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FINANCIAL LITERACY AND HOUSEHOLD FINANCIAL SECURITY IN RURAL HARYANA: A STUDY OF A VILLAGE WORKSHOP PROGRAMME

Lakshya Khurania

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ABSTRACT

This paper sets out how to estimate the effect of a free, Hindi language financial literacy workshop programme on the financial security of rural households in Haryana, India. India has largely solved the problem of access: the great majority of households now hold a bank account. It has not solved the problem of use. Roughly a quarter of the accounts opened under the Pradhan Mantri Jan Dhan Yojana (PMJDY) have fallen inoperative, informal loans are still struck without a written rate, and land records that govern access to credit remain unread by the people they name. The programme studied here, founded after more than eight workshops delivered in villages of Kaithal district, addresses that gap with six practical modules taught in the village itself. Because villages and participants select themselves into the workshops, attendance is endogenous; the paper therefore proposes a propensity score matching design, strengthened by a waitlist comparison and a follow up visit four to six weeks after each session. Drawing on NABARD's 2021/22 survey of one lakh rural households and a meta-analysis of 76 randomised trials, it calibrates a simulated evaluation of 2,400 households in 40 villages, in which the true effect is known. A naive comparison overstates the knowledge effect more than twofold; matching removes part of that bias, and a difference in differences on baseline scores recovers the true effect of about 0.2 standard deviations. Effects on behaviour and on worry over essential expenses are too small to detect at this scale, as the power calculations predict. The results are illustrative and are not observations of the programme's participants.

Keywords: financial literacy, financial inclusion, rural India, Haryana, Jan Dhan Yojana, propensity score

matching, simulation

1. INTRODUCTION

India has opened more bank accounts in a decade than most countries hold in total. Launched in August 2014, the PMJDY gave a zero-balance account to every unbanked adult, and by November 2025 the scheme counted 56.98 crore accounts. The Reserve Bank of India's composite Financial Inclusion Index rose to 67.0 in March 2025, from 64.2 a year earlier, and the Bank attributed part of that gain to sustained financial literacy initiatives.

Access, however, has not translated into use. The government told Parliament that 15.07 crore PMJDY accounts, about 26 percent, had seen no customer-initiated transaction for over two years as of 12 November 2025. Many beneficiaries withdraw a direct benefit transfer the day it arrives and leave the account dormant until the next one. An account used only to receive transfers does little to build savings. The rural household in Haryana illustrates this gap well. It is banked, often has a smartphone, and manages tight cash flows with real precision. What it frequently lacks is the paperwork that turns that competence into security: a written interest rate, a clear record of who owns the plot, the name of the scheme it is entitled to, the knowledge that a bank will never ask for an OTP.

This paper studies a programme built to close that gap.
Founded by Lakshya Khurana after an internship with the Shinning India Foundation for Global Welfare in Kaithal, it runs free workshops, taught in Hindi, in villages, self-help groups and schools. Its curriculum covers household budgeting, savings and banking, safe borrowing, government schemes, digital payments and fraud, and land records.

We ask two questions. First, does attending the workshops measurably improve a household's financial knowledge, behaviour and security? Second, which barriers to the active use of formal finance, whether literacy, documentation, trust or distance, does a village workshop actually remove? We propose a propensity score matching strategy and, since the programme's own records are not yet available, test it on simulated data built to match published Indian and international evidence. A simulation has one advantage over field data: the true effect is known, so one can see which estimator recovers it.

2. THE CASE STUDY

Haryana is a prosperous state with a persistently informal rural economy, which makes it a revealing case. Its agriculture is commercial and its villages lie close to Delhi, Chandigarh and the Grand Trunk Road; yet credit outside the bank branch, cash savings and unwritten loan terms remain ordinary features of village life. The programme names three problems it works against: accounts that are held but little used, informal borrowing at rates rarely put in writing, and scheme rules and land records that exist on paper but do not reach the village in plain language.

The dormancy problem is not new in the state. As early as June 2018, 6.50 lakh of the 65.82 lakh PMJDY accounts opened in Haryana held a zero balance, and banks were warning holders that inoperative accounts faced closure. In 2025 the Haryana Chief Secretary asked the State Level Bankers Committee to run a special campaign to reactivate inactive Jan Dhan accounts and to raise awareness of digital payments.

The programme itself is simple in form. A panchayat member, a self-help group, an anganwadi worker or a school teacher requests a session by telephone or WhatsApp. The village fixes the date, the place (a chaupal, a classroom, the group's usual meeting spot) and an hour that fits around field and household work. A session lasts ninety minutes to two hours, is taught in Hindi with questions throughout, and ends with a printed sheet. Participants fill in their own budget and work out the cost of one real loan offer. A call or second visit follows after four to six weeks.

Sessions began in Kaithal, where the founder first taught the material as a social development intern with the Shinning India Foundation for Global Welfare, an NGO working in the district; there he delivered more than eight workshops to over a hundred participants. The programme lists Kurukshetra, Jind, Karnal, Hisar, Fatehabad, Sirsa and Bhiwani as further districts, and counts no district as served until a session has taken place there. Each of the six modules ends with a question the participant should be able to answer (Table 1). These questions also give the study a readymade instrument for measuring knowledge gains.

Table 1: The six workshop modules

Module	Time	Question the participant should answer
1. Household budgeting	90 min	What was my largest expense last month, and what share of my income was it?
2. Savings and banking	90 min	If I set aside Rs 500 a month, where will it sit and how much will there be after a year?
3. Credit and borrowing safely	2 h	Across two loan offers, how much is repaid in total, and which costs less?
4. Government schemes	90 min	Which scheme is my household eligible for, and which office is the next step?

5. Digital payments and fraud	60 min	What do I do when someone asks for an OTP, and where do I report money already lost?
6. Documents and land records	90 min	Whose name is on the record for my house or land (jamabandi, nakal, SVAMITVA card), and how is an error corrected?

The founder's own research explains the curriculum's emphasis on documents. His work on alternative credit scoring, using utility payments and household cash flow where formal income proof is absent, and on land title risk in rural mortgages, looks at the same household from the lender's side of the table. The workshops teach the household what the lender will ask for before it walks into the branch.

3. LITERATURE REVIEW

The evidence on financial education shows a consistent pattern: it reliably raises knowledge, but changes behaviour less often and less durably. Lusardi and Mitchell (2014) establish the economic case, showing that low financial literacy is widespread and is associated with poorer saving, borrowing and planning decisions. Whether teaching closes that gap is the harder question.

Two large meta-analyses frame the expectation. Fernandes, Lynch and Netemeyer (2014) find that interventions explain only a small share of the variance in downstream financial behaviour, and that their effects decay over time. Kaiser, Lusardi, Menkhoff and Urban (2022), drawing on a larger set of randomised experiments, find positive effects on both knowledge and behaviour, of a size comparable to other educational interventions. Read together, they suggest that practical teaching works, but that its effect must be measured soon and checked again later.

The Indian evidence points the same way. Cole, Sampson and Zia (2011), studying India and Indonesia, find that a financial literacy course had no effect on account opening for the average household, while small subsidies did; education helped only those with the least prior knowledge. Carpena, Cole, Shapiro and Zia (2019), in an experiment with low-income households in urban India, find that financial education changed attitudes and awareness but had limited effect on numeracy or behaviour unless paired with goal setting and individual counselling. The programme's follow up visit, and its insistence that participants work on their own budget and loan offer, are close to the complements that made the difference in that study.

A second literature concerns use rather than access. Dupas and Robinson (2013) show in Kenya that giving people a place to save, even an unattractive one, raised savings and business investment for some groups. Field, Pande, Rigol, Schaner and Troyer Moore (2021) show in Madhya Pradesh that depositing women's

MGNREGA wages into their own accounts, combined with training on how to use them, raised women's labour supply and shifted gender norms. De Mel, McIntosh, Sheth and Woodruff (2018) find in Sri Lanka that mobile linked accounts were adopted slowly even when fees were waived.

Finally, the mobile money literature shows what becomes possible once formal tools are actually used. Suri and Jack (2016) attribute a 22 percent fall in extreme poverty among female headed households in Kenya to the spread of mobile money, and Riley (2018) finds better consumption smoothing after village shocks among mobile money users. India's Unified Payments Interface already gives rural Haryana comparable infrastructure. The remaining constraint is the household's confidence to use it safely, which Module 5 addresses.

4. HYPOTHESES

Following the literature, we expect a clear effect on knowledge and a smaller, more uneven effect on behaviour and security.

H1, knowledge. Participants answer more of the six module questions correctly at follow up than comparable non participants. We expect this effect to be large and statistically significant, consistent with Kaiser et al. (2022).

H2, behaviour. Participants are more likely to have made a customer-initiated transaction in a dormant account, started a recurring deposit, or obtained a written statement of the interest rate on their most recent loan. We expect a positive but smaller effect, concentrated among those who received the follow up visit, as Carpena et al. (2019) would predict.

H3, security. Participants report less worry about meeting essential expenses, including food, in the past seven days. We expect any effect here to be small and imprecisely estimated over a four-to-six-week horizon.

H4, heterogeneity. Effects are larger for women, for households with the least prior financial knowledge (Cole et al., 2011), and for households further from a bank branch.

Attendance is not random. Villages request sessions; within a village, the more motivated, better connected or more literate members are likelier to come. This self-selection would bias a naive comparison upward, and the methodology is built around it.

5. DATA

The study combines the programme's own records with national survey data for context, and tests its estimators on simulated data.

Primary data. At each session, a short registration sheet in Hindi records, for every participant, the household characteristics used for matching and a baseline score on the six module questions. The same questions, plus the behaviour and security outcomes, are asked again at the follow up call or visit four to six weeks later. Non participants are surveyed with the same instrument in two groups: residents of the

same villages who did not attend, and residents of villages that have requested a workshop but have not yet received one (the waitlist).

Secondary data. The NABARD All India Rural Financial Inclusion Survey 2021/22 (NAFIS), covering one lakh rural households, provides the benchmark. It reports that the share of rural respondents with good financial literacy rose from 33.9 percent in 2016/17 to 51.3 percent in 2021/22, and that 66 percent of rural households reported saving in 2021/22, up from 50.6 percent.

Simulated data. Because the programme's records are not yet compiled, the estimators in Section 6 are applied to a simulated sample of 2,400 households in 40 villages spread over the eight districts the programme lists, 60 households per village. Covariates are drawn to resemble rural Haryana: about 80 percent of households have a literate member, 72 percent own a smartphone, 31 percent belong to a self-help group, and the median distance to a bank branch or business correspondent is about four kilometres. Attendance depends on these observed characteristics and on an unobserved trait, motivation, which also raises outcomes; this is the self-selection described in Section 4. The true effects on knowledge (0.20 standard deviations) and behaviour (4 percentage points) follow Kaiser et al. (2022); the effect on worry over essential expenses is deliberately set small, at 1.5 percentage points. Summary statistics are reported in Table A2.

Limitations. A field sample will be small and clustered in a few districts, so standard errors must be clustered at the village level. Self-reported behaviour is vulnerable to social desirability bias after a workshop; where possible, passbook entries and loan papers should be checked with consent. A four to six week follow up captures knowledge and early behaviour, not long run security.

Consent and privacy. Participation in the survey is voluntary and separate from the workshop. No bank details or OTPs are ever requested, and records are kept under household codes rather than names.

6. METHODOLOGY

We estimate the average treatment effect on the treated (ATET) of attending a workshop, using propensity score matching, with a difference in differences on knowledge and a waitlist comparison as checks.

6.1 Variables

Outcomes. Three outcomes correspond to H1 to H3: a knowledge score (number of the six module questions answered correctly, 0 to 6); a binary behaviour indicator (any customer-initiated transaction in a previously dormant account, a new recurring deposit, or a written interest rate on the latest loan, since the session); and a binary indicator of whether the household worried about meeting essential expenses, including food, in the past seven days.

Treatment. A binary variable equal to 1 if the respondent attended at least one module. A secondary specification counts the number of modules attended (1 to 6) to test for a dose response.

Controls. Characteristics that predict attendance and outcomes but are not changed by the workshop:

literacy, highest education in the household, sex, distance to the nearest bank branch or business correspondent, self-help group membership, smartphone ownership, landholding and district. A field evaluation would add age, main occupation and social category.

6.2 Econometric Model

Let Y^1 be the outcome if household i attends and Y^0 the i i outcomes if it does not. The effect of interest is:

$$\text{ATET} = E[Y^1 - Y^0 \mid D = 1] \text{ i i i}$$
$$= E[Y^1 \mid D = 1] - E[Y^0 \mid D = 1] \text{ i i i i}$$

The second term is never observed. Matching estimates it from non-participants who resembled the participants before the workshop. Each household's probability of attending is estimated with a probit:

$$\Pr(D = 1) = \Phi(\, c + \beta_1 \text{ literacy}_i + \beta_2 \text{ education}_i + \beta_3 \text{ female}_i + \beta_4 \text{ distance}_i + \beta_5 \text{ SHG}_i + \beta_6 \text{ smartphone}_i + \beta_7 \text{ land}_i + \gamma_d \,)$$

where γ_d is a district fixed effect. Treated households are matched to the nearest control on this score, with replacement. Standard errors are estimated by bootstrap with 400 replications, resampling whole villages, which accounts both for the matching step and for clustering. Balance is checked by the standardised bias of each covariate, and common support by comparing score densities before and after matching; both are reported in the appendix.

Because baseline knowledge is recorded at registration, the knowledge outcome can also be estimated as a difference in differences on the matched sample, which removes fixed, unobserved differences between the groups. The waitlist comparison is the strongest check in the field: villages that have asked for a workshop but not yet received one share the motivation that drives selection. The simulation does not model the waitlist, which remains a check for the field evaluation.

7. RESULTS AND DISCUSSION

7.1 Expected Effects

Two sources anchor the expected effects: NABARD's NAFIS 2021/22, which fixes where the average rural household starts (Table 2), and the meta-analysis by Kaiser et al. (2022) of 76 randomised trials covering over 160,000 people, which fixes how large an effect is plausible (Table 3).

Table 2: Where the average rural household starts (2016/17 value in brackets)

Indicator	Value	Source
Respondents with good financial literacy	51.3% (33.9%)	NAFIS 2021/22
Households reporting any saving	66.0% (50.6%)	NAFIS 2021/22
Households with outstanding debt	52.0% (47.4%)	NAFIS 2021/22
Average annual household financial savings	Rs 13,209 (Rs 9,104)	NAFIS 2021/22
PMJDY accounts inoperative, November 2025	26% (15.07 of 56.98 crore)	Reply to Parliament

Nearly half of rural respondents still fall short of good financial literacy, more than half of households carry debt, and one Jan Dhan account in four lies idle. These are the margins on which the workshops operate.

Table 3: Expected effects and the sample needed to detect them

Outcome	Effect (SD)	Participants needed per group
Knowledge, average effect	0.20	≈ 393
Knowledge, corrected for publication bias	0.15	≈ 698
Behaviour, average effect	0.10	≈ 1,570
Behaviour, corrected for publication bias	0.06	≈ 4,361

Effects from Kaiser et al. (2022). Sample sizes assume a two-sided test at the 5% level with 80% power and individual comparison; with clustering in villages, each figure is multiplied by the design effect, 1 + (m − 1)ρ, where m is participants per village and ρ the correlation of outcomes within a village.

Three expectations follow. First, the knowledge effect is within reach. At the scale of the founder's first internship (more than eight workshops, over a hundred participants), the programme needs roughly four to seven times that reach, with as many matched non participants, to measure its effect on knowledge with confidence.

Second, behaviour is harder to measure. An effect of 0.06 to 0.10 standard deviations requires between 1,570 and 4,361 participants per group before clustering. A single season of workshops will not measure it reliably; pooling several seasons, or concentrating on one sharply defined behaviour such as reactivating a dormant account, is the realistic path.

Third, the expected pattern is uneven. A clear gain in knowledge with a small, imprecise change in behaviour would not indicate failure: it is what the evidence from 33 countries leads one to expect, and it would point to the follow up visit, rather than the session, as the place to invest. Conversely, a large fall in worry over essential expenses within six weeks would be suspicious, more likely the result of reporting bias or a seasonal income shock than of the workshop.

7.2 Simulated Evaluation

Of the 2,400 simulated households, 1,058 attended at least one module. Matching uses 600 distinct non participants out of 1,342. Before matching, participants were markedly more likely to be literate, female and members of a self-help group, and lived closer to a bank; after matching, every covariate in Figure A1 is within 5 percent standardised bias, and the two score distributions overlap closely (Figure A2).

Table 4: Effect of attendance on the treated, simulated sample (n = 2,400)

Outcome	True effect	Naive diff.	Matched (SE)	95% CI
Knowledge score (points)	0.23	0.52	0.35 (0.05)	[0.24, 0.45]
Knowledge, difference in differences	0.23	0.24	0.26 (0.03)	[0.19, 0.32]
Behaviour change (share)	0.040	0.045	0.015 (0.023)	[−0.030, 0.060]
Worry about expenses (share)	−0.015	−0.026	−0.032 (0.022)	[−0.076, 0.012]

Nearest neighbour matching on the propensity score, one match with replacement; village cluster bootstrap standard errors (400 replications). Data are simulated; see Section 5.

The simulated results are consistent with the preceding sections. The naive comparison puts the knowledge gain at 0.52 points, more than twice the true 0.23, because motivated households both attend and learn more.

Matching on observed characteristics brings the estimate down to 0.35, but it cannot see motivation, and its confidence interval only just excludes the true value. The difference in differences, which nets out each household's baseline score, gives 0.26, with an interval of 0.19 to 0.32: close to the true value, which makes it the preferred estimator for a field evaluation.

The behavioural results are weaker. The true effect of 4 percentage points is real but small, and with about a thousand participants the matched estimate of 0.015 has an interval running from −0.030 to 0.060: the data cannot distinguish a modest success from no effect at all. The same holds for worry over essential expenses. This is what Table 3 predicted, and it carries the main practical implication of the exercise: a programme of this size can show that it teaches, but it will need either many more participants or a randomised order of visits to show that what it teaches changes what households do.

8. EVALUATION

Matching removes bias only from what is observed. Motivation, trust in banks and the social standing of the person who invited the facilitator are not on the registration sheet, and they plausibly drive both attendance and outcomes. The waitlist comparison narrows this problem but does not close it, since villages that ask first may differ from those that ask later.

The cleanest improvement would be a randomised phase in: when more villages request workshops than can be served in a season, the order of visits is drawn by lot. Every village is eventually served, so no one is denied the programme, and the villages not yet served form a valid control group. This is the design used in much of the work cited in Section 3, and it would allow a simple comparison of means.

Two further extensions are worth pursuing. A second follow up at six months would test whether gains decay, as Fernandes et al. (2014) found elsewhere. And comparing account holders who attend with account holders who do not would separate the effect of knowledge from the effect of merely being banked.

The case is also local. Haryana's mobile coverage, bank network and largely Hindi speaking villages make the programme portable to neighbouring states, but not automatically to regions with different languages, land record systems or credit markets.

9. CONCLUSION

India's financial inclusion problem now lies less in access than in use. The accounts exist; what is missing is the confidence and the paperwork to use them, to borrow on written terms, and to claim what the state already offers. A village workshop in Hindi, built around a household's own budget and its own loan offer, is a plausible and inexpensive response.

This paper has proposed how to test that claim: a propensity score matching design anchored by baseline knowledge scores, a waitlist comparison, and ideally a randomised order of visits. The evidence from India and elsewhere suggests the programme will raise knowledge clearly and change behaviour more modestly, and that its follow up visit may matter as much as the session itself. A simulated evaluation calibrated to that evidence shows why the design matters: a naive comparison would double the programme's apparent effect, while a difference in differences on baseline scores recovers it. Whether rural Haryana confirms that expectation will be known only when the programme's own attendance sheets are counted.

APPENDIX

Figure A1: Standardised % bias before and after matching (simulated data)

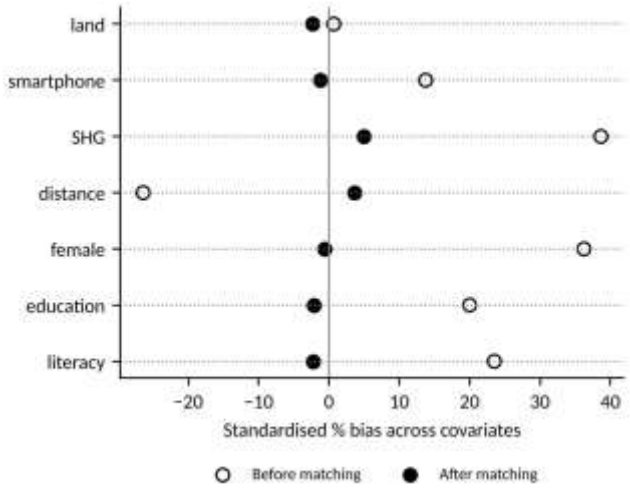


Figure A2: Kernel density of propensity scores, before and after matching (simulated data)

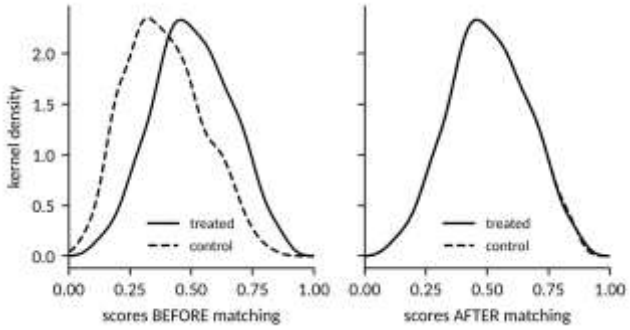


Table A1: Variables recorded at registration and follow up

Variable	Coding	Recorded at
Workshop attendance	1 attended at least one module, 0 otherwise	Registration
Modules attended	1 to 6	Each session
Knowledge score	Module questions answered correctly, 0 to 6	Baseline, follow up
Behaviour change	Dormant account used, recurring deposit opened, or written loan rate obtained	Follow up
Worry about expenses	Worried about meeting essential expenses, including food, in past 7 days	Baseline, follow up
Female respondent	1 female	Registration
Literacy	1 any household member literate	Registration
Household education	0 none to 5 graduate or above	Registration

Distance to bank or BC	Kilometres	Registration
SHG membership	1 member of a self help group	Registration
Smartphone	1 household owns a smartphone	Registration
Landholding	0 landless to 4 large	Registration
District	Eight districts of Haryana	Registration

Table A2: Summary statistics of the simulated sample (n = 2,400)

Variable	Mean	SD	Min	Max
Workshop attendance	0.441	0.496	0	1
Female respondent	0.493	0.500	0	1
Literacy	0.808	0.394	0	1
Education (0 to 5)	2.110	1.221	0	5
Distance (km)	4.816	3.083	0.7	35.2
SHG membership	0.306	0.461	0	1
Smartphone	0.726	0.446	0	1
Landholding (0 to 4)	1.992	1.405	0	4
Knowledge, baseline	3.320	1.128	0	6
Knowledge, follow up	3.517	1.206	0	6
Behaviour change	0.230	0.421	0	1
Worry about expenses	0.267	0.442	0	1

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