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Aid, Conflict and Gender: Heterogeneous Impacts of Agricultural Asset Transfers during the Syrian Civil War

Dorothee Weiffen[1,2,*], Ghassan Baliki[1] & Tilman Brück[1,2,3]

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Abstract
Conflict is a significant driver of global food insecurity, and agricultural assistance is a key policy tool to improve food security in such settings. However, evidence on whether and for whom such interventions are effective in conflict settings remains limited. Using panel data from 883 households in Syria and a quasi-experimental design, we estimate the average and heterogeneous treatment effects of an agricultural asset transfer on household food security. Applying the honest causal forest algorithm, we assess how gender, conflict intensity, and other household and contextual factors shape treatment heterogeneity. On average, the intervention increases the Food Consumption Score by 8.2%. The largest gains (12%) are observed among female-headed households in moderately conflict-affected areas, while households in low- or high-intensity conflict zones show limited or no improvement. These findings underscore the importance of contextualized targeting in fragile settings and caution against relying solely on average treatment effects, which may obscure critical heterogeneity in program impact.

Keywords: Agricultural intervention, Asset transfers, Food security, honest causal forest, Impact evaluation, Machine learning, Syria, Violent conflict

JEL classification: D10, D60, O12, O22, Q12

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* Corresponding author: Dorothee Weiffen, Zero Hunger Lab, Thaer-Institute for Agricultural and
[1] ISDC - International Security and Development Center, Berlin, Germany
[2] Zero Hunger Lab, Thaer-Institute of Agricultural and Horticultural Sciences, Humboldt-University of Berlin, Germany
[3] Leibniz Institute of Vegetable and Ornamental Crops, Großbeeren, Germany

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Author contributions:

Dorothee Weiffen: Conceptualization, methodology, data curation, formal analysis, writing - original draft, writing - review & editing, visualization.

Ghassan Baliki: Conceptualization, investigation, methodology, data curation, writing - review & editing, project administration, funding acquisition.

Tilman Brück: Conceptualization, investigation, methodology, writing - review & editing, funding acquisition.

1 Introduction

Violent conflict is the leading driver of food insecurity in the world (FAO et al., 2024). Conflict severely disrupts agrifood systems by damaging infrastructure, depleting agricultural assets and interrupting supply chains (Adelaja & George, 2019; Brück et al., 2019b; Vesco et al., 2021). Additionally, conflict increases household vulnerability to additional shocks (Brück et al., 2019b) and undermines the delivery of humanitarian assistance (Ghorpade, 2020), exacerbating food insecurity from multiple angles. At the same time, food insecurity is not only a consequence of conflict but also a driver, contributing to the onset and escalation of violence in fragile settings (Bellemare, 2015; Brück & d'Errico, 2019; Martin-Shields & Stojetz, 2019).

The consequences of conflict are structurally gendered (Brück et al., 2024; Justino, 2018; Vesco et al., 2025). While men are more often recruited, injured or killed in war, women disproportionately bear the social and economic burden (Kreft & Agerberg, 2024; Ormhaug et al., 2009). Repeated exposure to conflict has been shown to reduce men's engagement in agricultural activities (Bloem et al., 2025), whereas women frequently assume the sole responsibility for managing the household and its livelihood under adverse conditions during and after war (Ronzani et al., 2025). As a result, women and female-headed households are especially vulnerable to hunger and malnutrition during protracted crises (Kayaoglu et al., 2024; Ronzani et al., 2025; Stojetz & Brück, 2024).

Assistance programs are widely recognized as a critical strategy for restoring food security during and after crises. A growing body of literature covers the role of food and cash transfers in enhancing food security and related outcomes during conflict episodes with largely positive impacts (for example, Altındağ & O'Connell 2023; Ecker et al. 2024; Kurdi 2021; Gupta et al. 2024; Mastrorillo et al. 2024; Schwab 2019; Tranchant et al. 2019; and for reviews Brück et al. 2019a; Hirvonen et al. 2024; and Jeong & Trako 2022). Though there is limited evidence on whether and under what conditions agricultural asset transfers contribute to restoring food security in the context of active armed conflict (Al

Daccache et al., 2024; Baliki et al., 2023b), even though agriculture is known to be an effective coping strategy in times of instability (Arias et al., 2019; Bozzoli & Brück, 2009).

Existing studies assessing the impact of agricultural asset transfers and broader agricultural assistance in conflict-affected settings report mainly positive effects on food security (Baliki et al., 2018, 2024[1]; Doocy et al., 2018; Vallet et al., 2021), as well as when combined with food aid (Brück et al., 2019c). However, null effects have also been documented (Leuveld et al., 2018). In non-conflict contexts, agricultural interventions commonly have heterogeneous impacts depending on household characteristics and other contextual factors (Storm et al., 2019). For instance, studies have found that women particularly benefit from agricultural interventions in terms of food security (Anderson et al., 2021; Baliki et al., 2019, 2022) as well as resource-poor households (Carter et al., 2019; Mullally et al., 2021). However, none of these studies account for heterogeneity in treatment effects from variation in conflict intensity, leaving a potentially important dimension of program effectiveness unexplored.

Conflict not only affects food insecurity directly but also shapes households' responses to assistance by influencing risk preferences and decision-making behavior (Checchi et al., 2016; Mironova et al., 2019; Verwimp et al., 2019). Households receiving support might make different choices regarding food production and consumption compared to households living in non- or less-conflict settings. This could directly impact their food security. Comparing the impact of food aid across areas with varying levels of conflict, Tranchant et al. (2019) find that households directly exposed to armed groups increase their food expenditures more in response to assistance than other treated households in relatively stable areas. However, it remains unclear whether such differential impacts also hold for livelihood-building interventions or how different levels of conflict intensity interact with other factors that shape the effectiveness of agricultural programs.

[1]This study uses the same household survey dataset as the present study.

To address these knowledge gaps, we investigate two research questions: (i) whether agricultural asset transfers improve food security in an active conflict setting and (ii) how contextual factors, particularly gender and conflict intensity, shape these impacts. To do so, we study an agricultural asset transfer provided in Syria during the civil war. We collected panel data from 883 households from different regions across Syria. We measure food security using the Food Consumption Score (FCS). We use a quasi-experimental design comparing treatment and control households before and after the intervention could materialize. We combine the panel survey data with external data reflecting the incidence of violent conflict and population statistics at the subdistrict level. This unique dataset enables us to explore the relationship between different levels of conflict intensity and treatment effect heterogeneity. In our model estimation, we apply the honest causal forest algorithm, a machine-learning technique that recursively partitions the data into subgroups (Athey & Imbens, 2016). We first estimate the average treatment effect of the intervention. Then, we investigate which characteristics and contextual factors shape the heterogeneity in our model and examine these differences in conditional average treatment effects across subgroups, with a particular focus on how gender and the intensity of violent conflict shape differential impacts.

Regarding our first research question, we find that agricultural asset transfers significantly and robustly improve food security, increasing the FCS on average by 8.2% (4.7 points, (90% CI [2.1, 7.4], $p < 0.01$), emphasizing the strong potential of livelihood interventions in settings of active conflict. Regarding our second research question, the treatment effects are highly heterogeneous, with variation in conflict intensity being the most prominent driver of differential impacts in our setup. Specifically, the intervention has no statistically significant effect in areas with particularly low or high levels of violent conflict. In contrast, female-headed households in moderately conflict-affected regions benefit the most, with FCS increases of up to 12% (6.8 points, 90 % CI[2.6, 11], $p < 0.01$), while male-headed households exhibit only marginal or no improvements. These findings underscore the importance of contextualized designs of assistance in fragile settings, considering the specific needs of the targeted population. The results also caution against relying solely on average treatment effects, which may obscure critical heterogeneity in program impact.

Our study contributes to the growing literatures on the microeconomics of violent conflict (Verwimp et al., 2019), with a particular focus on gendered dynamics, agriculture and the effectiveness of assistance in fragile settings. We provide novel evidence on the differential impacts of conflict intensity on treatment effect heterogeneity, offering rare empirical insights from panel data collected in Syria during the war. By unpacking the heterogeneity in program impacts, our findings enhance the understanding of both the strong and null effects of agricultural interventions in fragile contexts. This has important implications for designing, particularly for the targeting of development and humanitarian programs tailored to context-specific needs.

The remainder of this chapter is structured as follows: Section 2 describes the study location, the intervention and the theoretical framework. Section 3 outlines our empirical approach. Section 4 presents the findings, followed by a discussion in Section 5. Section 6 concludes.

2 Background

2.1 The Context

We study the case of Syria from 2018 to 2020, a period marked by protracted civil war and its severe impacts. The war began in 2011 and is rooted in state repression, severe socioeconomic grievances and the broader regional upheaval of the Arab Spring, displacing more than 14 million people (UNHCR, 2025). Beyond its severe humanitarian consequences, the Syrian conflict severely damaged the country's infrastructure and trade foundations, with particularly severe disruptions in the agrifood sector (Baliki et al., 2023a; Jaafar et al., 2017; Mohammed et al., 2020). The economic contraction resulted additionally in hyperinflation and high unemployment during our study period in Syria (Mohammed et al., 2020; World Bank, 2022). At the same time, recurrent droughts critically undermine water availability (Mathbout et al., 2025; Mohammed et al., 2020) while crop pests are common due to low availability and quality of agro-chemicals (Syria Cross Border Humanitarian Leadership Group, 2023). These compound crises severely undermine agricultural production in Syria, resulting in 60% of the population being food insecure at the end of our study period (Food Security Cluster, 2022).

The situation in Syria has severe implications for women, particularly for widows. They disproportionately bear the burden of displacement or assume sole responsibility for their households amid extreme insecurity and economic hardship (European Asylum Support Office, 2020). Additionally, deeply rooted patriarchal structures further restrict their participation in economic activities and decision-making processes, often limiting access to critical resources and opportunities (Gallagher, 2012), further exacerbating their hardship.

2.2 The Intervention

In response to the adverse impacts of the war on the Syrian agrifood systems, the United Nations Food and Agriculture Organization (FAO) implemented an agricultural asset transfer to restore short-term food production and support rural livelihoods. Between July 2018 and June 2019, FAO distributed vegetable support comprising tomato, cucumber, eggplant, sweet pepper, broad bean, spinach and lettuce seeds, alongside agricultural tools and drip irrigation kits to irrigate 400-600 square meters. All beneficiary households received the same package. The intervention targeted 3,400 vulnerable smallholder farming households, prioritizing (i) female-headed households, (ii) unemployed young men at risk of militia recruitment and (iii) farmers with depleted assets or limited access to inputs. FAO applied the following selection criteria: (i) access to land between 500 and 1000 square meters for vegetable cultivation; (ii) agriculture must be the main livelihood and (iii) the household should not have a steady income source (Baliki et al., 2023a). The intervention was implemented in five governorates: Al-Hasakah and Deir-Ez-Zor in the North-East, Aleppo in the North-West and As-Sweida and Quneitra in the South.

This vegetable package formed part of a larger FAO-led emergency and recovery program implemented between 2018 and 2021, targeting households in eight governorates, the five listed above, as well as Dar'a, Hama and Homs. This larger intervention reached over 30,000 households with various assistance components, including poultry kits, livestock vaccinations, beekeeping materials, seed multiplication, sprout production kits and irrigation rehabilitation. However, the households included in our study did not receive support other than the vegetable kits. Hence, we do not evaluate these components in this study.

2.3 Theoretical Framework

We posit that providing seeds, tools and drip irrigation kits to vulnerable households in a crisis setting enhances their food security. The intervention is expected to increase homestead food production, which should directly increase the availability and diversity of food for beneficiary households. Furthermore, homestead food production reduces the dependence on external food sources, which is particularly relevant in crisis contexts where markets are often restricted. The intervention may also indirectly improve food security by increasing household income from selling surplus produce, enabling households to purchase other food and enhancing the quality and diversity of their dietary intake. We expect the transfer of durable components, such as tools and irrigation kits, to facilitate continued production beyond the initial support period, contributing to improved food security in the long run.

We also hypothesize that the impact of an agricultural asset transfer on food security is likely to vary across different households, as demonstrated in peaceful areas (for example, Storm et al. (2019) or Carter et al. (2019)). First, the literature demonstrates that conflict and other adverse shocks significantly disrupt local agrifood systems (Vesco et al., 2021). In areas of high conflict intensity, input and output markets are often less functional and critical infrastructure, such as irrigation systems or key assets, like land or machinery, may be damaged or inaccessible. As a result, households exposed to higher conflict intensity may be less able to engage in agriculture and benefit from agricultural asset transfers.

Moreover, a conflict bears more production risk, for example, through destruction, which may decrease the beneficiaries' willingness to engage in agriculture. At the same time, conflict itself changes risk preferences (Verwimp et al., 2019), potentially discouraging long-term engagement in agriculture. However, given the absence of alternative income-generating opportunities and the restricted access to food, beneficiary households may also be more reliant on agriculture in high-conflict settings (Bozzoli & Brück, 2009), increasing the relative importance of agricultural interventions. Similarly, other shocks, such as crop pests, droughts or a severe event in the household, such as an illness or an accident involving a household member, might disrupt agricultural production, undermining the effectiveness of asset transfers. However, beneficiary households may still be better off than they would be without the intervention.

Second, household characteristics may influence the response to an agricultural asset transfer and its impacts on food security, particularly when interacting with conflict. For instance, evidence from low-income settings suggests that women often benefit substantially from small-scale agricultural interventions (Al Daccache et al., 2024). However, especially in crisis contexts, widowed or female-headed households often face distinct institutional and economic barriers, such as limited access to resources, restricted rights or social exclusion (Brück et al., 2024; Justino, 2018; Vesco et al., 2025), that may hinder their ability to benefit from such interventions. Moreover, the age of the household head may be a key determinant of the effectiveness of such transfers. Younger farmers may be more open to adopting new practices, while older farmers may have greater agricultural experience. Young men are also more likely to engage in (para-)military groups compared to other groups (Mironova & Whitt, 2020), decreasing their capacity to engage in agriculture in conflict settings. Similarly, literacy may also play a crucial role in the utilization of new approaches, such as the drip irrigation system. Conversely, literate beneficiaries may also have access to other livelihood activities, which may negatively impact their interest in the intervention.

Third, the initial asset endowment of a household may shape the effectiveness of an agricultural asset transfer. For instance, limitations in access to land or basic farm tools may restrict the optimal use of support, while households with access to such assets may experience smaller marginal increases in their agricultural production as a result of the support. Similarly, households already engaged in agriculture may use the support to reinforce their existing production. Again, their marginal gains might be smaller. In contrast, for households engaged in livelihoods other than agriculture, an agricultural asset transfer may diversify food sources and income streams. However, those with more stable or higher income opportunities may be less interested in engaging in small-scale agriculture, which could reduce the uptake of the transfer and its overall impact.

In summary, agricultural asset transfers are likely to improve household food security in crisis settings. However, the effectiveness of such interventions is likely to vary depending on contextual and household-level factors. The impact of many of these factors can be positive or negative, non-linear and interacting with other factors in complex ways. Understanding these heterogeneous effects is essential for identifying who benefits from such support under which conditions, thereby enabling more effective targeting and designing of interventions.

3 Empirical Approach

3.1 The Study Design

We employ a quasi-experimental design to address our research questions (i) whether agricultural asset transfers improve food security in an active conflict setting and (ii) which contextual factors, particularly gender and conflict intensity, shape these impacts. We use two waves of household survey data ("Baseline" and "Endline") that we collected from households that received the asset transfer, the treatment group, and households that did not receive the asset transfer, the control group, before and after the intervention could materialize. Our design accounts for unobserved factors that may impact the outcome differently for the treatment and the control group, enabling us to estimate the causal treatment impact of the intervention on food security. We combined the household survey data with conflict event data and population statistics at the subdistrict level. This integrated data structure enables us to examine how both household-level and contextual characteristics shape the treatment effects of the intervention.

We selected the treatment and the control group for our study sample to be as comparable as possible using the following strategy: First, we identified all beneficiaries of the intervention by subdistrict before Baseline. Second, we randomly drew samples from these subdistricts proportionally representative of the full pool of beneficiaries. Third, we randomly selected a set of treatment villages from which the enumerator team selected every second household from a list of beneficiaries. Treatment households were randomly replaced by another household from the same village if they were not available at Baseline. Fourth, enumerators selected control households on-site from nearby villages to ensure comparability. Within each sub-district, the enumerators selected control villages similar to the pre-identified beneficiary villages. Then, respondents were selected from each of the control villages based on FAO's selection criteria as for the treatment households. The control group also served as a reference group for other assistance packages under FAO's emergency and recovery program.

3.2 Data and Variables

We conducted the Baseline in November 2018 before any treatment effect could have unfolded and the Endline took place two years later in January 2021. We collected both waves during the rainy autumn and winter seasons, thereby reducing concerns about seasonal variation in agricultural production or food access that could bias comparisons over time.

Our control group includes 273 additional households beyond those originally planned. These households were initially targeted to receive other FAO-led assistance packages and were surveyed for another study, but due to adjustments made after Baseline data collection, they ultimately did not receive any support. This discontinuation has two reasons: First, a beekeeping intervention was relocated based on post-Baseline contextual analyses. The potential of conflict contradicted the "do no harm" principle. Second, technical targeting requirements for seed multiplication and sprout production intervention did not align with the pre-identified implementation areas. Adding these 273 households, of which we already collected Baseline data, has three advantages: First, we increase statistical power to run more complex models. Second, more observations potentially improve covariate overlap between the treatment and control group, increasing the credibility of causal inference. Third, we enhance the external validity of our findings by covering a wider range of locations. In the robustness checks, we evaluate different sample compositions to rule out any systematic confounding due to these ex-post adjustments. Additionally, to rule out the possibility that assignment to FAO assistance is systematically related to food security outcomes, independent of receiving the intervention, we conduct a placebo test as part of our robustness checks. In this test, we exclude true beneficiaries and assign treatment status to the additional control households.

Table 1: Study Sample Composition

	Control	Treatment
Baseline	699	235
Endline	663	229
Panel	656	227

Notes. The table reports the Baseline and Endline rollout as well as the panel households that could be interviewed in both waves, in terms of the number of observations, disaggregated by treatment and control groups.

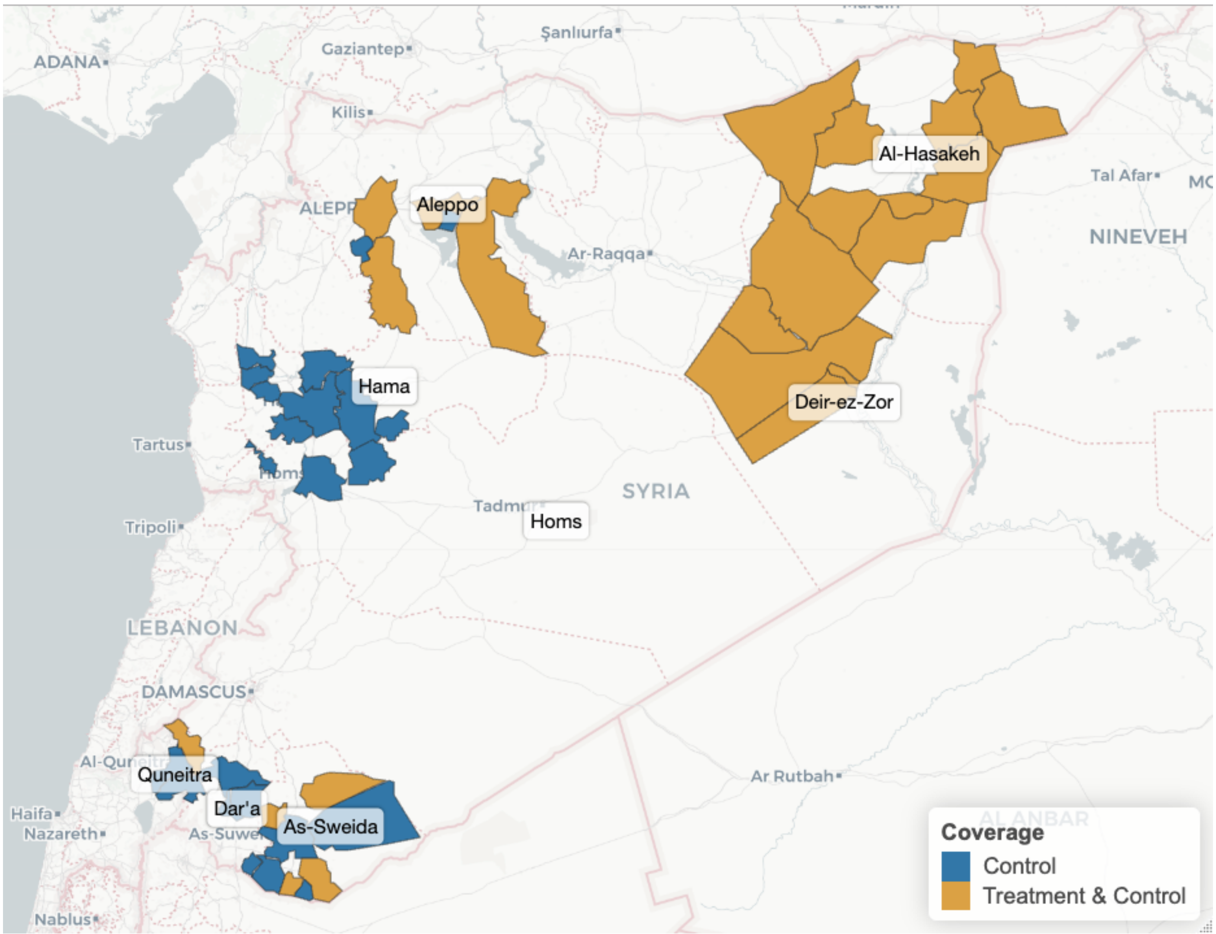
Table 1 displays the Baseline and Endline roll-out for the control and the treatment group. At Baseline, enumerators completed interviews from 699 control and 235 treatment households. At Endline, the enumerator team completed interviews from 663 control households and 229 treatment households. A total of 656 control and 227 treatment households form our final panel dataset. The attrition rate is 4.5%. Table A1 in the

Appendix details an attrition analysis. The fact that the outcome variables at Baseline are balanced between attrited and non-attrited households suggests that attrition is unlikely to introduce systematic bias to our estimates. Only 2 out of 30 Baseline variables differ significantly between attrited and non-attrited households at the 5% significance level: female-headed households and those owning chickens were slightly more likely to drop out of the panel. The dropout of female-headed households might be attributed to higher vulnerability and a higher likelihood of displacement. Additionally, herding households from subdistricts with lower shares of disabled households are more likely to drop out of the sample at the 10% significance level.

The study covers 46 subdistricts across eight governorates in Syria. Table A2 in the Appendix provides a detailed overview of the distribution of households by treatment status, governorate and subdistrict. Figure 1 shows the geographic coverage at the subdistrict level, showing that control households are present in all subdistricts where the assistance was distributed. In addition, the sample includes control households from 23 non-treatment subdistricts. Notably, several of these subdistricts, particularly those located in Hama and Homs in the Western-Center of the country, are geographically distant from the treatment areas. To ensure that geographic distance does not bias our results, we conduct robustness checks by excluding households from Hama and Homs, households from all governorates that did not receive treatment and households from all non-treatment subdistricts.

Together with FAO's monitoring and evaluation unit for Syria, the research team provided extensive training to local enumerators in Arabic before each data collection round. The research team ensured that the training covered ethical research principles and practical advice on data collection. Before participating in the interviews, all respondents provided verbal informed consent, in accordance with the ethical guidelines for research involving human subjects. The informed consent form provided a comprehensive explanation of the study's purpose and the participants' rights in clear and understandable language, ensuring that they fully understood the nature of their involvement. The enumerators conducted interviews using paper-based questionnaires and the monitoring and evaluation team manually entered the data into Microsoft Access. Such a panel dataset collected through face-to-face interviews is novel and unprecedented for Syria.

Figure 1: Group Coverage at Subdistrict Level



Notes. The Figure presents the geographic coverage of our study by control and treatment group at the subdistrict level.

Our main outcome variable is the Food Consumption Score (FCS), which measures food security. The FCS, developed by the World Food Programme, captures both the frequency and diversity of food consumption over the past seven days. The FCS is a valid proxy for adequate caloric intake (Wiesmann et al., 2009). It assigns scores based on the relative nutritive importance of the following food groups with weights in parentheses: main staples (2), pulses (3), vegetables (1), fruits (1), meat and fish (4), dairy products (4), sugar (0.5) and oil (0.5). The FCS score is the sum of the number of days each food group was consumed multiplied by its respective weight, yielding a total score between 0 and 112. In the Syrian context, scores above 28 are considered as borderline food consumption, while scores above 42 are classified as acceptable (WFP, 2008). Related studies commonly apply the FCS (for example, George et al., 2020; Mastrorillo et al., 2024).

The weighting for nutritive importance in FCS might understate the impact of the intervention, as we expect the most change in nutrition to occur in vegetable consumption, which receives a weight of one, compared to meat, which receives a weight of four. Therefore, we use FCS with an unweighted indicator as a robustness test. We constructed the measure with the same food items from FCS. However, we omitted the food group weights and divided the score by 7 to generate an average daily dietary diversity score, which takes a value between 0 and 8. We refer to this indicator as Dietary Diversity (DD).

We also included household characteristics and contextual factors that we theorize to shape treatment heterogeneity in treatment effects in crisis settings. First, we account for household head characteristics that might drive differences in response to agricultural asset support, including gender, age and literacy. Second, we include household-level variables: if the household earned income with crop farming or herding at Baseline, the household's Baseline endowment of agricultural capital, namely, the irrigated and rainfed land size, if they own any poultry, sheep or cattle, if they have constraints to water and if they keep a home garden. Third, we draw on self-reported experiences of shocks that are likely to impact farm productivity. Households were asked if they experienced the following shocks in the previous twelve months before the interview: drought, crop pests, livestock diseases, high costs for agricultural inputs, low prices for agricultural outputs, illness of an income earner and theft of agricultural assets.

We expand our pool of contextual factors by aggregating geo-coded conflict event data and population statistics at the subdistrict level. First, we construct variables on conflict incidence and intensity from the Armed Conflict Location and Event Data Project (ACLED) (Raleigh et al., 2010). We generated two indicators: the aggregate numbers of fatalities through violent events that occurred one month and one year before the interview to measure the short-term and medium-term exposure to violent conflict, respectively. This enables us to investigate the relationship between varying intensity levels of conflict and the program's effectiveness in enhancing food security. The number of fatalities per 100,000 inhabitants due to violent conflict is the most suitable available indicator of conflict, as it accounts for event severity relative to population size. Second, and complementary to the above, we merged information on demographic characteristics from 2019 provided by FAO (2019), also at the subdistrict level, to our dataset. We include shares of widowed, disabled or female people in the population, which we interpret as proxies of regional long-run fragility and conflict exposure. We are unable to disaggregate the conflict data to a smaller administrative level, as the survey data do not include geo-codes and village names are not consistently identifiable.

3.3 Descriptive Statistics

Table 2 presents descriptive statistics of the panel dataset. Columns (1) and (2) show means with standard deviations in parentheses or percentages for the control and treatment group, while Columns (4) and (5) show the same for male- and female-headed households. Columns (3) and (6) display p-values from t-tests or chi-square tests to assess the statistical significance of group differences. Female-headed households account for 33% of the treatment group and 17% of the control group ($p < 0.01$). The literacy rate of household heads is considerably higher among male-headed households (83%) than among female-headed households (49%), reflecting the average disadvantaged position of women. The average age of household heads at Baseline is 49 years.

Table 2: Sample Balance Between Treatment and Control Households and Male- and Female-headed Households

	(1) Control (Mean (SD)/ Share)	(2) Treatment (Mean (SD)/ Share)	(3) P-value	(4) Male HHH (Mean (SD)/ Share)	(5) Female HHH (Mean (SD)/ Share)	(6) P-value
Selection Criteria						
HH has access to land	76.8%	79.7%	0.355	75.1%	87.0%	< 0.01
Main income agriculture	67.5%	73.6%	0.081	69.3%	68.1%	0.749
Main income other stable source	22.6%	20.7%	0.556	23.4%	17.3%	0.061
Household-Level Characteristics at Baseline						
Female HHH	16.8%	33.0%	< 0.01			
Age of HHH	49.3 (12.9)	49.4 (12.6)	0.913	49.1 (12.3)	50.5 (14.5)	0.214
HHH is literate	78.0%	70.0%	0.021	83.1%	49.2%	< 0.01
Income from crop farming	89.6%	92.1%	0.259	90.3%	90.3%	0.996
Income from herding	36.4%	33.0%	0.353	35.8%	34.6%	0.757
Irrigated land (ha)	0.33 (0.63)	0.25 (0.53)	0.075	0.32 (0.62)	0.25 (0.52)	0.145
Rainfed land (ha)	0.78 (1.42)	0.68 (1.38)	0.340	0.78 (1.45)	0.66 (1.25)	0.287
Water access constraints	27.6%	21.6%	0.065	26.9%	22.7%	0.230
HH owns chicken	22.0%	17.6%	0.151	21.3%	18.9%	0.460
HH owns cattle	15.2%	11.0%	0.093	14.9%	11.4%	0.190
HH owns sheep	20.1%	22.9%	0.385	21.2%	19.5%	0.598
HH owns home garden	50.6%	68.7%	< 0.01	51.7%	68.6%	< 0.01
Exogenous Shock Experience 12 Months before Endline						
Drought	12.5%	12.8%	0.915	14.5%	5.4%	< 0.01
Crop pests	9.0%	8.4%	0.772	10.2%	3.8%	< 0.01
Livestock disease	10.5%	6.6%	0.056	10.2%	7.0%	0.155
High agr. input costs	77.9%	80.6%	0.379	79.8%	74.1%	0.109
Low agr. output price	11.3%	14.1%	0.284	12.8%	9.2%	0.151
Illness income earner	5.0%	2.2%	0.030	3.7%	6.5%	0.158
Theft agr. assets	3.8%	3.5%	0.842	3.3%	5.4%	0.242
Direct and indirect Intensity of Conflict at Subdistrict Level						
Prop. widowed	0.05 (0.04)	0.05 (0.03)	0.885	0.05 (0.04)	0.05 (0.04)	0.634
Prop. female	0.50 (0.03)	0.51 (0.02)	< 0.01	0.50 (0.03)	0.50 (0.03)	0.713
Prop. disabled	0.24 (0.08)	0.24 (0.08)	0.267	0.24 (0.07)	0.25 (0.09)	0.094
Fatalities (month)	14.8 (30.4)	11.7 (26.3)	0.154	13.8 (29.0)	14.5 (30.8)	0.809
Fatalities (year)	173.2 (289.5)	140.2 (233.2)	0.086	169.3 (284.0)	147.5 (245.5)	0.300
Food Security Outcomes at Baseline						
FCS	59.8 (18.7)	50.1 (16.2)	< 0.01	57.5 (18.1)	56.6 (20.1)	0.610
FCS = Acceptable	80.0%	62.1%	< 0.01	77.1%	69.2%	0.078
FCS = Borderline	17.2%	32.6%		19.6%	27.0%	
FCS = Poor	2.7%	5.3%		3.3%	3.8%	
Dietary Diversity	4.8 (1.1)	4.3 (0.9)	< 0.01	4.7 (1.0)	4.6 (1.1)	0.328
Observations	656	227		698	185	

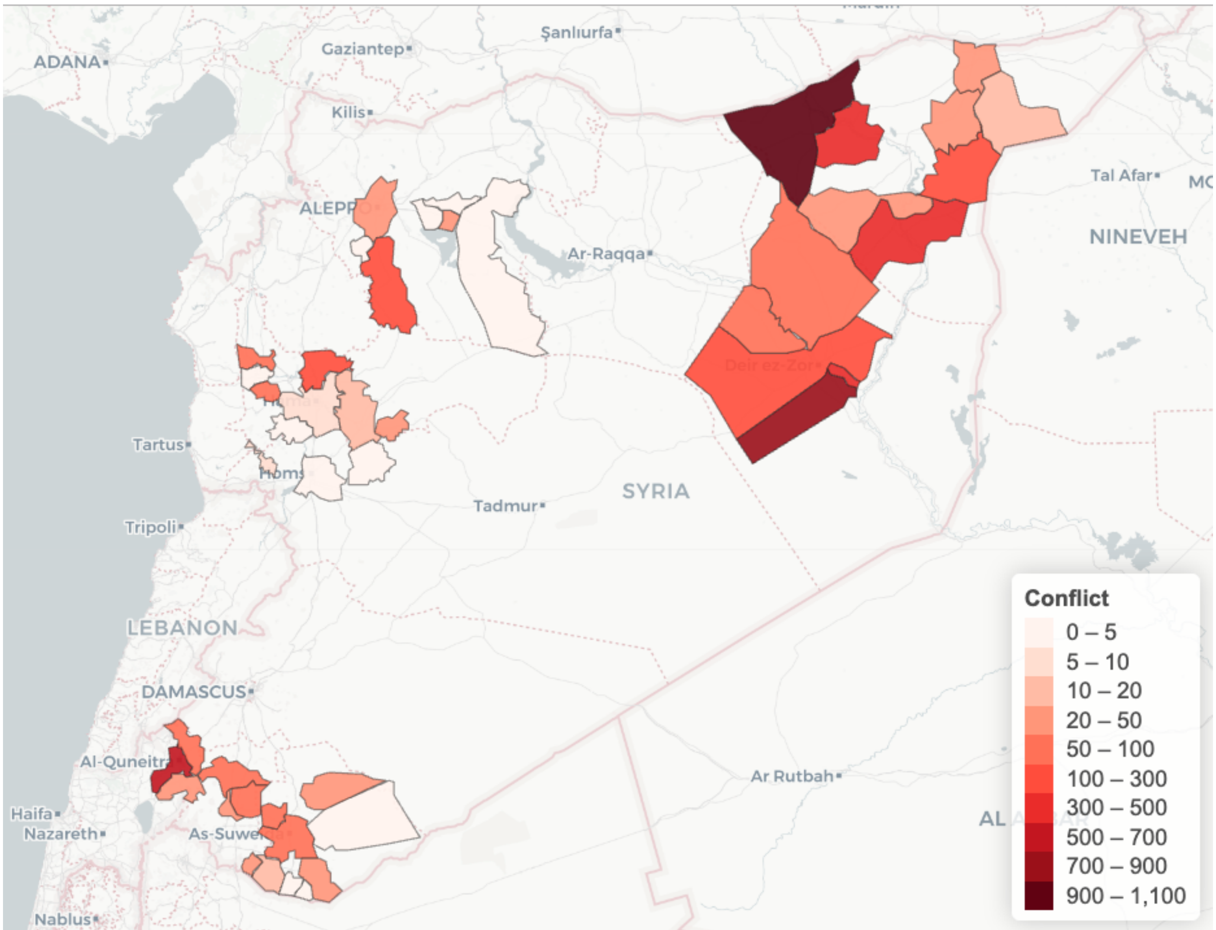
Notes. Columns (1)-(2) and (4)-(5) display values of each variable for the Control and Treatment groups, and for male- and female-headed households, respectively. Continuous variables are displayed by means with standard deviations in parentheses. Categorical variables are shown in percentages. Columns (3) and (6) report the p-values from two-sided t-tests for continuous and binary variables or chi-square tests for factor variables. HH=household, HHH=household head. The main income source is defined as contributing at least 50% of total household income. Fatalities are measured as the number of conflict-related deaths per 100,000 inhabitants before Endline.

Approximately 90% of households reported income from crop farming and 35% from herding. On average, households own 0.3 hectares of irrigated land and 0.7 hectares of rainfed land at Baseline, with male-headed households holding, on average, more land than female-headed households. About 25% of households reported constraints to water access at Baseline. Additionally, 20% of households keep chickens, 13% keep cattle and 21% keep sheep. Home gardens are more common among treatment and female-headed households (both 69%) compared to control households (51%) and male-headed households (52%)(both differences $p < 0.01$).

In the 12 months before the Endline survey, a substantial share of households reported experiencing shocks. Most notably, 79% faced high agricultural input costs, which we attribute to the hyperinflation in Syria in 2020. Additionally, 13% of households were affected by drought and 9% by crop pests, both more common among male-headed households, likely due to their larger average farm sizes. Livestock diseases affected 11% of control households and 7% of treatment households, while illness among income earners was reported by 5% of control households and 2% of treatment households. Furthermore, 13% of households experience low output prices and 4% reported theft of agricultural assets.

Subdistrict-level data reveal a slight overrepresentation of women and girls in treatment areas, with an average share of 51% compared to 50% in control subdistricts ($p < 0.01$). Our sample households reside in subdistricts with a high average prevalence of individuals with disabilities (24%) and a relatively low share of widowed individuals (5%). In terms of direct exposure to violence, treatment households live in subdistricts where an average of 12 fatalities per 100,000 inhabitants was recorded one month before Endline, compared to 15 for control households. One year before Endline, treatment households were exposed to 140 fatalities per 100,000 inhabitants on average, compared to an average of 173 in control areas. Figure 2 displays this measure at the subdistrict level. Notably, violence was particularly intense in the North-East and South of the country, while in the North-West, relatively fewer fatalities occurred. The subdistrict-level variables are mainly balanced between male- and female-headed households, indicating a well-distributed spatial spread of household head gender across the study area.

Figure 2: Conflict Exposure at Subdistrict Level



Notes. The Figure presents the number of registered fatalities per 100,000 inhabitants for the year before Endline data collection at the subdistrict level.

With respect to food security, Baseline data indicate a higher average FCS of 60 among the control group, compared to 50 among the treatment group ($p < 0.01$). This corresponds to 80% of control households and 62% of treatment households being classified as having acceptable food security. Male-headed households are more likely to have acceptable food consumption than female-headed households (77% compared to 69%, respectively).

In terms of dietary diversity, control households reported consuming an average of 4.8 food groups per day, while treatment households reported 4.3 food groups on average ($p < 0.01$).

Based on our study design, the sampled households are expected to comply with three selection criteria: (i) access to land for vegetable production, (ii) agriculture as the primary source of livelihood and (iii) absence of any other stable income source. However, the data reveal some discrepancies between these criteria and the actual Baseline characteristics: 78% of the households reported access to land at Baseline. Among female-headed households, this share is higher (87% compared to 75% among male-headed households). 68% of the control group and 74% of the treatment group state that they earn at least half of their income from agriculture and 78% of households reported that they do not gain the majority of their income from a steady, non-agricultural source. 8 households ($< 1\%$) do not comply with any of the selection criteria, all of them are in the control group (not displayed). This discrepancy reveals a systematic difference between what was reported during the selection process and what was later stated during the interview itself. Such misreporting is not unusual when it comes to social protection interventions (Martinelli & Parker, 2009). Still, as long as such misreporting is not systematically different between the treatment and control group, it does not compromise the internal validity of our evaluation. To address this concern, we conduct a robustness check only comparing households that reportedly comply with the selection criteria across groups.

In summary, systematic gender differences are evident in our sample. Male-headed households tend to have larger agricultural businesses, while female-headed households are more likely to have access to a home garden or land at all. At the same time, women appear to be more vulnerable, as expressed through lower literacy rates and a higher likelihood of food insecurity. Similarly, the treatment group appears more vulnerable than the control group, which we attribute to a larger share of female-headed households within the treatment group. Ensuring comparability between the control and treatment group is essential for constructing credible counterfactuals. Systematic differences between the groups may introduce bias in the estimated treatment effects. In our sample, we identify several statistically significant imbalances between the control and treatment group. In the next section, we present a method that addresses these sample imbalances and enables robust causal inference.

3.4 Econometric Approach

To estimate the causal impact of the intervention and examine treatment effect heterogeneity, we use the honest causal forest algorithm (Athey & Imbens, 2016). Through recursive splitting of the sample based on covariates, this approach enables us to assess whether the treatment effect varies across subgroups and which characteristics shape this heterogeneity. The algorithm calculates the conditional average treatment effect (CATE) by aggregating differences between treatment and control observations within subgroups of similar characteristics. The algorithm uses different observations to estimate effects than the ones used to determine the partitions. This "honest" approach reduces bias (Athey & Imbens, 2016; Wager & Athey, 2018).

More specifically, an honest causal forest comprises many iterations ("trees"), each constructed using a sample-splitting strategy. The data are randomly divided into two halves: a splitting sample and an estimation sample. In the splitting sample, the algorithm recursively groups observations into subgroups ("leaves"). By selecting split points on covariates, the algorithm maximizes heterogeneity in predicted treatment effects between the subgroups. This strategy enables comparisons of treated and control households within relatively homogeneous subgroups, thereby approximating conditional unconfoundedness. Once the subgroups are defined, the algorithm uses only the estimation sample to compute treatment effects. Equation 1 shows the CATE estimator $\hat{\tau}(X)$, which is the difference in outcomes of treatment and control households, conditional on subgroup characteristics where $W \in \{0, 1\}$ is a binary treatment indicator, Y is the outcome, X is the covariate matrix and L denotes a subgroup within an iteration. The algorithm obtains the final estimate for each observation by averaging across iterations in which the observation is part of the estimation sample.

$$\hat{\tau}(x) = \frac{1}{|\{i : W_i = 1, X_i \in L\}|} \sum_{\{i:W_i=1,X_i \in L\}} Y_i - \frac{1}{|\{i : W_i = 0, X_i \in L\}|} \sum_{\{i:W_i=0,X_i \in L\}} Y_i \quad (1)$$

In our model, we apply augmented inverse propensity weighting (AIPW) to the CATEs, which adjusts for differences between the treatment and control group based on covariates. The approach weights observations by their importance in treatment heterogeneity (Athey et al., 2019). This method is particularly adequate for non-randomized samples, like ours, because the AIPW estimator ensures a consistent estimation as long as either the propensity score model or the regression model is correctly specified (Athey et al., 2019; Glynn & Quinn, 2010). In other words, this method reduces bias from non-random treatment assignment.

To estimate the honest causal forest, we collapse the panel data into one cross-section. Our main objective for collapsing the data is to ensure that Baseline and Endline observations from one household end up in the same subgroups. We select the time-invariant household information as well as household variables potentially affected by the treatment from the Baseline survey. For the outcome variable, we take the simple difference for each respondent between the two waves. For exposure to shocks and conflict, we use the Endline values, as we are interested in the occurrence of these events during and after the intervention phase. For the population data, we use information from 2019, which falls during or after the intervention. We test our results by adding pre-treatment shocks to our vector of covariates and dropping single covariates from the model to show that our results are not driven by one single contextual factor.

The algorithm provides three estimates for the treatment effect: the average treatment effect, the average treatment effect of the treated and the average treatment effect of the overlapped sample. Given the significant differences in some variables between the control and treatment group as shown in Table 2, the average treatment effect of the overlapped sample is the most precise estimate for the treatment effect. It constructs the counterfactuals by comparing observations with the highest degree of similarity. Furthermore, the algorithm provides two calibration tests. First, the mean forest prediction test uses held-out data to indicate if the model precisely estimates the average treatment effect. Second, the differential forest prediction shows if the model detects treatment heterogeneity reliably. A value of 1 indicates a perfect calibration for both indicators (Chernozhukov et al., 2018). We first developed a pilot forest that included 24 covariates that we theorize to potentially moderate treatment heterogeneity. Based on a pilot simulation, we can then assess the frequency of the covariates being applied for splitting. This is a valuable indicator for the variable importance in treatment heterogeneity. Then, by using the tuning configuration, the algorithm determines the optimal shaping parameters for the model. Tuning prioritizes covariates that most strongly influence splitting decisions.

We examine heterogeneity with four approaches. First, we interpret the algorithm's variable importance output, which displays the frequency with which each covariate was selected for partitioning. Second, we plot continuous variables against the predicted CATEs for each observation in a scatter plot. We group the observations into three equal groups of low, medium and high covariate values and derive linear trend lines in relationships between the CATEs and the selected outcome for each of the three groups. Third, we split the sample by the size of CATEs into five groups and examine the differences in within-group covariate means between high- and low-impact households. Fourth, we split the sample two-way by exposure to conflict and by the gender of the household head. Then, for each of the ten combinations, we predict the within-group average treatment effects holding all other covariates fixed at their median.

The honest causal forest algorithm offers several advantages compared to other methods that we would traditionally apply for our case: First, the honest causal forest algorithm works non-parametrically. The drivers of heterogeneity do not have to be defined ex-ante because the algorithm tests a wide range of coefficients relative to the overall sample size (Athey & Imbens, 2016). Like this, the algorithm selects informative variables. This is a significant advantage for our setup, as we test 24 covariates with only 883 observations from households. Traditional linear regression models with interaction terms would lack statistical power when accounting for all this information at the same time (Chernozhukov et al., 2018).

Second, the honest causal forest algorithm can handle sample imbalances between the treatment and control group because the observations are matched with each other based on their characteristics within subgroups in a similar fashion to other non-parametric approaches like kernel methods and nearest-neighbor matching (Wager & Athey, 2018). However, due to the additional weighting of the covariates by their relative importance, the honest causal forest algorithm delivers more efficient locally-weighted estimators, which account for the dimensionality of the set of covariates (Athey et al., 2019).

Third, through the tree structure, we can assess non-linear relationships between the variables. This is particularly relevant in our setup, as agricultural interventions are known to exhibit complex treatment interaction effects (Storm et al., 2019).

Fourth, the data splitting makes the approach honest: the structure of tree splits and leaf groupings are defined by the splitting data and, therefore, exogenous to the estimation data. Like this, the method generates unbiased and asymptotically normal estimates. Nevertheless, the splitting decreases the precision of the model since only half of the data is used for estimation (Wager & Athey, 2018).

4 Results

4.1 Average Treatment Effects

Table 3 presents the average treatment effect of the agricultural asset transfer on the FCS. We report three estimates: the average treatment effect for the overlap population (ATO) (Column 2), which represents the most conservative estimate by focusing on the sample with common support, and the average treatment effect (ATE) for the full sample (Column 3) as well as the average treatment effect on the treated (ATT) (Column 4). The intervention increased the FCS by 4.7 points (90% CI [2.1, 7.4], $p < 0.01$), as indicated by our main estimate, corresponding to a relative improvement of 8.2%. The alternative treatment effect definitions indicate a similar impact of 4.7 to 5.4 points (90% CI [2.7, 6.7], $p < 0.01$ and 90% CI [2.8, 7.8], $p < 0.01$, respectively). Columns (5) and (6) provide calibration tests for our model. The mean forest prediction in Column (5) indicates that the tuned model is well-calibrated to test average treatment effects ($p < 0.01$). The differential forest prediction in Column (6) provides strong evidence of treatment effect heterogeneity ($p < 0.01$). However, the estimate of 1.7 indicates that our model understates the existing heterogeneity.

Table 3: Average Treatment Effects on FCS and Calibration Goodness

	(1) Mean (SD) at Baseline	(2) ATO	(3) ATE	(4) ATT	(5) Mean forest prediction	(6) Differential forest prediction
FCS	57.27	4.71***	4.72***	5.41***	1.01***	1.68***
	(18.5)	(1.63)	(1.21)	(1.51)	(0.33)	(0.36)

Notes. Model with tuned parameters. Standard deviation or standard errors in parentheses. ATO= Average treatment effect of the overlapped sample, ATE=Average treatment effect of the overall sample, ATT= Average treatment effect of the treated. The coefficients for the mean forest prediction in (4) indicates the goodness of the CATE prediction and the differential forest prediction in (5) indicates if the model assesses heterogeneity appropriately. A value of 1 in both predictions indicates a precise estimation. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Several tests confirm the robustness of our estimate for the average treatment effect. Table A3 in the Appendix reports the average treatment effects of the overlapped sample across nine different model specifications and sample compositions. First, Test (1) demonstrates that a placebo test, treating households originally selected to other support that did not receive any assistance as the treatment group and excluding true treatment households, does not result in an ATO significantly different from zero. This indicates that assignment alone, independent of actual treatment, did not significantly impact food security. Notably, the slight positive tendency may reflect more favorable time trends for households initially targeted for assistance compared to control households. Second, with Tests (2)-(6), we estimate the ATO using different sample compositions. The estimates range from 2.8 points ($p < 0.1$) to 5.8 points ($p < 0.01$), spanning closely around the estimate from our main model. Third, Test (7) emphasizes that our results are not sensitive to the selection of tuned parameters. Fourth, Tests (8) and (9) show that different variable selections do not alter the interpretation of the results. We confirm the latter with Table A5 in the Appendix, where we sequentially exclude each covariate from the model. The ATOs range between 3.6 and 4.8 ($p < 0.05$), again, close to our main result. Fifth, Table A4 in the Appendix presents ATTs from different specifications of Difference-in-Differences and Fixed Effects models. The Difference-in-Differences estimates suggest notably larger ATTs, indicating that our main results should be interpreted as conservative. The Fixed Effects models produce estimates that closely align with our main estimate for the average treatment effect.

4.2 Heterogeneous Treatment Effects

We detect heterogeneity in the predicted treatment effects of an agricultural asset transfer on food security. To identify the factors contributing to this variation, we analyze the frequency of selection for each variable in our model, used to distinguish subgroups with differing treatment effect magnitudes in Table 4. Note that since correlated variables may act as substitutes in the splitting process, their relative importance should be interpreted only suggestively for treatment effect heterogeneity. First, both direct measures of conflict intensity (the number of fatalities at the subdistrict level for one month and one year) account together for 26% of all splits. When combined with the indirect indicators of local conflict exposure, the shares of disabled, widowed and female individuals at the subdistrict level, the model selects conflict-related variables for partitioning in 50% of the time.

Table 4: Variable Importance for Treatment Heterogeneity by Group

Variable	Importance
Household-Level Characteristics at Baseline	42.6%
Age of HHH	11.9%
Female HHH	10.8%
Irrigated land (ha)	4.3%
HH owns home garden	3.3%
HH owns sheep	2.9%
Rainfed land (ha)	2.7%
HH owns chicken	2.7%
Income from herding	1.5%
HHH is literate	<1%
HH owns cattle	<1%
Water access constraints	<1%
Income from crop farming	<1%
Exogenous Shock Experience 12 Months before Endline	7.8%
Crop pests	4.8%
Livestock disease	1%
Low agr. output price	<1%
High agr. input costs	<1%
Drought	<1%
Theft agr. assets	<1%
Illness income earner	<1%
Direct and indirect Intensity of Conflict at Subdistrict Level	49.6%
Fatalities one year before Endline (per 100,000)	13.5%
Fatalities one month before Endline (per 100,000)	12.4%
Prop. disabled	11.4%
Prop. widowed	8.3%
Prop. female	4.1%

Notes. The table displays an output from the honest causal forest algorithm without tuned parameters ordered by the frequency of slits conducted with each covariate along with the corresponding share of usage for splits in parentheses. A high frequency of splitting should not be interpreted as evidence of treatment mediation, as many covariates are highly correlated. Correlated variables are likely to act as substitutes for splitting.

Second, household-level characteristics account for 43% of the splits to separate the sample by its treatment effect heterogeneity. The household head's age is the most frequently selected variable, accounting for 12% of splits, followed by the head's gender with 11% of the splits. Agricultural assets, such as sheep or chicken ownership, home garden ownership and the size of agricultural land, each contribute to 3–4% of the splits.

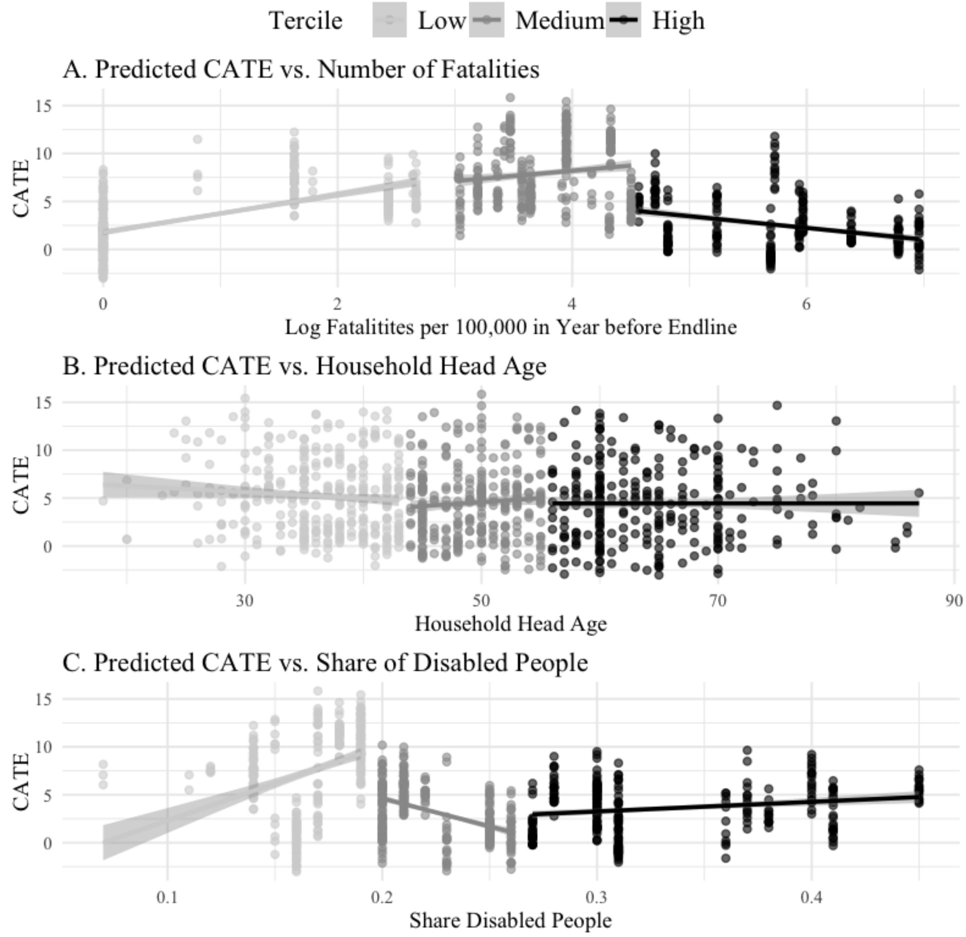
Third, and notably, apart from exposure to crop pests (selected in 5% of splits), exogenous shocks experienced in the past 12 months are rarely used in the splitting process. This suggests that household-level exposure to shocks is not a significant factor in treatment effect heterogeneity in our model.

As a robustness test of these findings, we test the sensitivity of splitting variable selection. Table A5 in the Appendix presents the top three variables for treatment heterogeneity when sequentially excluding one covariate from each model. The results confirm that the number of fatalities in the past year is the most influential covariate in this setup, appearing as the most frequently used splitting variable in 20 out of 23 models. The proportion of disabled individuals and the number of fatalities in the past month at the subdistrict level also show strong predictive power in treatment heterogeneity, ranking among the top three variables in 20 and 19 out of 23 specifications, respectively. Notably, the two variables accounting for fatalities are highly correlated and are likely to be substitutes in splitting. In contrast, the age and gender of the household head are far less influential, appearing among the top three variables in seven and two specifications, respectively.

To verify that subdistrict fixed effects did not artificially inflate the importance of conflict variables, we re-estimate the model by replacing subdistrict-level variables with subdistrict fixed effects. Table A6 in the Appendix presents the relative importance of each subdistrict for treatment heterogeneity alongside the conflict intensity one year before Endline. The results reveal that most subdistrict fixed effects are irrelevant for sample splitting, with only three subdistricts exhibiting a splitting importance above 1%. The levels of conflict intensity varied substantially between the subdistricts and the importance of subdistrict-level variables declines sharply from 50% to 9% in this robustness test, demonstrating that conflict variables capture substantial additional variation beyond what is explained by subdistrict-fixed effects alone.

To understand how the treatment effects of the asset transfer on FCS vary across different subgroups, we examine the relationship between the predicted CATEs and continuous covariates that are frequently selected for variable splitting in Figure 3. We divide the sample into terciles according to their covariate values, for which we plot separate linear trendlines. The plots cover (A) the logarithmic number of fatalities in the past year before Endline per 100,000 inhabitants at the subdistrict level, (B) the age of the household head and (C) the share of disabled people in the subdistrict population. Note that CATEs are predicted from a model that includes these covariates. This may introduce bias to the associations presented. Nevertheless, the substantial vertical dispersion of the predicted CATEs across all three graphs highlights the multidimensional nature of the model's estimation rather than the large predictive power of one dominant covariate. Hence, this illustration provides meaningful insights into treatment heterogeneity.

Figure 3: Relationship between CATEs and Selected Continuous Covariates



Notes. The Figures present the relationship between the predicted CATEs for each observation with respect to their values of continuous covariates. The trendlines indicate trends for subgroups split into terciles with 90% confidence intervals.

First, we observe an increasing trend in CATEs with higher fatality rates for the lower two terciles, followed by a decreasing trend in the highest tercile. This pattern suggests that the largest predicted CATEs of the transfer occurred in areas with moderate conflict intensity. Second, we find no clear association between the age of the household head and CATEs, which may indicate more complex, possibly non-linear interactions or limited treatment heterogeneity driven by the age of the household head. Third, we also identify a rather non-linear relationship between CATEs and disability prevalence at the subdistrict level. The largest CATEs occurred for households residing in subdistricts with disability rates between 15% and 20%, with a diminishing association observed at lower levels and a rather stable association observed at higher levels of disability in the community.

Table 5: Covariate Means by Conditional Treatment Effect Size

Covariate	Low CATE	Lower Moderate CATE	Moderate CATE	Upper Moderate CATE	High CATE	P-Value
CATE	-0.30 (1.03)	2.03 (0.64)	4.61 (0.6)	6.57 (0.74)	10.9 (1.75)	-
Household-Level Characteristics at Baseline						
Female HHH	0%	10.17%	16.95%	28.98%	48.86%	<0.01
Age of HHH	50.96	49.06	50.51	47.74	48.47	0.093
	(12.38)	(13.11)	(13.08)	(11.96)	(13.35)	
Income from herding	29.38%	41.81%	30.51%	36.93%	39.2%	0.058
Irrigated land (ha)	0.29	0.39	0.32	0.26	0.25	0.187
	(0.59)	(0.64)	(0.66)	(0.54)	(0.57)	
Rainfed land (ha)	0.16	0.59	1.08	1.17	0.75	<0.01
	(0.76)	(1.45)	(1.65)	(1.57)	(1.21)	
HH owns chicken	12.99%	25.42%	16.95%	19.89%	28.98%	<0.01
HH owns sheep	11.86%	30.51%	18.64%	25%	18.18%	<0.01
HH owns home garden	31.07%	42.37%	54.24%	62.5%	86.36%	<0.01
Exogenous Shock Experience 12 Months before Baseline						
Crop pests	0%	2.82%	10.73%	13.07%	17.61%	<0.01
Livestock disease	0%	2.82%	9.6%	17.61%	17.61%	<0.01
Direct and indirect Intensity of Conflict at Subdistrict Level						
Prop. widowed	0.034	0.04	0.066	0.062	0.039	<0.01
	(0.03)	(0.03)	(0.05)	(0.04)	(0.02)	
Prop. female	0.508	0.511	0.501	0.502	0.498	<0.01
	(0.03)	(0.03)	(0.04)	(0.03)	(0.02)	
Prop. disabled	0.244	0.258	0.266	0.238	0.190	<0.01
	(0.06)	(0.07)	(0.08)	(0.09)	(0.05)	
Fatalities 1 month	29.16	21.55	6.46	2.59	10.02	<0.01
	(42.57)	(37.3)	(20.97)	(11.06)	(8.13)	
Fatalities 1 year	308.56	309.30	86.11	59.88	58.47	<0.01
	(379.19)	(354.54)	(164.49)	(111.84)	(59.13)	
Food Security at Baseline						
FCS	52.83	55.43	59.47	59.72	58.94	<0.01
	(19.16)	(17.38)	(16.84)	(18.19)	(20.14)	

Notes. Table reports group means with standard deviations in parentheses for continuous variables and group percentages for categorical variables. Groups are based on quintiles of predicted CATE. Only covariates with a splitting variable importance above 1% are shown. P-values from ANOVA tests comparing means across quintiles. Fatalities are measured as the number of conflict-related deaths per 100,000 inhabitants before Endline

To further investigate the characteristics of beneficiaries who experienced greater or smaller to no impacts, Table 5 displays means and percentages for all covariates that are relevant for sample splitting (>1% of splits), as well as Baseline food security. We divide the sample into five groups based on their predicted CATE size, which ranges from -0.3 (the lowest average group impact) to 10.9 (the highest average group impact). Again, the estimation of the CATEs is based on these variables, hence, this comparison may be biased.

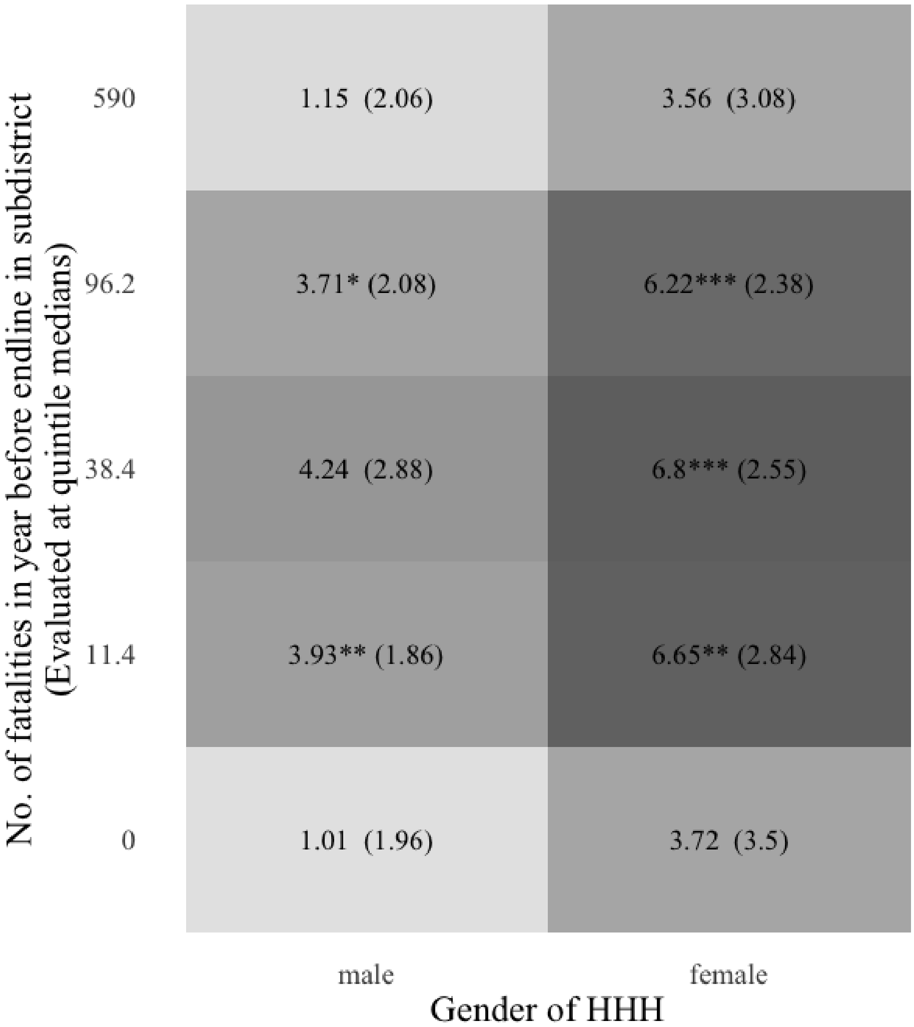
Nearly all covariates differ significantly across impact size groups, confirming the high degree of treatment effect heterogeneity. The first notable pattern is a sharp increase in the share of female-headed households, rising from 0% in the lowest-impact group to 48.9% in the highest-impact group. Predicted CATEs also increase with small-scale agricultural asset ownership: while only 13% of households in the lowest impact group reported owning chickens and 31.1% a home garden, these figures rise to 29% and 86.4%, respectively, in the highest impact group. These variables correlate with female-headed households. Notably, the strong association between garden ownership and higher impacts is intuitive, given that the asset transfer program included vegetable seeds. Regarding larger-scale agricultural assets, such as sheep ownership, herding income or land size, we observe rather non-linear, inconclusive patterns.

In terms of adverse shocks and consistent with our previous findings, the group with the highest predicted CATEs resided on average in subdistricts experiencing moderate levels of conflict (10 fatalities per 100,000 in the past month and 59 in the past year). In contrast, the group with the lowest predicted impact resided in areas with the highest average conflict intensity (29 fatalities per 100,000 in the past month and 309 in the past year). Notably, the high-impact group is also characterized by the lowest subdistrict-level share of people with disabilities. Another noteworthy finding is that households with a high predicted CATEs are more likely to have experienced crop pests and livestock diseases during the intervention period (both, 18% in the high-impact group compared to only 0% in the low-impact group).

Regarding Baseline food security, households with the largest gains began with an average level of food security, whereas those in the low-impact group exhibited the lowest food security at Baseline.

The gender of the household head and conflict intensity are important drivers of treatment heterogeneity of the agricultural asset transfer. To better understand their interactive heterogeneity in treatment response, in other words, how treatment effects differ for male-versus female-headed households under varying levels of conflict intensity, we estimate the CATE for each combination of household head gender at five levels of conflict intensity, represented by the median values of conflict intensity quintiles in Figure 4. All other covariates are held constant at their median to isolate the interactive heterogeneity. The Figure highlights that female-headed households exhibited substantially larger predicted CATEs on food security than male-headed households, regardless of conflict intensity. The transfer increased the food security of male-headed households only under upper-moderate and lower-moderate conflict intensity, with a 4-point increase on the FCS ($p < 0.1$). In contrast, female-headed households experienced significant gains across lower-moderate to upper-moderate levels of conflict intensity, ranging from 6.2 to 6.8 points on the FCS ($p < 0.05$). This corresponds to an increase of 11–12% relative to the average FCS for female-headed households at Baseline. At both, the lowest and highest levels of conflict intensity, agricultural asset transfers did not yield statistically significant impacts on FCS for either household type.

Figure 4: CATEs by Gender of the Household Head and Incidence of Violence



Notes. CATEs are estimated for combinations of household head gender (male or female) and the median value of each quintile of fatalities from violent events per 100,000 inhabitants, keeping other covariates fixed at their median. Standard errors in parentheses. CATEs are based on the honest causal forest model with tuned parameters and an adjusted set of covariates. HHH=Household head. Included covariates: age of HHH, if the HHH is a herder, land size of rainfed and irrigated land, if the households own chickens or sheep, if they have a home garden, if they were affected by crop pests and droughts, proportion of disabled, widowed, female persons in the sub-district. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

To validate these findings, we estimate subgroup average treatment effects using a fixed-effects model in Table A7 in the Appendix. We divide the sample into three groups based on levels of conflict intensity and into two groups by household head gender. Due to limitations in sample size, we were unable to conduct meaningful analyses for two-way interactions or further disaggregate the sample. The results align with our prevailing finding: households in subdistricts with moderate conflict intensity experienced large and significant improvements in food security following the agricultural asset transfer (p <0.01), as well as female-headed households (p< 0.01). In line with our main findings, male-headed households and those in areas with low or high levels of conflict did not show statistically significant treatment effects.

5 Discussion

Our analysis demonstrates that agricultural asset transfers have strong, significant and positive average treatment effects on food security in a conflict setting, highlighting the potential of such interventions to support vulnerable populations in times of crisis. However, our findings also reveal that while certain groups benefit substantially, other groups do not seem to benefit from such interventions in a conflict setting.

The studied asset transfer intervention is designed as an emergency support measure prioritizing women, aligning with a broader body of evidence suggesting that gender-sensitive agricultural programs can lead to substantial returns in food security (Al Daccache et al., 2024; Baliki et al., 2019, 2022). Our findings reinforce this evidence, revealing significantly larger improvements in food security among female-headed households compared to male-headed households. This gender-differentiated impact reflects structural differences: men tend to have stronger agricultural business capacity, while women, particularly female household heads and widows, face greater vulnerability in crisis contexts (Bloem et al., 2025; Kayaoglu et al., 2024; Ronzani et al., 2025). In our sample, female-headed households had lower Baseline food security, leaving more room for improvement. Moreover, they were more likely to maintain home gardens, a key requirement for benefiting from small-scale agricultural asset transfer. Conversely, male-headed households in our sample managed larger-scale agricultural businesses. They were less aligned with the intervention's focus on small-scale agricultural assets, which may explain the limited impact. Our study contributes to the literature by demonstrating that not only do men and women face different vulnerabilities in conflict settings (Justino, 2018; Vesco et al., 2025), but they also respond differently to agricultural support in a crisis setting.

Our findings also highlight that exposure to and intensity of adverse shocks, particularly crop pests and violent conflict, shape the effectiveness of agricultural asset transfers. Previous research has emphasized that such shocks often exacerbate each other's adverse effects (Brück et al., 2019b). However, we find that households facing moderate levels of violent conflict and those affected by crop pests and livestock benefited more from the intervention than those not exposed to these stressors. In the case of crop pests, the pattern may reflect two mechanisms: first, households actively engaged in agriculture are naturally more vulnerable to such shocks; and second, the agricultural asset transfer might have substantially buffered food security losses for beneficiaries compared to what would have happened without it. For violent conflict, we find that households exposed to either particularly low or particularly high levels of violence did not benefit significantly from the intervention. In contexts of active conflict, agricultural production tends to be severely constrained (Adelaja & George, 2019). Our results suggest that in such a conflict setting, a simple asset transfer is insufficient to raise farm productivity to a level that translates into improved food security. These null effects may be attributed to the challenges faced by households living under extreme stress or in areas with severely disrupted agricultural systems and markets. In these environments, alternative forms of support, such as food aid or cash transfers (for example, Tranchant et al. (2019)), may be more effective in improving nutritional and food security outcomes. Conversely, our results also suggest that asset transfers alone are insufficient to generate meaningful improvements in relatively peaceful areas. In these contexts, more comprehensive, resilience-building agricultural strategies may yield more sustained impacts on household welfare than short-term emergency assistance (Arias et al., 2019; Bozzoli & Brück, 2009; Chapter 2 of this dissertation).

Methodologically, our study highlights the value of using the honest causal forest algorithm for impact evaluations in setups like ours, which are based on a non-randomized design that introduces moderate imbalances between the treatment and control group and have many covariates of interest but a limited number of observations. One of the key advantages of our model is its ability to address multidimensional covariate imbalances without relying on strict model specifications. While our estimates of average treatment effects are broadly consistent with those produced by traditional econometric models, the true strength of our approach lies in its capacity to reveal rich, non-parametric insights into treatment effect heterogeneity. The method enables the simultaneous exploration of a large number of potential drivers of treatment heterogeneity without pre-specifying interaction terms or filtering variables based on theoretical assumptions. This data-driven flexibility is particularly valuable in contexts where relationships among variables can be complex and non-linear. By identifying the key drivers of heterogeneous treatment effects and assigning a treatment effect estimate to each observation, the honest causal forest algorithm enhances our understanding of who benefits most from the intervention and under what conditions, supporting better-targeted and more cost-effective intervention designs.

Still, we acknowledge several limitations. First, while our analysis identifies key household and contextual characteristics that shape the effectiveness of agricultural asset transfers, it does not include other potentially important dimensions such as detailed production data, market access, infrastructure or local governance. Although covering all sources of potential treatment heterogeneity was never the focus of our study, exploring these factors constitutes important avenues for future research. Second, we were unable to conduct more fine-grained spatial analyses, such as comparing distances to conflict events, due to the lack of precise geospatial information in our household data. This underscores the importance of spatial information in household data. Third, our study relies on a quasi-experimental design with notable Baseline imbalances. Although our model helps mitigate this issue by comparing households with similar observed characteristics and although we conduct extensive robustness checks, the possibility of unobserved confounding remains. As with any observational study, unmeasured factors may still bias our estimated effects.

6 Conclusion

Our study provides rare empirical evidence on the average and heterogeneous treatment effects of agricultural asset transfers in a conflict-affected setting, drawing on panel data from 883 households in Syria merged with aggregated conflict event data and population statistics. Using an honest causal forest model, we estimate an average increase in food security by 8.2%, with substantial heterogeneity across subgroups. Female-headed households in areas with moderate conflict intensity benefit most, while effects are limited among male-headed households and those in areas with very low or high conflict exposure. These findings caution against one-size-fits-all approaches, underscoring the importance of incorporating needs at the micro- and at the macro-level into program design. In fragile settings, where vulnerabilities and local conditions vary widely, understanding for whom and under what conditions interventions work is crucial for ensuring effectiveness for all assistance beneficiaries.

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A Appendix

Table A1: Sample Balance between Non-attrited and Attrited Households

	(1) Non-attrited (Mean (SD)/ Share)	(2) Attrited (Mean (SD)/ Share)	(3) P-Value
Treatment HH	25.7%	15.7%	0.151
Selection Criteria			
HH has access to land	77.6%	76.5%	0.858
Main income agriculture (>=50%)	69.1%	64.7%	0.531
Main income other stable source (>=50%)	22.1%	21.6%	0.932
Household-Level Characteristics at Baseline			
Female HHH	21.0%	39.2%	0.012
Age of HHH	49.4 (12.8)	51.3 (13.2)	0.321
HHH is literate	76.0%	82.4%	0.259
Income from crop farming	90.3%	82.4%	0.155
Income from herding	35.6%	49.0%	0.069
Irrigated land (ha)	0.31 (0.60)	0.27 (0.57)	0.662
Rainfed land (ha)	0.75 (1.41)	0.65 (1.47)	0.622
Water access constraints	26.0%	25.5%	0.930
HH owns chicken	20.8%	39.2%	0.012
HH owns cattle	14.2%	15.7%	0.773
HH owns sheep	20.8%	21.6%	0.903
HH owns home garden	55.3%	56.9%	0.825
Exogenous Shock Experience 12 Months before Baseline			
Drought	62.9%	66.7%	0.581
Crop pests	47.0%	45.1%	0.794
Livestock disease	17.0%	13.7%	0.519
High agr. input costs	60.2%	64.7%	0.524
Low agr. output price	36.9%	39.2%	0.747
Illness income earner	10.9%	9.8%	0.806
Theft agr. assets	17.6%	15.7%	0.726
Direct and indirect Intensity of Conflict at Subdistrict Level			
Prop. widowed	0.05 (0.04)	0.05 (0.04)	0.480
Prop. female	0.50 (0.03)	0.50 (0.03)	0.156
Prop. disabled	0.24 (0.08)	0.22 (0.07)	0.073
Fatalities one month before Endline (per 100,000)	13.97 (29.37)	17.97 (38.78)	0.472
Fatalities one year before Endline (per 100,000)	164.70 (276.37)	184.26 (326.64)	0.677
Food Security Outcomes at Baseline			
FCS	57.27 (18.54)	60.55 (18.90)	0.247
FCS = Acceptable	75.4%	78.4%	0.137
FCS = Borderline	21.2%	9.8%	
FCS = Poor	3.4%	5.9%	
Dietary Diversity	4.70 (1.05)	4.94 (0.99)	0.100
Observations	883	51	

Notes. Columns (1-2) display average values of variables for the non-attrited and the attrited sample, respectively. Continuous variables are displayed by means with standard deviations in parentheses. Categorical variables are shown in percentages. Column (3) reports the p-values from two-sided t-tests for continuous and binary variables or chi-square tests for factor variables. HH=household, HHH=household head.

Table A2: Distribution of Observations across Governorates and Subdistricts by Treatment Status

Subdistrict	Control	Treatment	Subdistrict	Control	Treatment
			Al-Hasakeh		
Areesheh	7	10	Be'r Al Hulo Al W.	3	18
Hole	10	13	Quamishli	13	1
Ras Al Ain	17	8	Shadadah	5	8
Tal Hmis	10	10	Tal Tamer	17	4
			Aleppo		
Al Khafsa	38	18	Dayr Hafir	11	
Eastern Kwaires	5	15	Hadher	11	
Jebel Saman	4	16	Rasm Haram ElImam	19	11
Tall Eddaman	23	6			
			As-Sweida		
As Sweida	3		Gharyeh	5	
Mashnaf	7		Mazra'a	26	14
Salkhad	14	4	Shaqa	16	8
Thibeen	10	3			
			Dar'a		
As Sanamayn	12		Busra Esh Sham	5	
Izra'	5		Jizeh	6	
Sheikh Miskine	6				
			Deir-ez-Zor		
Deir-ez-Zor	25	6	Khasham	8	14
Kisreh	5	1	Muhasan	52	4
Tabni	8	19			
			Hama		
As Salamiyeh	49		As Suqaylabiyah	2	
Eastern Bari	16		Hama	47	
Harbanifse	6		Jeb Ramleh	12	
Suran(Hama)	20		Tell Salhib	19	
			Homs		
Al Makhrim	4		Homs	4	
Shin	3				
			Quneitra		
Al Khashniyyeh	2		Khan Arnaba	45	16
Quneitra	21				

Notes. The table displays the number of observations in each governorate and subdistrict by treatment and control group.

Table A3: Robustness Tests: Different Specifications of the Average Treatment Effect on FCS

Specification	ATO	N
(1) Placebo test: additional control as treated	2.51 (1.64)	656
(2) Without additional control	5.08*** (1.81)	631
(3) Dropping Hama and Homs	3.76** (1.63)	701
(4) Dropping Dar'a, Hama, and Homs	3.66** (1.63)	667
(5) Dropping non-treated subdistricts	2.81* (1.65)	614
(6) Full compliance with selection criteria	5.33** (2.52)	445
(7) Not-tuned model	4.71*** (1.63)	883
(8) Including Baseline shock covariates	4.36*** (1.64)	883
(9) Outcome: Dietary Diversity	0.25*** (0.09)	883

Notes. Table displays the average treatment effect on the overlapped sample (ATO) using different sample and model specifications. If not stated otherwise, models use tuned parameters. Standard errors in parentheses. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
(1) Treats households that were originally selected to receive support under other treatment arms of the broader emergency and recovery program but received no assistance as treated. True treatment households are excluded from this analysis.
(2) Excludes the group of households originally selected for other treatment arms of the broader emergency and recovery program who received no assistance from the control pool.
(3) Excludes households from Hama and Homs from the control pool.
(4) Excludes households from Dar'a, Hama, and Homs from the control pool.
(5) Excludes all control households not located in a treated subdistrict.
(6) Excludes all households that do not comply with at least one of the selection criteria at Baseline.
(7) Uses the honest causal forest algorithm without tuned parameters and with a fixed number of iterations equal to the number of observations.
(8) Expands the covariate pool by including Baseline shock exposure.
(9) Replaces the FCS outcome with the unweighted Dietary Diversity indicator. The score ranges from 0-1 instead of 0-112.

Table A4: Robustness Tests: Average Treatment Effects with Traditional Models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Diff-in-diff models				Fixed effects			
ATT	8.61***	7.88***	8.23***	7.22***	4.42***	5.47***	4.42***	4.85***
	(1.90)	(1.65)	(2.35)	(2.10)	(1.49)	(1.52)	(1.50)	(1.74)
NN PSM	No	No	Yes	Yes	No	No	Yes	Yes
Control variables	No	Full	No	Full	No	Restricted	No	Restricted
Num. Obs.	1766	1766	908	908	1766	1766	908	908
R^2	0.031	0.293	0.046	0.299	0.010	0.186	0.019	0.204

Notes. ATT=Average treatment effect of the treated. For regressions in columns with NN PSM equal to "Yes", control households are matched using nearest neighbour propensity score matching. Matching is based on the following Baseline variables: gender, age and literacy of the household head, whether farming contributes to their livelihoods, if they keep a backyard garden, whether the household faces water constraints, their landsize of rainfed and irrigated land, and whether they keep chicken; and on the following Endline values if they were affected by drought, high input costs, livestock diseases, a severe illness of an income earner and theft. Control variables for the restricted set include if they keep a backyard garden, whether the household faces water constraints, their landsize of rainfed and irrigated land, whether they keep chicken, sheep or cattle, if they were affected by drought, crop pests, high input costs, low output prices, livestock diseases, a severe illness of an income earner and/or theft and the following variables at the locality level: the total number of fatalities per capita one month and one year before each data collection. Control variables for the full set additionally include gender, age and literacy of the household head, whether farming and/or herding contribute to their livelihoods, and at the locality level, the share of widowed, female and disabled population. All models are adjusted for robust standard errors. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Robustness Tests: Average Treatment Effects and Top 3 Important Variables with Sequential Exclusion of Variables

Dropped	(1) ATO	(2) Top 1	(3) Top 2	(4) Top 3
Female HHH	4.44*** (1.63)	Fatalities one year (14.7%)	Fatalities one month (11%)	Prop. disabled (10.7%)
Age of HHH	3.99** (1.63)	Fatalities one year (22.5%)	Fatalities one month (20.5%)	Prop. disabled (8.6%)
HHH is literate	4.22*** (1.6)	Fatalities one year (17.3%)	Fatalities one month (13.3%)	Prop. disabled (11.8%)
Income from crop farming	4.22*** (1.61)	Fatalities one year (15.9%)	Fatalities one month (13.3%)	Prop. disabled (10.5%)
Income from herding	4.13** (1.63)	Fatalities one year (18.5%)	Fatalities one month (13.8%)	Age of HHH (11.2%)
Irrigated land (ha)	4.05** (1.62)	Fatalities one month (19%)	Fatalities one year (16.6%)	Prop. disabled (9.7%)
Rainfed land (ha)	4.24*** (1.61)	Fatalities one year (23.1%)	Female HHH (14.3%)	Prop. disabled (13.7%)
Water access constraints	4.3*** (1.63)	Fatalities one year (19.7%)	Prop. disabled (13.7%)	Fatalities one month (13.2%)
HH owns chicken	4.34*** (1.62)	Fatalities one year (17.3%)	Fatalities one month (14%)	Age of HHH (11.7%)
HH owns cattle	4.43*** (1.62)	Fatalities one year (16.9%)	Fatalities one month (14.5%)	Age of HHH (11.1%)
HH owns sheep	4.35*** (1.62)	Fatalities one year (13.8%)	Fatalities one month (10.9%)	Prop. disabled (10.1%)
HH owns home garden	4.25*** (1.61)	Fatalities one year (13.7%)	Fatalities one month (10.9%)	Prop. disabled (10.3%)
Drought	4.24*** (1.63)	Fatalities one year (13.6%)	Fatalities one month (10.7%)	Prop. disabled (10.6%)
Crop pests	3.96** (1.62)	Fatalities one year (10.8%)	Prop. disabled (9.3%)	Fatalities one month (8.9%)
Livestock disease	4** (1.63)	Fatalities one year (13.9%)	Fatalities one month (11%)	Prop. disabled (10.7%)
High agr. input costs	3.61** (1.61)	Prop. disabled (6%)	Age of HHH (5.9%)	Prop. female (5.8%)
Low agr. output price	4.15** (1.63)	Fatalities one year (13.7%)	Fatalities one month (10.8%)	Prop. disabled (9.4%)
Illness income earner	4.15** (1.62)	Fatalities one year (16.1%)	Prop. disabled (13.1%)	Age of HHH (10.3%)
Theft agr. assets	4.1** (1.62)	Fatalities one year (13.3%)	Prop. disabled (10.8%)	Age of HHH (9.9%)
Prop. widowed	4.53*** (1.63)	Fatalities one year (15.1%)	Fatalities one month (12.9%)	Prop. disabled (11.3%)
Prop. female	4.25*** (1.6)	Fatalities one year (13.1%)	Prop. disabled (11.2%)	Fatalities one month (11.1%)
Prop. disabled	4.57*** (1.67)	Fatalities one year (17.8%)	Female HHH (13.6%)	Fatalities one month (12.6%)
Fatalities one month	4.29*** (1.62)	Fatalities one year (24.6%)	Age of HHH (13.6%)	Prop. disabled (9.2%)
Fatalities one year	4.75*** (1.61)	Fatalities one month (13.6%)	Prop. disabled (12.3%)	Widow (11.4%)

Notes. The table displays outputs from the honest causal forest algorithm, excluding one variable from each calculation. Column (1) displays the average treatment effect on the overlapped sample (ATO) using tuned parameters. Standard errors in parentheses. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns (2)-(4) display the three most frequently used variables for splitting in the respective model, along with the corresponding share of usage for splits in parentheses. A high frequency of splitting should not be interpreted as evidence of treatment mediation, as many covariates are highly correlated. Correlated variables are likely to act as substitutes for splitting.

Table A6: Robustness Tests: Variable Importance of Subdistrict Fixed Effects instead of Subdistrict-specific Covariates

Subdistrict	(1) Importance	(2) Conflict Intensity	Subdistrict	(3) Importance	(4) Conflict Intensity
Al Khafsa	4.4%	0.0	Harbanifse	0%	0.0
Mazra'a	2.5%	75.8	Homs	0%	2.2
Khan Arnaba	1.2%	51.9	Izra'	0%	64.5
Rasm Haram El Imam	0.5%	0.0	Jeb Ramleh	0%	75.0
Ras Al Ain	0.3%	1051.5	Jebel Saman	0%	24.4
Deir-ez-Zor	0.2%	296.7	Jizeh	0%	20.7
Hole	0.1%	187.7	Khasham	0%	379.1
Tabni	0.1%	90.2	Kisreh	0%	51.8
Al Makhrim	0%	0.0	Mashnaf	0%	0.0
Al Khashniyyeh	0%	28.4	Muhasan	0%	881.8
Areesheh	0%	32.2	Quamishli	0%	21.0
As Salamiyeh	0%	11.4	Quneitra	0%	589.7
As Sanamayn	0%	96.2	Salkhad	0%	28.9
As Suqaylabiyah	0%	55.4	Shadadah	0%	307.2
As Sweida	0%	50.0	Shaqa	0%	38.1
Be'r Al Hulo Al Wardeyyeh	0%	35.6	Sheikh Miskine	0%	30.7
Busra Esh Sham	0%	14.1	Shin	0%	6.0
Dayr Hafir	0%	24.4	Suran(Hama)	0%	110.8
Eastern Bari	0%	38.4	Tal Hmis	0%	14.4
Eastern Kwaires	0%	0.0	Tall Eddaman	0%	123.4
Gharyeh	0%	0.0	Tall Eddaman	0%	123.4
Hadher	0%	0.0	Tell Salhib	0%	0.0
Hama	0%	5.1	Thibeen	0%	0.0
Overall	9.3%				

Notes. Columns (1) and (3) display the variable importance of subdistrict-fixed effects from an honest causal forest algorithm, excluding all subdistrict-specific covariates. The model uses not-tuned parameters and a fixed number of iterations equal to the number of observations. Columns (2) and (4) display the conflict intensity based on the number of fatalities one year before Endline per 100,000 inhabitants for comparison.

Table A7: Robustness Tests: Subgroup Average Treatment Effects with Fixed-Effects Models

	(1)	(2)	(3)	(4)	(5)
	Conflict Intensity			HHH Gender	
Group/ Tercile	1st	2nd	3rd	Female	Male
ATT	3.54	7.32***	1.86	12.92***	0.38
	(2.36)	(2.21)	(2.83)	(2.80)	(1.79)
Num.Obs.	228	356	324	285	603
R^2	0.020	0.059	0.003	0.137	0.000

Notes. Observations divided into subsamples by conflict intensity based on the number of fatalities in the year before Endline per 100,000 inhabitants (terciles) or the gender of the household head (binary). ATT=Average treatment effect of the treated. Fixed-Effects Regression with nearest-neighbour propensity score matching. Matching was conducted separately for each subset based on the following Baseline variables: gender, age and literacy of the household head, whether farming contributes to their livelihoods, if they keep a backyard garden, whether the household faces water constraints, their landsize of rainfed and irrigated land, and whether they keep chicken; and on the following Endline values if they were affected by drought, high input costs, livestock diseases, a severe illness of an income earner and theft. Control variables include if they keep a backyard garden, whether the household faces water constraints, their landsize of rainfed and irrigated land, whether they keep chicken, sheep or cattle, if they were affected by drought, crop pests, high input costs, low output prices, livestock diseases, a severe illness of an income earner and/or theft and the following variables at the locality level: the total number of fatalities per capita one month and one year before each data collection. All models are adjusted for robust standard errors. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.