Can Restoration of the Commons Reduce Rural Vulnerability? A Quasi-Experimental Comparison of COVID-19 Livelihood-based Coping Strategies among Rural Households in Three Indian States

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ABSTRACT

India has been hard hit by the COVID-19 pandemic. In the context of a larger quasi-experimental impact assessment, we assess the pandemic's effects on household coping behavior in 80 villages spread across four districts and three states (n = 772). Half of these villages were targeted by a largescale common land restoration program spearheaded by an NGO, the Foundation for Ecological Security (FES). The other half are yet to be targeted but are statistically similar vis-à-vis FES's village targeting criteria. Analyzing the results of a phone survey administered eight to ten months into the pandemic and its associated lockdowns, we find that the livelihood activities of households in both sets of villages were adversely impacted by COVID-19. Consequently, most households had to resort to various negative coping behaviors, e.g., distressed asset sales and reduced farm input expenditure. From the same mobile survey data, we construct a Livelihoods Coping Strategies Index (LCSI) and find that households in villages targeted by FES's common land restoration initiative score 11.3% lower on this index on average, equating to a 4.5 percentage point difference. While modest, this statistically significant effect estimate ($p < 0.05$) is consistent across the four districts and robust to alterative model and outcome specifications. We find no empirical support that our observed effect was due to improved access to common pool resources or government social programs. Instead, we speculate that this effect may be driven by institutional factors, rather than economic, a proposition we will test in future work.

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1. INTRODUCTION AND BACKGROUND

India has been significantly and adversely affected by the COVID-19 pandemic. At the time of writing (February, 2022), India had recorded over 42.5 million cases and more than 500,000 COVID-related deaths (Worldometers, 2022). While the health impacts are severe, the economic and livelihood impacts, particularly among India's 800 million poor, have also been devastating. To slow down the spread of the virus, the Government of India imposed some of the toughest lockdown measures in the world (Mishra & Rampal, 2020). These were introduced from March 22, 2020 over six phases. The first four phases lasted up to June 8, 2020. During this time, India's 1.3 billion citizens were advised to stay indoors. Air and road transport and industrial activity were seriously curtailed, with only health and other essential services remaining in operation. Agricultural operations were, however, permitted from April 15 (Phase II) (Pathakoti et al., 2021). The last two phases—June 1 to July 31, 2020—involved an incremental easing of these restrictions, but with their continued imposition in designated 'containment areas' (Saha & Chouhan, 2021).

While arguably important for containing the virus, these lockdown measures severely disrupted supply chains and led to widespread losses in employment and income and, consequently, rising food insecurity (Mishra & Rampal, 2020; Rawal et al., 2020). With limited formal sources of social security, India's over 400 million informal workers were disproportionally affected, many with no choice but to forgo the waning economic opportunities that had originally drawn them to urban centers and return to their home villages (R. Suresh et al., 2020). Yet, there too, agricultural activities were gravely impacted. Due to mobility restrictions, farm inputs and labor were often in short supply, and farmers faced significant challenges marketing their produce (Dev, 2020; Kumar et al., 2021; Singh, 2020). Important social protection programs, such as the Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA), were also significantly disrupted during the lockdown (Rawal et al., 2020). The COVID-19 pandemic in India, including measures to address it, can therefore be treated as a major covariate shock (Barrett, 2011), affecting India in general and its rural population in particular. As is the case with climate-related and other exogenous shocks, it is of interest to explore the extent to which people's livelihoods were adversely affected in general and their coping behaviors in particular, including those pertaining to distressed asset sales and cuts in family consumption (Janzen & Carter, 2019).

One largescale effort to strengthen rural livelihoods, while simultaneously striving to achieve multiple environmental outcomes, is being spearheaded by the Foundation for Ecological Security (FES). Since the early 2000s, FES has been working with rural communities in the states of Rajasthan, Andhra Pradesh, Karnataka, Orissa, Madhya Pradesh, and Gujarat, as well as in India's North eastern region. A key focus involves the facilitation of collective action to secure community rights to common-pool resources—such as grazing land, water bodies, and forests—while promoting their effective management and restoration, thereby improving their ecological integrity. FES's interventions seek to build inclusive, democratic village institutions that promote collective action and regulate access to and use of commonly shared natural resources. FES also acts as a broker in the sense that it helps villages to access government programs, such as MGNREGA, to finance village activities associated with the restoration and management of common-pool resources that local people depend on (Meinzen-Dick et al., 2021). The purpose of this paper is to assess the effects of FES's community-level intervention model on household-level coping behavior in the wake of government policy to contain the COVID-19 pandemic.

This study is part of a bigger research project that is implementing a quasi-experimental impact evaluation of FES's work more broadly. Using secondary data representing measures of FES's village targeting criteria, we used propensity score matching (PSM) to identify 288 villages both exposed and not exposed to the organization's intervention model for at least five years (from 2000 to 2015) in six districts across four states (Figure 1). In each site, field teams digitally mapped common land areas, collected ecological data, and listed households in the matched treated and untreated villages. We used the household lists to randomly sample households in both treated and untreated villages and implemented a telephone survey to assess how households were affected by the pandemic.

In the next section, we describe FES's intervention model in greater detail, including two mechanisms of how it could potentially mitigate negative livelihood-based coping behavior in the face of large covariate shocks, such as those associated with COVID-19. In Section 3—Methods—we present our causal identification strategy underpinning our study and our data collection and analysis procedures. We present our results in Section 4. This includes a presentation of how well treated and untreated households are balanced vis-à-vis their non-intervention related characteristics; the extent to which the livelihoods of these households were affected by COVID-19; the coping strategies they employed in response; and the extent to which our hypothesized mechanisms of how FES's work may have mitigated negative household coping behavior are supported by the data. In Section 6—Discussion and Conclusion—we

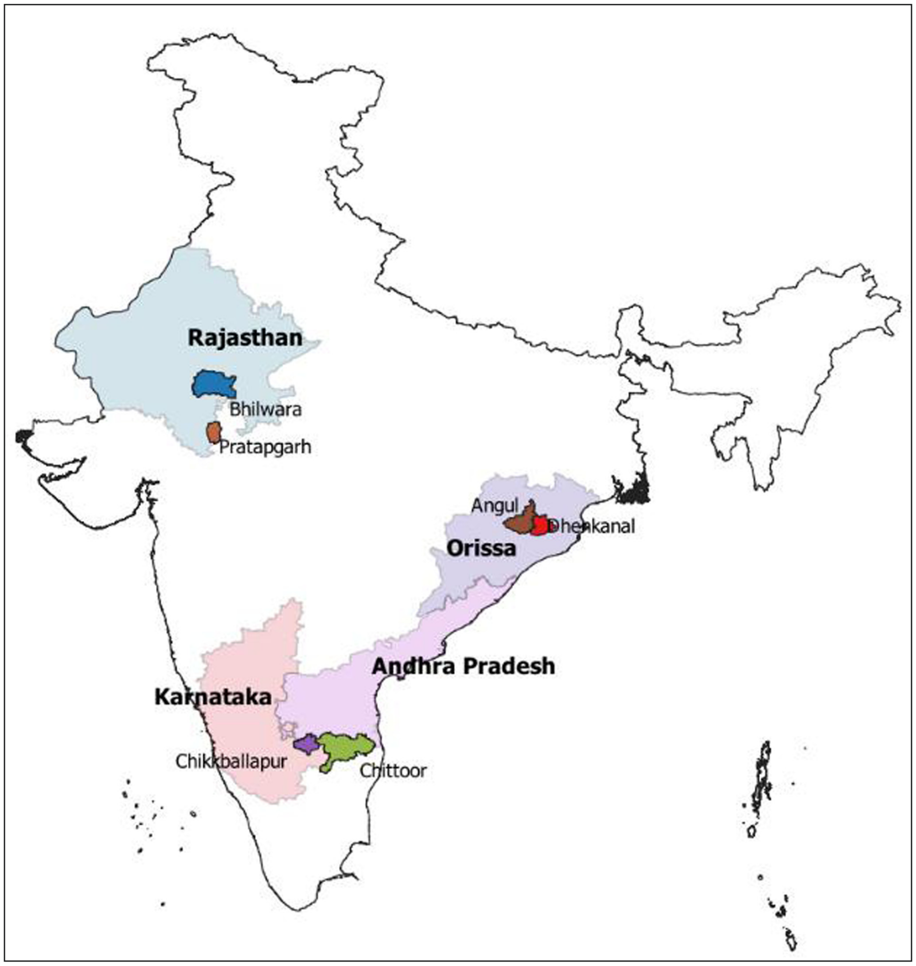

Figure 1 Map of main impact study sites. Silhouette represents boundaries of India for representational purposes only, with translucent and shaded areas, representing location of study states (in bold text) and districts, respectively.

summarize our key results and highlight the limitations our study and our next steps to address these, followed by concluding remarks.

2. FES'S INTERVENTION MODEL & HYPOTHESIZED EFFECTS

2.1 FES'S INTERVENTION MODEL

Approximately one-quarter of India's land area (205 million acres) constitutes common-pool resources (Chopra & Gulati, 2001) or, more simply, the commons. Examples of commons include forests, pasturelands, groundwater, fisheries, lakes, and rivers among others (Jodha, 1986). Being such a dominant feature in rural landscapes, many households rely on the resources that the commons generate (i.e., their 'provisioning services'), including fuelwood, fodder, timber, medicinal herbs, oils, and resins (Agarwal, 1997). Indeed, such resources have been estimated to contribute USD $5 billion per year to the incomes of poor rural households in India, equivalent to about 12% of their income (Beck & Nesmith, 2001). Yet, since the middle of the 20th century, the commons have been in a state of decline, both in terms of area and quality. The reasons are multiple, including population pressure, mechanization, land reform programs accelerating private land ownership, and the undermining of local common property management regimes in favor of more centralized state management approaches (Narain & Vij, 2016; Thapliyal et al., 2019).

Drawing heavily on the work of Elinor Ostrom (1990) and the CGIAR Systemwide Program on Collective Action and Property Rights (CAPRi, 2010), a central tenant of FES's work is that the 'tragedy of the commons' (Hardin, 1968) is not inevitable. Indeed, there are many examples where local people have independently devised local governance institutions to manage common pool resources effectively (Ostrom, 2008).

FES's intervention model (Figure 2) comprises three complementary core components, leading to two primary impacts—improved ecological health and more resilient livelihoods. The first component involves supporting communities to secure their rights to the commons.

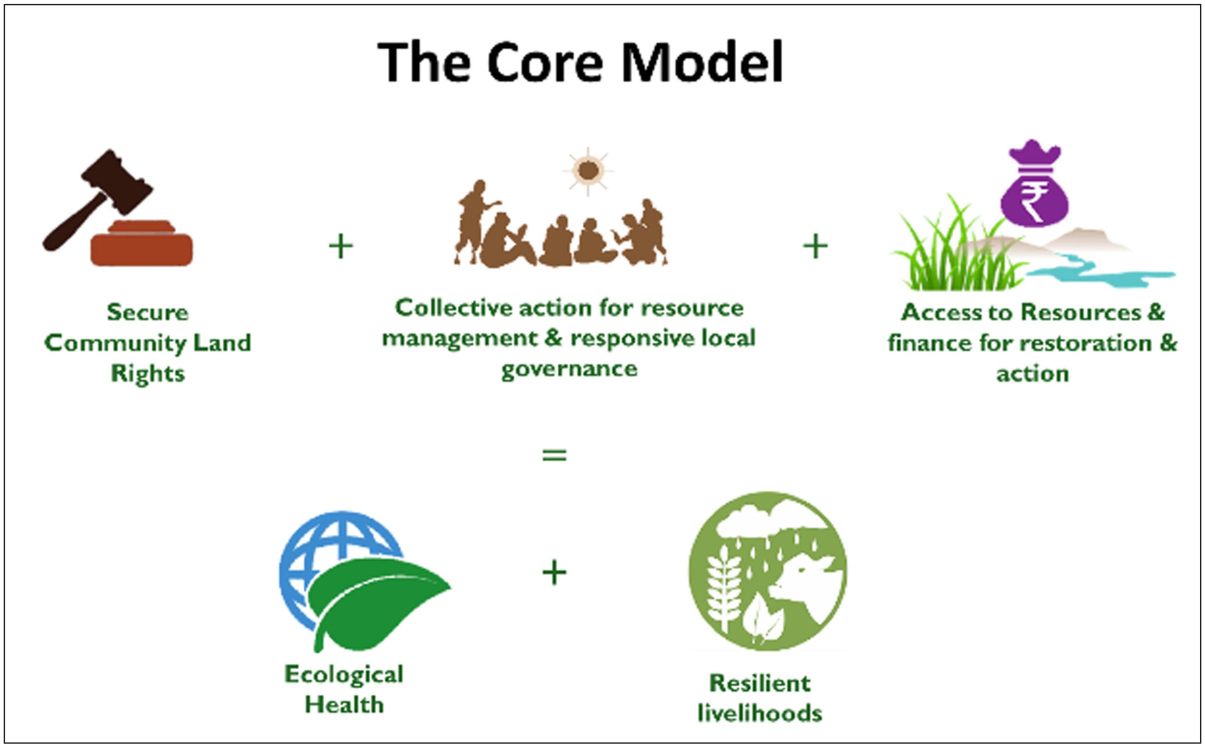

Figure 2 FES's Core Intervention Model (FES, 2021).

This work ranges from mapping common resources and facilitating conflict resolution to strengthening local capacity for rights claiming and supporting village institutions to file and negotiate ordinances. The second component focuses on facilitating collective action and strengthening village institutions for both the restoration and long-term management of the commons. Key activities under this component include carrying out awareness campaigns, assessing the status of the commons and devising restoration plans, supporting the development or strengthening of by-laws, leadership capacity building, and networking village institutions at the block (sub-district) and district levels. Recognizing that much of the commons is degraded and requires (financial) investment for restoration brings us to the third component of FES's intervention model, which focuses on linking village institutions to government social programs, such as MGNREGA. (See Supplementary Figure A.1 for a more elaborate Theory of Change for FES's core intervention model.)

To date FES's core model has been implemented in approximately 20,000 villages (habitations) in the above geographical areas, comprising over 5.5 million acres of land and 6.25 million people. Approximately 7,000 villages were intervened directly by FES, while partner organizations have implemented FES's core model in the 13,000 remaining villages.

2.2 HYPOTHESIZED EFFECT OF FES INTERVENTION

Our central hypothesis is that household's residing in villages exposed to FES's intervention model were less likely to engage in negative livelihood-based coping behaviors following the Government of India's policy measures to contain the spread of COVID-19. We offer two economic reasons for this expectation.

First, we expect that household's in FES treated villages had better and more equitable access to goods and services available from the commons. These products could be for direct consumption, selling, or trading, thereby enabling them to cope better during this period of unprecedented stress. Indeed, there is an extensive literature on the role of the commons in supporting the rural poor to cope during times of stress (see, for example, Beck & Nesmith, 2001).

A second economic reason pertains to FES's work to link villages to government organizations and programs. Hence, it is possible that the village institutions supported by FES were already better linked to government social security schemes in the pre-COVID-19 era or were better able to draw in the support offered by these schemes. Given the sheer magnitude of the COVID-19 shock, many advocate that scaling up India's social security and protection programs is critical. However, some reports suggest that several major programs were not able to continue their normal operations during the pandemic, much less increasing their coverage and magnitude of assistance (Mishra & Rampal, 2020; Summerton, 2020).

3. METHODS

3.1 CAUSAL IDENTIFICATION STRATEGY

A key challenge faced by the overall impact evaluation of FES's intervention model—and, by extension, this more narrowly focused COVID-19 study—is that FES, understandably as a development organization, did not randomly target the villages it chose to work in. Hence, finding differences in selected outcomes among units in villages where it worked and where it did not may be reflective of pre-existing baseline differences or differences in how these outcomes evolved over time, independent of FES intervention.

As is the case with any well executed quasi-experimental impact evaluation, how units came to be treated must first be understood in order to devise an appropriate strategy for addressing potential self-selection bias or program placement bias (Steinman et al., 2008; White, 2010). In this context, FES purportedly targeted villages based on its 'official' targeting criteria, e.g., presence of common land in need of restoration and significant representation of India's Scheduled Caste and Scheduled Tribe communities (see Supplementary Table A.1). Once these villages were selected, common-pool resources within them were mapped, and the communities prioritized those to be first targeted for restoration. In this context, participant self-selection did not take place as would be the case, for example, in a job training program. Consequently, the key type of bias we need to mitigate in this context is program placement bias (Ravallion & Wodon, 1999).

Given the above targeting criteria, our causal identification strategy involves comparing units within villages targeted by FES and those residing in other "non-FES" villages located in the same districts that are statistically similar vis-à-vis these criteria. The validity of our results, therefore, rests significantly on the Conditional Independence Assumption (CIA) (Morgan & Winship, 2015); that is, FES did not systematically target villages based on some other consideration(s), i.e., 'unobservables', particularly those correlated with our outcomes of interest.

While non-experimental approaches have been criticized in their inability to address bias (see, for example, LaLonde, 1986), work using four-arm experiments that first randomize participants into experimental and non-experimental groups, while then randomizing the treatment in the former and allowing participants to self-select their preferred treatment in the latter reveal important insights (Glazerman et al., 2002; Michalopoulos et al., 2004). The main one is that experimental results can be replicated non-experimentally when the selection process is both understood, measured, and appropriately modelled. Diaz and Handa (2006), for example, constructed propensity scores using the same variables used to determine eligibility for participation in Mexico's large-scale anti-poverty program, PROGRESSA. This enabled them to accurately model program participation and, hence, control for selection bias, thereby enabling the replication of the experimental effect estimates of the program.

Given the absence of experimental data for benchmarking, the extent to which our approach eliminates the possible presence of unobservable program placement bias is, unfortunately, untestable. That said, the following three preconditions enables us to at least significantly mitigate such bias: (A) explicit program placement targeting criteria; (B) substantive application of these criteria; and (C) existence of non-targeted villages that are similar to treated FES villages with respect to the same targeting criteria and that were also subjected to the same set of COVID-19 lockdown restrictions.

Precondition A is satisfied as explained above. Drawing largely from India's census and geographic attribute data, we obtained various direct and proxy measures of FES's targeting criteria. To assess the extent precondition B is met, we compare villages from three states targeted by FES from 2000 to 2015 and other villages located in the same targeted districts (Supplementary Table A.2). Because the data are population based (rather than derived through random sampling), we report standardized differences, as opposed to the results of statistical significance tests.[1] That said, we present the results of chi-squared tests of joint orthogonality to evaluate the extent to which the variables jointly predict whether a village was targeted by FES. We find this joint predictability to be highly significant across the three states. Thus, we find that FES followed its stated targeting criteria when selecting villages to work with.

With precondition B satisfied, we are left with precondition C, i.e., the existence of a significant number of candidate comparison villages that are similar in relation to FES's targeting criteria and subjected to the same set of COVID-19 lockdown restrictions. In the context of propensity score matching (PSM)—a procedure that matches units on the basis of their conditional probability of being treated given a set of observable characteristics (Rosenbaum & Rubin, 1983)—this is equivalent to there being a significant area of common support. In Stata (StataCorp, 2017), we computed propensity scores separately for each district using logit regression, with an FES treated village dummy as our response variable and the direct and proxy measures of FES's targeting criteria presented in Supplementary Table

A.1 as our predictors. We then examined the distributions of the propensity scores in the form of density plots (Figure 3). As expected, we find that the treated villages are more likely to have higher propensity scores. There is, nevertheless, a significant degree of overlap among the two sets of villages, particularly those of Chittoor and Bhilwara districts. We see that this is less so in the case of Chikballapur and Pratapgarh districts, but there are, fortunately, many potential comparison villages.

3.2 VILLAGE-LEVEL PROPENSITY SCORE MATCHING

We performed PSM at the district level for all the six districts presented in Figure 1. We are aware of other matching approaches, e.g., Coerced Exact Matching (CEM), and that PSM has been criticized due to its relative inefficiency in obtaining covariate balance (King & Nielsen, 2019). We experimented with CEM, simply to explore how it would perform in balancing our treated and comparison villages. However, it failed to generate the requisite number of matched treated and comparison villages required for our study. Consistent with recent literature on CEM (Ripollone et al., 2020), this is largely due to the nature of our data vis-à-vis FES's targeting criteria, which includes a mix of 16 binary and continuous variables. Indeed, CEM can result in the dropping of large numbers of treated observations, thereby resulting in the misidentification of average treatment effects (Black et al., 2020). As illustrated below, we found that PSM works reasonably well in balancing our treatment groups against FES's targeting criteria.

We conducted three rounds of one-to-one matching (caliper < 0.05) to obtain a ratio of three matched comparison villages for every treated village (288 villages in total, 72 treated and 216 comparison). Our aim was to have three times as many comparison villages to serve as baseline data for a randomized phase-in design going forward, as well as to implement a second round of one-to-one matching using additional primary and remote-sensing data obtained during the primary impact study's first full round of data collection.

For our telephone survey, we focused on the 72 matched treated villages and 72 of the best-matched comparison villages from the overall matched set of 216. However, when implementing this protocol, we encountered two challenges. First, during the common land mapping exercise associated with our primary impact study, we were forced to replace several comparison villages. This was either due to the absence of common land in these matched villages or a refusal of village leaders to participate in the study. Second, prior to mobile data collection, household listing had not been done in all 216 matched comparison villages, thereby reducing the pool from which to select our 72 matched set. We therefore repeated the PSM matching exercise for those villages with household lists and where

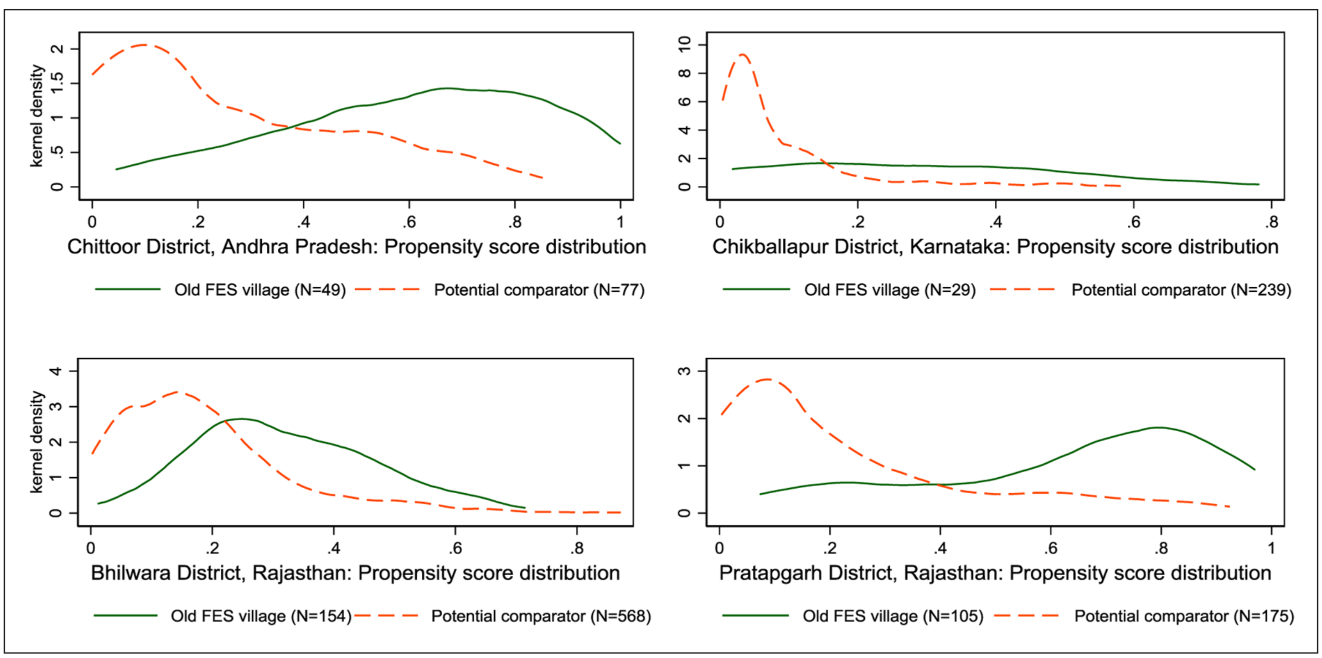

Figure 3 Propensity score density plots for treated & potential comparator villages.
Note: Secondary data was used for matching in all districts, save Chittoor. Primary data on FES's targeting criteria had to be compiled for this district, given that many villages can fall under an official Revenue Village in the state this district falls under (Andhra Pradesh). Graphs show kernel smoothed distributions for the propensity scores computed by district for both 'treated' and 'untreated' villages. The area of overlap indicates where villages can be found between the two groups with similar propensity scores.

village leader cooperation was assured, relying on the same data associated with FES's targeting criteria. Another complication was that during the telephone survey, we found respondents from the tribal communities of Odisha highly reluctant to be interviewed by telephone, ultimately forcing us to exclude the two districts from this state for this interim study.

We produced new density plots (Figure 4) for the re-computed propensity scores for the revised set of villages, both before and after matching. We found, again, that the matching exercise better aligns the distributions and is, consequently, reasonably successful in achieving statistical balance vis-à-vis FES's targeting criteria. However, we failed to reach our target of 12 treated and 12 comparison villages in three of the four districts. In Chittoor and Pratapgarh, we dropped several treated villages in order to balance the propensity score distributions. In Chikballapur, we were unable to administer the telephone survey in several villages, given that local elections were being held and local leaders were, consequently, unable to compile the telephone numbers of sampled households. In the end, we implemented our telephone survey in 80 matched treated and comparison villages, as described in Section 3.

3.3 DATA COLLECTION

From the lists of household names compiled during the common land mapping and ecological data collection exercise, we selected 10 households at random from each of the 80 matched villages, with the sex of the adult household respondent also selected at random. We further constructed reserve lists of households for each village in the same way. We contacted leaders in the matched villages to obtain phone numbers of these respondents. We programmed our survey instrument using OpenDataKit (ODK) (Hartung et al., 2010), and we trained enumerators at the state level to administer this instrument using mobile devices.

The enumerators administered the telephone survey from October 6 to December 20, 2020. After introductions, the enumerator asked to speak with the main male or female decision-maker of their household, depending on which sex had been randomly selected for that household. Once the respondent of the appropriate sex was identified, the purpose of the survey was explained. They were additionally informed that, while some of their answers could be recorded for quality control purposes, their responses will be kept confidential and their participation

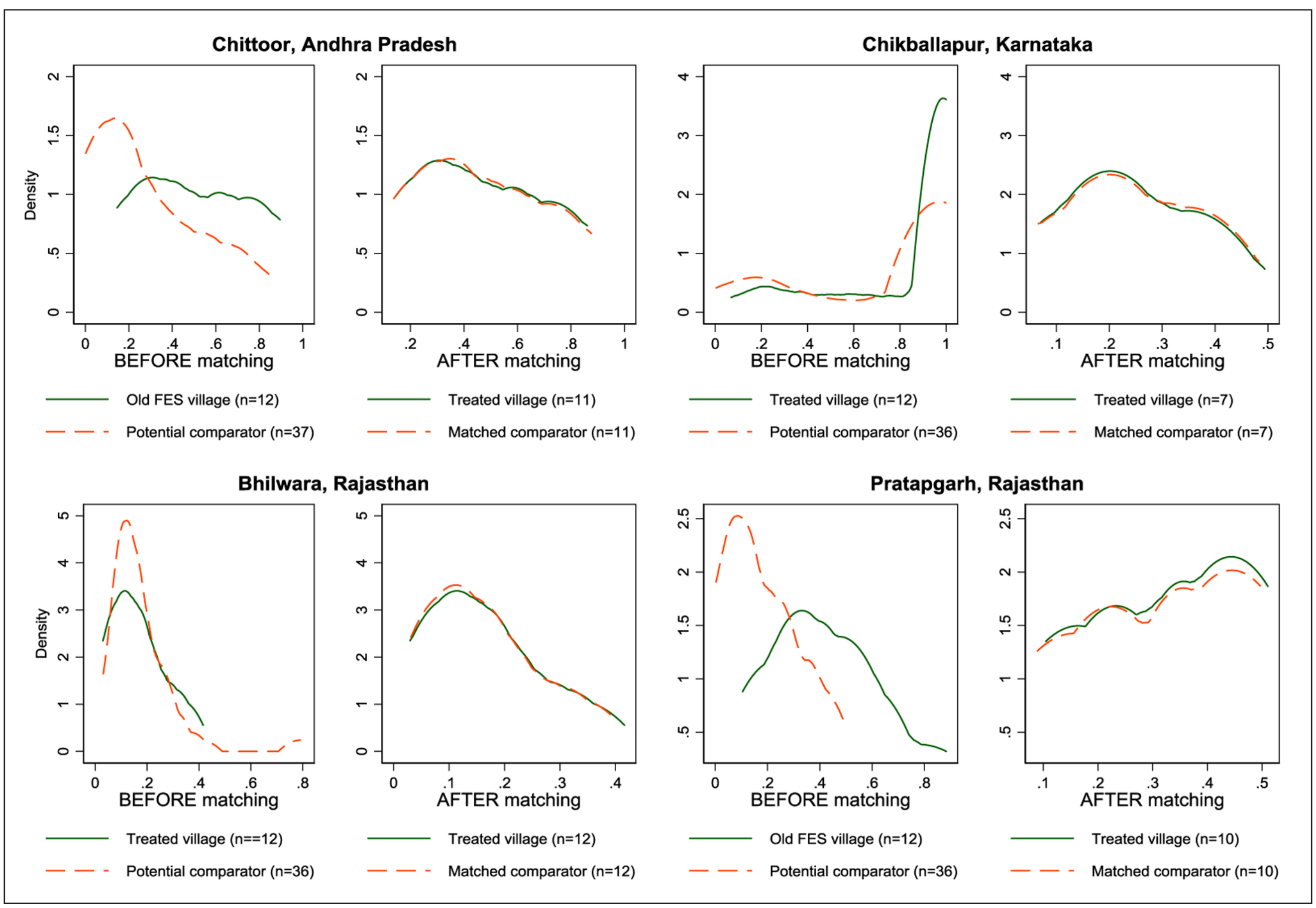

Figure 4 Propensity score density plots before and after matching for telephone survey villages.

was voluntary. Thereafter, their consent to participate in the survey was obtained.

We programmed an audio audit function into the survey instrument, such that random segments of the interviews were recorded for 30 seconds at a 25% probability. We monitored these audio audits continuously during data collection, with FES's Study Team also listening to the recordings in the local languages. We intensely monitored the timing of overall survey and module delivery as well. Four enumerators were caught making up data and immediately disengaged and replaced. Their uploaded survey forms were discarded as well.

We encountered several challenges during data collection. First, the audio audit functionality only worked on specific android devices, so we took significant time at the beginning of the exercise to resolve this issue. Second, many respondents were reluctant to allocate their time for the telephone interviews. We mitigated this by having senior members of FES's Study Team call the selected respondents in advance to explain the survey exercise and request them to positively engage with the enumerators when they called. However, in the tribal communities of Odisha, many sampled respondents were, nevertheless, reluctant to participate. Observing COVID-19 prevention protocols, we attempted to carry out face-to-face interviews in the two districts located in this state. However, given that some villages were inaccessible due to an outbreak of COVID-19, coupled with identified data quality concerns and the differing nature of data collection methods, we decided to exclude these two districts from this interim study. In the end, we used data collected from 402 households residing in 40 treated villages and 370 households residing in 40 matched comparison villages (Table 1).

3.4 DATA

What follows is a brief description of the main data captured through our telephone survey.

Respondent and household characteristics

Once consent was solicited, demographic information was obtained from the respondents including their age, marital status, highest level of education, principle occupation, religion, caste category, household size, as well as whether

	OLD ('TREATED') VILLAGE	NEW ('COMPARISON') VILLAGE	TOTAL
Overall			
Households sampled	402	370	772
Female respondent	205	185	390
Male respondent	197	185	382
Chittoor District, Andhra Pradesh			
Households sampled	112	94	206
Female respondent	57	52	109
Male respondent	55	42	97
Chikballapur District, Karnataka State			
Households sampled	54	50	104
Female respondent	27	21	48
Male respondent	27	29	56
Bhilwara District, Rajasthan State			
Households sampled	132	123	255
Female respondent	68	63	131
Male respondent	64	60	124
Pratapgarh District, Rajasthan State			
Households sampled	104	103	207
Female respondent	53	49	102
Male respondent	51	54	105

Table 1 Sample sizes by district and respondent sex.

any migrant workers had returned since the onset of the COVID-19 pandemic.

Access to the commons and government programs

Respondents were additionally asked whether their households had collected timber and non-timber products since the beginning of 2020 and, if so, the types of products and whether any had been sold. We then asked if the quantity of products collected from the commons had changed from previous years and, if so, how and the reasons for the change. We further asked if their household had received support from any social security program since the beginning of 2020. If so, we followed-up with questions about the nature of this support, including its estimated cash value.

COVID-19 livelihood impacts

The enumerators asked respondents if their households had experienced any atypical on-farm and off-farm related challenges since the beginning of 2020 and, if so, what these were specifically. They were further requested to self-report how they expected their 2020 on-farm and off-farm income would compare to that earned in 2019.

Primary outcome measure: The Livelihood Coping Strategies Index (LCSI)

We adapted the World Food Program's (WFP) Livelihoods Coping Strategies Index (LCSI) (WFP, 2015), which is based on the original Coping Strategies Index (CSI) (Maxwell & Caldwell, 2008). We obtained data required to construct the LCSI by asking respondents if their households had engaged in various coping strategies due to a lack of resources to meet domestic needs since the beginning of 2020. WFP stresses flexibility on what specific coping strategies should be included in the module to ensure their appropriateness for the local context. It recommends that the analyst choose 10 strategies to construct the LCSI—four from the Stress category and three each from the Crisis and Emergency categories. Drawing on WFP's master lists and making adaptations to the local context, we collected data on 21 coping strategies (Supplementary Table A.3).

To construct our primary outcome measure—the weighted LSCI—we followed WFP's approach but selected the most commonly reported strategies under each of the three categories, irrespective of treatment status. We did this to capitalize on the variability in our data, as well as to justify our inclusion of some coping strategies in the index and not others.

As a robustness check, we also compared households in the matched treated and comparison villages against a raw score comprising all 21 coping strategy items and another index also comprising all items but constructed using principle component analysis (PCA). This latter procedure enabled the 21 items to be reduced into a single index, while retaining much of the variance in item responses (Delchambre, 2015).

Given that lockdown measures were common across districts, we anticipated that the livelihood activities of households in both villages targeted by FES and those in the matched comparison villages would have experienced similar levels of disruption. However, we hypothesize that the resulting effects should be less severe among the former, given their better access to common pool resources and government programs. Specifically, we expected households in villages where FES has intervened to be less in need of engaging in negative coping behaviors, such as distress asset sales.

3.5 Data analysis

As was the case for village-level PSM, we used Stata (StataCorp, 2017) to conduct our analysis. A standard initial analytical procedure in both experimental and quasi-experimental studies is to compare treated and control observations vis-à-vis relevant baseline and time invariant variables. If the two groups are statistically similar in relation to these variables, this generates confidence in the effectiveness of the randomization procedure or quasi-experimental strategy in question (Brooks & Ohsfeldt, 2013). While we did not have access to baseline data at the individual or household levels, our first step in the analysis of our dataset involved comparing the two treatment groups vis-à-vis12 covariates. These are variables that are either time invariant or assumed unlikely to be affected by FES's intervention. They include a) the proportion of respondents who are female, married, fulltime farmers, and fulltime laborers; b) the proportion of households caring for under-five children, with no adults under the age of 60 years, of the Hindu faith, and belonging to either the Schedule Caste or Scheduled Tribe social groups; c) the respondent's age; and d) household size, including numbers of children and adults less than 60 years of age.

We further tested whether all 12 variables jointly predict a household's treatment status. Given that an individual covariate can be statistically different among treatment groups due to chance (as opposed to a systematic source of bias), this test of joint orthogonality is important and complementary (Özler et al., 2018).

As was the case for our outcome measures, we clustered standard errors at the village level (our pseudo unit of assignment) in our statistical comparison of these covariates. We further employed sampling weights to adjust for deviations from our targeted sample size of 10 households in some of the study villages, thereby ensuring their equal representation. Moreover, given that we performed the village matching exercises within districts, district dummy variables were included in all our statistical models.

We compared households in the treated and matched comparison villages vis-à-vis our primary outcome measure (described above) using ordinary least squares (OLS) regression. As a robustness check, we did so both with and without the above covariates. We further checked the robustness of our results by using robust regression. This approach allocates less weight to extreme observations, thereby generating results that better represent the bulk of the distribution (Funk et al., 2011). Finally, we implemented quantile regression to compare median, as opposed to average, values between the treated and comparison observations. Potentially differential treatment effects across districts were further tested through the implementation of Wald tests.

Moreover, given that we collected data related to the two hypothesized economic mechanisms described in Subsection 2.2, i.e., on household collection of products from the commons and participation in government social programs, we explored the extent to which the variation in the data are consistent with these. We did this using causal mediation analysis (MacKinnon, 2008).

A simple one mediator (M) variable model—X → M → Y—is founded on three foundational regression equations:

$$Y = \alpha_1 + cX + e1 \tag{1}$$

$$M = \alpha_2 + aX + e2 \tag{2}$$

$$Y = \alpha_3 + c'X + bM + e3 \tag{3}$$

Mediation is possible if:

(1) X co-varies with Y, i.e., parameter c in equation 1 is statistically significant
(2) X co-varies with M, i.e., parameter a in equation 2 is statistically significant
(3) parameter b in equation 3 is statistically significant, i.e., the variation that both X and M share explains variation in Y.

A statistically significant effect estimate would indicate that Condition 1 is met. Our next step was to then interrogate the second and third conditions using Stata's *sem* command (StataCorp, 2017). We used five alternative measures of the mediator variable associated with the enhanced commons resource access hypothesized mechanism, as well as three others for the enhanced access to social safety nets hypothesized mechanism.

If the variability in our data supports either hypothesized mechanism, we expected to see a correlation between the treatment dummy (X) and the various mediator variable measures (Condition 2). Moreover, the variation shared by X and M should significantly predict variation in our primary outcome measure (Condition 3).

4. RESULTS

4.1 COVARIATE COMPARISON

For the overall sample, we found no statistically significant differences between individuals and households in the treated and matched comparison villages vis-à-vis our 12 covariates (Table 2). Rubin and Imbens (2015) argue that a standardized mean difference (SMD) less than 0.25 is indicative of reasonable balance. From our chi-squared test of joint orthogonality, we find that all 12 variables fail to jointly predict a household's presence in a FES treated village.

4.2 LIVELIHOOD IMPACTS OF COVID-19

We examine atypical on-farm and off-farm livelihood challenges reported by the surveyed respondents since the onset of the COVID-19 pandemic (Figure 5). Approximately 70% of respondents in both the treated and matched villages reported experiencing at least one challenge in relation to both their on-farm and off-farm activities.

We observe that the inability to carry out farming activities as usual is the most commonly reported farm-related challenge (~60% of households), followed by challenges related to accessing farm inputs (~50% of households). We further note that approximately one-third of household respondents reported unusual challenges marketing their products (both crops and livestock). However, those in the comparison villages were approximately 8% more likely to report this challenge ($p = 0.02$). We decompose these results by respondent sex and district (Appendix A, Figure A.2). While the reported challenges differ little by respondent sex, we see that households in both treated and comparison villages in the two Rajasthani districts were more likely to report challenges carrying out farming activities and selling farm produce. However, we see that similar numbers of households across the four districts reported unusual difficulties accessing farm inputs.

We further observe that approximately 75% of respondents in both the treated and comparison villages reported that their households were significantly reliant on income from daily wage labor, and over half of respondents reported fewer wage labor opportunities in 2020 as compared with other years (Figure 5). Moreover, in both treated and comparison villages, household

	TREATED VILLAGE	COMPARISON VILLAGE	DIFFERENCE	
	MEAN (μ1)	MEAN (μ2)	$\mu1 - \mu2$	SMD
Respondent, Female ($\hat{p}$)	0.504	0.488	0.015	0.02
			(0.023)	
Respondent, Married ($\hat{p}$)	0.898	0.907	–0.0095	–0.018
			(0.022)	
Respondent, Farmer ($\hat{p}$)	0.788	0.767	0.018	0.031
			(0.037)	
Respondent, Laborer ($\hat{p}$)	0.115	0.121	–0.0034	–0.013
			(0.029)	
Household has under 5 children ($\hat{p}$)	0.403	0.396	0.0069	0.004
			(0.036)	
Elderly headed ($\hat{p}$)	0.047	0.045	0.003	0.019
			(0.014)	
Household, Hindu ($\hat{p}$)	0.935	0.992	–0.056	–0.305
			(0.034)	
Scheduled, Tribe/Caste ($\hat{p}$)	0.495	0.51	–0.014	–0.07
			(0.071)	
Respondent age	39.136	39.345	–0.2	–0.024
			(0.97)	
Household size	5.378	5.299	0.07	0.041
			(0.19)	
Number of children	1.728	1.699	0.022	0.008
			(0.11)	
Number of productive age adults	2.915	2.906	0.011	0.025
			(0.12)	
Chi-2 test of joint orthogonality			11.65	
p-value			0.474	
N (households)	**402**	**370**	**772**	**772**
N (villages)	**40**	**40**	**80**	**80**

Table 2 Covariate Comparison—Respondents & Households in Matched Treated and Comparison Villages.
* p < 0.1 ** p < 0.05 *** p < 0.01; Standard errors in parentheses and clustered at village cluster level (matching unit);
Sampling weights used to adjust for deviations from target sample size of 10 households in some villages;
District fixed effect used (matching strata); SMD = Standardized Mean Difference.

respondents reported job loss (~20%), delayed salary and wage payments (~15%), and cuts in salaries and wages (~10%). Atypical challenges accessing business supplies and equipment and selling goods and services were also reported by approximately 15% of respondents in both sets of villages also.

We note further that households in the matched comparison villages were 7% more likely to report experiencing at least one off-farm challenge but find this difference to be statistically insignificant when district effects and village-level clustering are considered (p = 0.280). We decompose the results by respondent sex and district (Supplementary Figure A.4). We observe, again, that the two Rajasthani districts stand out, particularly in terms of a reported reduction in wage labor opportunities. Very few respondents in these two districts reported that their

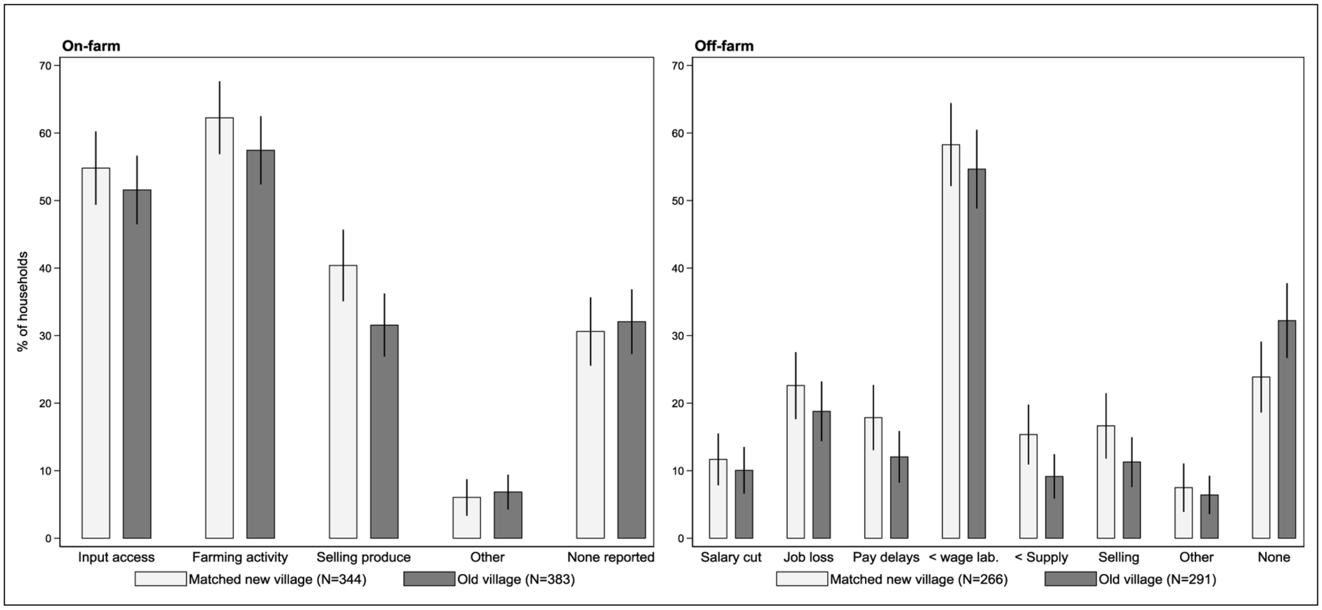

Figure 5 Reported farm & off-farm related challenges experienced since onset of COVID-19.
With 95% confident intervals Sampling weights used to adjust for deviations from target sample size of 10 houscholds in some villages
Results reported only if household reported on-farm or off-farm activities/income sources.

household had not experienced any challenge pertaining to their off-farm livelihood pursuits. Additionally, we see that over half of the respondents in Bhilwara reported that someone in the household had lost their job.

We additionally observe that approximately 40% of respondents from both the treated and comparison villages expected that their income from both on-farm and off-farm sources would drop by half or more compared to that earned in 2019, with over 75% reporting at least some loss (Figure 6). Approximately, 10% in both sets of villages reported either no change or some improvement. Approximately, 6% more respondents from the treated villages reported an expected drop in farm-income by half or more, but this difference, again, is not statistically significant ($p = 0.261$).

4.3 ENGAGEMENT IN MALADAPTIVE COPING BEHAVIORS

Having had both their on-farm and off-farm activities adversely affected, we expect to see households in both the treated and comparison villages resorting to various coping strategies, many of which are likely to impede their ability to recover, i.e., those that can be considered as 'maladaptive'. We examine the percentages of respondents who reported that their households had engaged in each of the 21 coping strategies since the start of 2020 (Figure 7).

Under WFP's Stress coping category, we observe that over half of households in both the intervention and comparison villages reported that they had depleted their savings, switched to less preferred foods, and took out loans with high uncertainty about their ability to pay these back (presumably exacerbating indebtedness). Moreover, under the Crisis coping category, we see that approximately 75% of respondents reported reducing household expenditure to only essential items, such as food, and, thereby forsaking expenditure on other items, such as healthcare and education. Moreover, and still under this category, we see that approximately half of households in both treatment groups either consumed seed stock or reduced agricultural input expenditure, thereby adversely affecting their future production. Finally, under the Emergency category, we observe that approximately one-third of households coped with the impacts of the pandemic by mortgaging land and other major assets and one-fifth migrated from their home villages in search of work.

Overall, we see that similar percentages of respondents in both the treated and comparison villages reported that their households had undertaken each of the 21 coping strategies. However, for most strategies, slightly more respondents from the comparison villages reported that their households had done so. In Figure 8 below, we present the distributions of our weighted LCSI in the form of density plots decomposed by treatment group.

For both treatment groups, we see significant variation across households. However, the distribution for the households in the treated villages is more to the left, with about a 10% difference in the distributions' respective medians.

We produced disaggregated density plots by sex of respondent and district (Supplementary Figure A.6). In

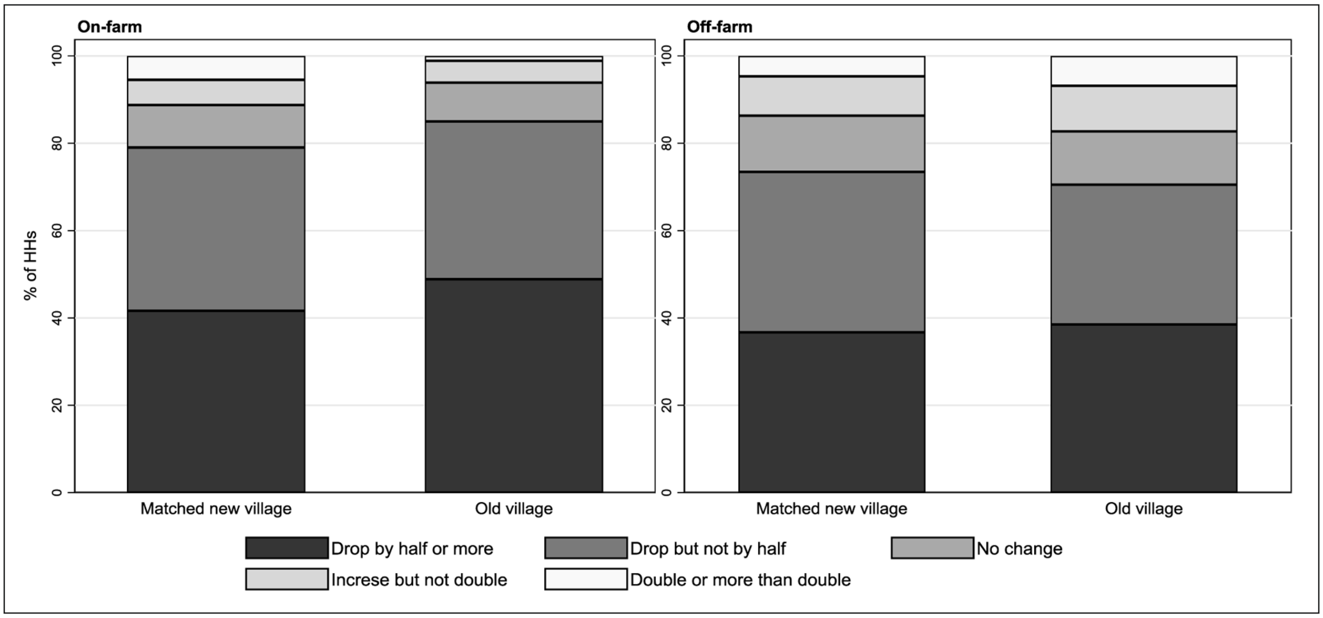

Figure 6 Expected changes in 2020 versus 2019 on-farm and off-farm income.

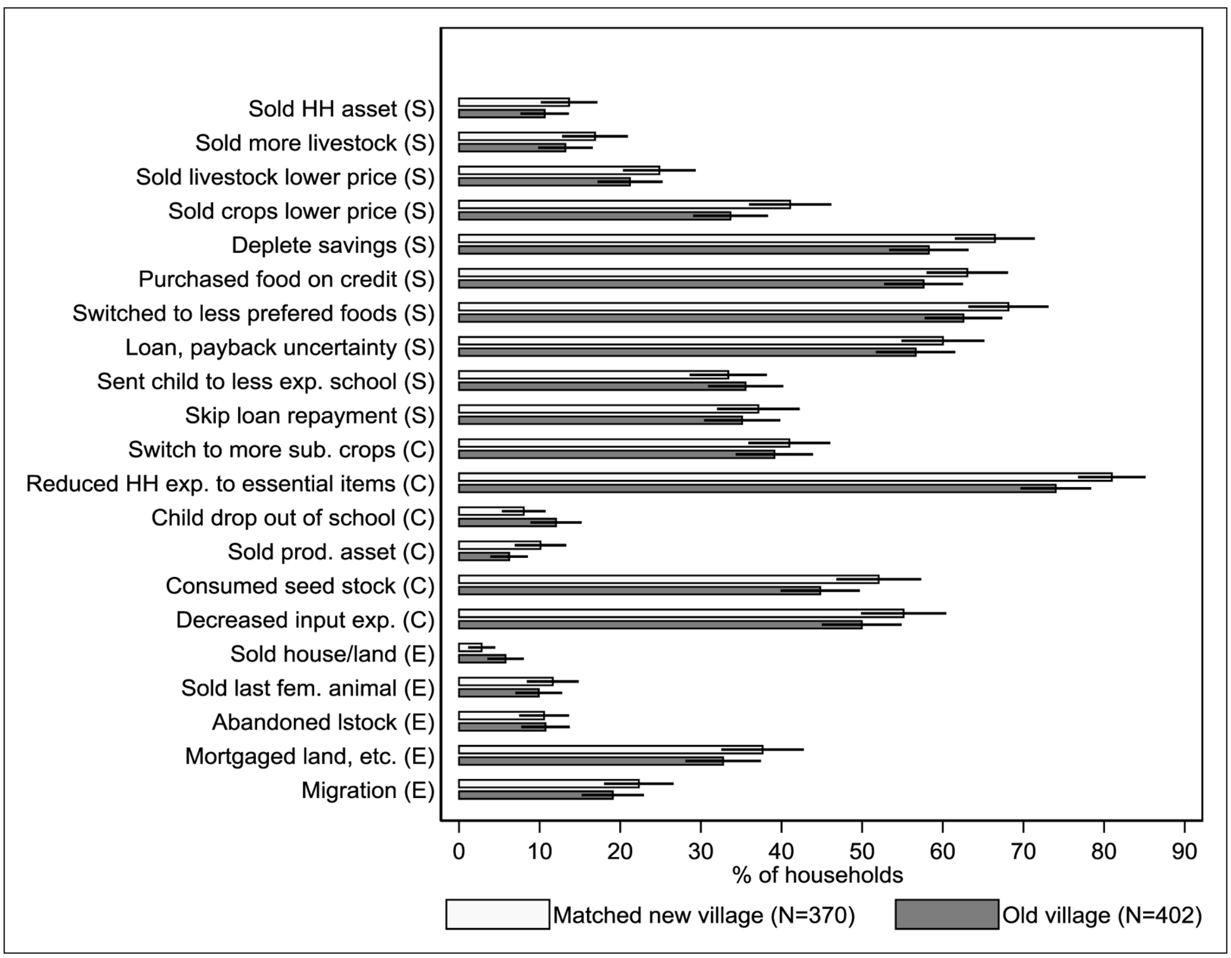

Figure 7 Coping strategies undertaken since beginning of 2020.
With 95% confident intervals Sampling weights used to adjust for deviations from target sample size of 10 households in some villages S = Stress category; C = Crisis category; E = Emergency category.

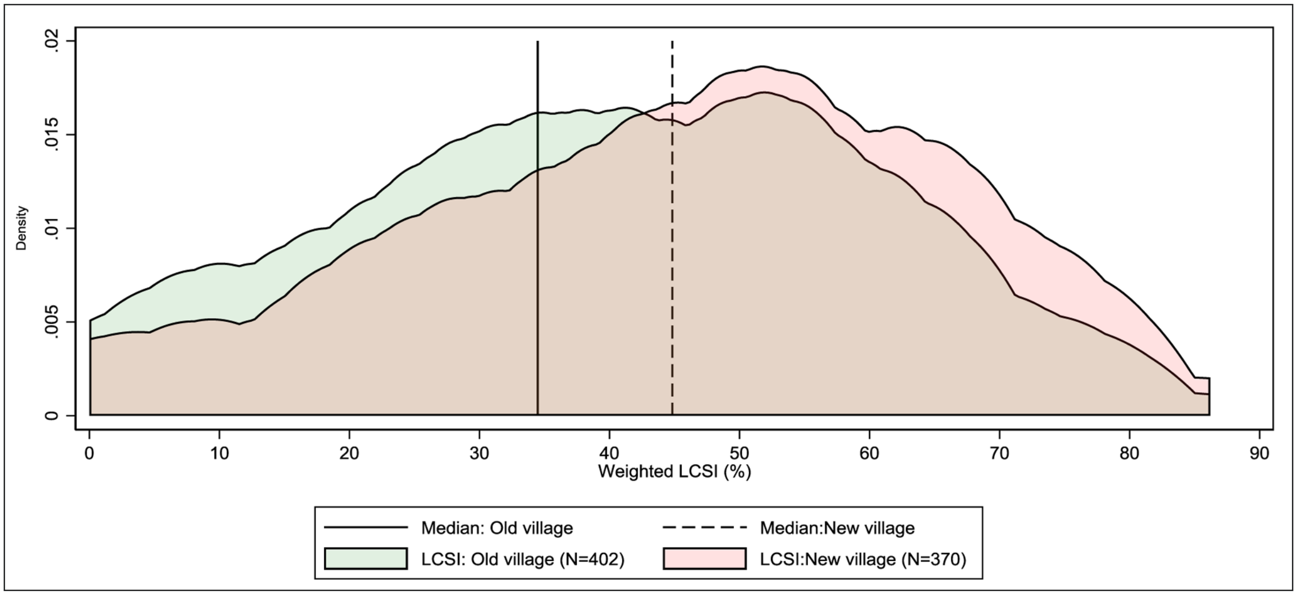

Figure 8 Density Plots for Weighted Livelihoods Coping Strategy Index (LCSI).
Sampling weights used to adjust for deviations from target sample size of 10 households in some villages.

general, we see a similar pattern. However, the LCSI scores for both the treated and comparison households are significantly higher in the two Rajasthani districts. We find this consistent with how the respondents from these districts reported that their livelihood activities had been more greatly affected by COVID-19.

We appraise the statistical significance of these graphically observable differences (Table 3). We find that the average difference between households in the treated and comparison villages is 4.5 percent points, meaning that households in the former scored 11.3% lower on the LSCI. This is a modest effect size (Cohen's d = –0.21), but statistically significant ($p < 0.05$). We further find that the results are robust across the different models and outcome measure specifications. We find the 7% median overall difference also noteworthy. We observe variation in the estimated effect sizes across the four districts (Supplementary Table A.4), with those of the two Rajasthani districts being larger. However, all district-level estimates are in a consistent direction. We test whether the effect sizes between the two northern and two southern districts are statistically different from zero and find this not to be the case ($p = 0.5428$).

Using mediation analysis, we next explore the extent to which the variation in the data are consistent with our two explanatory hypotheses for how FES's intervention model may have given rise to this effect. We discover that our treatment dummy variable co-varies with only two of the mediator measures associated with the commons product access mechanism (Condition 2, Table 4). Specifically, 40% of respondents in the treated villages reported having had collected at least one product from common land in the last 12 months, as compared with 27% in the comparison villages. In addition, 11% of treated households reported that they relied more on the commons in 2020 than in 2019, against 7% among their untreated counterparts. We further find that almost all households in both treated villages (96%) and comparison villages (93%) accessed one or more social safety net programs over the survey recall period. With such low variation, we therefore focus our mediation analysis on income earned through such programs, as well as estimated changes in such income. We find that no mediator variables for the access to safety net mechanism co-vary with our treatment dummy variable and therefore fail to meet this condition.

For Condition 3—i.e., the variation shared by both our treatment dummy (X) and mediator measures (M) predict variation in our primary outcome measure (Y), we find only one statistically significant coefficient (column 5). This pertains to our treated dummy variable and the 'any product collected from common land in 2020' dummy variable. Moreover, we see that the direction of this coefficient indicates that residing in both a treated village and having had collected products from the commons in 2020 is positively associated with our primary outcome measure. That is, while households in treated villages were more likely to report collecting products from the commons, those that did were also more likely to engage in negative coping behaviors. As a consequence, we see that the associated direct effect estimate (column 6)—i.e., X's effect on Y independent of the correlation it shares with M—is slightly inflated as compared with our overall estimated treatment effect.

	TREATED VILLAGE MEAN (µ1)	COMPARISON VILLAGE MEAN (µ2)	OLS (NO COVARIATES)	OLS (COVARIATES)	ROBUST REGRESSION	QUANTILE REGRESSION
Weighted LCSI (%)	39.55	44.03	–4.48**	–4.35**	–4.83***	–6.90***
			(1.89)	(1.87)	(1.43)	(1.76)
LCS PCA Index	1.51	1.69	–0.18**	–0.18**	–0.16***	–0.22***
			(0.079)	(0.076)	(0.058)	(0.070)
Raw LCS (21 items)	6.89	7.57	–0.68*	–0.67**	–0.59**	–1***
			(0.34)	(0.33)	(0.25)	(0.28)
Observations	**402**	**370**	**772**	**763**	**772**	**772**

Table 3 Overall & district comparisons of treated & comparison villages against various variations of LCSI.
*p < 0.1 **p < 0.05; Standard errors in parentheses with standard errors clustered at the village level for OLS models.
District fixed effects used in all models (strata used in village matching).
Scheduled Tribe/Scheduled Caste; respondent's age; household size; # of children; # of working age adults.
Sample weights to adjust for deviations from target village sample size (n = 10) used in all models, save robust regression.
OLS = Ordinary Least Squares Regression; LCSI = Livelihoods Coping Strategies Index; PCA = Principal Component Analysis.

(1)	(2)	(3)	(4)	(5)	(6)	(7)
MEDIATOR MEASURE	TREATED VILLAGE	COMPARE VILLAGE	CONDITION 2	CONDITION 3	DIRECT EFFECT	% MEDIATED
	MEAN ($\mu 1$)	MEAN ($\mu 2$)	X → M	XM → Y	X – XM	VIA XM
Enhanced commons resource access hypothesized mechanism						
Any commons product collected in 2020 ($\hat{p}$)	0.40	0.27	0.13***	0.798**	–5.28***	–17.83
			(0.043)	(0.37)	(1.84)	
# of commons products collected in 2020	0.70	0.52	0.1801	0.50	–4.98***	–11.14
			(0.11)	(0.32)	(1.80)	
Any common product sales in 2020 ($\hat{p}$)	0.05	0.04	0.013	0.014	–4.491***	–0.31
			(0.019)	(0.03)	(1.88)	
More products collected in 2020 than 2019 ($\hat{p}$)	0.08	0.07	0.015	0.150	–4.623**	–3.33
			(0.024)	(0.24)	(1.81)	
Relied more on commons in 2020 than 2019 ($\hat{p}$)	0.11	0.07	0.038**	0.20	–4.677**	–4.46
			(0.019)	(0.13)	(1.89)	
Enhanced access to safety net programs hypothesized mechanism						
Estimated social safety net income, 2020 (INR)	15,982	14,543	1439	0.263	–4.74**	–5.87
			(1372)	(0.28)	(1.88)	
Estimated soc. safety net income dif. 2020 –	530	1888	–1358	–0.006	–4.47**	0.12
2019 (INR)			(1489)	(0.091)	(1.90)	
MGNREGS Income 2020 (INR)	6274	5487	787	0.31	4.89**	–6.93
			(763)	(031)	(1.90)	
Observations	**402**	**370**	**772**	**772**	**772**	

Table 4 Results of mediation analysis evaluating candidate mediator variables.
* p < 0.1 ** p < 0.05 *** p < 0.01.
District fixed effects used in all models (strata used in village matching).
Y = outcome variable; X = treatment dummy; M = mediator variable; XM = variation shared by X and M.
Direct effect = X's effect on Y independent of XM.

5. DISCUSSION

Large numbers of households in both the treated and matched comparison villages experienced significant and atypical challenges in relation to both their on-farm and off-farm livelihood activities during the first eight to nine months of the COVID-19 pandemic. This, in turn, adversely affected household income and, presumably, subsistence food production. These findings reinforce those of other studies on the impacts of the pandemic in rural areas of India (Jaacks et al., 2021; V. Suresh et al., 2022). However, consistent with our overall expectation, households in villages where FES had intervened for at least five years were less likely to engage in negative coping behaviors, as compared with their counterparts in the comparison villages.

The extent to which we can confidently conclude that this was caused by one or more facets of FES's intervention model rests on how successfully we addressed program placement bias, given FES's non-random targeting of the areas in which it worked. We sought to mitigate this form of bias by matching intervention and comparison villages vis-à-vis measures of FES's explicit area-level targeting criteria. This matching effort was largely successfully, and the fact that the associated statistical balance also replicated for individual- and household-level covariates gives further credence to our causal identification strategy.

That said, given the absence of random assignment, we cannot conclusively rule out the possibility that our estimated effect is simply indicative of systematic bias, either program placement or response bias, i.e., the tendency for respondents to respond inaccurately or untruthfully to survey questions (Furnham, 1986). However, for such bias to affect our results, it would need to have been present across all four districts. The fact that a similar effect was observed in all districts meets the consistency criterion, one of several criteria some use to evaluate the plausibility of an observed association being causal (Höfler, 2005). Moreover, if systematic response bias was responsible—e.g., resulting from respondents in either the treated or comparison villages over- or under-reporting the coping strategies they pursued—we would expect reported livelihood impacts associated with COVID-19 to be dissimilar as well. Yet, these impacts were reported in similar levels of severity by respondents in both treated and comparison villages.

Nevertheless, one key limitation of our study is that we are unable to explain—quantitatively—how our observed effect came about. Indeed, a purported causal relationship is more convincing when the mechanism(s) through which the hypothesised cause generated the observed effect is evidenced (Reynolds et al., 2004). Our causal conclusions pertaining to the impact of FES's would, therefore, be much stronger if we had evidence supporting how this impact came about. Unfortunately, our causal mediation analysis does not support our two proposed economic-oriented mechanisms—enhanced access to common pool resources and government safety net programs. If the variation in our data supported mediation vis-à-vis either hypothesized mechanism, our direct effect estimates would be significantly smaller than our estimated overall effect. We found this not to be the case, and we are, consequently, unable to explain how FES's intervention model may have reduced negative coping behavior among households in the intervention villages in the wake of the livelihood disruptions associated with the COVID-19 pandemic.

It is, of course, possible that some other mechanism led to this effect, for which we did not capture data. It could also be the case that we measured our hypothesized mediating mechanisms too crudely, e.g., by failing to capture data on the quantity and quality of the specific products collected from the commons. Indeed, the results of mediation analysis is well known to be highly susceptible to measurement error (VanderWeele et al., 2012).

Another key limitation of our interim study is that we did not capture data on the extent to which FES's intervention model was implemented in the treated villages. There is likely considerable heterogeneity, for example, on the extent to which targeted common lands have been restored, as well as the nature and enforcement of rules regulating their access. Further, some efforts to secure formal community rights over the commons or link the targeted villages to social security programs may have been more successful than others. The absence of such data handicaps our ability to explore how variation in the rollout of FES's core model affects our treatment effect estimate. If we were to find less maladaptive coping behavior among households in villages where such rollout has been particularly successful, the veracity of our overall treatment effect estimate becomes more plausible. Such dose-response analysis supporting (as opposed to conclusively evidencing) causal inference has been advocated for and used elsewhere in the literature (Bessinger et al., 2004; Dunn et al., 2005; Hill, 1999). Indeed, examining a program's 'change model' to disentangle critical and non-critical features driving outcomes is not foreign to the evaluation literature (Donaldson & Lipsey, 2006; Lee et al., 2008; Vartuli & Rohs, 2009).

Having failed to produce quantitative evidence in support of the two economic mechanisms, we propose an institutional explanation. The logic is as follows: FES's intervention model involves facilitating the setting up of inclusive and democratic decision-making processes for how common resources should be managed and by whom.

With stronger local institutions for governing the commons, local people possess a potentially valuable institutional foundation for organizing and conducting village affairs in a more effective and inclusive manner. This includes prioritizing collective action, identifying needs and possible responses, addressing differences of opinion, managing conflicts, negotiating benefit-sharing agreements, and deliberating about how to deal with crises. Villages with solid institutional frameworks in place are better equipped to deal with external shocks, such as COVID-19, because such institutions provide the means for people to work together in a more strategic and cost-effective manner (Andersson et al., 2018; Torpey-Saboe et al., 2015; Valdivieso et al., 2021). Since we did not ask respondents in the phone survey about these institutional variables, we leave it to future work to evaluate this potential institutional causal mechanism.

6. CONCLUSION

The COVID-19 pandemic, and particularly the drastic measures governments have taken to control it, has affected people's lives and livelihoods in unprecedented ways. India, with its 400 million informal workers and 119 million farmers (Das, 2020; Roy & Bhattacharyya, 2020), is a stark example as demonstrated by our study. Leveraging a quasi-experimental research design and using a telephone survey, we evaluated the short- and medium-term impacts of FES's intervention model on household coping behavior in the wake of this largescale covariate shock.

Consistent with emerging evidence in the literature (Ceballos et al., 2020; Kumar et al., 2021), we find that both the on-farm and off-farm livelihoods of households residing in both the treated and matched comparison villages were adversely affected by COVID-19. The effects range from impeding access to agricultural inputs, services, labor, and markets through to a dramatic reduction in both formal and informal wage labor opportunities. Understandably, households in both sets of villages had to engage, consequently, in various coping strategies, such as the depletion of savings, reduced expenditure on agricultural inputs, and consumption of seed stock. Many of these behaviors are likely to exacerbate their longer-term vulnerability. However, we found that households in the villages where FES had intervened for at least five years were less likely to do so, and this was the case across all the four districts where our telephone survey was administered. Specifically, households in FES intervention villages scored 11.3% lower on our primary outcome measure—the Livelihoods Coping Strategies Index (LCSI)—than those residing in matched comparison villages, equating to a 4.5 percentage point difference.

We are challenged, however, to present quantitative evidence on the specific factors behind our estimated treatment effect. This points to the need for other rigorous studies evaluating the potential of environmental protection, restoration, and village institutional strengthening interventions in fostering positive coping behavior. We plan to contribute to this effort in the context of our larger impact assessment of FES's work.

NOTE

1 There is no firm rule with respect to what counts as a significant standardized difference. However, Rubin and Imbens (2015) suggest that anything above 0.25 can be considered as a significant.

ADDITIONAL FILES

The additional files for this article can be found as follows:

- **Supplementary tables and figures.** These tables and figures provide additional detail on FES's targeting criteria and associated village matching indicators; how these indicators compare between treated villages and all potential comparison villages; the full list of coping strategies for which data were obtained during the study's survey; FES's Theory of Change for its core model at village level; and disaggregated results by respondent sex and district. DOI: https://doi.org/10.5334/ijc.1155.s1
- **Data collection instrument.** PoC Mobile Survey. DOI: https://doi.org/10.5334/ijc.1155.s2

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COMPETING INTERESTS

The authors have no competing interests to declare.

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