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Violent conflict moderates food security impacts of agricultural asset transfer in Syria: A heterogeneity analysis using machine learning

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HiCN Working Paper 381

December 2022

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

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

Abstract

Agricultural interventions are one of the key policy tools to strengthen the food security of households living in conflict settings. Yet, given the complex nature of conflict-affected settings, existing theories of change might not hold, leading to misinterpretation of the significance and magnitude of these impacts. How contextual factors, including exposure to conflict intensity, shape treatment effects remain broadly unconfirmed. To address this research gap, we apply an honest causal forest algorithm to analyse the short-term impacts of an agricultural asset transfer on food security. Using a quasi-experimental panel dataset in Syria, comparing treatment and control households two years after receiving support, we first estimate the average treatment effect, and then we examine how contextual factors, particularly conflict, shape treatment heterogeneity. Our results show that agricultural asset transfers significantly improve food security in the short-term. Moreover, exposure to conflict intensity plays a key role in determining impact size. We find that households living in moderately affected conflict areas benefited significantly from the agricultural intervention and improved their food security by up to 14.4%, while those living in no or high conflict areas did not. The positive effects were particularly strong for female–headed households. Our findings provide new insights on how violent conflict determines how households benefit from and respond to agricultural programming. This underscores the need to move away from one-size-fits-all agricultural support in difficult settings towards designing conflict-sensitive and inclusive interventions to ensure that no households are left behind.

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Conflict of interest: The authors declare no conflict of interest.

Funding: This work is supported by the Centre of Excellence for Development Impact and Learning (CEDIL) - which is funded by UK Aid.

Acknowledgement: We would like to thank Melodie Al Daccache for providing support in data processing. We would also like to thank all participants from the ISDC seminar, the 2022 CEDIL Workshop on New Methods for Impact Evaluations in Conflict Settings and Humanitarian Emergencies in Berlin, the 2022 Household in Conflict Network Annual Workshop in Warwick, the 2022 21st Jan Tinbergen European Peace Science Conference in London, and the 2022 SEEDS for Recovery: Research & Policy Workshop in Beirut for their valuable feedback on this work.

1. Introduction

This paper provides evidence on how contextual factors shape the impact of agricultural aid on food security in conflict-affected settings. 139 million food insecure people live in settings affected by conflict (FAO et al., 2022). Today, all ten countries with the highest level of food crisis experience some form of conflict (Fiertz, 2021). Not surprisingly, 80% of the needs for humanitarian assistance is concentrated in conflict-affected settings (World Bank, 2022a). Apart from the direct costs to lives, violent conflict destroys physical capital and infrastructure, disrupts farm production and breaks down value chains, which directly affects food insecurity (George et al., 2020). Accordingly, individuals highly exposed to violent conflict exhibit lower calorie intake and dietary diversity (Tranchant et al., 2021). Inversely, high levels of food insecurity drive the likelihood of onset and intensity of violent conflict (Bellemare, 2015; Brück & d'Errico, 2019; Bush & Martiniello, 2017). Hence, in order to break out of the vicious violent conflict-food insecurity cycle, external aid is crucial, for example in the form of food or agricultural support (Abraham & Pingali, 2020), where food aid is shown to decrease the incidence of violent conflict (Mary & Mishra, 2020).

Although the evidence on the relationship between violent conflict and food insecurity is well established in the literature (see Martin-Shields & Stojetz, 2018 for a detailed review) and agriculture is known to be a coping strategy during and after conflict (Arias et al., 2019; Bozzoli & Brück, 2009), there is less research on how effective agricultural interventions are in strengthening food security in these settings. Due to challenges in conducting studies and collecting data in difficult settings (Puri, et al., 2017), it is not surprising that most of the evidence to date stems from non-conflict-affected regions. This literature provides clear evidence that agricultural interventions yield positive impacts on nutrition and food security (Bizikova et al., 2020; Ruel et al., 2018; Wordofa & Sassi, 2020), particularly for women (Andersen et al., 2021; Baliki, et al 2022; 2019). Existing research from conflict-affected settings focuses mainly on the nutritional benefits of food aid or cash, rather than agricultural support (Altındag & D. O'Connell, 2022; Brück et al., 2019a; Ecker et al., 2019; Kurdi, 2021; Tranchent et al., 2019; Tusiime et al., 2013). Doocy et al. (2018) show with a quasi-experimental design that agricultural capacity building programmes improve household food security in DRC. Other recent evidence of agricultural interventions combined with other programme arms indicates mixed results on the impact on food security outcomes in conflict settings (Bedoya et al., 2019; Vallet et al., 2021). However, there is a dearth of evidence on the effectiveness of agricultural asset transfers in complex conflict settings when delivered alone.

Moreover, there is a gap in the literature on how agricultural interventions in settings are affected by violent conflict, specifically in how household characteristics and contextual factors shape treatment heterogeneity (Fiorella et al., 2016). Households living in challenging settings, might respond systematically differently at various levels of conflict compared to peaceful settings since their risk preferences and behaviour are directly shaped by conflict (Verwimp et al., 2019, Mironova et al., 2019), which is particularly evident for women and youth (Justino, 2018). This implies that households who receive agricultural support might undertake different choices regarding food production and consumption compared to households living in non-conflict settings, which in turn could directly impact their food and nutritional security. Tranchant et al. (2019) show that the nutritional impacts of a food assistance programme in Mali change depending on the exposure to conflict, but they do not address how conflict in and of itself contributes to impact heterogeneity. Furthermore, households exposed to violent conflict become less resilient to other shocks (Brück et al., 2019b), and access and endowment of physical capital are adversely shaped by conflict (Adelaja and George, 2019).

From other settings, research shows that accounting for heterogeneity in agriculture interventions is important and factors such as household characteristics, agricultural endowment and exposure to shocks shape responses to agricultural support. Female and young farmers behave differently in dealing with food insecurity (Kairiza and Kembo, 2019) and accessing agricultural resources (Johnson et al., 2015; Kristjanson et al., 2017), and resource-poor households benefit substantially more from rural development programmes, training and transfer programmes (Carter et al., 2018; Mullally et al., 2021). However, how these household characteristics and factors interact with and develop in conflict settings remain unclear.

This paper contributes to closing these research gaps by testing (1) if agricultural asset transfers improve food and nutritional security in conflict-affected settings; (2) how contextual factors, particularly exposure to violent conflict, moderate the intervention impacts on food security. To do so, we use a panel household survey data from Syria based on a quasi-experimental design comparing treatment and control households before and after the intervention. The treatment households received an agricultural asset transfer in the form of vegetable seeds, agricultural tools and drip irrigation kits. We merge the panel survey data with additional information including the incidence of violent conflict and population data at the sub district-level. These additional variables enable us to explore the relationship between conflict exposure and treatment effect heterogeneity. We measure food security through the food consumption score (FCS).

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 conditional average treatment effect of the whole sample. Then, we examine whether and how contextual factors, particularly exposure to violent conflict, mediate treatment heterogeneity and how the predicted treatment effect varies across different contexts and characteristics. We find that agricultural asset support leads to significant and robust improvements in food security as measured by the food consumption score. In fact, agricultural asset support increases food security on average by about 9% on the FCS. Moreover, the treatment impact is strongly heterogeneous: Female-headed households in moderately conflict-affected settings benefit most from the intervention. Male-headed households living in regions with low or very high violent conflict show little or no treatment impacts. These findings provide novel insights on how agricultural asset transfers work in conflict settings and open the black box in better understanding how different levels of violent conflict among other factors shape the response and the success of agricultural interventions. It highlights that agricultural interventions based on one-size-fits-all solutions might not work as theorised. Understanding how different factors shape a programme's success and fine-tuning programming accordingly are key to reach and benefit all households living in challenging settings.

The remainder of this paper is structured as follows. Section 2 describes the intervention, the research design and the data. Section 3 introduces the methodological approach, particularly the honest causal forest algorithm. Section 4 presents the findings and section 5 discusses the results and concludes.

2. Intervention, research design and data

In this paper, we analyse the causal impacts of an emergency support in the form of an agricultural asset transfer including the provision of vegetable seeds, agricultural tools and drip irrigation kits provided by the UN Food and Agriculture Organization (FAO) in Syria. The support was provided in the third and fourth quarter of 2018. This intervention is part of a larger programme implemented by FAO, where other beneficiary households also received poultry kits, beekeeping support, seedlings, livestock vaccinations and livestock feed, and rehabilitation of irrigation systems addressing in total more than 30.000 households, all which are not part of this study. The agricultural asset transfer support was delivered to 3500 vulnerable smallholder farming households, prioritising female-headed households, residing in various regions of Syria, namely in the Governorates of Al-Hasakah and Deir-ez-Zor in the North-East, Aleppo in the North-West, and As-Sweida and Quneitra in the South-West.

Syria is affected by protracted violent conflict, macroeconomic instability and extreme weather shocks, which are the main drivers of food insecurity in the country (FAO et al., 2022). In 2022, 12 million people in Syria were threatened by food insecurity (WFP, 2022). Since the start of violent conflict in 2011, the level of food insecurity has increased immensely (FAO et al., 2022). Moreover, Syria's recurrent episodes of drought severely affected the agriculture sector and the access to drinking water (ibid). Additionally, the country is plagued by an economic crisis reinforced by the financial crisis in neighbouring Lebanon, which blocks large amounts of Syrian funds, high unemployment rates and inflation (ibid).

In order to analyse the causal impacts of the agricultural support on food security in Syria, we use a quasi-experimental design comparing households that received the intervention (the "treatment group") with households who did not receive the intervention (the "control group"). For the treatment assignment, we first identified potential beneficiaries per sub-district at baseline. Second, we randomly drew samples from these sub-districts proportionally representative of the full pool of beneficiaries. Third, we randomly selected a set of treatment villages from which the enumeration team selected every second household from the list of beneficiaries. Treatment households were randomly replaced if they could not be reached. Lastly, control villages and control households were selected on-site from nearby villages to ensure comparability while avoiding spillover biases. Within each sub-district, the enumerators selected control villages similar in number and size to the pre-identified beneficiary villages. Then, respondents were selected from each of the control villages based on the same eligibility criteria of the intervention villages. The purpose of this quasi-random sampling process is to build credible counterfactuals for the treatment group by ruling out any observed changes in outcomes among beneficiaries that are caused by seasonality or due to events that might skew the treatment effect during the implementation period, such as drought.

We collected a panel of household survey data including a baseline collected before the treatment was provided (November 2018), a midline, almost one year after the treatment was provided (January 2020) and an endline, almost two years after the support was provided (January 2021). All three waves were collected in the rainy autumn/winter season. Hence, we do not expect any notable challenges in comparability of the surveys due to seasonality between the waves, which could affect agricultural production and access to diverse food. The enumerators were trained before baseline and conducted the interviews with paper-based questionnaires. At baseline, the enumerators collected complete interviews from 684 control and 235 treatment households. The attrition rate is 12%. Female-headed households, not involved in crop farming are more likely to drop out of the survey. The final panel dataset consists of 591 control and 222 treatment households. Such a panel dataset is novel and unprecedented for Syria.

We use the Food Consumption Score (FCS) as an outcome indicator to measure food security. The FCS is calculated as the number of days in the past seven days during which food items from different food groups were consumed, and then weighted by their nutrient density. The indicator reflects dietary diversity as well as food frequency (WFP, 2008). The weighting for nutritive importance in FCS might skew the results since this discriminates, for example, against vegetables with the weight of one compared to meat with the 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 the average daily diversity score, which takes a value between 0 and 8.

For the moderating and explanatory variables, we selected household characteristics and contextual factors that we theorise to shape treatment heterogeneity in conflict settings. First, we account for household head characteristics that might drive differences in response to agricultural asset support, including gender, age, their literacy and if the household head's main occupation was crop farming or herding at baseline. Second, we include 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 cultivate a home garden. Third, we draw on self-reported experiences of shocks that affect farm productivity. Households were asked if they experienced the following shocks in the previous twelve months before endline: 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. Fourth, we use publicly available information on conflict incidence and intensity from ACLED - Armed Conflict Location & Event Data Project (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 survey to measure the short-term and medium-term exposure to violent conflict, respectively. This allows us to test the moderation effects of violent conflict on the programme's success in improving food security.

The number of fatalities per 100.000 inhabitants through violent conflict is the most adequate available indicator of conflict, since it accounts for event severity relative to the population size. Fifth, and complementary to the above, we merged estimated information on demographic characteristics provided by FAO, also on sub-district 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. Table 1 lists all included covariates.

Table 1 gives an overview over these covariates in our sample. The share of female household heads is 33% in the treatment group and 17% in the control group. The share of illiterate household heads is 30% in the treatment group and 22% in the control group. The household heads are on average 49-50 years old in both groups, around 91% do crop farming and 35% do herding. Both groups own on average about 0.3 ha of irrigated land and 0.7 ha of rainfed land at baseline. 21% of the treatment households and 29% of the control households faced constraints to water at baseline. Around 20% of the whole sample kept chicken, 13% kept cattle and 21% kept sheep at baseline. 50% of the control group owned a home garden while 68% of the treatment group had a home garden at baseline.

Table 1: Sample balance between treatment and control households.

	Control	Treatment	p-value
N	591	222	
Individual and household- level characteristics			
Prop. of female HHH	0.17 (0.38)	0.33 (0.47)	<0.001
Age of HHH (years)	49.53 (12.90)	49.54 (12.62)	0.989
Prop. of illiterate HHH	0.22 (0.42)	0.30 (0.46)	0.018
Prop. of crop farmers	0.90 (0.30)	0.92 (0.27)	0.457
Prop. of herders	0.36 (0.48)	0.33 (0.47)	0.354
Average irrigated land size at baseline (ha)	0.31 (0.60)	0.25 (0.53)	0.212
Average rainfed land size at baseline (ha)	0.76 (1.41)	0.66 (1.39)	0.366
Prop. with constraints to water at baseline	0.29 (0.45)	0.21 (0.41)	0.026
Prop. that own chicken at baseline	0.22 (0.42)	0.17 (0.38)	0.114
Prop. that owns cattle at baseline	0.15 (0.36)	0.11 (0.31)	0.133
Prop. that owns sheep at baseline	0.20 (0.40)	0.23 (0.42)	0.455
Prop. that owns home garden at baseline	0.50 (0.50)	0.68 (0.47)	<0.001
Exogenous shocks 12 months before endline			
Drought	0.10 (0.30)	0.12 (0.32)	0.474
Crop pests	0.09 (0.29)	0.09 (0.28)	0.688
Livestock Disease	0.11 (0.31)	0.07 (0.25)	0.071
High input costs	0.79 (0.41)	0.82 (0.38)	0.257
Low output price	0.12 (0.33)	0.14 (0.35)	0.397
Illness income earner	0.05 (0.23)	0.02 (0.15)	0.054
Theft of agricultural assets	0.03 (0.17)	0.03 (0.18)	0.937
Direct and indirect exposure to conflict at sub-district level			
Prop. widowed people (2019)	0.05 (0.04)	0.05 (0.03)	0.920
Prop. females (2019)	0.50 (0.03)	0.51 (0.02)	0.065
Prop. disabled people (2019)	0.24 (0.08)	0.24 (0.08)	0.733
Fatalities through violent events in month before endline (per 100.000 inhabitants)	10.93 (34.70)	3.97 (17.50)	0.004
Fatalities through violent events in year before endline (per 100.000 inhabitants)	80.04 (145.18)	77.58 (147.94)	0.830

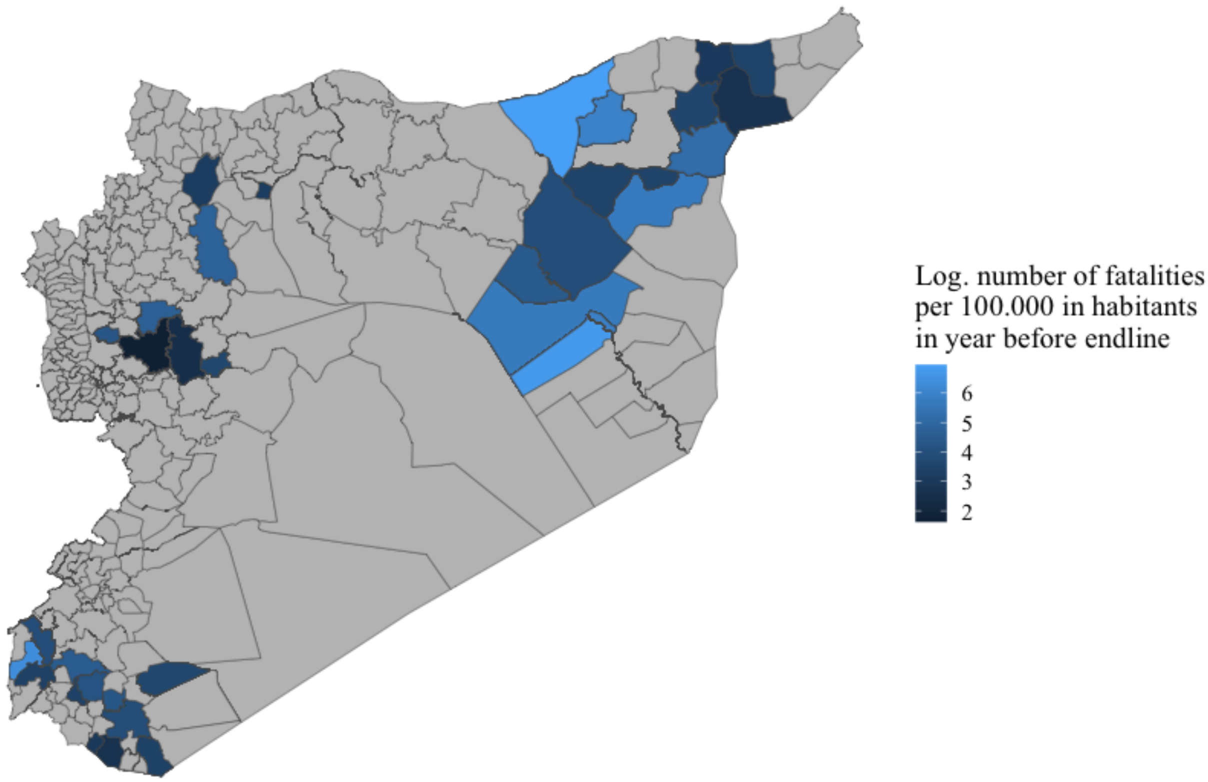
Notes. Standard deviations in parenthesis, p-values based on t-tests. HHH=household head, HH=household, prop=proportion.

During the 12 months before the endline survey, large shares of the households experienced shocks. Around 80% of households faced high agricultural input costs, which is driven by the hyperinflation taking place in Syria in 2020 at the endline of this study (World Bank, 2022b). Around 11% of the households were affected by drought and 9% faced crop pests. In the control group, 11% were affected by livestock diseases and 5% faced a higher incidence of ill income earners compared to the treatment group with 7% and 2%, respectively. Around 13% of the sample households faced low output prices and 3% experienced theft of agricultural assets.

From the data on sub-district level, we see a slight overrepresentation of females in the treatment sub-districts with 51% on average compared to 50% in the control group. The share of people with disabilities of 24% is remarkably large while the share of widowed people of 5% is remarkably low. Treatment households witnessed on average 4 fatalities through violent events per 100.000 inhabitants in their sub-district in the month before endline compared 11 fatalities per 100.000 on average in the sub-districts of the control group. In the year before endline, households faced on average 77-80 fatalities per 100.000 inhabitants through violent events in their sub-district.

Figure 1 shows the 43 studied sub-districts with the corresponding logarithmic number of fatalities through violent events per 100.000 inhabitants from our sample. The figure indicates the large degree of variation in exposure to violence within regions. This is a necessary condition for our study for assessing the mediating effect of exposure to violence since the captured correlation subject to exposure to violence is less connected to regional fixed effects.

Figure 1. Map of Syria illustrating number of fatalities through violent events at sub-district level (per 100.000 inhabitants, logarithmic).



Comparability between the control and the treatment group is key to construct credible counterfactuals in the absence of a random assignment. Systematic imbalances across the two groups might lead to biases in impact estimates. In our sample, we found some statistically significant imbalances as shown in Table 1. Most remarkably, the shares of households with female household heads and those with access to a home garden are significantly higher in the treatment group than in the control group (p<0.01). Furthermore, there are significant disparities in literacy of the household head, the household's constraints to water, the exposure to livestock diseases, and the incidences of violence one month before endline at the sub-district level. The revealed sample differences underscore the need to account for these disparities. In the next section, we will present a non-parametric method that deals with this sample imbalance and construct valid counterfactuals.

3. Methodological approach

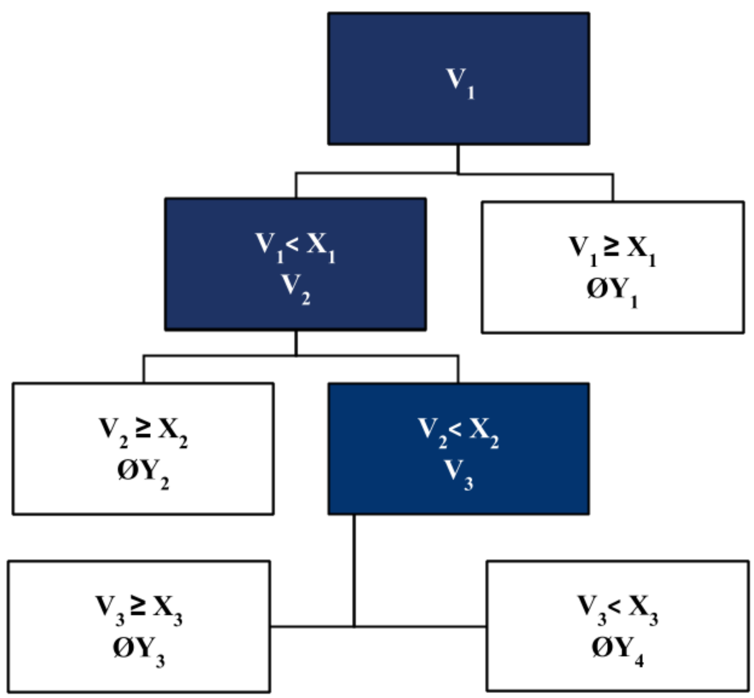
In order to analyse the causal impact of the intervention, we use the honest causal forest algorithm (Athey and Imbens, 2016). We assess the conditional average treatment effects (CATE) through a machine learning algorithm stemming from the family of generalised random forests, which divides the data via recursive partitioning into subgroups based on their predicted treatment effect size. This allows us to examine if the treatment had any heterogeneous impact on our outcome of interest as well as to disentangle the characteristics and contextual factors that moderate the treatment heterogeneity, if it exists.

The honest causal forest consists of many honest causal trees. To fit a causal tree, the data are split into two equal halves. In the first half, the splitting sample, the data are divided via recursive partitioning. The observations are grouped in the tree leaves, maximising the predicted treatment effect size difference between subgroups. The splitting criteria are assigned based on covariate values. Like this, the variance of the treatment effect is minimised within one leaf, so that the observations within one leaf resemble a randomised treatment assignment. With the second half of the data, the CATE is derived by running the data through the trees defined by the first half and then subtracting the change in the outcome of the treated observations from the control observations within each leaf (Athey & Imbens, 2016; Wager & Athey, 2019). Equation (1) displays the derivation of the CATE denoted here by $\hat{\tau}$. W is the treatment dummy, which takes a value of 1 for treatment households, Y is the outcome variable, X the covariate matrix, and L is a subgroup leaf within one tree (Wager & Athey, 2018).

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

Figure 2 provides an illustration of the algorithm procedure. The sample is split through recursive partitioning subject to the covariate variables V_i. Depending on the observations' covariate value being lower or higher/equal to a certain threshold X_i, the observations are partitioned into subgroups. $ØY_i$ represents the subgroup average treatment effect.

Figure 2. Illustration of an honest causal tree.



Notes. Graphic illustrates the construction of an honest causal tree. V_i = splitting variable, X_i= splitting threshold, $ØY_i$= subgroup conditional average treatment effect.

The causal forest aggregates the average of the treatment effect predictions from the different trees (Athey et al., 2019). In our model, we weight the CATE by augmented-inverse propensity weighting, which weights the observations by overlap in key covariates, which are additionally weighted by their importance in treatment heterogeneity (ibid.). This method is particularly adequate for non-randomized samples, like ours, because through these double robust estimators, we ensure a consistent estimation as long as either the propensity score model or the regression model is correctly specified (Athey et al., 2019, Glynn and Quinn, 2010). In other words, this method reduces the treatment assignment bias.

For estimating 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 tree leaves. We select the time-invariant information from the endline survey, except for those variables that are potentially affected by the intervention, which were selected 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 take the endline values since 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.

The algorithm provides three estimates for CATE: 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 1, the average treatment effect of the overlapped sample is the most precise estimate to predict the treatment effect because it constructs the counterfactuals 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 all 24 variables that we theorise 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 applying the tuning configuration, the algorithm determines the optimal shaping parameters for the model. The tuned version only includes covariates that are relevant for model heterogeneity. First, we examine how the CATE is spread around the predicted average treatment effect of the whole sample. Second, splitting the sample into observations with high, medium and low CATE, we examine mean characteristics of households who benefited most and least from the intervention. We test if the averages differ significantly between the groups by Pearson's chi-square tests or ANOVA tests, depending on the variable type. Third, we split the sample by exposure to conflict and the gender of the household head and calculate the within group average treatment effects.

The honest causal forest algorithm has several advantages compared to other methods which would be applied for our case otherwise: First, the honest causal forest algorithm works non-parametrically. The drivers of heterogeneity do not have to be defined ex-ante because the algorithm is able to test a wide range of coefficients relative to the overall sample size (Athey & Imbens, 2016). The algorithm sorts out uninformative variables (Storm et al., 2020). This is a great advantage for our setup since we test 24 covariates while only having observations from 813 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 causal forest algorithm can cope with sample imbalances between the treatment and control group because the observations are matched with each other based on their characteristics in the leaves, in a similar fashion to other non-parametric approaches like kernel methods and nearest-neighbour matching (Wager and Athey, 2018). However, due to the additional weighting of the covariates by their importance, the 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 are able to assess non-linear relationships between the variables. This is particularly relevant in our setup since agricultural interventions are known to show complex treatment interaction effects (Storm et al., 2020). Fourth, the data splitting makes the approach honest: the shape of the model is defined by the spelling data and, therefore, exogenous for 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 are used for estimation (Athey & Imbens, 2016).

4. Results

4.1. Average treatment effects

Table 2 displays the coefficients of the outcomes using augmented inverse-propensity weighting. Here, we include three specifications: ATE - average treatment effect on the overall sample (column 1), ATT - average treatment effect on the treated (column 2), and ATO - average treatment effect on the overlapping sample (column 3). First, we estimate an ATE of 5.3 points ($p<0.01$) and an ATT of 5.0 points ($p<0.01$) of the intervention on food security as measured through FCS. Only including the overlapped observations, we find an ATO of 4.6 points ($p<0.01$) on the FCS scale, which is an increase by 9% from the baseline values of the treatment group. Columns (4) and (5) display calibration tests. The mean forest prediction indicates that the tuned model is fitted well to assess the treatment impact for FCS ($p<0.01$). The differential forest prediction emphasises treatment effect heterogeneity ($p<0.01$).

Table 2. Average treatment effects of the intervention using honest causal forest.

	(1) ATE	(2) ATT	(3) ATO	(4) Mean forest prediction	(5) Differential forest prediction
FCS	5.259*** (1.218)	4.985*** (1.525)	4.617*** (1.653)	1.126*** (0.342)	1.189*** (0.258)

*Notes. Standard errors in parenthesis; * =p<0.1, **=p<0.05, ***=p<0.01. ATE=Average treatment effect, ATT= Average treatment effect of the treated, ATO= Average treatment effect of the overlapped sample. Model with tuned parameters. The coefficients for the mean forest prediction in column 4 indicates the goodness of the CATE prediction, while the differential forest prediction in column 5 indicates if the model assesses heterogeneity appropriately. A value of 1 in both predictions indicates a precise estimation.*

4.2. Heterogeneous treatment effects

We detect heterogeneity in the predicted treatment effects of the food consumption score. In this section, we examine the treatment heterogeneity to understand which covariates play a key role, as well as how the impact varies across different subgroups.

We begin with examining the drivers of treatment heterogeneity. In Table 3, we display the frequency with which the covariates are used for splitting the initial causal forest. The most frequently applied covariate is the household head's age which was used for 19.5% of the splits. Another relevant variable is the gender of the household head, driving 7% of the splits. We see that both measures of exposure to violent conflict at the sub-district level are used for 24.4% of the splits. Taken together with the shares of disabled, widowed and female individuals at the sub-district - which are indirect indicators for a region's exposure to conflict - direct and indirect conflict exposure variables account for 50.3% of the splits. Furthermore, agricultural asset indicators like owning sheep or a home garden and land size at baseline are less frequently used for the splitting process. Particularly striking is that exogenous shocks during the past 12 months are seldom applied in the splitting process. In other words, these shocks are not driving impact heterogeneity in our sample.

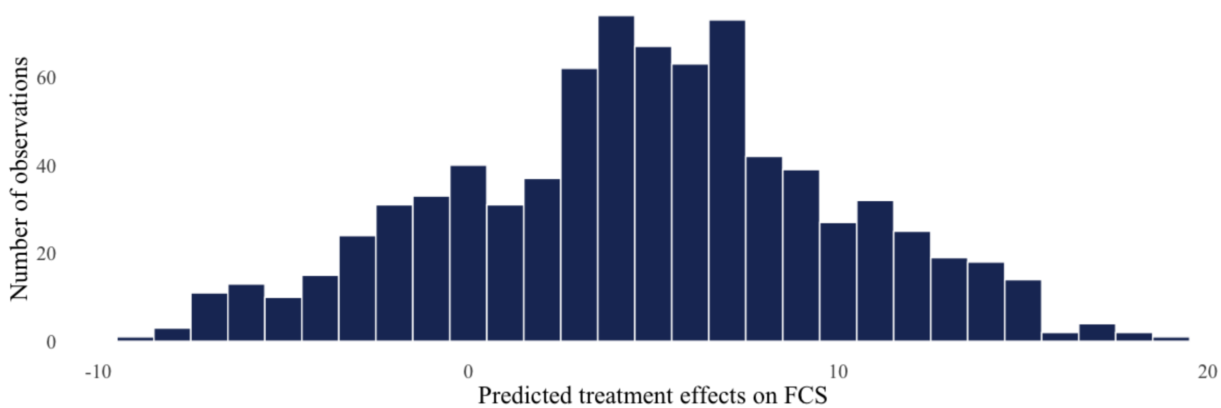
Table 3. Frequency of variable selection for splitting.

Individual and household- level characteristics	**44.5%**	**Exogenous shocks 12 months before endline**	**5.1%**
HHH age	19.5%	Crop pests	2.4%
Female HHH	7.0%	Drought	1.0%
Own sheep	4.0%	Low output prices	<1%
HHH is herder (baseline)	1.6%	High input costs	<1%
Own home garden	3.4%	Livestock disease	<1%
Rainfed land size	3.1%	Illness income earner	<1%
Irrigated land size	2.2%	Theft of agricultural assets	<1%
Own chicken	1.8%	**Direct and indirect exposure to conflict at sub-district level**	**50.3%**
Illiterate HHH	<1%	Fatalities through violent events (last year, per 100.000)	13.7%
HHH is crop farmer (baseline)	<1%	Prop. widows	11.0%
Constraints to water	<1%	Prop. disability	11.0%
Own cattle	<1%	Fatalities through violent events (last month, per 100.000)	10.7%
		Prop. females	3.9%

Notes. The assignment for splitting is based on the initial model. A high frequency in splitting alone should not be interpreted as an indicator for treatment mediation, since many covariates are highly correlated. Correlated variables are likely to be substitutes for splitting.

Next, we show the distribution of the predicted CATE based on the constructed counterfactual within the tree leaves from our tuned honest causal forest model for FCS (Figure 3). As expected, we detect a concentration in predicted treatment effect size spread between 0 and 10 points, which aligns with the average treatment effect. However, the dispersion in the distribution of the predicted CATE underscores that a notable number of households did not benefit, and even were harmed. We observe a notable amount of predictions with a negative treatment effect size down to -10 points on the food consumption score. Likewise, there are households who benefit a lot with a treatment effect size of up to 20 points.

Figure 3. Distribution of predicted conditional treatment effects on FCS.



To better understand which households benefited most or least from the intervention, we now examine the households' characteristics and contextual factors according to their predicted CATE. To do so, we divide the sample by its predicted treatment effect size into three equal sized groups. In Table 4, we calculate the within-tercile averages for the covariates that we found to be applied for at least 1% of the splits in Table 3 separately. We find that the CATE on FCS is -1.1 points for the low tercile (*Low CATE*), 4.9 points for the middle tercile (*Medium CATE*), and 10.4 points for the highest tercile (*High CATE*).

Households who benefit most from the agricultural asset transfer support are more likely to be female-headed. We find that 38.4% of households in *High CATE* are female-headed, compared to 8.5% in *Low CATE*. The mean age of the household head is 6 years younger in *High CATE* and

Medium CATE compared to *Low CATE*. Furthermore, households with strong initial capital and agricultural endowments (such as owning livestock and home gardens) are more likely to benefit from the intervention. The intervention also benefited households who faced issues with crop pests and households who did not experience drought episodes in the past 12 months.

Table 4. Comparison of household characteristics and contextual factors according to predicted treatment effect size.

	Low CATE	*Medium CATE*	*High CATE*	p-value
CATE	-1.108 (0.144)	4.877 (0.144)	10.37 (0.144)	
Individual and household- level characteristics				
Prop. of female HHH	0.085 (0.024)	0.192 (0.024)	0.384 (0.024)	<0.01
Age of HHH (years)	56.06 (0.734)	49.99 (0.734)	49.07 (0.734)	<0.01
HHH is herder (baseline)	0.262 (0.029)	0.428 (0.029)	0.373 (0.029)	<0.01
Size of rainfed land (ha) (BL)	0.411 (0.084)	1.038 (0.841)	0.761 (0.084)	<0.01
Size of irrigated land (ha) (BL)	0.317 (0.036)	0.287 (0.036)	0.284 (0.036)	0.763
Prop. that owns sheep (BL)	0.151 (0.025)	0.229 (0.025)	0.244 (0.025)	0.018
Prop. that owns chicken (BL)	0.114 (0.024)	0.225 (0.024)	0.284 (0.024)	<0.01
Prop. that owns home garden (BL)	0.347 (0.029)	0.568 (0.029)	0.738 (0.029)	<0.01
Exogenous shocks 12 months before endline				
Crop pests	0.026 (0.017)	0.044 (0.017)	0.207 (0.017)	<0.01
Drought	0.162 (0.018)	0.089 (0.018)	0.063 (0.018)	<0.01
Direct and indirect exposure to conflict at sub-district level				
Prop. females (2019)	0.505 (0.002)	0.503 (0.002)	0.504 (0.002)	0.797
Prop. disabled people (2019)	0.257 (0.005)	0.253 (0.005)	0.217 (0.005)	<0.01
Prop. widowed people (2019)	0.037 (0.002)	0.051 (0.002)	0.054 (0.002)	<0.01
Fatalities through violent events in month before endline (per 100.000 inhabitants)	20.16 (1.661)	12.27 (1.661)	6.94 (1.661)	<0.01
Fatalities through violent events in year before endline (per 100.000 inhabitants)	237.9 (15.71)	172.9 (15.71)	65 (15.71)	<0.01

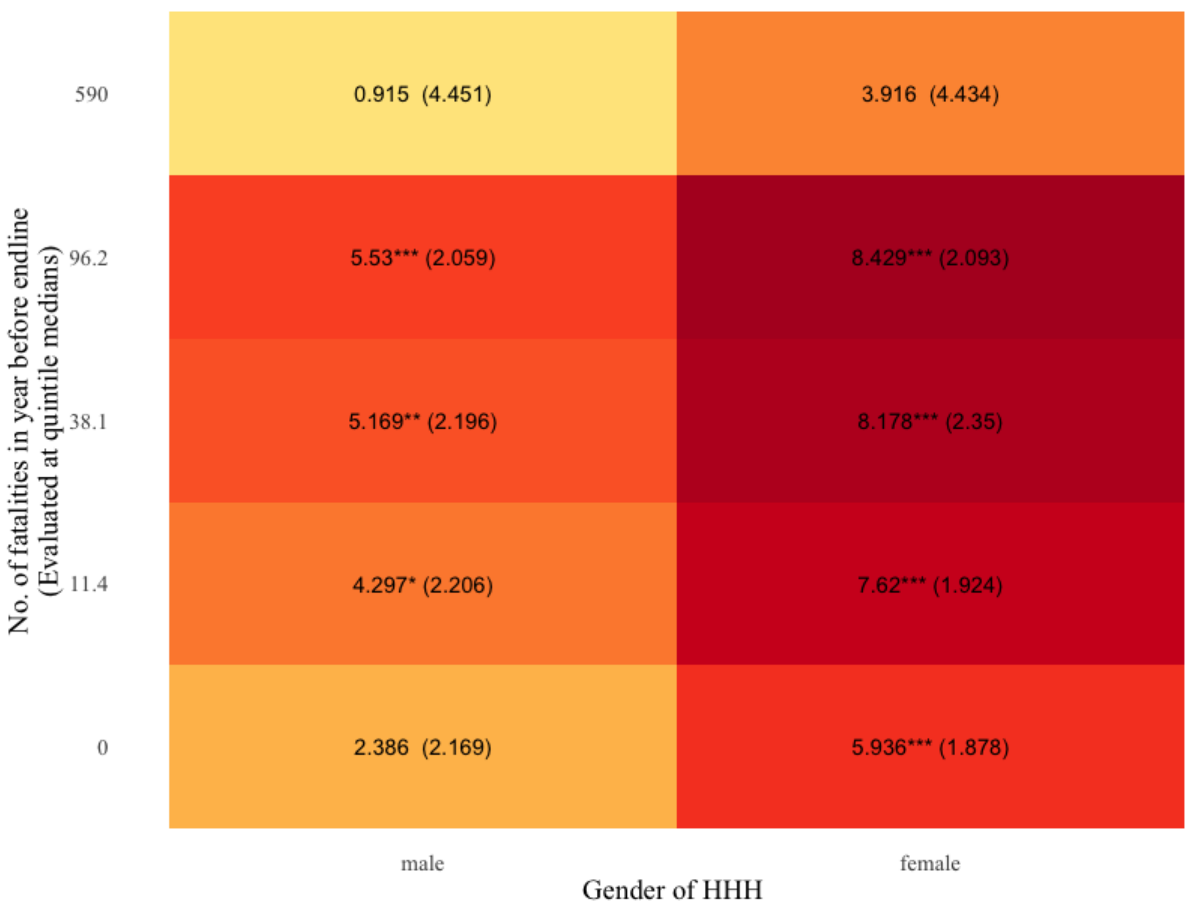
Notes. BL=Baseline, Within-group means based on partitioning through predicted treatment effect size. Standard errors in parenthesis. P-values for binary variables from Pearson's chi-square tests, for continuous variables through ANOVA tests. Table only includes variables that are applied for at least 1% of the initial model splits (see Table 3).

Moreover, we observe that the sub-district level share of the population with disabilities is marginally lower in the *High CATE* while the share of widowed individuals is higher. The latter implies that particularly in areas with a high share of conflict-affected and vulnerable households, agricultural asset transfers are beneficial. Finally, both indicators for direct exposure to violence show that the average number of fatalities from violent events per 100.000 inhabitants is significantly higher for *low CATE*.

In summary, the heterogeneous findings emphasise that agricultural asset transfer benefits younger female-headed households with agricultural capital endowments who were not exposed to intense levels of violent conflict. To better understand how different levels of intensity of violent conflict moderate treatment heterogeneity, we further divided the sample into quintiles based on the number of fatalities per 100,000 in the past year. We also divide this sample by the gender of the household-head to understand how the household's profile shapes the treatment response under different levels of exposure to violent conflict. Figure 4 displays the predicted conditional average treatment effect for each subgroup while holding all other covariates fixed at their medians.

First, we observe that female-headed households strengthen their food security compared to male-headed households as a result of receiving support, regardless of the intensity of violent conflict. Second, when the intensity of violent conflict is highest, we find no significant impacts of asset transfers on FCS. Similarly, at very low levels of conflict exposure, the predicted treatment effect size is small and insignificant for male-headed households at 2.4 points. For female-headed households, the CATE at low levels of exposure to violent conflict is at 5.9 points ($p<0.01$). This implies that on average, male-headed households who experienced few or no episodes of violent events in the past 12 months, did not benefit from the intervention. Third, both male- and female-headed households who experienced moderate levels of violent events in the past 12 months, as shown in the three middle quintiles, benefited most from the intervention. Female-headed households who experience moderate to high levels of (quintile 4) improved their food security the most due to the intervention.

Figure 4. CATE by gender of the household head and incidence of violence.



*Notes. CATE for No. of fatalities through violent events per 100.000 inhabitants split by quintiles and gender of the household head keeping other covariates fixed at their median. Standard errors in parenthesis. * =p<0.1, **=p<0.05, ***=p<0.01. CATE is based on the adjusted honest causal forest model* *with tuned parameters. 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 chicken 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.*

4.3. Robustness tests

To verify the validity of our findings, we conducted several robustness tests. First, we run the analysis using a fixed effects model to check if both approaches lead to the same results. The advantage of a fixed effects estimation is that it accounts for all unobserved time-fixed confounders within each household. To ensure comparability between the two models, we balanced the covariates between the treatment and the control groups by matching households using nearest-neighbour propensity scores. Figure A1 in the appendix shows the covariate balance before and after matching. We also control for conflict exposure and the same household-level covariates that were included in the forest estimation. Hence, the estimates of the matched fixed-effect model can be compared to those from the overlapped honest causal forest model. Column (1) in Table 5 shows that the fixed effects model estimates a treatment effect of 5.6 points which corresponds to an increase of 11% in food security from baseline values of the beneficiary group. This result is broadly in line with our main estimations. The subgroup analysis emphasises stronger treatment effects for female-headed households and households exposed to moderate levels of violent conflict. The direction and significance of the findings align with our estimate from the honest causal forest model, but the effect sizes on food security are much larger, which could be driven by the small sample sizes. The subgroup analysis of the fixed-effect linear model should only be used indicatively since small sample sizes in linear models could lead to biased estimates. This also reiterates the reliability and validity of the honest causal forest specification for heterogeneous analysis with small sample sizes.

Table 5. Average treatment effects on FCS with fixed effects model.

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	Female household head	Male household head	Low conflict	Moderate conflict	High conflict
ATE	5.612*** (2.151)	20.832*** (3.955)	-0.055 (2.575)	-0.941 (3.215)	10.709*** (3.447)	5.873 (4.796)
n	888	272	616	296	296	296

*Notes. Fixed effects model. Standard errors in parenthesis. * =p<0.1, **=p<0.05, ***=p<0.01. HHH= household head, HH=household. Control variables: gender, age, literacy of the HHH, if they are crop farmers or herders, size of owned irrigated and rainfed land, if they face constraints to land water access, if the HH owns chicken, sheep or cattle. Matching variables: gender, age, literacy of the HHH, if they are crop farmers, if the HH had a home garden, size of owned irrigated land, size of owned rainfed land, if they face constraints to land water access, if the HH owns chicken, if the HH was exposed to drought, high agricultural input costs, livestock diseases or a severe illness of an income earner in year before endline.*

Second, we run the analysis on the unweighted food consumption score instead of FCS, as an alternative measure of food security. Row (1) of Table 6 displays the average treatment effects on the unweighted score, which are similar in direction and magnitude to FCS. The ATO translates into an increase in food security by 7% compared to baseline values of the treatment group compared to 9% for FCS.

Third, we test if the disparity between the control and treatment group at baseline could have biased our findings. In addition to the robust augmented-inverse propensity weighting for sample imbalance, we run our honest causal forest model with only the matched sample. Row (2) of Table

6 shows that the estimates using matched data are closely similar to our main specification, detecting an increase of the FCS of the beneficiary group by 8%.

Finally, in our main model, we included households from sub-districts, from which we do not have both, treatment and control observations. Regional disparities can create sample imbalances in unobserved variables. Row (3) in Table 6 shows the estimates of the model where we only use observations from sub-district containing both treatment and control households. Despite a small reduction in the effect size, the sign and significance of the impact remain the same.

Table 6. Average treatment effects with different model specifications.

		(1)	(2)	(3)	(4)	(5)
	Model specification	ATE	ATT	ATO	Mean forest prediction	Differential forest prediction
(1)	Outcome: unweighted FCS (N=813)	0.312*** (0.067)	0.313*** (0.082)	0.282*** (0.089)	1.108*** (0.293)	1.347*** (0.292)
(2)	Matched sample (N=444)	4.94*** (1.637)	5.077*** (1.989)	3.953** (2.109)	1.09*** (0.462)	0.911*** (0.32)
(3)	Sub-district overlap (N=539)	2.993** (1.51)	3.961*** (1.658)	2.868** (1.707)	0.976** (0.554)	1.249*** (0.302)

*Notes. Standard errors in parenthesis; * =p<0.1, **=p<0.05, ***=p<0.01. ATE=Average treatment effect, ATT= Average treatment effect of the treated, ATO= Average treatment effect of the overlapped sample. Model with tuned parameters. The coefficients for the mean forest prediction in column 4 indicates the goodness of the CATE prediction, while the differential forest prediction in column 5 indicates if the model assesses heterogeneity appropriately. A value of 1 in both predictions indicates a precise estimation.*

5. Discussion

Using panel survey data from Syria, we examine if an agricultural asset transfer intervention improves food security of households living in a conflict-affected setting and show how household characteristics (such as gender and age of household) and contextual factors (such as conflict intensity) shape treatment effects. We find that households living in conflict settings who receive agricultural assets such as seeds and tools exhibit improvement in their food security by around 9%. Moreover, we find that these effects are more pronounced for younger female-headed households living in areas with moderate levels of conflict intensity.

The asset transfer intervention was designed as an emergency support, with a targeting prioritisation for women. Hence, per design, the intervention has worked as intended for the selected target groups. On one hand, our results are aligned with evidence from other settings which show that women benefit considerably from small-scale asset transfer interventions (Baliki et al., 2022; 2019), particularly in improving nutritional and food security outcomes (Ruel et al., 2018, Anderson et al., 2021). On the other hand, the findings underscore the importance of incorporating the contextual heterogeneity in designing and testing agricultural interventions aiming to improve food security. This implies that theories of change of simple asset transfer interventions in volatile and challenging settings might not always work as theorised. Recent evidence shows that women behave differently in times of crises (Justino, 2018). Our paper additionally shows that they also respond differently to agricultural aid, and that such response is shaped by experiences of violent conflict.

Moreover, in contrast to other work from non-conflict settings, our findings show that households with initial endowments in agricultural capital benefited more from agricultural support than resource-poor households (Carter et al., 2018; Mullally et al., 2021). We also find that households exposed to high levels of violent conflict did not benefit from the intervention. Taken together, these results show that agricultural support might not be the right tool to improve the livelihood and food security of resource-poor households living under extreme stress, where farm production and productivity is low (George & Adelaja, 2021). Other types of support such as food aid or cash transfers are more effective in improving nutritional and food security outcomes in emergency and high intensity conflict-affected settings (Altındag & D. O'Connell, 2022; Brück et al., 2019a; Ecker et al., 2019; Kurdi, 2021; Tranchent et al., 2019; Tusiime et al., 2013).

Our results also show that asset transfers alone are not sufficient to generate meaningful impacts for households residing in relatively peaceful areas, where more resilience-building agricultural approaches can have stronger impacts (Arias et al., 2019; Bozzoli & Brück, 2009). Future research should explore in detail the role of other key contextual factors that can potentially shape treatment heterogeneity of agricultural asset transfer interventions, such as access to markets or to access critical infrastructure. To that end, our study generates novel insights on the importance of incorporating heterogeneity in understanding if and how agricultural asset transfer programming works in conflict-affected settings. We provide novel evidence from a country which remains markedly understudied, and also apply the honest causal forest approach which helps us to unravel the impact of heterogeneity in difficult conflict-affected settings.

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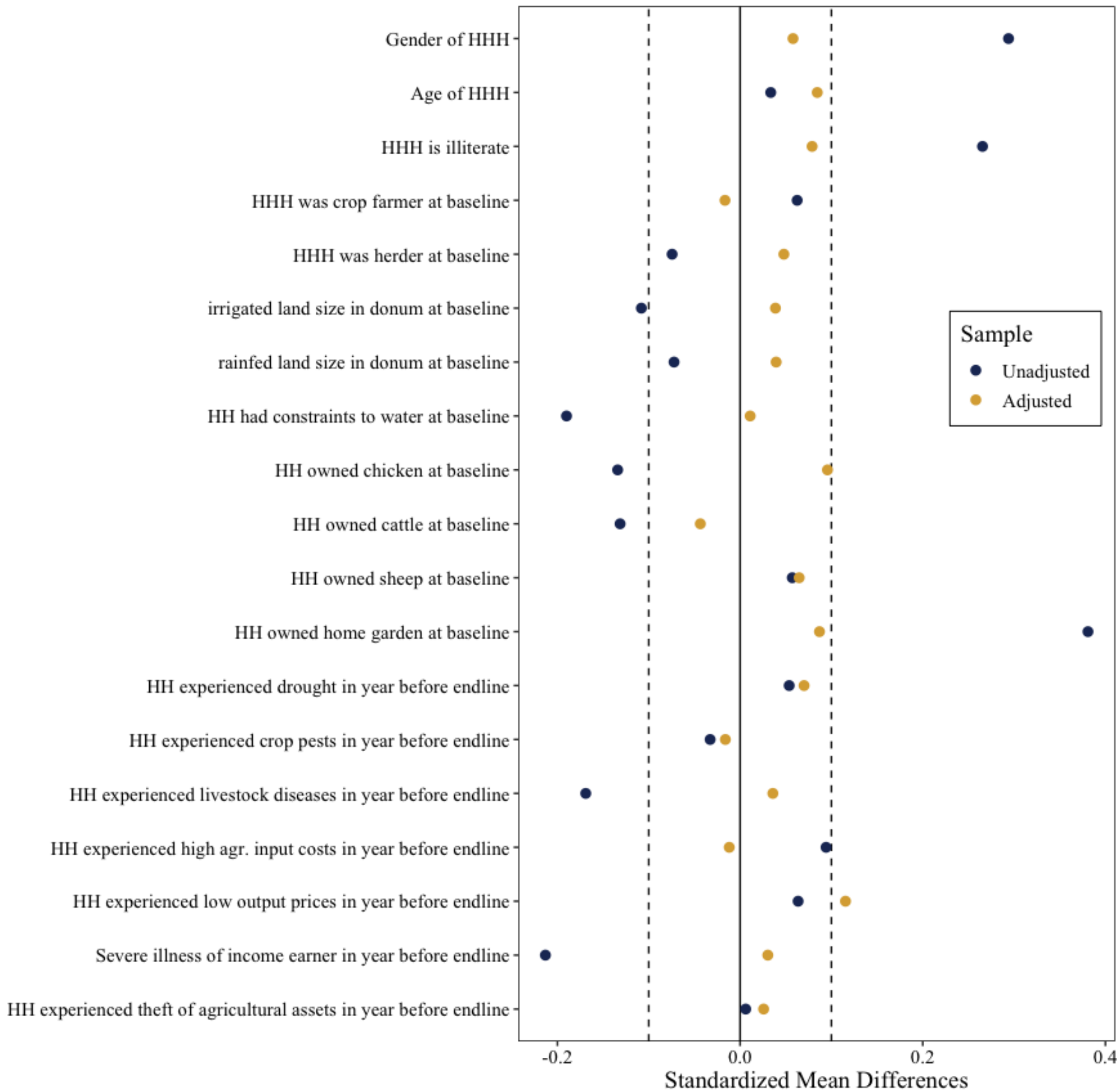
Appendix

Figure A1. Covariate balance of unadjusted and matched sample



Notes: HHH=household head, HH=household. Matching variables: gender, age and literacy of the HHH, if they are crop farmers, if the HH had a home garden, size of owned irrigated and rainfed land, if they face constraints to land water access, if the HH owns chicken, if the HH was exposed to drought, high agricultural input costs, livestock diseases, a severe illness of an income earner or theft of agricultural assets in year before endline.