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The Journal of Development Studies

ISSN: 0022-0388 (Print) 1743-9140 (Online) Journal homepage: https://www.tandfonline.com/loi/fjds20

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To cite this article: Tilman Brück, Oscar Mauricio Díaz Botía, Neil T. N. Ferguson, Jérôme Ouédraogo & Zacharias Ziegelhöfer (2019) Assets for Alimentation? The Nutritional Impact of Assets-based Programming in Niger, The Journal of Development Studies, 55:sup1, 55-74, DOI: 10.1080/00220388.2019.1687876

To link to this article: https://doi.org/10.1080/00220388.2019.1687876

The Journal of Development Studies, 2019
Vol. 55, No. S1, 55–74, https://doi.org/10.1080/00220388.2019.1687876

Assets for Alimentation? The Nutritional Impact of Assets-based Programming in Niger

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ABSTRACT *A recent strand of aid programming aims to develop household assets by removing the stresses associated with meeting basic nutritional needs. In this study, the authors posit that such nutrition-sensitive programmes can reduce malnourishment by encouraging further investment in diet. To test this hypothesis, they analyse the World Food Programme's (WFP) Protracted Relief and Recovery Operation (PRRO), in Niger, a conflict-affected, low-income country with entrenched food insecurity. Under the PRRO, a household falls into one of three groups at end line: receiving no assistance, receiving nutrition-specific assistance, or receiving nutrition-specific assistance and nutrition-sensitive food for assets-based programming. If provided alone, food aid has no nutritional impact relative to receiving no assistance. However, the study observes pronounced positive effects if food aid is paired with assets-based programming. The authors conclude, first, that certain forms of food aid function well in complex, insecure environments; second, that assets-based programmes deliver positive nutritional spillovers; and, third, that there are theoretical grounds to believe that assets-based nutrition-sensitive programmes interact positively with nutrition-specific programming.*

1. Introduction

The theories of change that link food aid to improved nutrition are intuitive (Barrett & Maxwell, 2007; Kennedy & Alderman, 1987; Maxwell & Singer, 1979), but empirical studies share three features that inhibit generalisability. The first is that most look at the performance of food aid in the aftermath of shocks (del Ninno, Dorosh, & Subbarao, 2007; Gilligan & Hoddinott, 2007; Quisumbing, 2003; Tusiime, Renard, & Smets, 2013; van der Veen & Gebrehiwot, 2011; Yamano, Alderman, & Christiaensen, 2005). By contrast, less is known about how food aid performs in situations of chronic malnutrition. The second is a tendency to focus on nutrition-specific programming (IOD PARC, 2014; van der Veen & Gebrehiwot, 2011; World Food Programme [WFP], 2016), which aims to address the immediate determinants of nutrition, such as calorie intake. There is macro-level evidence on the positive outcomes of nutrition-sensitive aid, which aims to boost nutrition by targeting the root causes of malnutrition (Mary, Saravia-Matus, & Paloma, 2018; Ruel & Alderman, 2013), but less is known about how these outcomes are supported at the programme level.[1] The third is a focus on scenarios that are ideal for food aid to perform well (IOD PARC, 2014; WFP, 2016), specifically, locations with stable populations, access to health centers, and no threats of fragility.

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This study poses three research questions to address these concerns. First, it tests the performance of a nutrition assistance programme in the context of chronic food insecurity. Second, it compares the relative performance of nutrition-specific and asset-based nutrition-sensitive programme arms. Third, it contextualises the findings in a highly fragile environment. It hypothesises that gains from direct food provision will be lost once the programme cycle ends because the causes of food insecurity have not been tackled. By contrast, the increase in assets provided by nutrition-sensitive interventions adds to household wealth and therefore to the capacity of households to sustain gains. Because assets can be liquidated to provide coping strategies, this form of provision is well placed to overcome external risks in highly fragile environments, which can interrupt both the supply of and the demand for nutrition-specific aid.

To test these hypotheses, the study investigates the World Food Programme's (WFP) Protracted Relief and Recovery Operation (PRRO) in Niger. From 2014 until 2016, the PRRO provided poor households with ready-to-use therapeutic food (RUTF). A subset of these households also benefited from a Food for Assets (FFA) Programme, which builds assets, first, by providing seedlings to households, which then grow trees that can be sold or used as lumber and, subsequently, through the rehabilitation of the land on which the trees are grown. The study collected two waves of household panel data, the first shortly before the programme began and the second three months after it finished. At end line, households fall into three distinct groups: those that received no assistance in the previous year, those that received only nutrition-specific assistance, and those that received both nutrition-specific assistance and nutrition-sensitive assistance.

Rollout could not be randomised because of the complex scenario, and sample attrition was comparatively high, which is not atypical in difficult environments. For these reasons, the study adopted a tailored set of econometric analyses to mitigate biases. It constructed two versions of the database: a child-level panel, on which the main analyses were based, and a household panel, in which nutrition indicators are averaged across all children in the household. The study used propensity score matching (PSM) approaches to account for non-random rollout, with an instrumental variable (IV) based on the spatial lag of insecurity used to ensure robustness. Attrition is treated as a Heckman-style selection problem. A quirk of the scenario studied provides the criterion. Households in villages scheduled for enumeration on Fridays are significantly more likely to leave the sample than households scheduled on other days. Niger has a majority Muslim population and a high degree of religiosity, resulting in reduced productivity (including among the enumerators) during the Jumu'ah (Friday prayer).

Using these corrections, the analyses follow difference in difference estimations to test for the relative performance of both the nutrition-specific-only (NSO) and nutrition-specific–nutrition-sensitive (NSNS) treatment arms against the reference group. They show no statistical differences between the NSO and reference groups. By contrast, the mid-upper arm circumference (MUAC) is, on average, 1.916 millimeters larger among the NSNS group than among the children in the reference group and 2.687 millimeters larger among the NSNS group than among the children in the NSO group.[2] A marginal exogenous increase in assets would boost the MUAC by about 0.67 millimeters, suggesting that the impact of the NSNS assistance is substantial. The results are robust across various specifications, across IV and PSM analyses, and across the individual and household panels. They confirm the hypothesis that, in situations of high fragility and of entrenched, chronic malnutrition, medium-term effects can be delivered by a combination of nutrition-sensitive programming and nutrition-specific programming, but not by nutrition-specific programming provided in isolation.

In the case of chronic malnutrition, this finding is intuitive. In emergency scenarios, short-term assistance bridges temporary food security problems, rather than tackling the underpinning causes. Moreover, in a scenario characterised by security threats and weak institutional capacities, different intervention typologies offer different implications.[3] Low capacities or damage to existing capacities likely disrupt the supply chains that provide RUTF, while also inhibiting demand. Individuals become less willing to travel long distances if infrastructure is weak or if travel is dangerous. Compared with nutrition-specific programmes, such threats are less likely to disrupt asset-based nutrition-sensitive programming, at least once the programming has been rolled out, because supply lines and the process of demand are less complex.[4]

The remainder of this presentation is structured as follows. The next section offers a brief theoretical background on why nutrition-specific programmes and nutrition-sensitive programmes might perform differently in this scenario. Section 3 discusses the PRRO and the related theories of change, implementation, and rollout. Section 4 presents the data and the analytical methods. Section 5 examines the results. Section 6 concludes.

2. Background

Nutrition-specific programmes are characterised by efforts to increase calorie intake and thereby enhance nutrition either by the direct provision of supplements or indirect provision through, for example, food stamps (Beaton & Ghassemi, 1982; Mora, Herrera, Suescun, de Navarro, & Wagner, 1981). By contrast, nutrition-sensitive programming relies on more complex theories of change. Impacts arise from income effects (Cunha, 2014) and associated impacts on the food Engels curve (Cruz & Ziegelhöfer, 2014; Fiszbein & Schady, 2009). Such programmes are accompanied by assumptions about household behaviours, especially if all children are to benefit (Jacoby, 2002). Although more complicated, the study here argues that the theories of change implicit in nutrition-sensitive programming are more tractable in complicated, fragile, and chronically food insecure places.

To illuminate this issue, the analysis draws lessons from literature that compares in-kind provision and cash transfers. The logic is as follows: nutrition-specific assistance is an in-kind transfer, directly providing RUTF. The nutrition-sensitive programme, then, corresponds to the logic of an unconditional cash transfer because households can use gains as they please, without any requirement to invest the gains in diet.

Both cash transfers and in-kind assistance are known to boost health and nutrition indicators (Akresh, de Walque, & Kazianga, 2016; Gentilini, 2016; Hidrobo, 2014; Hidrobo, Hoddinott, Peterman, Margolies, & Moreira, 2014; Skoufias, Unar, & González-Cossío, 2008). Cash transfers perform at least as well and, often, better (Afkar & Matz, 2015). Large proportions of cash transfers are consumed as food (Barrett & Lentz, 2013; Harvey & Marongwe, 2006). When the transfer is insufficient to cover all needs, impacts can be attributed to income effects (Cunha, 2014), but increases in food consumption are often larger than this effect (Cruz & Ziegelhöfer, 2014; Fiszbein & Schady, 2009).

In the case of chronic malnutrition, any programme that does not tackle the root causes of food insecurity is unlikely to have impacts outside the programme period (Maxwell, Webb, Coates, & Wirth, 2010; Ruel & Alderman, 2013). Especially if the malnutrition involves capacity constraints, rather than knowledge constraints (Baird, Ferreira, Özler, & Woolcock, 2014), unconditional programming should work well in any situation. By contrast, the nutrition-specific assistance arm examined here tackles neither the income constraints nor the production constraints that underpin chronic food insecurity in Niger. If assistance is removed, one would therefore anticipate a convergence to previous trends. However, the semipermanent increase in income implied by the nutrition-sensitive programme suggests that income constraints are partially relaxed. This provides the basis for gains to be sustained into the medium term in chronically food insecure environments.

In cases of fragility, the analysis is more practical in its considerations. Nutrition-specific programmes place restrictions on what is provided, how what is provided can be accessed, and how what is provided can be used. These modalities must be supported by meaningful delivery structures and oversight. Beneficiaries must travel – sometimes, long distances – to centers to receive their entitlements. Given the damage to normal economic interactions because of violence and fragility (Barbieri & Levy, 1999; Jha, 2013), it would be naive to believe that such threats do not interact with nutrition-specific provision. Supply lines are damaged during episodes of violence (Coward, 2009), and individuals are less likely to travel to distant health centers if they perceive the way to be dangerous. Displacement could further inhibit access.

While assets may be targeted, violence and fragility interrupt the basic provision of nutrition-sensitive programming to a lesser degree. First, such forms of treatment do not require ongoing provision or deep support structures, suggesting that there is less disruption on the supply side. Similarly, there are no ongoing commitments among recipients to travel and thus less room for demand-side disruption. Additional assets provide coping strategies that are not available in the case of in-kind provision.[5]

Based on this intuition, the analysis develops three hypotheses, as follows:

- Nutrition-specific programming leads to a temporary improvement in nutrition indicators that converges to underlying levels in the postprogramme period. The empirical prediction depends on the duration of this convergence period.[6]
- The addition of nutrition-sensitive programming allows these temporary boosts to be sustained in the medium term. Recipients of NSNS assistance will exhibit elevated nutrition indicators compared with the reference group. If households experience income constraints, rather than knowledge constraints, the NSNS group will also exhibit elevated nutrition indicators relative to the NSO group.
- Nutrition-sensitive programming increases household food consumption capacity through income effects, leading to improvements in nutrition status, regardless of the provision of nutrition-specific programming. Nutrition-sensitive programming is well placed to boost nutrition in situations of chronic food insecurity and political fragility.

Broadly, these hypotheses correspond to the conclusions of Langendorf et al. (2014), who propose that mixed strategies, involving food supplements and a cash transfer, should perform best in scenarios such as the one in Niger.[7]

In the case of both nutrition-specific assistance and nutrition-sensitive assistance, the provisions of the programme under study are inframarginal. This suggests that the effects hypothesised are driven by income effects (Cunha, 2014). The analysis draws a contrast between temporary (nutrition-specific) and semipermanent (nutrition-sensitive) income effects.

3. The intervention

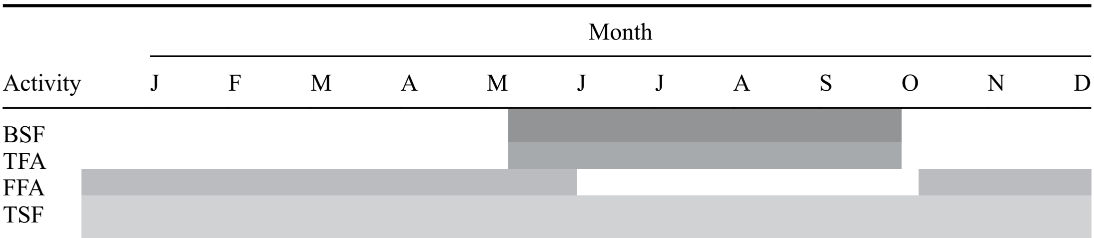
3.1. Programmes

Malnutrition in Niger is multicausal.[8] Consequently, WFP assistance works towards three objectives that encompass these causes: (a) to reduce constraints and adverse seasonal impacts on lives and livelihoods, (b) to support integrated safety nets, and (c) to increase the access of the poor to assets and food. The first two objectives are targeted through nutrition-specific RUTF, including targeted food assistance (TFA), blanket supplementary feeding (BSF), and targeted supplementary feeding (TSF). The TFA and BSF are available to all families in target villages; the TSF is available only to households with children below the moderate acute malnutrition threshold. The study formed the NSO group from households that reported they had received the BSF, TFA, or TSF during the previous calendar year.

The third objective is targeted through the nutrition-sensitive FFA Programme, which operates in two steps. First, households are supplied with tree seedlings and grow the trees on arid land, providing a lumber asset. Second, the growth of the trees facilitates land rehabilitation. At end line, all households that have qualified for the FFA also report that they receive at least one form of nutrition-specific assistance, thus forming the NSNS treatment group. Different forms of assistance are variously provided throughout the year (Table 1).

In the case of the BSF and TFA, all households in identified villages (see below) receive an RUTF allowance. Those that receive the BSF receive an allowance valued at 200 calories per child per day; TFA households receive an allowance valued at 620 calories per day. TSF recipients receive 92 calories per day per child under age 5. The FFA aims to deliver 620 calories per day through the use of assets and farming on the rehabilitated land.

Table 1. Sequencing intervention types throughout the year



Source: Construction based on WFP (2016).

3.2. Targeting

Rollout takes place geographically in three stages. First, priority districts are defined using indicators of food security, nutrition, livelihoods, population movement, and infrastructure. Aid is provided, according to the budget, to the districts with the worst characteristics. The criteria led to the identification of 70 priority districts, although budget cuts meant that assistance only reached 37 of the districts during the period of the study; the 33 districts exhibiting the least poor indicators were deselected. This phase-out is the backstop in the empirical strategies.

Second, villages within priority districts are targeted on the basis of food availability and local adjustment capacity. This includes villages in which more than 30 per cent of the population is food insecure and global acute malnutrition has been above the 15 per cent emergency threshold at least twice in recent years. All qualifying villages in priority districts receive assistance. The final stage selects very poor households in these villages. These households are identified by implementation partners, village committees, and WFP according to a household economy approach. Assistance is available to all very poor households in target villages.

The budgets available for NSNS programming differ based on priorities set by WFP and donors. Thus, NSO is available to all very poor households in all identified villages in the 37 priority areas. NSNS is also available in all 37 priority areas, but is restricted to the villages with the worst relevant nutrition indicators.

4. Data and methods

The study draws on panel data from a two-wave survey supported by WFP. Each wave includes household socioeconomic and demographic information and anthropometric measurements of each child. The anthropometric measurements are matched to questions about whether households had received certain forms of WFP assistance during the previous year. Analyses then evaluate the relevant dynamic differences in nutrition status between baseline and end line using a difference in differences model. The following formula is thus estimated:

$$Nutrition_{ihvt} = \beta_1 After + \beta_2 Treat_{ihvt} + \beta_3 Treat * Treat_{ihv} + \beta_4 X_{ihvt} + \in_{ihvt} \quad (1)$$

where *Nutrition* captures the nutrition status of child i, in household h, in village v, at time t; *After* is a dummy variable taking the value of 1 for all end line observations and 0 for baseline observations; *Treat* is a variable that defines the assistance group into which a given child falls; $After * Treat$ is the interaction of these two variables and isolates the programme impact; X is a vector of control variables; and $\in$ is the regression error term.

The baseline was collected in March 2014 before the PRRO was rolled out, while the end line was collected at the end of September 2017 after the PRRO period. The baseline is a representative sample of very poor households in the 70 priority areas ($m = 3,517$ households). Anthropometric measures are taken for each child ages 0–59 months ($n = 5,527$ children in 236 villages). The end

Table 2. Defining the assistance receiving groups

		End line status		
		No assistance	No FFA but at Least one of TFA, BSF	FFA and at least one of TFA, BSF
Baseline status	FFA only	Reference group	NSO	NSNS
Prop. sample, %		52.28	19.00	25.31

line resampled all households and children in the baseline ($n = 3,482$ children from $m = 1,694$ households in 200 villages).[9] The treatment group splits at end line are shown in Table 2.

Because of the intrahousehold movement of children and high mortality and birthrates, the study has a balanced panel $n \times T = 2,804$ children. The reasons for attrition beyond these local factors can be split into exogenous and structural reasons. Two events took place during data collection that exogenously reduced end line response. First, security in Diffa deteriorated because of the activities of Boko Haram, and enumerator teams did not visit this area. Second, an enumeration team was attacked and robbed in Tillaberi, which resulted in the loss of the data collected in this region. In both cases, all observations within those areas were lost.

There remain significant differences at baseline between remainer and attritor households (excluding those lost in Diffa and Tillaberi), many of which – such as agroecological zone – are worse for attritors and hold obvious ties to household nutrition outcomes (Table 3).[10]

To overcome attrition, the analysis treated it as a selection problem (Heckman, 1979). It posited that the day a village was scheduled for enumeration represents a valid criterion for this approach. If a household is in a village that was visited on a Friday, it is significantly less likely to be present in the end line. Niger is an overwhelmingly Muslim country, and a large share of the population observes Friday prayers, with an associated reduction in the productivity of the enumeration activities on that day. Because data collection took place in a formalised manner, with teams travelling in circles from regional capitals and visiting one village per day, the only determinant of whether a given village was visited on a Friday was the day the enumeration in a region started, which was random. There are thus no reasons to believe that nutrition status determines whether or not a village was visited on a Friday.

The analysis estimated the following selection equation by probit:

$$Attrition^{*}_{(t-1|t)} = \delta_1 Friday_{vt} + \delta_2 Friday_{vt} + \delta_2 X_{ivt-1} + \in_{it-1} \tag{2}$$

where $Attrition*$ is a binary variable taking the value of 1 if a household leaves the sample and 0 otherwise; $Friday_{vt}$ is a binary variable taking the value of 1 if a household is resident in village v that was visited on a Friday at end line and 0 otherwise; X is other exogenous control variables at baseline time $t-1$; δ_j are the regression coefficients for variables j; and $\in$ is the regression error term for a given household.

The results from Equation (2) are presented in Table 4. This approach is used to generate the inverse Mills ratio, which is included directly in the analysis.

Given local norms, the level of attrition may be structured differently at the individual and household levels. The nutrition indicators are therefore averaged over the household, and further analyses are conducted on the panel. This generates a balanced (household) panel $n \times T = 2,446$.[11] As before, the inverse Mills ratio from the Friday selection criterion is included to account for attrition.

The targeting of the PRRO on regions and villages with the worst observable nutrition indicators raises a second source of endogeneity. To investigate this bias, the study compared the mean of the key variables in each group. In these analyses, a random distribution of assistance would imply no differences between these means. The difference is tested using a standard t-test (Table 5).

Table 3. Attrition analysis: T-test comparison of sample means

Variables	(1) Remainers	(2) Attritors	(3) Difference
Poverty status	1.172622	1.016892	0.1557***
Agroeconomic zone	2.037867	1.172297	0.8656***
Male child <5 months	0.0583111	0.0709459	−0.0126
Male child 6 < 59 months	0.5788444	0.6182432	−0.0394
No. male family members	3.018667	3.111486	−0.0928
Female child <5 months	0.0583111	0.0540541	0.0043
Female child 6 < 59 months	0.5488	0.4594595	0.0893**
No. female family members	3.25	3.140444	0.109556
Gender household head	1.231289	1.25	−0.0187
Marital status household head	1.630044	1.712838	−0.0828
Age household head	44	45.78258	−1.7826**
Education household head	1.5984	1.658784	−0.0604
Employment household head	4.264356	4.358108	−0.0938
Income household head	1.440356	1.442568	−0.0022
Water source	1.703289	1.719595	−0.0163
Toilet type	4.029867	4.135135	−0.1053
Energy supply	2.874667	2.959459	−0.08479***
House tenure type	1.646044	1.307432	0.3386***
Owns chair	1.049422	1.084459	−0.0350***
Owns carpet	0.1701333	0.1351351	0.0350*
Owns table	0.8312889	0.4527027	0.3786***
Owns bed	0.0311111	0.0202703	0.0108
Owns mat	0.6792889	0.4358108	0.2435***
Owns jewelry	0.9308444	0.9831081	−0.0523***
Owns iron	0.1139556	0.0472973	0.0667***
Owns sewing machine	0.0186667	0.0202703	−0.0016
Owns telephone	0.0083556	0.0067568	0.0016
Owns TV	0.4243556	0.3614865	0.0629**
Owns radio	0.0060444	0.0168919	−0.0108**
Owns hoe	0.2007111	0.1824324	0.0183
Owns plough	0.8954667	0.847973	0.0475***
Owns motorbike	0.1431111	0.1047297	0.0384**
Owns bike	0.0344889	0.027027	0.0075
Owns lamp	0.0177778	0.0168919	0.0009
Owns other asset	0.9367111	0.8614865	0.0752***
Has animals	0.0307556	0.9831081	−0.9524
Has cows	0.6449778	0.3716216	0.2734***
Has sheep	0.5847111	0.2837838	0.3009***
Has goats	1.314667	0.4560811	0.8586***
Has camels	2.128356	0.902027	1.2263***
Has donkeys	0.0698667	0.0067568	0.0631**
Has horses	0.5363556	0.1047297	0.4316***

Note: ***p < .01; **p < .05; *p < .1.

Households in the reference group tend to be larger than average, while households in the NSNS treatment group are smaller, although households in both treatment groups have more children aged 0–5. NSNS households are more likely to have a woman head of household, are less likely to be polygamous, and display higher than average access to livestock and assets. The study overcame the associated biases in two ways. The first is an IV in which an instrument that is correlated with the endogenous variables is included, but not the regression error term.

Each endogenous variable is first regressed on all exogenous regressors and the IV:

Table 4. Testing the correlation between the Friday selection criterion and attrition

Non-random selection, Probit	
	(1)
Variables	*1 child is in both waves 0 only baseline*
Cluster visited on Friday in 2016	−0.268***
	(0.0599)
Constant	−0.179**
	(0.0718)
Observations	3,447

Notes: Standard errors are shown in parentheses. ***p < .01; **p < .05; *p < .1.

Table 5. Group-based mean differences

	(1)	(2)	(3)
Variables	Reference group	NSO	NSNS
Household size	0.294*	0.269	−0.552***
	(2.47)	(1.68)	(−4.17)
Children ages 0–5	0.0216	0.0621**	0.0700***
	(1.39)	(2.98)	(−4.03)
Female household head	−0.0132	−0.0362*	0.0415**
	(−1.09)	(−2.22)	(3.07)
Number of wives	0.0759***	0.00873	−0.101***
	(3.9)	(0.33)	(−4.64)
Livestock index	−0.276***	−0.00854	0.352***
	(−5.05)	(−0.12)	(5.76)
Assets index	0.0767	−0.338***	0.139*
	(1.52)	(−5.02)	(2.47)
Region	−0.155	0.434**	−0.107
	(−1.46)	(3.1)	(−0.93)
Child, moderate acute malnutrition	0.00573	0.00662	−0.0117
	(0.75)	(0.64)	(−1.37)
Child, mid-upper arm circumference	−0.906*	0.331	0.901
	(−2.16)	(0.59)	(1.92)
Height-for-age Z	−0.07	−0.0922	0.152**
	(−1.36)	(−1.33)	(2.63)

Notes: Table shows t-test comparisons of end line means of key variables according to group status. Column (1) shows the comparison of the mean for the reference group, compared to pooled mean of the NSO and NSNS Groups; column (2) for NSO, compared to the reference group and NSNS; and column (3) for NSNS compared with the reference group and NSO. T-statistics in parentheses. Significance level: * = 10 per cent, ** = 5 per cent, *** = 1 per cent.

$$Treat_{ihv} = \phi_o + \phi_1 X_{it} + \phi_2 IV + \in_{ivht} \quad (3)$$

where $Treat_{ihv}$ is the treatment status of individual i in household h and village v; X is the vector of exogenous control variables; IV is the instrument; ϕ_j are regression coefficients; and $\in$ is the regression error term.

The analysis identified the spatial lag of violence during the period shortly before the end line survey was collected (see Ferguson & Michaelsen, 2015, for baseline work using this instrument; Brück & Ferguson,

Table 6. Testing the correlation between group belonging and the instrumental variable

	(1)	(2)	(3)
Variables	NSOvRG	NSNSvRG	NSNSvNSO
Spatial lag	−0.000233***	0.000924***	0.00122***
Insecurity	(5.75e–05)	(5.94e–05)	(8.11e–05)
Observations	3,646	3,631	3,631

Notes: Standard errors are shown in parentheses. ***p < .01; **p < .05; *p < .1.

2018, for the use of the instrument in impact evaluation). The analysis uses the distance from a village to the nearest violent event (other than those events took place in the village), as recorded by the United Nations security team, in the three months before the end line. It includes the instrument alone and the interaction of the instrument with time. Table 6 shows the correlations between the instruments and each treatment group. As can be seen, ϕ_4 is significantly different from zero in all settings.[12]

The analysis also qualitatively justified the exclusion restriction. It relies on violence that took place in a village that is not part of the unit of analysis. There are no grounds to believe that violence happening in other villages should influence nutrition in the analysis village directly. Because violence may be spatial, the analysis controls for violent events that took place in the community of observation.

Second, the study used PSM across treatment groups (Rosenbaum & Rubin, 1983). This approach accounts for the covariates that predict receipt of assistance. It matches on the full range of covariates in the main econometric specifications. It fits a multinomial logit model on each household's treatment status, estimating the following:

$$Treat_{ihv2} = \psi_0 + \psi_1 X_{ihv1} + \psi_1 M_{ihv1} + \in_{ih2} \quad (4)$$

where the treatment status $Treat_{ihv2}$ of individual i, in household h, village v, and, at period two, takes values 1, 2, or 3; X is the vector of exogenous variables at baseline; M is the inverse Mills ratio; ψ_j are regression coefficients; and $\in_{ih2}$ is the error term. Once the probability of the presence of an observation in each group is predicted, a Kernel-based PSM difference in differences is performed, following Leuven and Sianesi (2018). Figure 1 shows the common support of this approach. The results of this step are shown in Appendix A, Table A4.

The study thus involved four main analyses that generate the estimates on which the discussion of the results is based. These are summed up in Equations (5) and (6):

$$N_{ihvt} = \beta_1 After + \beta_2 Treat_{ihv} + \beta_3 After * Treat_{ihv} + \beta_4 X_{ihvt} + \beta_5 M + \in_{ihvt} \quad (5)$$

where N is the nutrition outcome of interest, and M is the included inverse Mills ratio. Otherwise, Equation (5) is as described in Equation (1).

$$N_{hvt} = \beta_1 After + \beta_2 Treat_{hv} + \beta_3 After * Treat_{hv} + \beta_4 X_{hvt} + \beta_5 M + \in_{hvt} \quad (6)$$

where N is the average nutritional status of all children in household h in village v at time t. The remainder of the equation is as described in Equations (1) and (5). Subsequently, Equations (5) and (6) are estimated using the instrumental variables and PSM approaches. X_{hivt} *and* X_{hvt} comprise the size of the household, the number of children under age 5 in the household, the gender of the head of household, the number of wives in the household, an index of household assets, and security data according to the requirements of the instrument.

Each equation is estimated three times. The first estimation compares the impact of receiving NSNS with the impact of no assistance (the reference group). The second estimation repeats this

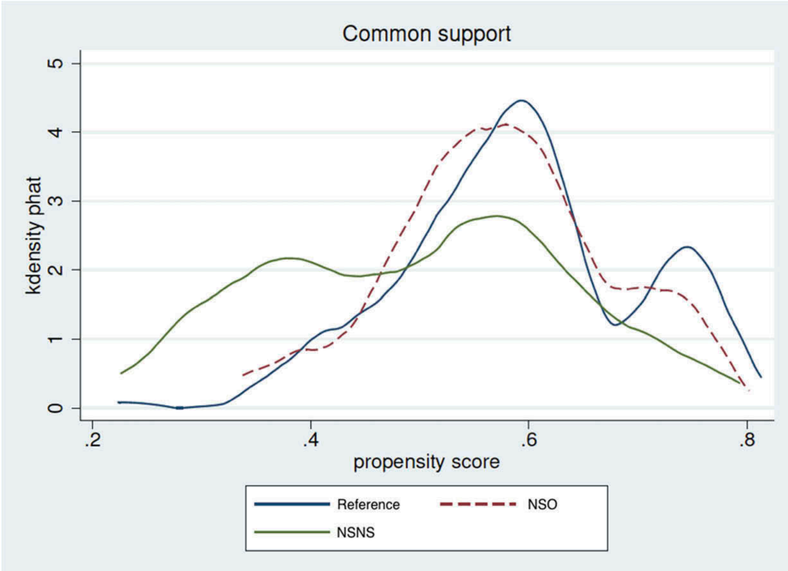


Figure 1. Common support in PSM across treatment groups.

process to compare the impact of NSO with the reference group. The third estimation compares the NSNS and NSO treatment groups to test the relative effects of the different treatment combinations.[13]

5. Results

The results of the PSM analyses are presented as the preferred specification because of the maximum extent of bias that might arise in instrumenting the NSO treatment. The results are shown in Tables 7–9. Table 7 compares the outcomes among the NSNS and reference groups; Table 8 compares the outcomes among the NSO and reference groups, and Table 9 compares the outcomes among the NSNS and NSO groups. The results of the instrumental variables analyses are presented in Appendix A, Tables A1–A3, but exhibit no major differences. Each table contains eight columns. Columns 1–4 exhibit the results of the individual-level analyses, and columns 5–8 show the household-level results. Columns 1 and 5 show results from the MUAC z-score; columns 2 and 6 from the continuous MUAC score; columns 3 and 7 from the weight-for-age z-score (WAZ); and columns 4 and 8 for the height-for-age z-score (HAZ). Both MUAC measurements capture variations in short-term nutritional outcomes; WAZ and HAZ capture longer-term impacts.

At the child level, three key trends emerge. First, the receipt of NSNS, certainly in terms of shorter-term indicators, boosts nutrition outcomes relative to the reference group. The scale of the effect is large compared with the other control variables. Recipients of NSNS show an increase in MUAC of 1.916 millimeters relative to the reference group. This is more than three times the impact that an exogenous, marginal increase in household assets would have. Although similar movements are not evident in weight-for-age, height-for-age is also positive and significant, suggesting longer-term gains.

Second, there are no differences between the NSO treatment group and the reference group. Given the timing of the end line survey, it is not clear if NSO assistance has no impact at all or if the impacts are lost over the medium term. It is also unclear if this finding relates to a general convergence in the nutritional status of the reference and NSO groups, to the relative effects of makeshift coping strategies, or to the ineffectiveness of these programmes if they are provided in isolation. Finally, there is strong evidence that children in the NSNS treatment group are significantly more well off than those in the NSO group across all nutritional indicators. Although this follows logically from the

Table 7. Child- and household-level PSM analyses of NSNS versus reference group outcomes

	Individual panel results				Household panel results			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	MUACZ	MUAC	WAZ	HAZ	MUACZ	MUAC	WAZ	HAZ
Time	−0.19**	6.41***	−0.23**	−0.21	0.10	3.92***	−0.28***	−0.03
	(0.08)	(0.90)	(0.09)	(0.13)	(0.09)	(1.25)	(0.10)	(0.16)
NSNS vs RG	−0.18**	−1.99*	−0.14	−0.53***	−0.10	−1.76*	−0.19**	−0.42***
	(0.09)	(1.03)	(0.09)	(0.14)	(0.08)	(0.90)	(0.08)	(0.13)
Time(NSNS vs RG)	0.28***	2.16**	0.13	0.40***	0.24**	2.46**	0.24***	0.35**
	(0.09)	(0.92)	(0.09)	(0.12)	(0.10)	(1.17)	(0.09)	(0.15)
IMR Friday visit	0.22	4.08	0.44	−0.83	−0.35	−0.18	−0.71**	−1.85***
	(0.46)	(5.52)	(0.44)	(0.65)	(0.36)	(3.65)	(0.30)	(0.46)
Observations	1,588	2,073	2,069	2,070	2,905	3,531	3,520	3,522
R-squared	0.02	0.14	0.01	0.06	0.04	0.07	0.01	0.04
Mean control t(0)	−1.344	136	−1.996	−0.998	−1	138.9	−0.920	−0.125
Mean treated t(0)	−1.525	134	−2.133	−1.527	−1.100	137.1	−1.107	−0.546
Diff t(0)	−0.181	−1.987	−0.138	−0.529	−0.100	−1.755	−0.187	−0.421
Mean control t(1)	−1.536	142.4	−2.223	−1.204	−0.899	142.8	−1.198	−0.160
Mean treated t(1)	−1.442	142.6	−2.228	−1.333	−0.762	143.5	−1.143	−0.230
Diff t(1)	0.0944	0.170	−0.00515	−0.130	0.137	0.701	0.0553	−0.0697

Notes: Child-level PSM difference in differences estimation on the balanced panel subsample of NSNS group and reference group households (columns 1–4) and household-level PSM difference in differences estimation on the balanced panel subsample of NSNS group and reference group households (columns 5–8). The dependent variables are children's anthropometrics. *NSNS vs RG* is a dummy that takes value 1 if the child lives in a household that belongs to the NSNS treatment and 0 if the child lives in a household that belongs to the reference group; *Time* takes value 1 in 2016 and 0 in 2014. The Inverse Mills Ratio (IMR) correction is based on whether the household is in a village that was visited on a Friday. Control variables include: household size, number of children under five, household head gender, household head education, and number of wives in the household. Robust standard errors are in parentheses. ***$p < .01$; **$p < .05$; *$p < .1$.

Table 8. Child- and household-level PSM analyses of NSO versus reference group outcomes

	Individual panel results				Household panel results			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	MUACZ	MUAC	WAZ	HAZ	MUACZ	MUAC	WAZ	HAZ
Time	−0.15*	6.08***	−0.31***	−0.19	0.08	4.21***	−0.27***	−0.22**
	(0.09)	(1.12)	(0.10)	(0.12)	(0.08)	(0.99)	(0.09)	(0.10)
NSO vs RG	−0.10	−0.78	−0.09	−0.10	−0.02	0.14	0.02	0.03
	(0.09)	(1.03)	(0.08)	(0.12)	(0.08)	(0.99)	(0.07)	(0.11)
Time(NSO vs RG)	0.10	−0.41	−0.07	0.08	0.02	−0.77	−0.13	−0.04
	(0.11)	(0.90)	(0.08)	(0.11)	(0.08)	(0.94)	(0.08)	(0.12)
IMR Friday visit	0.20	3.01	−0.71	−2.09**	−0.23	−2.23	−1.10**	−1.62**
	(0.54)	(6.12)	(0.70)	(0.83)	(0.51)	(5.15)	(0.55)	(0.63)
Observations	1,480	1,920	1,914	1,916	2,593	3,157	3,146	3,147
R-squared	0.01	0.11	0.03	0.06	0.01	0.04	0.03	0.03
Mean control t(0)	−1.414	136	−1.214	−0.307	−0.984	142.5	−0.763	−0.504
Mean treated t(0)	−1.512	135.2	−1.301	−0.408	−1.001	142.6	−0.739	−0.471
Diff t(0)	−0.0979	−0.780	−0.0871	−0.101	−0.0175	0.145	0.0243	0.0338
Mean control t(1)	−1.560	142.1	−1.526	−0.492	−0.908	146.7	−1.032	−0.720
Mean treated t(1)	−1.561	140.9	−1.682	−0.513	−0.904	146	−1.136	−0.724
Diff t(1)	−0.00121	−1.193	−0.156	−0.0211	0.00359	−0.625	−0.104	−0.00374

Notes: Child-level PSM difference in differences estimation on the balanced panel subsample of NSO group and reference group households (columns 1–4) and household-level PSM difference in differences estimation on the balanced panel subsample of NSO group and reference group households (columns 5–8). The dependent variables are children's anthropometrics. *NSO vs RG* is a dummy that takes value 1 if the child lives in a household that belongs to the NSO treatment and 0 if the child lives in a household that belongs to the reference group; *Time* takes value 1 in 2016 and 0 in 2014. The Inverse Mills Ratio (IMR) correction is based on whether the household is in a village that was visited on a Friday. Control variables include: household size, number of children under five, household head gender, household head education, and number of wives in the household. Robust standard errors are in parentheses. ***$p < .01$; **$p < .05$; *$p < .1$.

Table 9. Child- and household-level PSM analyses of NSNS versus NSO group outcomes

	Individual panel results				Household panel results			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	MUACZ	MUAC	WAZ	HAZ	MUACZ	MUAC	WAZ	HAZ
Time	−0.10	4.89***	−0.34***	−0.08	0.07	2.09**	−0.50***	0.27
	(0.11)	(1.10)	(0.11)	(0.11)	(0.08)	(0.85)	(0.11)	(0.28)
NSNS vs NSO	0.00	−0.52	0.04	−0.32**	−0.13	−1.68*	−0.21**	−0.18
	(0.10)	(1.13)	(0.11)	(0.14)	(0.10)	(0.90)	(0.08)	(0.19)
Time(NSNS vs NSO)	0.21*	2.87***	0.19*	0.26*	0.26***	3.57***	0.41***	0.14
	(0.12)	(1.06)	(0.10)	(0.14)	(0.09)	(0.89)	(0.11)	(0.25)
IMR Friday visit	−0.02	−2.26	−0.77	−1.91**	−0.31	−3.37	−0.21	0.05
	(0.45)	(5.13)	(0.74)	(0.90)	(0.41)	(4.56)	(0.66)	(0.73)
Observations	837	1,095	1,091	1,088	1,531	1,843	1,835	1,832
R-squared	0.04	0.16	0.04	0.10	0.06	0.10	0.04	0.06
Mean control t(0)	−1.239	140.7	−1.040	−0.317	−0.957	142.1	−1.343	−2.070
Mean treated t(0)	−1.235	140.1	−0.996	−0.637	−1.083	140.4	−1.555	−2.250
Diff t(0)	0.00364	−0.524	0.0436	−0.320	−0.126	−1.677	−0.212	−0.180
Mean control t(1)	−1.343	145.5	−1.377	−0.393	−0.889	144.2	−1.846	−1.803
Mean treated t(1)	−1.127	147.9	−1.144	−0.456	−0.758	146.1	−1.644	−1.847
Diff t(1)	0.217	2.351	0.233	−0.0629	0.131	1.895	0.202	−0.0436

Notes: Child-level PSM difference in differences estimation on the balanced panel subsample of NSNS group and NSO group households (columns 1–4) and household-level PSM difference in differences estimation on the balanced panel subsample of NSNS group and NSO group households (columns 5–8). The dependent variables are children's anthropometrics. *NSNS vs NSO* is a dummy that takes value 1 if the child lives in a household that belongs to the NSO treatment and 0 if the child lives in a household that belongs to the reference group; *Time* takes value 1 in 2016 and 0 in 2014. The Inverse Mills Ratio (IMR) correction is based on whether the household is in a village that was visited on a Friday. Control variables include: household size, number of children under five, household head gender, household head education, and number of wives in the household. Robust standard errors are in parentheses. ***$p < .01$; **$p < .05$; *$p < .1$.

previous two findings, it reinforces the importance of nutrition-sensitive programming and suggests these effects might also be traced to longer-term indicators.

These findings are more compelling if they are considered at the household level. In this case, the effects are more substantial in the short-term and long-term indicators among the NSNS treatment group relative to both the NSO treatment group and the reference group, and there is little difference between the NSO treatment group and the reference group. This is important for two reasons. First, it suggests that the child-level gains are not the result of intrahousehold shifts in resource allocation, but, rather, real gains based on either more effective use of food or increased food availability. Second, it provides grounds to believe that the FFA has impacts on longer-term nutritional trends, as well as on shorter-term nutritional trends.

6. Discussion and conclusions

The study shows that, although it may have resulted in a temporary improvement in nutrition indicators during the project period, nutrition-specific programming has otherwise had no effect. The study also shows that the combination of nutrition-sensitive assistance and nutrition-specific assistance has a pronounced positive impact on a range of nutritional outcomes relative to the receipt of no assistance. Finally, the study shows that the combination of nutrition-specific assistance and nutrition-sensitive assistance outperforms the receipt of only nutrition-specific assistance. Despite the strength of these findings, it is important to be precise about what the findings capture. Although the findings point to the key role of the FFA, attribution must be considered more carefully. At worst, the FFA plays a major role in boosting the performance of other forms of assistance. However, one cannot ascertain if these impacts are delivered by the FFA or by the interaction of the FFA with other forms of assistance. Future work might examine attribution more deeply.

The combination of results confirms the relationships hypothesised in the study and supports the notion that typical theories of change associated with nutrition programming are complicated in situations of chronic malnutrition or fragility. The application of this logic suggests that nutrition-sensitive programmes should play an important role in nutrition aid in such contexts. Despite longer causal chains and more complicated theories of change, these results are largely intuitive. Food insecurity and malnutrition are related as much to income constraints as knowledge constraints (Baird et al., 2014), while the relevant causal chains are less prone to interruption by violence or fragility.

Though intuitive, these results are important for a number of reasons. First, they show that nutrition-sensitive programmes appear to be more suited to deliver impacts in complicated fragile environments associated with chronic food security concerns.

Second, at least for as long as any impacts on asset accumulation endure, the results suggest that FFA-style nutrition-sensitive programming is well placed to deliver ongoing nutritional benefits, although the specific commodities provided and how they are delivered will require careful scrutiny in other environments. At least in cases where malnutrition is the outcome of an income constraint, it is plausible to believe that assistance bundles containing the FFA are optimally placed to deliver sustained impacts.

Third, little rigorous evidence is available on the performance of food aid programmes in challenging fragile and conflict-affected environments. This likely relates partly to the data-quality issues and rollout constraints faced in the study. The series of methodological innovations in the study help overcome these concerns and produce theoretically valid, empirically confirmed, and robust findings. Not only do the findings suggest that certain forms of food aid boost nutrition in challenging environments, but also that these improvements may endure into the medium term.

The findings demonstrate that positive spillovers accompany food aid programmes in particular and development and humanitarian aid projects more generally. They show that development interventions intended for one purpose, such as, in the case of the FFA, the development of household assets, can develop positive outcomes elsewhere, such as nutrition status in this case. Although this partly fits in with a wider literature, the modality under investigation here is rather different. It implies that providing a small amount of nutrition-sensitive assistance may induce additional positive investments in nutrition in recipient households.

Acknowledgements

The research team gratefully acknowledges funding from the International Initiative for Impact Evaluation (3ie), received through the UK Department for International Development (DFID) and the World Food Programme (WFP). The authors are specifically thankful to the WFP Niger Country Office for its support throughout the project. They thank M. Hambali Moussa for leading a qualitative follow-up of the project, Myroslava Purska for excellent organisational support, and Sveva Vitellozzi for research assistance. They are grateful for the comments of participants of the Household in Conflict Network (HiCN) Annual Workshop in 2017, a project meeting in Kampala, Uganda, and a series of presentations in Niger. They express their appreciation to reviewers at 3ie, DFID, and WFP for feedback; to Amber Peterman and Ugo Gentilini for additional comments; and to two anonymous reviewers, whose input has significantly strengthened the work. They are grateful to the National Statistics Institute of Niger for assistance in collecting baseline and end line surveys and to the High Commission of Nigeriens Feeding Nigeriens for support throughout the research process. The usual disclaimer applies.

Funding

This work was supported by the International Initiative for Impact Evaluation (3ie) [HATW-TW6.1023].

Disclosure statement

No potential conflict of interest was reported by the authors.

Notes

1. Some debate has questioned whether or not these interventions might distort production incentives or other aspects of local food markets (Abdulai, Barrett, & Hoddinott, 2005; Barrett, Mohapatra, & Snyder, 1999; Bezu & Holden, 2008; Gelan, 2007; Schultz, 1960; Stevens, 1978; Tadesse & Shively, 2009). A recent review (Ruel, Quisumbing, & Balagamwala, 2018) discusses the role of nutrition-sensitive agriculture. However, agriculture is only one of multiple key domains in which nutrition-sensitive programming can exist. Boosts to assets and income feature less prominently in the programme-level literature.
2. The results are significant at 5 per cent and are robust across three other nutrition indicators: a z-score of the MUAC, a z-score of weight-for-age, and a z-score of height-for-age.
3. Although interesting work measures conflict at the individual level (Brück, Justino, Verwimp, Avdeenko, & Tedesco, 2015; Justino, Brück, & Verwimp, 2013), the collection of relevant data has not been possible. Testing how private exposure to violence interrupts programme benefits would be revealing, but is beyond the scope and capacity of this study.
4. There is some risk that assets could be misappropriated or destroyed by violent actors, although this is not unique to nutrition-sensitive programming. There is no evidence that this occurred in Niger.
5. This is not to suggest that conflict is not associated with the risk of the onset of food crises (Brinkman & Hendrix, 2011), although this is common across the nutrition-specific and nutrition-sensitive typologies.
6. If the convergence period is longer than three months after the programme (when the end line data are collected), the NSO group exhibits better nutrition outcomes than the reference group (H_1). If it is shorter, no difference between the groups is observed (H_2).
7. Langendorf et al. (2014) also suggest the need for additional rigorous evidence to confirm these findings. This is particularly important because Hoddinott, Sandstrom, and Upton (2018) suggest that households receiving food baskets showed larger impacts on dietary diversity than households that received cash. This study addresses this important programmatic level gap in Niger.
8. A reliance on rain-fed agriculture and weather shocks in Niger (Shideed, 2017) has led some of the worst malnutrition indicators in the world. Among children, 14.8 per cent suffer global acute malnutrition (Institut National de la Statistique, Niger [INS] & ICF International, 2013). Half the country's regions show levels of global acute malnutrition above the emergency threshold set by the World Health Organisation. One child in three is underweight, and chronic malnutrition affects 42 per cent of children under 5 (Concern Worldwide, 2013).

9. The data include all children ages 0–59 months at the time of baseline data collection and all children who were newly born in the households in the baseline sample.
10. Because the study only observed the assistance received by households at end line after the collection of the survey, it could not observe the treatment groups to which attritor households belonged. Thus, the analysis cannot directly show that attrition is not a function of the treatment received. Other results produced through the empirical specifications suggest, however, that attrition should be unrelated to treatment status. Tables 3 and 5 show some of the determinants of attrition and assistance receipt, respectively. The variables that are different between attritor and remainer households seldom overlap with those that determine group membership. Where they do, the implication of the signs of the coefficients are unclear. For example, attritors show elevated access to some household assets, and less access to others, with no obvious hierarchy in the indicators. By contrast, the NSO group has fewer assets than the pooled mean of the other groups; the reference group exhibits no difference; and the NSNS group has more assets.
11. This should not be taken to imply that there are households in the end line that were not present at baseline. Rather, it means that there were households in the baseline household survey on which there are no baseline anthropometric measurements; however, these appear at end line.
12. Comparison of these correlations against the Stock-Yogo thresholds suggests a maximum bias of less than 1 per cent in analysing NSNS versus NSO and NSNS versus the reference group and a maximum bias of about 12 per cent to 13 per cent in analysing NSO versus the reference group.
13. A more standard econometric model would include all treatment groups in a single equation. This is not done so for two practical reasons. First is the implied increase in the number of instruments required; second is that many more data would be excluded in the PSM approaches.

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Appendix A. Additional tables

Table A1. Child- and household-level IV analyses of NSNS versus reference group outcomes

	Individual panel results				Household panel results			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	MUACZ	MUAC	WAZ	HAZ	MUACZ	MUAC	WAZ	HAZ
NSNS vs RG	−0.72***	−6.17***	−0.11	−0.59**	−0.88***	−8.16***	−0.15	−0.82***
	(0.17)	(2.03)	(0.19)	(0.26)	(0.15)	(1.92)	(0.18)	(0.24)
Time(NSNS vs RG)	0.92***	5.59	0.22	0.49	1.15***	8.23***	0.64**	1.26***
	(0.36)	(3.49)	(0.33)	(0.44)	(0.25)	(2.98)	(0.27)	(0.37)
Time	−0.30**	6.17***	−0.18	−0.13	−0.17*	3.16***	−0.29***	−0.36***
	(0.12)	(1.23)	(0.12)	(0.16)	(0.09)	(1.06)	(0.10)	(0.13)
IMR Friday visit	0.31	7.13**	0.36	−0.95**	−0.20	−0.24	−0.49*	−1.44***
	(0.33)	(3.56)	(0.34)	(0.45)	(0.25)	(2.85)	(0.26)	(0.35)
First-stage F-statistic	66.53	89.35	90.72	89.42	138.7	158.7	159.9	159.4
Observations	1,579	2,062	2,057	2,059	2,911	3,538	3,525	3,529
R-squared		0.12	0.01	0.04		0.04		0.01

Notes: Child-level IV difference in differences estimation on the balanced panel subsample of NSNS group and reference group households (columns 1–4) and household-level IV difference in differences estimation on the balanced panel subsample of NSNS group and reference group households (columns 5–8). The dependent variables are children's anthropometrics. *NSNS vs RG* is a dummy that takes value 1 if the child lives in a household that belongs to the NSNS treatment and 0 if the child lives in a household that belongs to the reference group; *Time* takes value 1 in 2016 and 0 in 2014. The Inverse Mills Ratio (IMR) correction is based on whether the household is in a village that was visited on a Friday. Control variables include: household size, number of children under five, household head gender, household head education, and number of wives in the household. Robust standard errors are in parentheses. ***$p < .01$; **$p < .05$; *$p < .1$.

Table A2. Child- and household-level IV analyses of NSO versus reference group outcomes

	Individual panel results				Household panel results			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	MUACZ	MUAC	WAZ	HAZ	MUACZ	MUAC	WAZ	HAZ
NSO vs RG	2.26	14.68	−1.06	−0.01	2.03***	16.64**	−0.92	0.56
	(1.44)	(11.77)	(1.28)	(1.35)	(0.69)	(7.63)	(0.77)	(0.93)
Time(NSO vs RG)	−6.39*	−31.22	−1.80	−1.37	−2.87***	−20.22**	−1.17	−2.27*
	(3.79)	(20.85)	(2.31)	(2.47)	(1.01)	(10.05)	(1.03)	(1.26)
Time	1.58	14.90***	0.30	0.27	0.80***	9.70***	0.06	0.37
	(1.03)	(5.40)	(0.60)	(0.64)	(0.25)	(2.45)	(0.25)	(0.31)
IMR Friday visit	0.75	6.20	0.13	−1.46***	−0.16	−0.07	−0.96***	−1.98***
	(0.84)	(4.52)	(0.48)	(0.52)	(0.32)	(3.24)	(0.33)	(0.40)
First-stage F-statistic	1.774	3.877	3.621	3.534	11.81	16.55	16.24	15.68
Observations	1,463	1,897	1,890	1,893	2,626	3,192	3,179	3,182

Notes: Child-level IV difference in differences estimation on the balanced panel subsample of NSO group and reference group households (columns 1–4) and household-level IV difference in differences estimation on the balanced panel subsample of NSO group and reference group households (columns 5–8). The dependent variables are children's anthropometrics. *NSO vs RG* is a dummy that takes value 1 if the child lives in a household that belongs to the NSO treatment and 0 if the child lives in a household that belongs to the reference group; *Time* takes value 1 in 2016 and 0 in 2014. The Inverse Mills Ratio (IMR) correction is based on whether the household is in a village that was visited on a Friday. Control variables include: household size, number of children under five, household head gender, household head education, and number of wives in the household. Robust standard errors are in parentheses. ***p < .01; **p < .05; *p < .1.

Table A3. Child- and household-level IV analyses of NSNS versus NSO group outcomes

	Individual panel results				Household panel results			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	MUACZ	MUAC	WAZ	HAZ	MUACZ	MUAC	WAZ	HAZ
NSNS vs NSO	−0.25	−3.18	−0.05	−1.03***	−0.50***	−5.56***	−0.23	−1.10***
	(0.17)	(2.00)	(0.19)	(0.26)	(0.14)	(1.73)	(0.16)	(0.22)
Time(NSNS vs NSO)	1.07***	8.31**	0.64*	1.22***	0.85***	7.62***	0.89***	1.65***
	(0.35)	(3.48)	(0.34)	(0.45)	(0.23)	(2.66)	(0.25)	(0.33)
Time	−0.53***	2.04	−0.56***	−0.61**	−0.30**	−0.30	−0.73***	−0.87***
	(0.20)	(2.05)	(0.20)	(0.26)	(0.15)	(1.70)	(0.16)	(0.21)
IMR Friday visit	−0.04	−2.48	−0.40	−1.53***	−0.52	−7.19*	−0.88**	−1.05**
	(0.41)	(4.47)	(0.43)	(0.57)	(0.33)	(3.82)	(0.36)	(0.48)
First-stage F-statistic	53.71	68.33	69.09	66.85	136.1	153.2	153.0	150.5
Observations	840	1,101	1,099	1,096	1,535	1,848	1,844	1,841
R-squared		0.15	0.02	0.06	0.03	0.07	0.00	0.02

Notes: Child-level IV difference in differences estimation on the balanced panel subsample of NSNS group and NSO group households (columns 1–4) and household-level IV difference in differences estimation on the balanced panel subsample of NSNS group and NSO group households (columns 5–8). The dependent variables are children's anthropometrics. *NSNS vs NSO* is a dummy that takes value 1 if the child lives in a household that belongs to the NSO treatment and 0 if the child lives in a household that belongs to the reference group; *Time* takes value 1 in 2016 and 0 in 2014. The Inverse Mills Ratio (IMR) correction is based on whether the household is in a village that was visited on a Friday. Control variables include: household size, number of children under five, household head gender, household head education, and number of wives in the household. Robust standard errors are in parentheses. ***p < .01; **p < .05; *p < .1.

Table A4. Propensity score matching results

	Individual panel		Household panel	
	(1)	(2)	(3)	(4)
Variables	NSO	NSNS	NSO	NSNS
Household size	−0.062	0.796**	−0.049	−0.014
	(0.040)	(0.033)	(0.033)	(0.030)
Children ages 0–5 years	0.137	0.292*	−0.035	0.264*
	(0.204)	(0.166)	(0.176)	(0.140)
Woman household head	0.093	−0.015	0.083	−0.044
	(0.213)	(0.205)	(0.182)	(0.192)
Education household head	−0.165**	0.103**	−0.090*	−0.006
	(0.068)	(0.050)	(0.008)	(0.048)
Number of wives in household	−0.002	0.235*	0.008	0.161
	(0.143)	(0.122)	(0.116)	(0.107)
Assets index	0.087	−0.101**	0.116**	0.029
	(0.054)	(0.052)	(0.045)	(0.046)
Security	0.056	−0.167	−0.078	3.325***
	(0.153)	(0.142)	(0.159)	(0.330)
Constant	−0.754***	−1.617***	−0.660***	−4.527***
	(0.257)	(0.226)	(0.224)	(0.357)
Observations	1,402	1,402	1,969	1,969

Notes: Child-level panel on household's treatment status at end line (columns 1 and 2) and household-level panel on household's treatment status at end line (columns 3 and 4), estimated using multinomial logit models. Columns 1 and 3 show determinants of belonging to NSO groups versus the reference group; columns 2 and 4 the determinants of belonging to the NSNS treatment group. ***p < .01; **p < .05; *p < .1.