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Can gender- and nutrition-sensitive agricultural programs improve resilience?

Medium-term impacts of an intervention in Bangladesh

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ABSTRACT

There are few studies that rigorously assess how agricultural and nutrition related interventions enhance resilience and even fewer that incorporate a gendered dimension in their analysis. Mindful of this, we address three knowledge gaps: (1) Whether agricultural interventions aimed at diversifying income sources and improving nutrition have sustainable impacts (on asset bases, consumption, gender-specific outcomes and women's empowerment, and on diets) that persist after the intervention ends; (2) whether such interventions are protective when shocks occur? and (3) whether these interventions promote gender-sensitive resilience. We answer these questions using unique data, a four-year post-endline follow-up survey of households from a cluster-randomized controlled trial of a nutrition-and-gender-sensitive agricultural intervention in Bangladesh. We find that treatment arms that included both agriculture and nutrition training had sustainable effects on real per capita consumption, women's empowerment (as measured by the pro-WEAI), and asset holdings measured four years after the original intervention ended. Treatment arms that included both agriculture and nutrition training (with or without gender sensitization) reduced the likelihood that households undertook more severe forms of coping strategies and reduced the likelihood that household per capita consumption fell, in real terms, by more than five percent between in the four years following the end of the intervention. The treatment arm that only provided training in agriculture had positive impacts at endline but these had largely faded away four years later. Our results suggest that bundling nutrition and agriculture training may contribute to resilience as well as to sustained impacts on consumption, women's empowerment, and asset holdings in the medium term. These have implications for the design of future gender- and nutrition-sensitive agricultural programs.

Keywords: resilience, agriculture and nutrition training, women's empowerment, Bangladesh

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ACRONYMS

ANCOVA	Analysis of Covariance
ANGeL	Agriculture, Nutrition, and Gender Linkages
BARI	Bangladesh Agricultural Research Institute
BCC	Behavior change communication
BIRTAN	Bangladesh Institute of Research and Training on Applied Nutrition
BRRI	Bangladesh Rice Research Institute
CNW	Community nutrition workers
GDQS	Global Diet Quality Score
hGDQS	Household-level GDQS
HKI	Helen Keller International
IFPRI	International Food Policy Research Institute
ITT	Intent-to-treat
LCSI	Livelihoods Coping Strategy Index
Pro-WEAI	Project-level Women's Empowerment in Agriculture Index
RCT	Randomized controlled trial
SAAO	Sub-assistant agricultural officer
WEAI	Women's Empowerment in Agriculture Index
WFP	World Food Programme

1. Introduction

There is a seemingly vast literature on resilience. Barrett, Hoddinott, and co-authors' (2021) systematic review of resilience to shocks in the context of low- and middle-income countries uncovered more than 9,000 published or grey literature studies. But as they note, a closer look reveals that this literature is not nearly as voluminous as it might first appear. Fewer than 250 of these papers meet their criteria for inclusion in their review.[1] Among the quantitative studies included in their review, the vast majority (77%) were household-level cross-sectional studies. There were fewer than ten studies that (a) have a clear definition of resilience and (b) use household panel data. While the importance of building up asset bases and diversifying income sources are recurrent themes in this literature, the Barrett et al. database contains virtually no studies that rigorously assess how interventions can enhance resilience to shocks. Their database contains few studies that incorporate a gendered dimension in their analysis of resilience; the literature on resilience has largely ignored the research and policy emphasis on gender and intra-household resource allocation that has taken place in the last 20 years. Moreover, although a small literature has examined the impact of idiosyncratic and covariate shocks on men's and women's asset holdings, existing studies (e.g. Quisumbing et al. 2018) have been associational, making it difficult to establish causality.

Our study seeks to bring evidence to bear on three knowledge gaps: (1) Whether agricultural interventions aimed at diversifying income sources and improving nutrition have sustainable impacts (on asset bases, consumption, gender-specific outcomes and women's empowerment, and on diets) that persist after the intervention ends; that is to say, do such interventions contribute to resilient escapes from poverty; (2) Are such interventions protective when shocks occur? That is, following the definition of resilience set out in Constas, Frankenberger, and Hoddinott (2014), do they enhance the "capacity that ensures adverse stressors and shocks do not have long-lasting adverse development consequences"; and

[1] Inclusion criteria were: Studies that are situated in low- or middle-income countries (per World Bank definitions); assess resilience to covariate or idiosyncratic shocks; examine resilience at the household or individual level; have some indicator of human wellbeing as an outcome of interest.

(3) Do these interventions promote gender-sensitive resilience; i.e., do they promote both the household's resilience generally and women's resilience specifically?

We attempt to answer these questions using data collected in a four-year post-endline follow-up survey of households from a cluster-randomized controlled trial of a nutrition-and-gender-sensitive agricultural intervention in Bangladesh. The initial evaluation (Ahmed et al. 2023a) found that treatments that combined agricultural training and nutrition behavior change communication (BCC) improved both production diversity and diet quality. Relative to treatments combining agriculture and nutrition training, Ahmed et al. (2023a) found that adding a gender sensitization component did not have a significant impact on measures of production diversity or diet quality. A companion study (Quisumbing et al. 2021) found that this intervention increased both women's and men's empowerment, raised the prevalence of households achieving gender parity, and led to small improvements in the gender attitudes of both women and men. Although they found significant increases in women's empowerment scores and empowerment status from all treatment arms there were no significant differences across these. The current paper seeks to ascertain whether these impacts persisted over the medium term, four years after the endline survey.

2. The Intervention

The intervention we study is the Agriculture, Nutrition, and Gender Linkages (ANGeL) project, a multi-arm cluster Randomized Control Trial (RCT) implemented over a 17-month period, from July 2016 to December 2017.[2] ANGeL consisted of the following treatment arms:

T-N(SAAO): Nutrition BCC

T-N(CNW): Nutrition BCC

T-A: Agricultural Production training

T-AN: Agricultural Production training and Nutrition BCC

T-ANG: Agricultural Production training, Nutrition BCC, and Gender Sensitization

[2] Additional details on the implementation of ANGeL can be found in Ahmed et al (2023a).

C: Control

The two T-N arms each consisted of 19 sessions. Topics included an introduction to the functional roles played by different types of foods, the importance of a balanced diet, micronutrients (vitamin A, iron, iodine, and zinc) and sources of food containing these, age-appropriate complementary foods, optimal breastfeeding practices, maternal nutrition and care, safe food preparation and preservation, hygiene, and handwashing. Activities included lectures, interactive discussions, games, and cooking demonstrations. Helen Keller International (HKI) developed the curriculum and training materials for the nutrition behavior change communication (BCC) with the Bangladesh Institute of Research and Training on Applied Nutrition (BIRTAN) and IFPRI. Training in the T-N(SAAO) arm was delivered by sub-assistant agricultural officers (SAAOs) – also referred to as agricultural extension agents – who are permanent employees of the Bangladesh Ministry of Agriculture. Training in the T-N(CNW) arm was delivered by Community Nutrition Workers (CNW) hired for the ANGeL study. Note that neither the T-N(SAAO) nor the T-N(CNW) treatment arms were included in the 2022 follow-up study.

The T-A arm consisted of 17 sessions. Topics covered an introduction to the cultivation of high-value crops (fruit and vegetables), using crop calendars to design a year-round system of cultivation, preparation of small plots and homestead gardens, water, pest and fertilizer management, harvest techniques, post-harvest storage, and marketing. Raising poultry, sheep and goats was also discussed, with attention to breed selection, feeding, vaccination, and diseases. The curriculum also included training on fishpond cultivation; fish is an important protein source in Bangladesh, and many Bangladeshi households have small fishponds in their homesteads or cultivate seasonal fishponds (Belton et al. 2019). Although these training sessions focused on agriculture, nutrition content was integrated by building competencies in identifying and cultivating nutrient-dense crops for household consumption. The curriculum and materials for the agricultural production training were developed by HKI in collaboration with the Bangladesh Agricultural Research Institute (BARI) and the Bangladesh Rice Research Institute (BRRI). Sessions included initial training, refresher training on key topics, and opportunities for participants to discuss their experiences applying their training. Training was delivered by SAAOs.

The T-AN treatment was also delivered by SAAOs. It consisted of the 17 T-N sessions and the 19 T-A sessions for a total of 36 training sessions.

The T-ANG arm received the 36 sessions associated with T-AN treatment arm and eight additional sessions on gender sensitization. Topics were based on HKI's *Nurturing Connections* curriculum (Helen Keller International Bangladesh 2017) and facilitated by staff hired by HKI. The gender sensitization sessions included structured activities aimed at improving intra-family respect, appreciation, and communication, as well as improving negotiation skills. These highly interactive sessions focused on gender relations, power dynamics, communication, and empowerment. The gender sensitization sessions invited mothers-in-law to participate along with husbands and wives, recognizing the role they play in decision-making around food and diets in rural Bangladesh.

Each training session conducted as part of ANGeL - lectures, interactive discussions, practical demonstrations, and question-answer sessions - lasted approximately 1.5 hours. Training took place either in meeting rooms or open courtyards in the villages where study participants resided; approximately 90 percent of participants reported that training sites were within one kilometer of their homes. Both husbands and wives were expected to attend each session, and care was taken to encourage active participation from both men and women. Participants received a small allowance for each training session to cover incidental costs of attending: 125 taka (approximately USD 1.50 at the time of the study) for one participant or 250 taka per household if both the husband and wife participated.

3. Study Design, data, sampling, and statistical methods

3.1 Study design and data

In the original study, ANGeL was designed to detect impacts of a 10% increase in households' per capita daily calorie availability and the Women's Empowerment in Agriculture Index (WEAI) score (Alkire et al. 2013), setting 80% power and 0.05 level of significance. Power calculations drew on data from the 2011/2012 round of the Bangladesh Integrated Household Survey, which is nationally representative of rural Bangladesh. The resultant sample size also provided 80% power at 0.05 level of significance to

detect an increase of one new food crop produced in homestead gardens and a 7.5% increase in the household Global Diet Quality Score – measures we use to assess impacts on production diversity and diets.

Because training would be conducted by SAAOs, and each SAAO was assigned to a "block," cluster-randomization was conducted at the block level, using blocks as clusters. Working with the Ministry of Agriculture, we identified all rural *upazilas* that were agro-ecologically suitable for agricultural diversification and had good market connectivity, thus considered appropriate for the ANGeL interventions. From a list of 484 such *upazilas*, 16 *upazilas* were purposively selected, such that each of the eight administrative divisions of Bangladesh was represented. From the list of all 525 blocks in 16 *upazilas*, we randomly selected 10 blocks from each *upazila*, yielding 160 blocks. Based on the power calculations, these were randomly assigned as follows: 25 blocks to each treatment arm (T-A, T-N, T-AN, T-ANG, as well as the additional treatment described in footnote 4), and 35 blocks to the control group. One village from each block was randomly selected.

In each selected village, we conducted a census. Villages ranged in size from 175 to 570 households; the average village contained 364 households. From the census, we identified households that had: (1) Engaged in crop production in the previous 12 months; and (2) had a child aged less than 24 months. From the list of all households who met these two criteria, we randomly selected 25 to take part in the training and the surveys. SAAOs contacted husbands and/or wives by mobile phone and invited them to attend the training sessions; in a few cases, invitations were made in-person.[3] This yielded 625 households in each treatment arm (2,500 households in total), and 875 households in the control group, for a total sample of 3,375 households.

[3] Only members of households (husbands and wives and small children to accompany their mothers) selected for ANGeL were allowed to participate in the agriculture and nutrition training sessions. Occasionally, when husbands from the selected households were not available, their brothers or fathers were allowed to attend the sessions. Other (non-invited) households/individuals were not allowed to participate in the sessions.

Baseline data were collected between November 2015 and January 2016.[4] Endline data were collected between January and March 2018, ensuring minimal seasonal difference between baseline and endline surveys. In each household, both the primary female beneficiary and primary male beneficiary were interviewed. Although the male and female beneficiaries were interviewed separately, some modules were answered by only the male (e.g., household demographics, assets and wealth, agricultural production), some were answered by only the female (e.g., food consumption and food security, diet data, anthropometry, women's status, and decision-making autonomy), and some were answered separately by each (e.g., data needed to construct measures of empowerment, gender attitudes, time preferences, agency).

In February and March 2022, we sought to re-interview study participants who were in the T-A, T-AN, T-ANG and Control groups.[5] The timing of the survey matched the timing of the 2016 (baseline) and 2018 (endline) surveys, meaning that seasonality considerations should not unduly confound our results.

3.2. Sample and attrition

The baseline sample included 3,994 households residing in 160 clusters. As we did not include the T-N(SAAO) and T-N(CNW) arms in this follow-up study (partly because the impact of these treatment arms had been small, see Ahmed et al. (2023a) and Ahmed et al. (2023b), and partly because the size of the survey budget meant that we had to prioritize those treatment arms where a priori, we would be most likely to observe sustainable effects), the relevant sample for the purposes of our participant flow diagram (Figure 1) are the 2,749 households in the T-A, T-AN, and T-ANG treatment arms and the control group. These households are found in 110 clusters. Between baseline and endline, 71 households attrit, leaving

[4] In the original ANGeL study, we received permission from the Ministry of Agriculture, Government of Bangladesh who issued Letters of Authorization to conduct these surveys. The surveys received ethical approval from the Institutional Review Board of IFPRI, (IRB approval number 00007490). The study was registered on the Registry for International Development Evaluations (RIDIE-STUDY-ID-5afbe43292b4c).

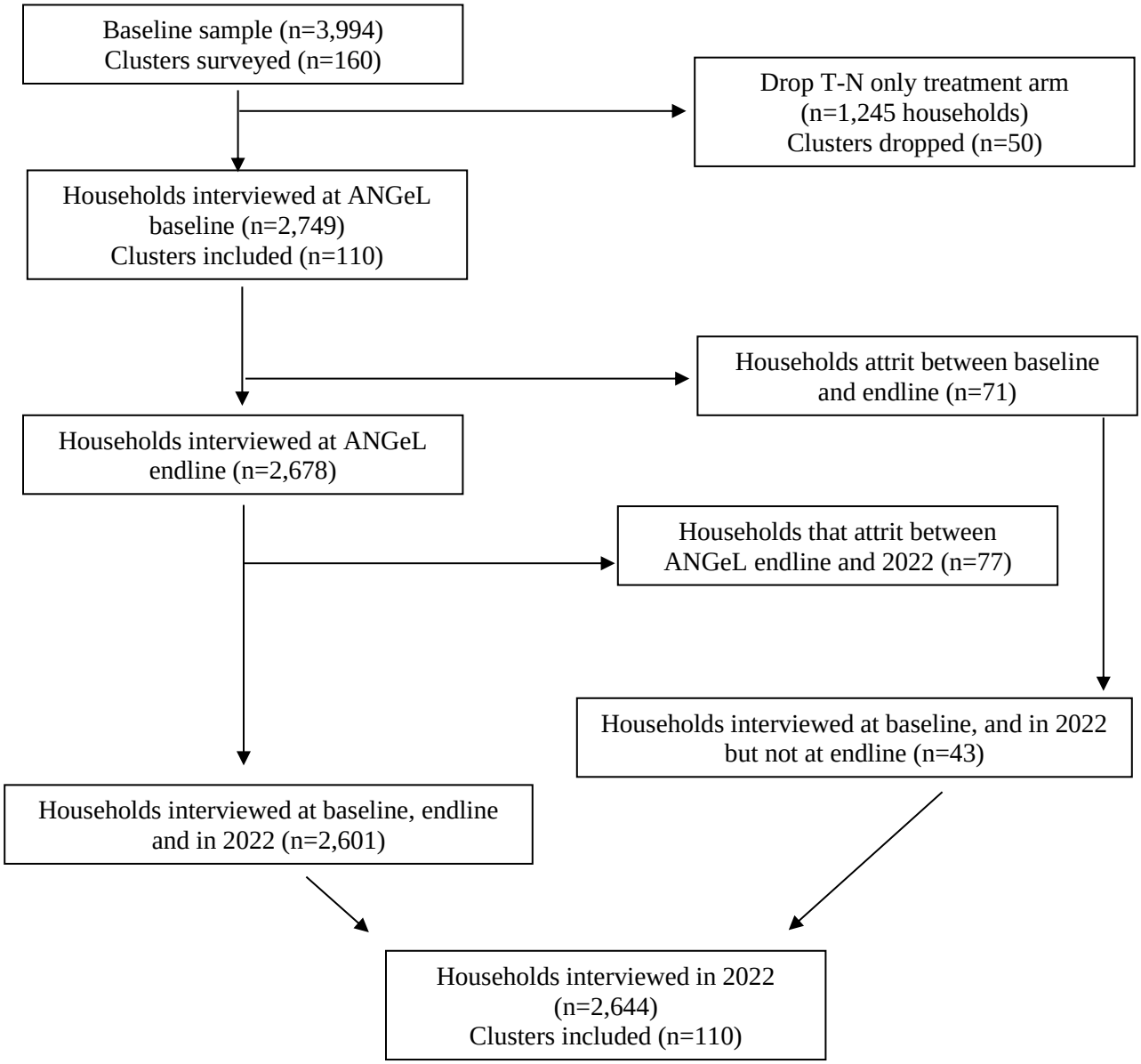
[5] We received permission from the Ministry of Agriculture, Government of Bangladesh who issued Letters of Authorization to conduct the 2022 survey. We received ethical approval from the Institutional Review Board of Cornell University (IRB approval number 2001009312). The study was registered on the Registry for International Development Evaluations (RIDIE-STUDY-ID-626008d5663c8).

2,678 households at endline. A further 77 attrited between the ANGeL endline and the 2022 survey, but we also were able to find and interview 43 households that had been interviewed as part of the ANGeL baseline but not endline. This yields a sample of 2,601 households in 110 clusters who were interviewed in all three rounds and 2,644 households that were interviewed at baseline and in 2022.

Attrition does not appear to differ markedly across treatment groups. We regress attrition status on treatment arms using a Linear Probability Model and clustering at the level of randomization, the block (Table 1), both without and with control variables. There is no indication that attrition over time is correlated with treatment assignment; the magnitudes of the point estimates are small, and none of the point estimates are statistically different from zero.

With the passage of time between the endline survey and the 2022 follow-up, some households split into two (or, much more rarely, three). When this occurred, we collected data on all these split households. However, for the purpose of this analysis, we "follow" the split household containing the individuals who participated in the ANGeL training. In the case of the control households, we "follow" the household containing the individuals who would have taken part in the training had they been included in a treatment group. Of the 2,644 households interviewed in 2022, 7.8 percent (206) are households that had split, and 92.2 percent (2438 households) never split. As was the case with attrition, there is no indication that the likelihood that household splits is correlated with treatment assignment; the magnitudes of the point estimates are small, and none of the point estimates are statistically different from zero (results available on request).

Figure 1. ANGeL2, Participant Flow Diagram



Source: Authors

Table 1. Correlates of attrition

	(1)	(2)
	Treatment status only	Treatment status and control variables
Treatment		
Agriculture (A)	0.006	0.003
	(0.014)	(0.010)
Agriculture & Nutrition (AN)	-0.013	-0.010
	(0.014)	(0.010)
Agriculture, Nutrition and Gender (ANG)	-0.012	-0.006
	(0.015)	(0.012)
R-squared	0.002	0.051
Sample size	2,749	2,749

Notes: Estimates are linear probability models where the dependent variable equals one if the household attrited between baseline and 2022. Standard errors adjusted for clustering at block level are in parentheses. *p<.10; **p<.05; ***p<.01. All specifications include as independent variables the treatment indicators. Column (2) also includes the following control variables: age and sex of household head, mean education levels of males and females 18 and older, number of adults, dependency ratio, wealth index, land owned at baseline, fishpond owned at baseline, baseline access to information as measured by (baseline) number of mobile phones owned, ownership of television, received extension visit for crop production, received extension visit for livestock or fish production, household has access to electricity, and baseline *upazila*.
Source: Authors

3.3 Statistical methods

Our principal approach to evaluating the impact of ANGeL on metrics of household resilience and welfare takes advantage of the RCT design of the intervention. We estimate intent-to-treat (ITT) impacts. Where we have baseline values for our outcomes of interest, we use an ANCOVA specification (McKenzie 2012):

$$Y_{ibt} = \alpha_t + \beta_Y Y_{ibt-1} + \beta_A TA_b + \beta_{AN} TAN_b + \beta_{ANG} TANG_b + \beta_X X_{ibt-1} + \varepsilon_{ibt} \qquad (1)$$

where Y_{ibt} is the outcome of interest for individual *i* residing in block *b* at time *t*; Y_{ibt-1} is the outcome in the prior period (usually baseline); TA_b, TAN_b, and $TANG_b$ are dummy variables that take the value of 1 if block *b* was assigned to T-A, T-AN, and T-ANG, respectively, and takes the value of 0 otherwise; X_{ibt-1} is a vector of baseline covariates; and ε_{ibt} is an error term. β_A, β_{AN}, and β_{ANG} represent the impact estimates for T-A, T-AN, and T-ANG, respectively.

A few outcomes were not measured at baseline. For these, we estimate equation (2):

$$Y_{ib} = \alpha + \beta_A TA_b + \beta_{AN} TAN_b + \beta_{ANG} TANG_b + \beta_X X_{ibt-1} + \varepsilon_i \quad (2)$$

All models include the following baseline covariates, intended to capture demographic and socioeconomic characteristics, human capital, land and labor availability, as well as access to information prior to intervention: age of household head, sex of household head, mean education level of males age 18 and older, mean education level of females age 18 and older, number of adults in the household, dependency ratio, wealth index, whether the household had access to electricity, amount of land owned at baseline, whether any fishponds were owned at baseline, the number of mobile phones owned, whether the household owned a television, whether the household had recently received an extension visit for crop production, whether the household had recently received an extension visit for livestock or fish production, and dummies for baseline *upazila* (the geographic unit above the unit of randomization). We also include a dummy variable if the household reported being adversely affected by the widespread flooding that occurred in Bangladesh in the 12-month period prior to the endline survey.

Means and standard deviations of the baseline covariates are presented in Table 2.

Table 2. Means and standard deviations of baseline covariates, by treatment arm

Covariates	Control		T-A		T-AN		T-ANG	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age of household head	41.37	13.84	40.80	13.49	41.77	14.30	41.11	13.79
Female-headed household	0.03	0.17	0.04	0.19	0.04	0.20	0.05	0.22
Average years of education of male (18+)	4.79	3.88	4.68	4.03	4.19	3.68	4.73	3.57
Average years of education of female (18+)	5.17	2.87	5.38	2.99	4.54	2.75	5.15	2.68
Number of adults (>=18 years) in HH at baseline	3.21	1.44	3.09	1.40	3.26	1.54	3.16	1.41
Dependency ratio	1.00	0.62	0.96	0.57	1.03	0.65	0.96	0.59
Wealth index	0.30	2.50	0.37	2.51	0.02	2.50	-0.08	2.38
HH has a cultivable pond (including rented/mortgaged/leased out ponds)	0.28	0.45	0.29	0.45	0.21	0.41	0.20	0.40
Total operated land of HH (acres)	1.08	1.09	1.15	1.24	1.09	1.19	0.96	0.80
Number of mobile phones owned	1.65	1.25	1.66	1.12	1.75	1.22	1.70	1.15
Ownership of television	0.36	0.48	0.34	0.48	0.33	0.47	0.32	0.47
Received extension visit for crop production	0.20	0.40	0.25	0.43	0.22	0.42	0.19	0.39
Received extension visit for livestock or fish production	0.06	0.25	0.06	0.24	0.04	0.20	0.02	0.15
Household has access to electricity	0.76	0.43	0.73	0.44	0.73	0.45	0.79	0.41
Number of observations	825		583		601		592	

Source: Authors

Unless otherwise noted, we estimate ordinary-least-squares regressions for all outcome variables, including those where outcomes are dichotomous (i.e., linear probability models). Standard errors are clustered at the block level, which is the level at which the randomization was conducted (Abadie et al. 2023). For each outcome, we conduct Wald tests to assess whether the difference in impacts estimated from various treatment arms are statistically significant. Specifically, we assess whether T-A = T-AN; T-A = T-ANG; and T-AN = T-ANG. These comparisons allow us to infer how combined interventions compare with the single intervention (T-A); and how adding gender sensitization to the combined agriculture and nutrition intervention changes impacts.

4. Results

4.1 Exposure to Cyclone Fani and to Covid-19

We begin by describing the extent to which households in the ANGeL study were exposed to two shocks, Cyclone Fani and Covid-19.

In May 2019, northeast India and Bangladesh were struck by Cyclone Fani. India suffered most from this storm as it initially made landfall in the Odisha state. Fani then travelled east and north, through Bangladesh. While wind speeds diminished as Fani moved northwards, gusts of up to 70km/hr were reported and there was heavy rain and widespread flooding. Four of the 16 districts (25 percent) included in the ANGeL study reported damage, primarily to cropland and housing. The Ministry of Disaster Management and Relief estimated that 13,000 houses were damaged nationwide, while the Ministry of Agriculture estimated that around 63,000 hectares of crops were affected in 35 districts. *Boro* rice—among the leading crops cultivated by the ANGeL participants—was the most affected (Daily Star 2019).

Our survey instrument contained a short module designed to elicit information on the effects of Cyclone Fani on assets and income. *However*, only 12 percent of households surveyed in all rounds (baseline, endline, 2022) reported any damage – mostly crop losses or damage to housing. There was relatively little loss of livestock or productive assets, see Table 3. It is possible that with the passage of time, and with the Covid-19 pandemic dominating respondents' memories, these are under-reports of damage caused by the cyclone. That said, we asked very specific questions (for example, about damage to housing) so as to prod respondents' memories and so the low percentages of households reporting any adverse impacts may well be correct.

Table 3. Impact of Cyclone Fani

Conditional on reporting any damage from Cyclone Fani, household reported:	Percent
Crop Loss	73.6
House Damaged	46.4
House Destroyed	18.8
Livestock Loss	6.4
Agricultural equipment damaged or destroyed	6.4
Equipment used in non-agricultural enterprises damaged or destroyed	5.2
Number of households reporting damage from Cyclone Fani	330

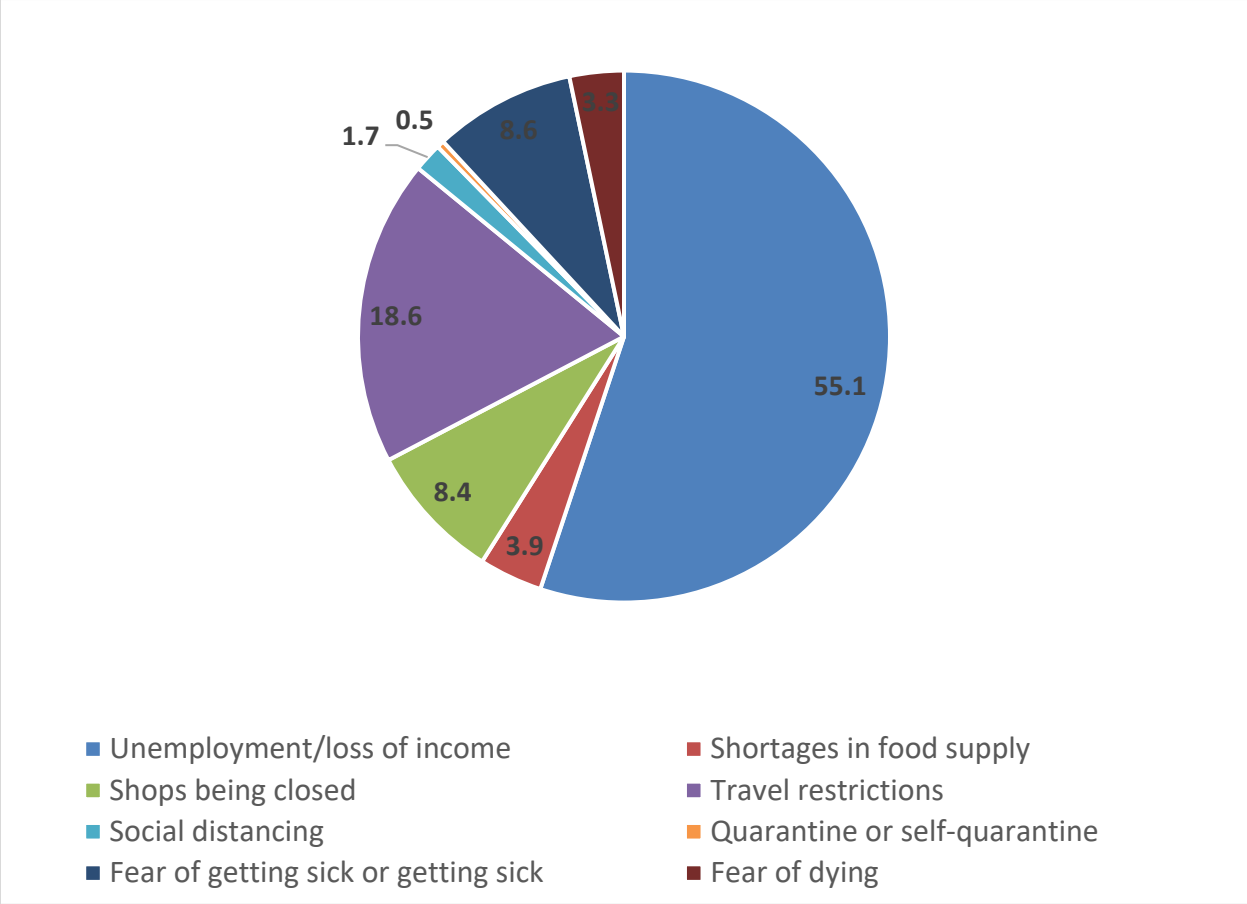
Note: Percentages do not add up to 100 because households may report multiple types of damage
Source: Authors

In Bangladesh, the pandemic took the form of two waves. The first, from March to June 2020, led to widespread lockdowns and significant restrictions on movement and on economic activity. The second wave, from March to August 2021, lasted longer and was accompanied by the delta variant of the virus, which affected south Asia particularly badly. There were intermittent lockdowns in Bangladesh, but these were not as severe as those that accompanied the first wave. By late 2021, the delta variant had run its course and lockdowns were largely ended. Results from phone surveys conducted in both rural and urban areas of Bangladesh during the pandemic showed that moderate-to-severe food insecurity increased dramatically in both rural and urban areas in June 2020 relative to one year before the pandemic, then declined to roughly pre-pandemic levels in January 2021 and September-October 2021; however, mild food insecurity remained much higher than in pre-pandemic times (Ahmed et al, 2023).

Our survey instrument included a module on Covid-19; whether respondents were aware of the pandemic and how it had affected them. All households in our sample had heard of Covid-19, 2.5 percent reported that at least one household member had been diagnosed with Covid-19, and 0.50% reported that a household member had died from Covid-19. But when asked “Which aspect of the Corona crisis had the greatest impact on you and your household?”, overwhelmingly, respondents described an economic

impact (see Figure 2), with 55.2 percent reporting unemployment or loss of income. Direct health effects, or fear of health effects were reported by less than 15 percent of respondents.

Figure 2. Aspect of the Covid-19 crisis that households reported as having the greatest impact



Source: Authors

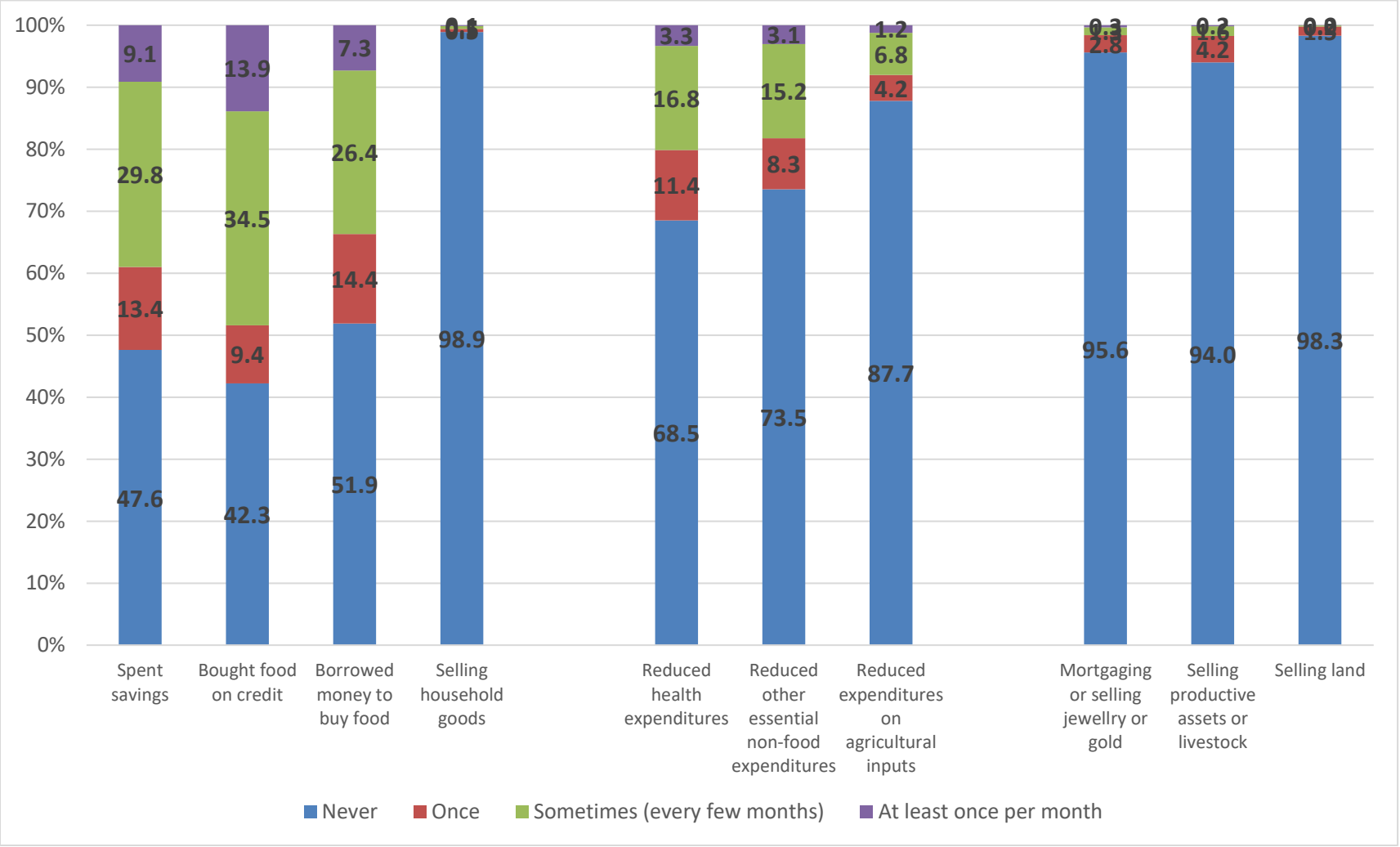
The implications of all this are the following. Our data on outcomes include outcomes (usually) measured in 2018 and 2022. We do not have data on outcomes that occur between these dates and, as such, we do not (for the most part, at least at this point in our analysis) distinguish between the malign effects of Cyclone Fani and the Covid-19 pandemic. That said, it is not unreasonable to argue that the major shock that affected our respondents over this period was the pandemic.

4.2 Coping with the Covid-19 pandemic

We assessed the immediate impacts of the Covid-19 pandemic by fielding an adapted version of the Livelihoods Coping Strategy Index (LCSI) (World Food Programme (WFP), 2023). The LCSI is designed to elicit how households respond to a shock. Behaviors are categorized across three levels: "Stress" - actions that increase access to food but reduce households' ability to deal with future shocks because of a reduction in current resources or an increase in debts; "Crisis" – actions that reduce future incomes or productivity, including human capital; and "Emergency" – actions that may generate increased access to food in the short term but are extremely difficult to reverse (WFP, 2023). More specifically, we asked the following question. "We would now like to ask whether, and how often, members of your household have to engage in any of the following behaviors due to a lack of food or a lack of money to buy food or meet other basic needs since the start of the coronavirus (Covid 19) pandemic in March 2020?" Behaviors were: "Stress" – spent savings; borrowed food on credit; borrowed money to buy food; selling household goods (radio, furniture, mobile, solar panel); "Crisis" - reduced health expenditures; reduced other essential non-food expenditures such as education, clothing; reduced expenses on agricultural, livestock or fisheries inputs; and "Emergency" - mortgaging or selling jewelry/gold; selling productive assets or means of transport (including livestock); selling land. Respondents were asked about the frequency with which they undertook these actions: Never; Once; Sometimes (every few months); Often (once a month); or Very frequently (more than once per month).

Figure 3 shows the use of Livelihood Coping Strategies, by strategy and frequency of use. Three "Crisis" strategies were used by many participants: spending savings; buying food on credit; or borrowing money to buy foods. Between 48 and 58 percent of respondents reported using one of these strategies and some, such as buying food on credit, were used every few months. Approximately 32 percent of households also reported reducing expenditures on health-related items and another 27 percent reduced expenditures on other essential non-food items. Only around 12 percent of households reduced expenditures on agricultural inputs and there was little evidence of households selling assets, either household goods, productive assets, livestock, jewellery, or land.

Figure 3. Use of Livelihood Coping Strategies, by strategy and frequency of use



Source: Authors

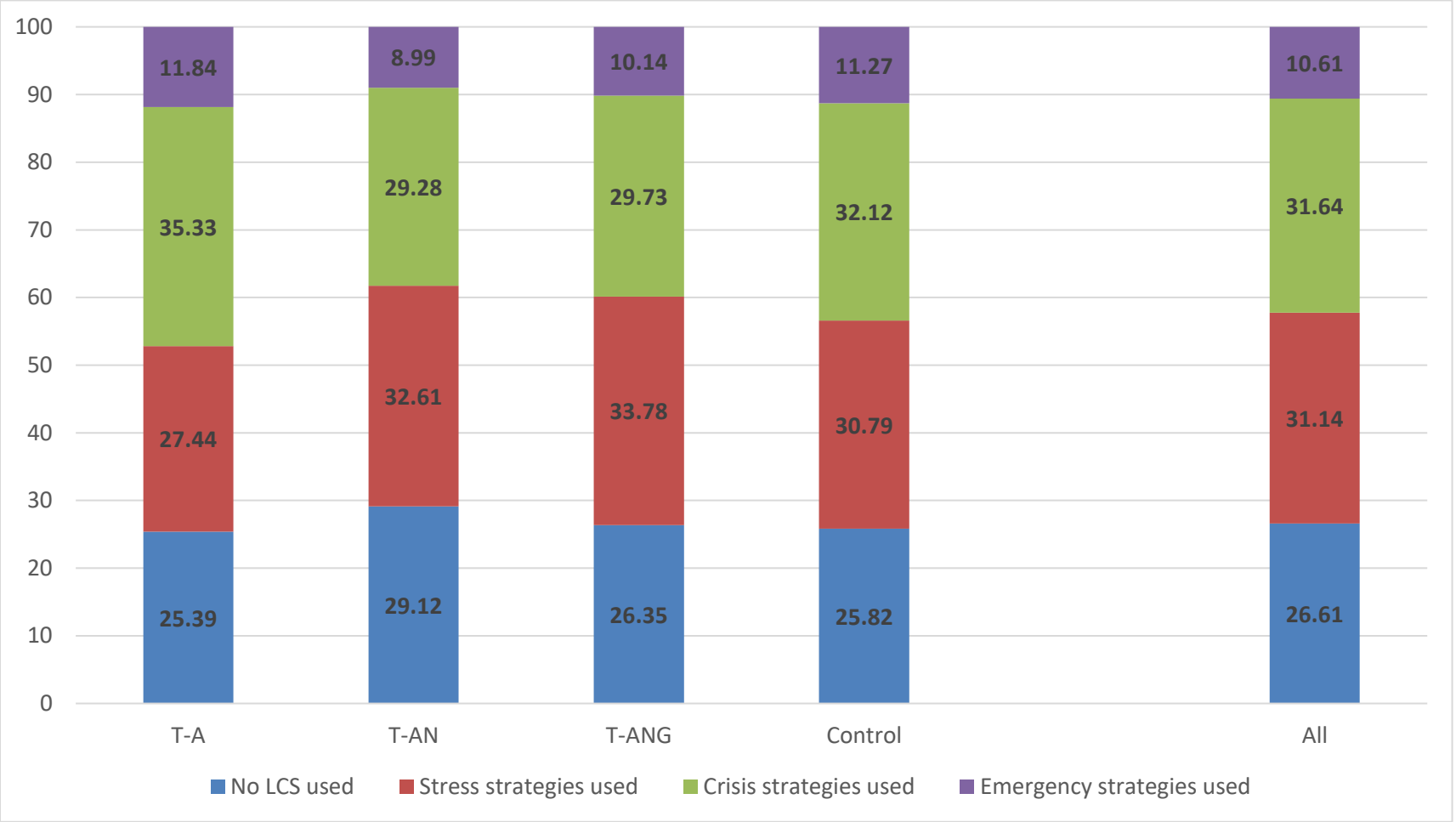
Figure 4 provides descriptive statistics on the most severe coping strategy used (stress, crisis, emergency) for the full sample and by treatment type. Across the full sample, 10.5 percent of households used an emergency strategy, 31.7 percent used a crisis strategy (but not an emergency strategy), 31.2 percent used a stress strategy (but not a crisis or emergency strategy) and 26.6 percent did not use any livelihood coping strategy. When we disaggregate by treatment group, we see little difference between usage of these strategies when we compare the T-A treatment group and the control group (the percentage of households in the T-A arm and the control arm reporting that they do not use any coping strategy is 25.39 and 25.82 percent, respectively). There is some suggestive evidence that, relative to the control group, the T-AN treatment group was less likely to use any coping strategy (29.12 percent of households in the T-AN treatment reported using no coping strategy, compared to 25.82 percent of those in the control group) and both T-AN and T-ANG treatment groups were less likely to use more severe strategies relative to the control group (8.99 percent and 10.14 percent, respectively, compared to 11.27 percent in the control group).

In light of these descriptive results, we estimated a single difference model to see whether pre-Covid treatment status affected the likelihood of using these coping strategies. Given the ordered nature of these (None, Stress, Crisis, Emergency) we estimate an ordered probit model with higher values denoting the use of a more extreme coping strategy. In addition to our treatment variables, we include the covariates described in section 3.3.

Results are reported in Table 4. This shows that both the T-AN and T-ANG treatment arms reduced the use of Livelihood Coping Strategies but T-A did not. We can reject the null that the impacts of T-A and T-AN, and T-A and T-ANG, are equal. We do not reject the null that T-AN and T-ANG have equal effects. A limitation of Table 4 is that the ordered probit parameter estimates are not directly interpretable. In Table 5, we convert these to marginal effects. These show that the T-AN and T-ANG increased the likelihood that households did not need to rely on these coping strategies during the Covid-19 pandemic by approximately five percentage points and they reduced the likelihood of using the more severe coping behaviours, crisis and emergency, by approximately three percentage points each. These

findings are consistent with the notion that the pre-Covid 19 treatment arms that combined both agricultural and nutrition training (with or without gender sensitization) enabled households to be more resilient to the malign effects of the pandemic.

Figure 4. Most severe coping strategy used by treatment group



Source: Authors

Table 4. Ordered probit estimates of the impact of ANGeL on use of Livelihood Coping Strategies

	(1)	(2)
Treatment		
Agriculture (T-A)	0.060	0.060
	(0.07)	(0.07)
Agriculture & Nutrition (T-AN)	-0.174**	
	(0.07)	
Agriculture, Nutrition and Gender (T-ANG)	-0.157*	
	(0.08)	
T-AN or T-ANG		-0.166**
		(0.07)
P values, equality of treatments		
T-A = T-AN	<0.01	
T-A = T-ANG	<0.01	
T-AN = T-ANG	0.83	
T-A = (T-AN or T-ANG)		<0.01
Number of observations	2,601	2,601

Notes. * p < 0.10; ** p <0.05; *** p < 0.01. All specifications include as independent variables the treatment indicators and the following baseline control variables: age and sex of household head, mean education levels of males and females 18 and older, number of adults, dependency ratio, wealth index, land owned, fishpond owned, access to information as measured by (baseline) number of mobile phones owned, ownership of television, received extension visit for crop production, received extension visit for livestock or fish production, household has access to electricity, and baseline *upazila*.
Source: Authors

Table 5. Marginal effects of ANGeL treatment arms on the use of Livelihood Coping Strategies

Treatment		None	Stress	Crisis	Emergency
T-A	Marginal effect	-0.018	-0.003	0.011	0.010
	Standard error	0.022	0.004	0.014	0.012
T-AN	Marginal effect	0.051	0.010	-0.032	-0.029
	Standard error	0.022	0.004	0.014	0.013
T-ANG	Marginal effect	0.046	0.009	-0.029	-0.026
	Standard error	0.024	0.005	0.015	0.014

Treatment		None	Stress	Crisis	Emergency
T-A	Marginal effect	-0.018	-0.003	0.011	0.010
	Standard error	0.022	0.004	0.014	0.012
T-AN or T-ANG	Marginal effect	0.049	0.010	-0.031	-0.028
	Standard error	0.020	0.004	0.012	0.011

Notes. Marginal effects are generated from results reported in Table 3.
Source: Authors

4.3 Impacts on consumption

Next, we turn to impacts on household consumption. We do so for several reasons. First, consumption is a measure of household welfare. Second, we observe it both before and after the pandemic, allowing us to assess the extent to which different ANGeL treatment arms did (or did not) allow households to maintain consumption levels in the aftermath of the pandemic. Third, the questions on Livelihood Coping Strategies required respondents to recollect events over the previous, event-filled, several years leading to concerns about the accuracy of recall.

In the ANGeL data set, consumption is measured as the sum of the value of total food consumption and total nonfood (nondurable and durable) expenses. The value of food consumption is calculated based on a seven-day recall module that asked questions about the quantity of food purchased, price of purchased food, quantity consumed from home production, food received from other sources. Quantities consumed out of own production and food received from other sources are valued using price data collected at the village level at the same time as the household survey. The food consumption module was exhaustive in scope, covering 321 food items. Non-food expenses are sub-divided into two categories. A one-month recall period was used for items that are purchased with some frequency: fuel and lighting; washing, cleaning and cosmetics; transport/travel. Items that were purchased with less frequency, or items whose purchases tended to be lumpy were asked with a one-year recall: clothing and footwear (male and female); housing expenses; medical expenses (male and female); education expenses (male and female); remittances sent, gifts, ceremonies; recreation and leisure; taxes, fees, insurance; cooking equipment; furniture and appliances; jewelry; and other household durables. The non-food consumption module included 225 items in total. The total value of food consumption and non-food expenses is converted to monthly values, then divided by the number of household members to obtain consumption expenditure per person per month. Note that: (a) lumpy and infrequent expenditures such as dowry, wedding costs, pilgrimage, etc. are excluded from these aggregates; (b) identical food and non-food consumption modules were used in all survey years, as were the methods for producing the

consumption aggregates; and (c) we use the rural general consumer price index (RGCPI) estimated by the Bangladesh Bureau of Statistics (BBS), to deflate all values to 2016 (baseline) Taka.

We begin with some descriptive statistics. Table 6 shows median monthly per capita consumption, by round, treatment group and region, expressed in 2016 Taka. We start with households in the control group. In real terms, median per capita consumption grew by 9.2 percent between 2016 and 2018, 14.4 percent between 2018 and 2022 with the result that real median per capita consumption was 24.9 percent higher in 2022 than it was at the time of the baseline survey in 2016. An implication of these changes is that increases in consumption levels in households that received support from ANGeL are not in themselves evidence of sustained impacts; evidence of sustainability comes from growth rates in consumption that are higher than those observed in the control groups.

Table 6. Median monthly real (2016) per capita consumption, by round, treatment group

	Median monthly per capita consumption, 2016 Taka			Real household per capita consumption declined by more than 5% (percent)
	Baseline (2016)	Endline (2018)	Four year post-program (2022)	Between 2018 and 2022
Control	2707	2955	3420	31.3
Agriculture	2748	3022	3492	33.3
Agriculture & Nutrition	2751	3396	3647	35.5
Agriculture, Nutrition & Gender	2772	3146	3615	31.8
All households	2741	3120	3547	32.8

Notes: Sample consists of households surveyed in 2016, 2017 and 2022. Sample size: 2,601.
Source: Authors

Table 6 shows that real per capita median consumption in the T-A treatment group grew by 10.0 percent between 2016 and 2018, 15.0 percent in the four years after ANGeL ended and was 26.4 percent higher in 2022 than it was at baseline. Consumption growth was faster in the arms that included both agriculture and nutrition training, 22.9 and 13.5 percent between 2016 and 2018, and 7.7 and 15.6 percent

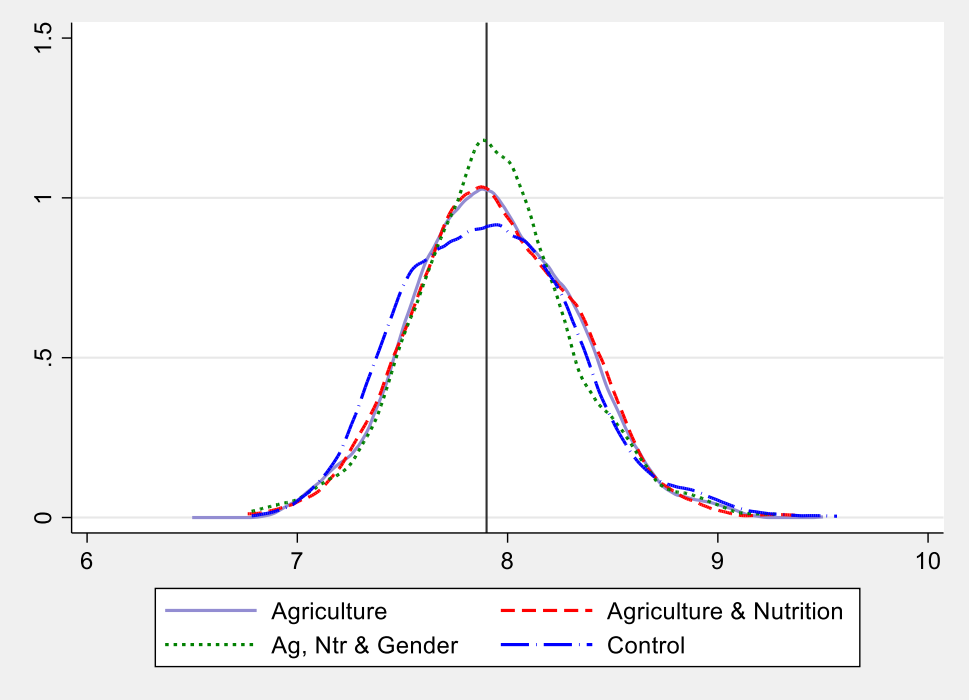
between 2018 and 2022, in T-AN and T-ANG respectively. In 2022, real per capita median consumption in the T-AN and T-ANG was, respectively, 32.4 and 31.2 percent higher than at baseline. This suggests two preliminary findings: (a) That the T-AN and T-ANG treatment arms had sustained impacts on real median per capita consumption – not only have these increased since 2016, but their values are also larger than that observed in the control group; and (b) real median per capita consumption is higher in 2022 (post-pandemic) than it was in 2018 (pre-pandemic).

That said, growth in consumption will have minimal effects on other welfare indicators if they are concentrated among better off households. With this in mind, we graph kernel density functions of log per capita monthly consumption (in 2016 Taka) by treatment group and survey round. We superimpose on these graphs the median log of real per capita monthly consumption for the control group at the time of the baseline survey in 2016.

Figure 5 shows that in 2016, at baseline, there is little difference in the distributions of consumption by treatment group, a result formally confirmed by the Exact Kolmogorov–Smirnov test of equality of distributions for all pairwise comparisons except for T-ANG and the control group (the former being somewhat more leptokurtic). In 2018, consumption in all treatment groups had shifted rightwards and we reject the null hypotheses that the distributions of each treatment group are equal to the Control group.

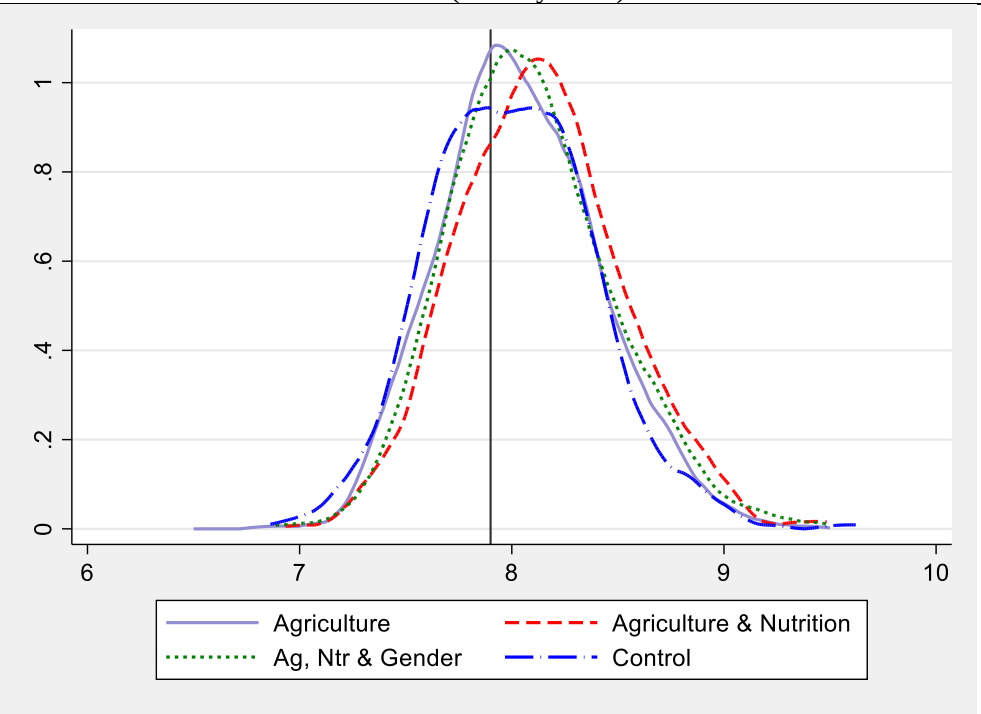
Figure 5. Density plots of log real per capita consumption, by survey round and treatment group

Baseline (January 2016)



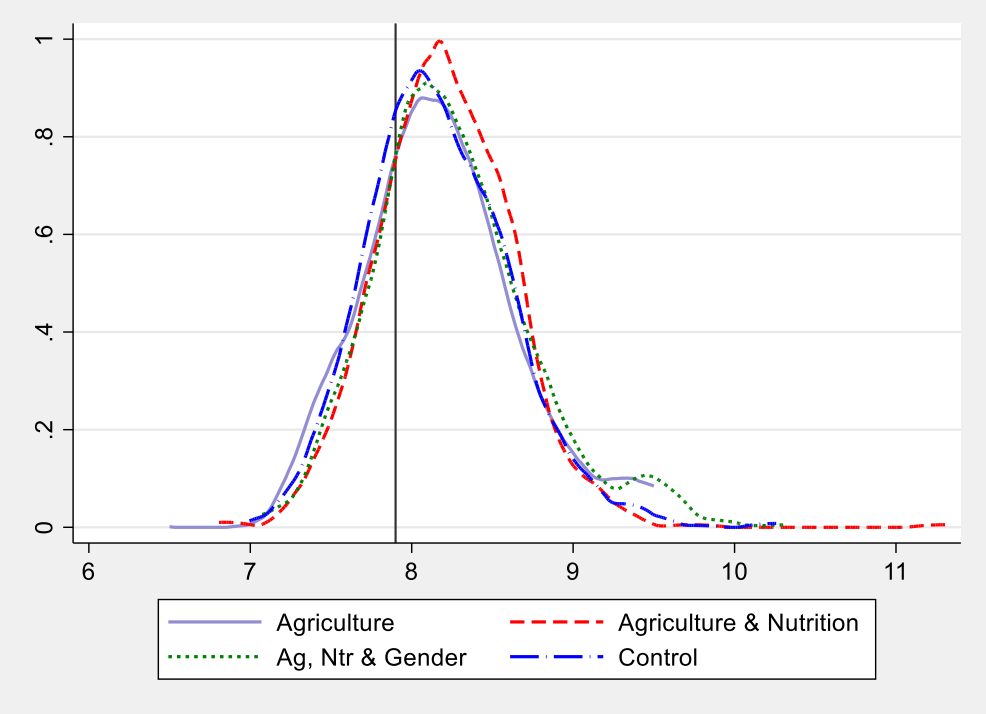
Exact Kolmogorov–Smirnov test of equality of distributions	P value
Agriculture = Agriculture & Nutrition	0.99
Agriculture = Agriculture, Nutrition & Gender	0.24
Agriculture & Nutrition = Agriculture, Nutrition & Gender	0.24
Agriculture = Control	0.23
Agriculture & Nutrition = Control	0.14
Agriculture, Nutrition & Gender = Control	0.04

Endline (January 2018)



Exact Kolmogorov–Smirnov test of equality of distributions	P value
Agriculture = Agriculture & Nutrition	<0.01
Agriculture = Agriculture, Nutrition & Gender	0.22
Agriculture & Nutrition = Agriculture, Nutrition & Gender	0.02
Agriculture = Control	0.04
Agriculture & Nutrition = Control	<0.01
Agriculture, Nutrition & Gender = Control	<0.01

Four year post-program follow-up (February 2022)



Exact Kolmogorov–Smirnov test of equality of distributions	P value
Agriculture = Agriculture & Nutrition	0.12
Agriculture = Agriculture, Nutrition & Gender	0.25
Agriculture & Nutrition = Agriculture, Nutrition & Gender	0.09
Agriculture = Control	0.33
Agriculture & Nutrition = Control	0.03
Agriculture, Nutrition & Gender = Control	0.03

Notes: Sample consists of households surveyed in 2016, 2017 and 2022. Sample size: 2,601. Vertical rule at 7.90 is median baseline consumption of the control group.
Source: Authors

Table 7 provides evidence of the impacts on treatment arms, relative to the Control group, by survey round. Columns (1) and (2) provide background and context. The outcome variable is the log real monthly per capita consumption in 2018 (endline). This shows that all three treatment groups increased real per capita consumption relative to the control group with the effect sizes larger (an approximately 10 percent increase) for the treatment groups that included both agricultural and nutrition training (T-AN and T-ANG) than the treatment that only included agriculture (a 3.5 percent increase). Four years after the ANGeL intervention ended, and a year after the worst effects of the pandemic were over, the agriculture only intervention no longer had a statistically significant impact on consumption. By contrast, in 2022,

real mean per capita consumption was 4.9 percent higher in the T-AN treatment group, relative to the control group and it was 9.2 percent higher, relative to the control group, in T-ANG; the latter is significant at the 10 percent level. If we combine the two treatment arms that included both agriculture and nutrition training (without and with gender sensitization), we see that these raise real per capita consumption by 6.8 percent relative to the control group. This impact estimate is statistically significant. Put differently, columns (3) and (4) show that treatments that combined training in both nutrition and agriculture led to, relative to the control group, sustained increases in real per capita consumption.

We wondered if treatment arms reduced the likelihood that real per capital consumption fell between 2018 and 2022. Columns (5) and (6) show that T-A had no effect on the likelihood that real per capita consumption fell by more than five percent over this period.[6] By contrast, the coefficients for T-AN and T-ANG are negative, indicating that these pre-pandemic treatment arms reduced the likelihood that consumption fell between 2018 and 2022 by approximately five percent (see columns 5 and 6).

[6] We chose the five percent cut-off to minimize the impact of random measurement error on the likelihood that consumption fell over this period.

Table 7. Impact of ANGeL on consumption levels and changes, endline and 2022

	(1)	(2)	(3)	(4)	(5)	(6)
	Log real per capita consumption, endline		Log real per capita consumption, 2022		Real household per capita consumption declined by more than 5%, 2018 to 2022	
Agriculture (T-A)	0.036**	0.036**	0.030	0.030	-0.005	-0.005
	(0.02)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)
Agriculture & Nutrition (T-AN)	0.102***		0.049		-0.055*	
	(0.02)		(0.03)		(0.03)	
Agriculture, Nutrition and Gender (T-ANG)	0.107***		0.092*		-0.045	
	(0.02)		(0.05)		(0.03)	
Agriculture & Nutrition or Agriculture, Nutrition & Gender		0.104***		0.068**		-0.051**
		(0.02)		(0.03)		(0.02)
P values, equality of treatments						
T-A = T-AN	<0.01		0.70		0.14	
T-A = T-ANG	<0.01		0.36		0.33	
T-AN = T-ANG	0.84		0.49		0.79	
T-A = (T-AN or T-ANG)		<0.01		0.45		0.15
Observations	2,601	2,601	2,601	2,601	2,601	2,601
R-squared	0.452	0.452	0.241	0.240	0.190	0.190

Notes. * p < 0.10; ** p <0.05; *** p < 0.01. All specifications include as independent variables the treatment indicators and the following baseline control variables: age and sex of household head, mean education levels of males and females 18 and older, number of adults, dependency ratio, wealth index, land owned, fishpond owned, access to information as measured by (baseline) number of mobile phones owned, ownership of television, received extension visit for crop production, received extension visit for livestock or fish production, household has access to electricity, and baseline *upazila*. Columns (1)-(4) control for baseline log per capita consumption. Columns (5)-(6) control for endline log real per capita consumption.
Source: Authors

4.4 Impacts on diets

Next, we turn attention to impacts on diets. We consider two measures: a quantity measure – per capita caloric availability; and a quality measure – the household Global Diet Quality Score (hGDQS).

First, we extract the quantities of food available for consumption from our detailed food consumption module (with a recall period of the last seven days). Using food composition tables specific to Bangladesh, we convert these quantities to calories, divide by household size and then by seven. This gives us daily per capita caloric availability.

Next, we construct the household Global Diet Quality Score (hGDQS). This is an adaptation of a recently developed indicator of diet quality (Bromage et al. 2021). Unlike other measures of diet quality, the Global Diet Quality Score (GDQS) is designed to be sensitive to diet-related outcomes associated with both undernutrition and overnutrition. The GDQS consists of 25 food groups: 16 healthy food groups, 7 unhealthy food groups, and 2 food groups (red meat, high-fat dairy) that are unhealthy when consumed in excessive amounts. For 24 food groups, three ranges of quantity of consumption are defined (in grams/day) and used in scoring the metric: low, medium, and high. For the final group, high-fat dairy, four ranges of quantity of consumption are used: low, medium, high, and very high. The points associated with the healthy GDQS food groups increase for each higher quantity of consumption category. The points associated with the unhealthy GDQS food groups decrease for each higher quantity of consumption category. For the two food groups that are unhealthy in excessive consumption (red meat, high-fat dairy), the points associated with the GDQS food group increase up to a certain threshold of quantity of consumption, then decrease. The overall GDQS is the sum of points across all 25 GDQS food groups. GDQS scores ≥23 are associated with a low risk of both nutrient adequacy, scores ≥15 and <23 indicate moderate risk, and scores below 15 indicate high risk (Bromage et al. 2021).

GDQS is defined at the individual-level, with points being given for each GDQS food group, according to the quantity of consumption consumed for that food group during the 24-hour reference period. Because our analysis of ANGeL is at the household level, and our household-level food consumption data are based on 7-day recall, we construct a variation of the GDQS at the household level, the household-level GDQS (hGDQS). We calculate household consumption of these 25 food groups, converting these quantities into daily per adult equivalent amounts and apply the scoring method described above.[7] This gives a hGDQS score that ranges 0 to 49.

[7] Our household-level calculations of hGDQS may not be directly comparable to the GDQS calculated at the individual-level using 24-hour recall food intake data. However, because we construct hGDQS in a consistent manner across all intervention arms in this study, this should not introduce bias for assessing treatment impacts.

Table 8 shows that at ANGeL endline, the T-AN and T-ANG treatment arms increased log per capita caloric availability by 3.7 and 6.0 percent. But four years later, both treatments no longer have a statistically significant impact on this diet measure when included individually (column 2) or jointly (column 3). However, we disaggregated our sample across two dimensions: land holdings (<0.54 cultivable acres, the bottom third of the land distribution; >1.06 cultivable acres, the top third of the land distribution); and whether the household had a homestead garden at baseline. When we restricted our sample to households with little cultivable land but also had homestead gardens at baseline, we find that the T-AN and T-ANG treatment arms increased per capita caloric availability by approximately seven percent. By contrast, there is no impact of these treatment arms on per capita caloric availability for households in the highest tercile of pre-intervention cultivable land.

At endline, all treatment arms increased log hGDQS (column 4), with the impacts ranging from 3.7 percent (T-A) to 8.3 percent (T-ANG). We can reject the null that the impacts of T-A and T-AN, and T-AN and T-ANG, are equal. Four years post-program, T-AN and T-ANG continue to have a statistically significant impact on hGDQS (column 5), but the effect size is attenuated (4.4 and 2.8 percent respectively). T-A no longer has a statistically significant impact on this outcome. When we combine these treatment arms (column 6), we can reject the null that their effect size, a 3.7 percent increase, is equal to the impact of T-A. These effect sizes seem small. However, when we again restrict our sample to households with little cultivable land but also had homestead gardens at baseline, we find that the T-AN and T-ANG treatment arms increased hGDQS by approximately 10 percent.

Table 8. Impact of ANGeL on caloric availability and household GDQS

	(1)	(2)	(3)	(4)	(5)	(6)
	Log per capita caloric availability			Log hGDQS		
	Endline (2018)	2022	2022	Endline (2018)	2022	2022
Agriculture (T-A)	0.012	-0.018	-0.020	0.037***	0.006	0.006
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Agriculture & Nutrition (T-AN)	0.039**	0.022		0.075***	0.044***	
	(0.02)	(0.02)		(0.01)	(0.01)	
Agriculture, Nutrition and Gender (T-ANG)	0.059***	0.015		0.083***	0.028**	
	(0.02)	(0.02)		(0.01)	(0.01)	
Agriculture & Nutrition or Agriculture, Nutrition & Gender			0.02			0.037***
			(0.02)			(0.01)
P values, equality of treatments						
T-A = T-AN	0.13	0.07		<0.01	<0.01	
T-A = T-ANG	0.01	0.13		<0.01	0.10	
T-AN = T-ANG	0.28	0.74		0.58	0.15	
T-A = (T-AN or T-ANG)			0.05			<0.01
Observations	2,598	2,598	2,598	2,599	2,599	
R2	0.08	0.07	0.07	0.25	0.21	0.21

Notes. * p < 0.10; ** p <0.05; *** p < 0.01. All specifications include as independent variables the treatment indicators and the following baseline control variables: age and sex of household head, mean education levels of males and females 18 and older, number of adults, dependency ratio, wealth index, land owned, fishpond owned, access to information as measured by (baseline) number of mobile phones owned, ownership of television, received extension visit for crop production, received extension visit for livestock or fish production, household has access to electricity, and baseline *upazila*. Columns (1)-(3) control for baseline per capita caloric availability. Columns (4)-(6) control for baseline household GDQS.
Source: Authors

4.5 Impacts on women's and men's empowerment and gender equality

Our measure of women's empowerment at endline is the pro-WEAI, an additive and decomposable index based on the Alkire-Foster methodology adapted from the WEAI (Alkire et al. 2013) for use in agricultural development projects (Malapit et al. 2019).[8] Pro-WEAI is based on a weighted adequacy count across 10 indicators. The 10 indicators seek to measure three types of agency: intrinsic; instrumental; and collective. The indicators of intrinsic agency comprise: autonomy in income, self-efficacy, and attitudes about IPV against women. Instrumental agency indicators are: input in productive decisions, ownership of land and other assets, access to and decisions on financial services, control over use of income, work balance, and visiting important locations. Finally, the collective agency domain includes one indicator: group membership. For each of these indicators, individuals are classified as adequate or inadequate based on pre-determined thresholds used in the pro-WEAI. The pro-WEAI is composed of the 3DE sub-index (three domains of empowerment, the pro-WEAI analogue of the five domains of empowerment (5DE) in the WEAI), which measures the extent and depth of empowerment, and the Gender Parity sub-index, which measures gender parity between women and men in the same household.

We use: (1) the individual empowerment score, defined as the weighted sum of the 10 pro-WEAI indicators; this score ranges from 0-1; and (2) the individual's empowerment status, which classifies an individual as empowered if his or her empowerment score is greater than or equal to 75% of the weighted sum of the 10 binary pro-WEAI indicators. In addition, we are also interested in gender parity. After calculating an empowerment score for the woman's partner, we assess whether the household has achieved *gender parity* if the woman is empowered, based on the above definition, or if she achieves at least the same empowerment score as her partner; thus, gender parity is a binary indicator at the household level. We set it equal to 0 if parity is attained, 1 if it is not attained.

[8] Note that this description draws heavily on Quisumbing et al (2021). The original version of pro-WEAI used 12 indicators. After subsequent validation and further testing, a revised version includes only 10 indicators (Seymour et al. 2023).

Table 9, Panel A, shows that at endline, all treatment arms increased women's empowerment (as measured by the pro-WEAI) by between 4.4 and 6.2 percentage points (column 1) and increased the likelihood that women were empowered by 7.4 to 11.8 percentage points (column 3). These effects attenuate by 2022, but the attenuation is uneven. The T-ANG treatment arm continues to increase women's empowerment, by 4.8 percentage points while the other two treatment arms have smaller effects (2.5 and 2.2 percentage points respectively for T-A and T-AN). T-ANG has the largest effect on whether women were empowered in 2022 (four years after the intervention ended) while reducing the intra-household inequality score (the gender parity index) by 4.6 points.

Table 9, Panel B, presents estimates of impact on male empowerment. Generally, effect sizes are smaller for men then for women and are less likely to be statistically significant. However, there is no evidence that they are negative, suggesting that the gains to women's empowerment did not come at the cost of disempowering men.

Table 9. Impact of ANGeL on empowerment

Panel A: Women

	(1)	(2)	(3)	(4)	(5)	(6)
	Empowerment Score		Whether empowered		Intra-household inequality score	
	Endline (2018)	2022	Endline (2018)	2022	Endline (2018)	2022
Agriculture (T-A)	0.043***	0.022**	0.069***	0.039**	-0.027**	-0.027**
	(0.01)	(0.01)	(0.03)	(0.02)	(0.01)	(0.01)
Agriculture & Nutrition (T-AN)	0.062***	0.024**	0.100***	0.019	-0.041***	-0.015
	(0.01)	(0.01)	(0.03)	(0.02)	(0.01)	(0.01)
Agriculture, Nutrition and Gender (T-ANG)	0.064***	0.044***	0.108***	0.052***	-0.037**	-0.041***
	(0.01)	(0.01)	(0.03)	(0.02)	(0.01)	(0.01)
P values, equality of treatments						
T-A = T-AN	0.09	0.88	0.29	0.4	0.35	0.36
T-A = T-ANG	0.09	0.06	0.2	0.58	0.46	0.3
T-AN = T-ANG	0.84	0.06	0.79	0.17	0.83	0.06
Observations	2,395	2,570	2,395	2,570	2,183	2,244
R-squared	0.18	0.11	0.13	0.07	0.13	0.13

Panel B: Men

	(1)	(2)	(3)	(4)
	Empowerment Score		Whether empowered	
	Endline (2018)	2022	Endline (2018)	2022
Agriculture (T-A)	0.016**	-0.010	0.028	0.005
	(0.01)	(0.01)	(0.03)	(0.03)
Agriculture & Nutrition (T-AN)	0.019**	0.010	0.037	0.065**
	(0.01)	(0.01)	(0.03)	(0.03)
Agriculture, Nutrition and Gender (T-ANG)	0.030***	0.008	0.084***	0.047
	(0.01)	(0.01)	(0.03)	(0.03)
P values, equality of treatments				
T-A = T-AN	0.67	0.03	0.77	0.09
T-A = T-ANG	0.06	0.05	0.06	0.28
T-AN = T-ANG	0.17	0.81	0.14	0.54
Observations	2,184	2,250	2,184	2,250
R-squared	0.12	0.14	0.10	0.10

Notes. * p < 0.10; ** p <0.05; *** p < 0.01. All specifications include as independent variables the treatment indicators and the following baseline control variables: age and sex of household head, mean education levels of males and females 18 and older, number of adults, dependency ratio, wealth index, land owned, fishpond owned, access to information as measured by (baseline) number of mobile phones owned, ownership of television, received extension visit for crop production, received extension visit for livestock or fish production, household has access to electricity, and baseline *upazila*.

Source: Authors

4.6 Impacts on assets

Lastly, we turn to impacts on assets. In all ANGeL survey rounds, the male respondent was asked questions about consumer durables, agricultural implements, livestock, jewelry, and other assets (e.g., sewing machines, rickshaws, boats, generators). This included questions on whether these were owned, who (within the household) owned them and their current value. We deflate values to 2016 using the rural CPI (just as we did in our analysis of consumption) and log these values.

Results are reported in Tables 10 and 11**.** Key findings are these: (1) At endline, across all asset categories, none of these treatment arms had a statistically significant impact on asset holdings (Table 10, column 1). However, by 2022, the value of all assets held by T-ANG households was approximately 19 percent higher relative to the control group and this effect was statistically significant (Table 10, column 2). Assets held by the T-AN treatment group were nine percent higher compared to the control group, though this effect is less precisely measured. Agriculture training alone (T-A) has no effect on asset accumulation; (2) women's share of assets (defined as assets owned solely by women and ½ the value of assets owned jointly) were not affected by any treatment arm (Table 10, columns 3 and 4). When we disaggregate across selected asset categories, this increase in real asset holdings is driven by accumulation of agricultural implements by T-AN and T-ANG participants (Table 11, columns 1 and 2).

Table 10. Impact of ANGeL on assets and women's share of assets

	(1)	(2)	(3)	(4)
	Log real total value of household assets		Share of women's total assets	
	Endline (2018)	2022	Endline (2018)	2022
Agriculture (T-A)	-0.049	0.048	-0.097	1.407
	(0.06)	(0.05)	(1.23)	(1.85)
Agriculture & Nutrition (T-AN)	0.051	0.089*	1.724	1.181
	(0.06)	(0.05)	(1.31)	(2.06)
Agriculture, Nutrition and Gender (T-ANG)	0.115	0.189***	0.993	-0.383
	(0.07)	(0.05)	(1.40)	(1.76)
Observations	2,601	2,601	2,601	2,601
R-squared	0.26	0.37	0.18	0.14
P values, equality of treatments				
T-A = T-AN	0.11	0.48	0.21	0.92
T-A = T-ANG	0.03	0.01	0.49	0.36
T-AN = T-ANG	0.36	0.08	0.66	0.44

Notes. * p < 0.10; ** p <0.05; *** p < 0.01. All specifications include as independent variables the treatment indicators and the following baseline control variables: age and sex of household head, mean education levels of males and females 18 and older, number of adults, dependency ratio, wealth index, land owned, fishpond owned, access to information as measured by (baseline) number of mobile phones owned, ownership of television, received extension visit for crop production, received extension visit for livestock or fish production, household has access to electricity, and baseline *upazila*.
Source: Authors

Table 11. Impact of ANGeL on assets, by selected asset categories

	(1)	(2)	(3)	(4)	(5)	(6)
	Log real total value of agricultural implements		Log real total value of livestock		Log real total value of other productive assets	
	Endline (2018)	2022	Endline (2018)	2022	Endline (2018)	2022
Agriculture (T-A)	0.140**	0.064	0.118	0.038	-0.047	-0.064
	(0.05)	(0.06)	(0.08)	(0.11)	(0.05)	(0.05)
Agriculture & Nutrition (T-AN)	0.112*	0.149**	0.110	-0.041	-0.033	0.066
	(0.06)	(0.06)	(0.11)	(0.13)	(0.04)	(0.06)
Agriculture, Nutrition and Gender (T-ANG)	0.217***	0.159**	0.380***	0.164	-0.066	-0.019
	(0.06)	(0.07)	(0.12)	(0.14)	(0.05)	(0.06)
Observations	2,601	2,601	2,601	2,601	2,601	2,601
R-squared	0.22	0.22	0.15	0.11	0.04	0.05
P values, equality of treatments						
T-A = T-AN	0.66	0.26	0.94	0.54	0.77	0.04
T-A = T-ANG	0.23	0.22	0.01	0.38	0.68	0.47
T-AN = T-ANG	0.13	0.89	0.04	0.15	0.45	0.15

Notes. * p < 0.10; ** p <0.05; *** p < 0.01. All specifications include as independent variables the treatment indicators and the following baseline control variables: age and sex of household head, mean education levels of males and females 18 and older, number of adults, dependency ratio, wealth index, land owned, fishpond owned, access to information as measured by (baseline) number of mobile phones owned, ownership of television, received extension visit for crop production, received extension visit for livestock or fish production, household has access to electricity, and baseline *upazila*.
Source: Authors

5. Summary and discussion

We address three knowledge gaps: (1) Do agricultural interventions aimed at diversifying income sources and improving nutrition have sustainable impacts (on asset bases, consumption, gender-specific outcomes and women's empowerment, and on diets) that persist after the intervention ends; that is to say, do such interventions contribute to resilient escapes from poverty? (2) Are such interventions protective when shocks occur? and (3) Do these interventions promote gender-sensitive resilience; i.e., do they promote both the household's resilience generally and women's resilience specifically? We answer these questions using unique data, a four-year post-endline follow-up survey of households from a cluster-randomized controlled trial of a nutrition-and-gender-sensitive agricultural intervention in Bangladesh.

Treatment arms that included both agriculture and nutrition training had sustainable effects on real per capita consumption, women's empowerment, and asset holdings measured in 2022. The magnitudes of these effects are meaningful. Treatment arms that included both types of training increased per capita consumption by 6.8 percent four years after the ANGeL intervention ended and the T-ANG arm increased asset holdings by approximately 19 percent. There were not sustained effects on caloric availability. By contrast, at endline, all treatment arms increased log hGDQS (our measure of household diet quality), with the impacts ranging from 3.7 percent (T-A) to 8.3 percent (T-ANG). Four years post-program, T-AN and T-ANG continued to have a statistically significant impact on hGDQS, with their effect sizes attenuated (4.4 and 2.8 percent respectively) but T-A no longer had a statistically significant impact on this outcome. When we combine the T-AN and T-ANG treatment arms, we reject the null that their effect size, a 3.7 percent increase, is equal to the impact of T-A. These effect sizes seem small. However, when we again restrict our sample to households with little cultivable land but also had homestead gardens at baseline, we find that the T-AN and T-ANG treatment arms increased hGDQS by approximately 10 percent.

At endline, all treatment arms increased women's empowerment (as measured by the pro-WEAI) by between 4.4 and 6.2 percentage points and increased the likelihood that women were empowered by 7.4 to 11.8 percentage points. These effects attenuate by 2022, but the attenuation is uneven. The T-ANG treatment arm continues to increase women's empowerment, by 4.8 percentage points while the other two treatment arms have smaller effects. T-ANG has the largest effect on whether women were empowered in 2022 while reducing the intra-household inequality score (the gender parity index) by 4.6 points.

At endline, the T-AN and T-ANG treatments largely had effects similar in magnitude. By 2022, there is some suggestion that the T-ANG treatment arm was having slightly larger impacts on some outcomes (for example, on log real per capita consumption, women's empowerment, and asset holdings), but not others (caloric availability and diet quality). Interestingly, while agriculture training alone (T-A) had increased household consumption, diet quality, and women's empowerment at endline, with the

exception of women's empowerment, impacts of T-A were smaller four years post program and no longer statistically significant.

We also sought to ascertain whether these interventions enhanced resilience. Following the definition of resilience set out in Constas, Frankenberger, and Hoddinott (2014), we investigated whether they enhanced the "capacity that ensures adverse stressors and shocks do not have long-lasting adverse development consequences". We found that treatment arms that included both agriculture and nutrition training reduced the likelihood that households undertook more severe forms of coping strategies (Crisis and Emergency). Similarly, treatment arms that included both agriculture and nutrition training reduced the likelihood that household per capita consumption fell, in real terms, by more than five percent between 2018 and 2022. Both these findings are consistent: the T-AN and the T-ANG treatment arms contributed to household resilience. By contrast, agriculture training alone (T-A), was not protective.

Understanding the mechanisms behind these sustained impacts—or lack thereof—across the different outcomes targeted by ANGeL is our agenda for future work. Additional work unpacking the reasons behind sustained impacts in the T-AN and particularly the T-ANG arm on women's empowerment measures would be valuable for future programming. It would be interesting to know how much individual indicators that comprise pro-WEAI changed over the medium- compared to the short-term. Examining impacts on time use, in particular, might shed light on unintended increases in workload, if any, or potential differences in opportunity costs of time that might be driving the differential impact on diets in low vs. high-landholding households. Complementary qualitative work on these issues would be desirable.

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