | (0.076) | (0.027) |
| Take-up (T4) [T3 + Internship] | 30.225*** | 4.414 | 1.651 | 0.663 | -0.364*** | -0.015 |
| | (9.527) | (4.452) | (1.877) | (0.814) | (0.100) | (0.035) |
| Observations | 2119 | 2119 | 2119 | 2119 | 2120 | 2120 |
| $R^2$ | 0.249 | 0.337 | 0.324 | 0.301 | 0.241 | 0.169 |
| Pure-control mean | 63.564 | 63.342 | 25.923 | 7.777 | 0.551 | 0.979 |
| $p$-value for joint significance | 0.002 | 0.081 | 0.116 | 0.520 | 0.002 | 0.542 |
| Training effect: T2 - T1 (beta coef.) | 6.711 | -1.970 | -0.793 | -0.079 | -0.102 | 0.017 |
| Training effect: T2 - T1 ($p$-value) | 0.383 | 0.554 | 0.625 | 0.882 | 0.070 | 0.379 |
| Stipend effect: T3 - T2 (beta coef.) | 3.246 | 3.494 | 1.267 | 0.254 | 0.016 | -0.023 |
| Stipend effect: T3 - T2 ($p$-value) | 0.611 | 0.178 | 0.294 | 0.560 | 0.791 | 0.256 |
| Internship effect: T4 - T3 (beta coef.) | 7.174 | -3.386 | -1.531 | -0.185 | -0.120 | -0.001 |
| Internship effect: T4 - T3 ($p$-value) | 0.227 | 0.185 | 0.213 | 0.662 | 0.034 | 0.963 |
| Outcome at baseline | Yes | Yes | Yes | Yes | Yes | Yes |
| Baseline covariates, phase FE, and village FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* Take-up means beginning or participating in the assigned treatment and does not require completion. The table reports IV estimates of the effects of treatment take-up, using randomized assignment to T1–T4 as instruments. The information-only arm is estimated separately and the pure control group is the omitted category. Specifications include baseline covariates, phase fixed effects, and village fixed effects. Village-clustered standard errors are in parentheses, $p$-values are in square brackets, and sharpened $q$-values are in braces. $*$, $**$, and $***$ denote statistical significance at 10, 5, and 1 percent levels, respectively.