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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 paper, we posit that such nutrition-sensitive programmes can reduce malnourishment by encouraging further investment in diet. To test this hypothesis, we study the World Food Programme's "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 endline: receiving no assistance; receiving nutrition-specific assistance; or receiving nutrition-specific assistance and nutrition-sensitive food for assets programming. When provided alone, food aid has no nutritional impact, relative to receiving no assistance. However, we observe pronounced positive effects when food aid is paired with assets-based programming. We 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 asset-based nutrition-sensitive programmes interact positively with nutrition-specific programming.

Keywords: Impact evaluation; nutrition; food for assets; food aid; Niger; World Food Programme.

JEL Codes: C F35; H84; I15.

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1. Introduction

The importance of nutrition status in a child's first 1,000 days is well-established (The Lancet, 2008). Due to high returns and associated cost-effectiveness, agencies focusing on nutrition have increasingly sought to target programming at children during this period (Navarro, 2013). Despite a long research history on the impact of food aid (see: Maxwell and Singer, 1979 and Barrett and Maxwell, 2007 for in-depth reviews), however, the theories of change and how impact is delivered are not, necessarily, clear cut in all contexts.

Literature to date has tended to exhibit three features. The first is a propensity to focus on what assists in the recovery from shocks or other emergencies (Tusiime et al., 2014; van der Veen and Begrehiwot, 2011; del Ninno, et al., 2007; Gilligan and Haddinot, 2007; Yamano et al., 2005; Quisimburg, 2003). Accordingly, even if we ignore arguments that a clear consensus on the impact of nutrition programming might not exist (Awokuse, 2011), it is not immediately obvious how it performs, or can be expected to perform, in situations of prolonged, chronic, food insecurity.

Second is that work has tended to focus on "nutrition-specific" programming (WFP, 2013a; 2016; van der Veen and Gebrehiwot, 2011). Even ignoring debates about whether such interventions might distort production incentives or other aspects of local food markets (Tadesse and Shively, 2009; Gelan, 2007; Barret et al., 1999; Stevens, 1978; Schultz, 1960) or not (Bezu and Holden, 2008; Abdulai et al., 2005), the impacts of nutrition-sensitive programming have tended to be ignored. While some evidence shows positive outcomes from such aid in a macro-sense (Mary et al., 2018; Ruel et al., 2013), little is known about how such outcomes are delivered or supported by individual programmes.

Third, is that studies to date have tended to focus on a small number of scenarios that are "ideal" for food aid to perform well (WFP, 2013a; 2016). That is, in locations with relatively stable populations, with regular access to health centres and with no threats of fragility or conflict (one counter-example is Tusiime et al., 2014, who show evidence of increased food consumption in conflict-affected Northern Uganda). There remains insufficient understanding and clarity about the impact of interventions (nutrition-specific or nutrition-sensitive) in environments where malnutrition treatment is constrained further by conflict and / or fragility, often due to data limitation and quality issues. Despite such constraints, it is important to analyse the performance of programmes in such locations as the theories of change - and thus the empirical findings - from more "ideal" scenarios may not be immediately relevant in more challenging places.

We therefore ask three research questions focused on addressing these respective knowledge gaps. First of all, we seek to understand the performance of nutrition-specific and nutrition-sensitive programming in the context of long-term, chronic malnutrition and food insecurity. Second, we contrast and compare the relative performance of two forms of programming: a bundle of nutrition-specific food-provision assistance programmes; and a nutrition-sensitive "food for assets" (FFA) programme that does not explicitly aim to boost nutrition. Third, we ask whether or not it is possible to trace positive nutrition impacts from one or other of these bundles of programming in a highly fragile environment.

To this end, we study the World Food Programme's "Protracted Relief and Recovery Operation (PRRO)" in Niger. To do so, we analyse two waves of panel survey data, collected at the household level. We combine this with anthropometric data for each child in those households, collected shortly before the treatment period began and shortly (c. 3 months) after it ended. At endline, households fall into three distinct treatment groups: those that receive no assistance - Group 1; those that receive nutrition-specific assistance (in this case, direct and indirect provision of nutritional supplements) - Group 2; and those that receive a combination of nutrition-specific assistance and a nutrition-sensitive programme aiming to boost households' assets through reductions in the stress of meeting basic nutritional requirements - Group 3.

Noting potential endogeneities in the treatment group into which a household falls and sample attrition, which is not atypical in the kind of difficult scenario we study, we adopt a tailored set of econometric analyses. We construct an instrumental variable, based on the spatial lag of recent insecurity threats, that strongly predicts treatment group. We augment this process by conducting propensity score matching to "balance" the treatment and control groups. We treat attrition as a Heckman-style selection problem and use a quirk of the scenario we study as the selection criterion. Households in villages scheduled for enumeration on Fridays are significantly more likely to leave the sample than those on other days. Niger has a majority Muslim population and a high degree of religiosity, resulting in reduced productivity (including amongst our enumerators) during the Jumu'ah.

Using difference-in-differences estimations corrected for these sources of bias, we test the relative performances of each assistance combination. These analyses suggest that nutrition-specific assistance (Group 2) does not lead to improved nutritional outcomes for children, when compared to Group 1. By contrast, we see a pronounced positive impact when we compare Group 3 to Group 1; and Group 3 to Group 2. Results are robust across multiple econometric specifications and two constructions of our data: a child-level balanced panel; and a household-level pseudo-panel.

Jointly, these results show three key findings:

First, we find some evidence that third party interventions can perform a positive role in boosting nutrition outcomes in chronically food insecure places, in addition to the role they perform in post-emergency scenarios.

Second, that - at least beyond the immediate programme period - these effects are delivered only by assistance bundles that include nutrition-sensitive programming. We find no evidence that nutrition-specific programming, when provided in isolation, improves nutritional status relative to the receipt of no assistance. Nutrition-sensitive programming - leading to the creation of assets - is effective in improving nutritional outcomes when provided with other forms of nutrition-specific programming. This outcome may be surprising, as it relies on a longer causal chain. At the same time, it suggests that not only is nutrition-sensitive programming effective in boosting childhood nutritional outcomes, matching recent macro-level evidence (Mary et al., 2018; Ruel et al., 2013) but also that provision of "food for assets" does not act as a disincentive to invest in childhood nutrition, or to produce food. Considering the accepted medical evidence (de Pee and Bloom, 2009; Nackers et al., 2010), these results indicate that constraints pertaining to malnutrition-specific programming weigh stronger in Niger than other scenarios; but that nutrition-sensitive interventions do not suffer the same concerns and, therefore, provide a stronger solution to on-going chronic malnutrition.

Third, we find some evidence that nutrition-based interventions can perform in highly constrained fragile environments. However, that these effects are not driven by nutrition-specific programming suggests an urgent need to revisit both programming modalities and the theories of change at operation in such places.

The remainder of this article is structure as follows. In the next section, we discuss a brief theoretical background to why nutrition-specific and nutrition-sensitive programmes might perform, drawing examples from the cash transfers for nutrition literature. In Section 3, we discuss the country context, including the challenges to nutritional programming in Niger. In Section 4, we discuss the PRRO, its implementation and theories of change. In Section 5, we present our data and analytical methods. In Section 6, we present and discuss our results, and in Section 7 we offer conclusions.

2. Background

In most cases, the theory of change behind nutrition-specific interventions is obvious. Through direct provision of food and other nutritional supplements and indirect provision (such as via vouchers or conditional cash transfers (CCTs), access to food and calories at the household level increases and, with it, average nutrition levels improve. Precisely how applicable such approaches are to situations of entrenched or long-term food insecurity, rather than more acute emergencies, requires further thought, however. In the case of direct and indirect provision of food aid and nutrition-specific programming, questions arise about the sustainability of any effects. If malnutrition is chronic or the consequence of underpinning structural weaknesses, the gains associated with providing nutrition supplements are likely to be lost unless it also tackles these weaknesses, or unless the assistance is provided in perpetuity.

By contrast, nutrition-sensitive programming, such as unconditional cash transfers (UCTs) or other programmes that assist in the development of assets may enable longer-term gains, through tacking (at least at the individual level) experience of these structural weaknesses. In turn, provided in combination, nutrition-specific and nutrition-sensitive programming might provide immediate boosts in nutrition status that households can then sustain in the longer-term.

In turn, while there is evidence of the efficacy of malnutrition treatment with ready-to-use therapeutic food in general[1] (de Pee and Bloem 2009) and in Niger in particular (Nackers et. al. 2010), more evidence is needed to evaluate the role, particularly at the level of individual interventions, played by nutrition-sensitive, as well as nutrition-specific, assistance (Ruel et al., 2013). This is especially so for situations of chronic malnutrition, due to the complex range of causes that result in such situations.

In Niger, for example, widespread poverty, an over-reliance on rain-fed subsistence farming, low education levels and some of the world’s highest rates of fertility and population growth have resulted in a highly food insecure population. Chronic malnourishment affects approximately half of all children, and the global acute malnutrition (GAM) prevalence rate among children aged 6-59 months is consistently above the WHO ‘warning threshold’. Malian refugees and individuals displaced by violence in northern Nigeria also contribute to the food

[1] De Pee and Bloom (2009) call for more evidence and comparisons with new or modified commodities and current fortified blended foods as compared to ready-to-use therapeutic foods. The latter have been proven effective to address severe acute malnutrition (SAM).

insecurity landscape. In turn, questions arise about how best to tackle these interconnected causes of entrenched malnutrition.

In this section, we seek to understand how nutrition-sensitive, as well as nutrition-specific, might be expected to work. To do so, we draw lessons from two forms of cash-transfers that aim to boost nutrition. CTTs, which we view as nutrition-specific, in the sense that such transfers can *only* be used to purchase food; and UCTs, which we view as nutrition-sensitive, as they do not constrain where the money can be spent. CCTs aim to provide cash to households, conditional on agreement that it will be used (at least in part) to boost children's outcomes (Akresh, de Walque and Kazianga, 2016), implying investment in children's outcomes. By contrast, UCTs do not specify behavioural conditions, meaning that any nutrition impacts rely on particular forms of decision-making within the household.[3] As noted by Baird et al. (2014), food insecure households might be more *income constrained* than *knowledge constrained*, suggesting some grounds to believe that nutrition-sensitive UCTs should perform at least as well as nutrition-specific CCTs in general circumstances.

Akresh, et al. (2016) show that CCTs and UCTs lead to improved health outcomes of children aged 0-5, as well as having positive impacts on other outcomes. Cruz and Ziegelhofer (2014) show positive impacts of CCT programming on the food Engels curve. Households increase private expenditure on food beyond what would be expected by the corresponding income effect from the programme. Fiszbein and Schady (2009) show improvements in the household food Engels curve in seven Latin American countries and show an income effect that allows households to invest in behaviours that are beneficial to children that is reinforced by a substitution effect that decreases the opportunity costs of good practices.

[3] An often-cited concern, particularly at the policy-level is that UCTs may be wasted, as they can be used to purchase alcohol, drugs and other temptation goods. In a review of 19 studies, however, Evans and Popova (2014) find little evidence of extensive consumption of such goods in the context of UCTs.

More directly, Barrett and Lentz (2013) show that a large proportion of transfers are consumed as food. Harvey and Marongwe (2006) suggest that between 84% and 91% of cash transfers is consumed as good in Zambia. Hidrobo et al. (2014) compare food transfers, vouchers and cash transfers in northern Ecuador and find that all treatments significantly improve the quantity and quality of food consumed. Afkar and Matz, 2015, show that CCTs had a bigger impact on food and nutrition outcomes than in-kind programming in Indonesia. Skoufias et al., 2008) show positive evidence from a food support programme in Mexico. Porter and Goyal (2015) find a positive medium-term nutritional impact of the programme on children aged 5-15 from a UCT programme in Ethiopia

In general, we observe positive impacts on nutrition status in line with improving households' access to income via UCTs, as well as more direct effects from CCTs. In turn, this suggests that there are good grounds to believe that an asset-building nutrition-sensitive intervention should lead to positive impacts on nutrition and health outcomes of children in any situation, via a similar theory of change. By increasing assets, the income constraint is relaxed and better decisions can be afforded by the household. Indeed, under the assumption that the asset-building aspect of the intervention is successful and endures in the long-term, there are good grounds to believe they could be better placed to perform well in contexts of long-term and entrenched malnutrition and food insecurity. In this setting, the effects of nutrition-specific programming may endure only for the duration of the intervention as they do not, in the long-term, tackle the income constraint.[4] Due to increases in assets, by contrast, income may be less constrained in the longer term, suggesting sustained improvements in nutrition status.

From this, we develop three hypotheses for the PRRO in Niger:

[4] By contrast, one might expect that in a post-emergency period, food production, availability and prices converge to equilibrium once the emergency period has passed.

1. Nutrition-specific programming should result in a temporary improvement in the nutrition status of recipient households that converges to underlying levels in the post-programmatic period. In turn, the empirical predication relies on the duration of this convergence period.[5]
2. The addition of nutrition-sensitive programming to nutrition-specific programming allows the temporary improvement of recipient households to be sustained in the medium- and longer-term (Group 3 > Group 1).
3. Nutrition-sensitive programming increases households' food consumption capacity, leading to an exogenous increase in nutrition status, regardless of the provision of nutrition-specific programming (Group 3 > Group 2).

3. The Case of Niger

Between its independence in 1960 and the return of constitutional civilian government in 2011, Niger suffered austere military rule. However, threats to stability in the country remain manifold. Borders are porous. This allows the easy movement of militants from north-eastern Nigeria and Mali to move in Nigerien territory, which is reflected in the growing influence of Islamic terrorist organisations. Every neighbouring country has suffered physical insecurity or outright war in recent years, with many armed groups operating from Niger and with refugees moving to the country for safety. The political situation remains uncertain, government institutions are usually weak and the army remains unable to enforce security.

Internal and external migration remain high, which itself strongly correlates with a high poverty rate. The median per capita income in the country is $360, 46.3% of the population lives under the absolute poverty threshold and the country sits at the very bottom of the Human

[5] This implies one of two empirical outcomes for our analysis, depending on the duration of this convergence period. If it is longer than three months after the end of the assistance period (when we collect the endline), we expect Group 2 to exhibit better nutrition outcomes than Group 1 (H_1). If it is shorter than three months, we expect to see no difference between Group 1 and Group 2 (H_2). Under H_1, Group 2 > Group 1. Under H_2, Group 2 = Group 1.

Development Index (United Nations, 2017). Although GDP growth is forecast at between 4.5% and 5.5%, Niger has one of the highest birth rates in the world, implying that per capita growth is stagnant (World Bank, 2015).

Although Niger is a main producer of uranium and began pumping oil in 2011, it remains challenged in terms of resources. Agriculture generally, and food production specifically, are hampered by recurrent droughts, with annual food shortages during the lean season. The low nutritional health of the population, a consequence of low and insecure food availability, is exacerbated by weak government spending on healthcare (Burki, 2013). Life expectancy is low and the fertility rate, at an average of eight live births per woman (Burki, 2013), is among the highest in the world. In addition to poverty and demographics, the country relies heavily on rain-fed agriculture (Shideed, 2017), which is particularly prone to weather shocks.

Demography, therefore, plays a key role in food crises in Niger. Women typically marry before they are 16 and short spacing between births means that many are anaemic and suffer micronutrient deficiencies. In turn, this affects the development of children in utero and causes low birth weights. Gender inequality is an important determinant of population growth and, accordingly, the political economy of hunger in Niger. At the institutional level, land commissions have sought to decentralise land access as well as to introduce statutory legislation to promote gender equality in land access. However, in practice, local structures are more often used to regulate and secure access to natural resources, while religious justifications remain important in excluding women from accessing land (Diarra and Monimart, 2006).

Compounding these legal and social realities, there appear to be dual trends of defeminisation of agriculture in Niger and the increasing feminisation of poverty (Diarra and Monimart, 2006). In southern regions, increased land pressures have created greater responsibilities for mothers and grandmothers to provide food. The result has been that food insecurity has increased for children, whose diet has worsened through a lack of variety. In northern regions, exclusion from pastoral production and a lack of non-agricultural alternative employment has forced women to farm for derisory returns under highly insecure conditions.

Malnutrition is widespread in the country, particularly among infants and young children. 14.8% of children aged 6-59 months suffer from global acute malnutrition (GAM), with 15.7%, in rural compared to 10.7% in urban areas (DHS, 2013). Children aged 6-23 months are also significantly more likely to experience GAM than older children. In half of the regions of Niger, GAM is above the WHO emergency threshold. One in three children is underweight, chronic malnutrition affects 42% of those under five years of age and, at any one time, up to three million people suffer food insecurity annually (Concern, 2013).

Fighting malnutrition and food insecurity sits high on the list of priorities for the government, with the president having initiated the High Commission for 'Nigeriens Nourishing Nigeriens' (HC3N). The Ministry of Health coordinates the overall approach to malnutrition with support from WFP and UNICEF (Burki, 2013). In this regard, multiple stakeholders have become involved in malnutrition interventions in the country, which are coordinated closely with WFP in the implementation of the PRRO in Niger. Despite the need for interventions and in spite of coordination between government and agencies, questions remain about the effectiveness of these programmes, not least because of the security situation in Niger and the frequent movements of potential beneficiary households. More generally, however, knowledge gaps also remain on the relative performance of different forms of assistance: on whether or not assets-based programmes deliver nutritional impacts; and whether or not food-assistance programmes perform at all in complicated, fragile and / or conflict-affected settings in Niger.

All of this significantly complicates the basic theories of change, especially for nutrition-specific programming. On the one hand, such programming is likely to boost nutrition in the short-term as they do elsewhere, through the direct and indirect provision of food and other supplements. In the medium-term, however, it is unclear how such improvements can be sustained, as such programming is unlikely to target the other underlying structures of child malnutrition. By contrast, nutrition-sensitive programming exhibits a longer causal chain, which itself complicates the theory of change. On one hand, increases in assets and income, especially under *income*, rather than *knowledge* constraints, performs the role of Sen's (1982) "access to food" and "utilisation of food" concerns, which should result in improved nutrition. By contrast, however, precisely how assets-based programming, itself, can be expected to perform in an environment as challenging is another question, entirely. In such regards, Niger is far from unique, yet such concerns raise questions about the relevance of other empirical results for such environments. In turn, testing the relative performance of both nutrition-specific and nutrition-sensitive programming in such locations is of paramount importance.

4. The Intervention

4.1 Programmes

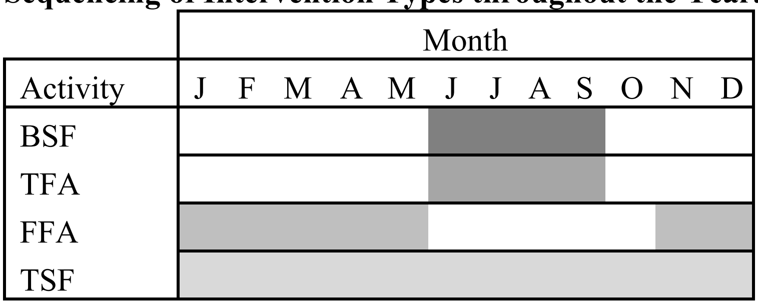
Given the chronic food insecurity and malnutrition in Niger, the World Food Programme has sought to work with the government of Niger to provide nutritional assistance through the Protracted Relief and Recovery Operation (PRRO). The operation under study in this impact evaluation took place over three years from January 2014 and was implemented by WFP through local service partners, including local and international NGOs. The intervention had three main strategic objectives and corresponding activities, spread across the year (Figure 1):

a) **To reduce the impact of constraints and adverse seasonal impacts on lives and livelihoods.** The intervention aimed to prevent a peak of acute malnutrition and mortality. Under this strategy, two major activities, Targeted Food Assistance (TFA) and Blanket Supplementary Feeding (BSF) were implemented during the lean season (June to September) and took the form of direct and indirect provision of food to qualifying households and / or regions.

b) **To increase access of poor to assets and food through land rehabilitation, water harvesting and local purchases.** The major activity under this objective was Food for Assets (FFA) programming. FFA focused on land rehabilitation, water harvesting and irrigation through partnerships in pastoral areas, while providing assistance to cover basic household nutritional needs. In this regard, it is important to note that the main purpose of FFA was not to boost nutrition but, rather, to allow households to accumulate assets by removing the burden of meeting basic nutrition requirements.

c) To support integrated safety nets. This objective aimed to treat acute malnutrition among children aged 6-59 months and pregnant and lactating women (PLW). This was achieved by implementing community-based interventions to prevent malnutrition. The main activity was implemented throughout the year. Targeted Supplementary Feeding (TSF) targeted moderate acute malnutrition (MAM) among children aged 6-59 months and PLW through government health facilities.

Figure 1: Sequencing of Intervention Types throughout the Year.



Source: Authors' construction based on WFP (2016).

The PRRO embodied a theory of change building on the explicit national Nigerien strategy to reduce malnutrition. The programme was launched on the basis of targeting the needs of vulnerable communities in accordance with the national priority plans of the country.

This established that malnutrition in Niger is multi-causal and has significant long-term developmental impacts. In this regard, the PRRO sought to screen for malnutrition; to prevent malnutrition; to treat malnutrition; and to engage in nutrition education. To do so, treatment (TSF) and prevention (BSF and TFA) programmes were offered to, respectively, treat and prevent the onset of malnutrition through the direct and indirect provision of nutrition supplements. In turn, through this provision, one would expect (at minimum) short-term boosts in the nutrition status of recipients.

Despite the intuitiveness of this theory, however, two concerns arise that might mitigate the attainment of the goal of boosted nutrition status. First is the uptake of offered assistance. In terms of uptake, a number of challenges were present, relating in particular to distance and access to assistance as well as gender norms. If households in particular need not avail themselves of the assistance available to them, we are unlikely to capture the anticipated impact of the intervention. Second is the use of so-called 'makeshift' coping strategies, such as migration, borrowing, sale of possessions or changes in food habits. Olivier de Dardan (2008) notes that, in Niger, these strategies (if not necessarily 'positive' behaviours) tended to be more effective approaches than food aid. In turn, if households that receive food aid behave differently from those who do not the nutritional benefits of assistance may not be captured.

A host of subsidiary concerns also arise. First are perceptions of unfairness in access to assistance (Olivier de Dardan, 2008; Issa, 2008); the requirement of beneficiaries to take an active role in their future, which complements the effectiveness of some programming (Bakou and Guillermet, 2008); and the strategies that households employ in order to capture food aid (Koné, 2008). Such strategies may not be universally positive, as suggested by related qualitative work undertaken in the context of our study (Hambali, 2018). For example, a household may redistribute food to certain children (knowing that a malnourished child will receive assistance) or may instigate illness such as dysentery. Each of these concerns provides a major challenge to the intuitive theory of change.

FFA, on the other hand, was not designed specifically to boost nutrition but, rather, aimed to remove concerns for households about meeting the most basic nutritional needs, in order to allow the accumulation of household assets. In this regard, while the programme did provide food, wider nutritional gains from the programme rely on investment in nutrition by the household. For that reason, steps were taken to boost the potential nutritional impacts of FFA, including: focusing on female beneficiaries; developing assets that protect against food shocks; and boosting access to markets. While FFA was hence not, inherently, a counter-MAM set of programmes, there are reasons to believe that it positively (or, indeed, negatively) interacted with the nutrition status of the individuals who received it - not least if Niger's experience with food insecurity is dominated by an income, rather than knowledge, constraint. More so, when provided in conjunction with other forms of WFP assistance, as it was in much of our sample, there were also theoretical grounds to believe that synergies could develop. In this case, a short-term boost from provision of nutrition supplements can be sustained in the long-term by improved income and, thus, increased investment in children.

4.2 Targeting

WFP targeted programmes geographically in three stages. First of all, 'priority districts' were defined using available indicators of food security, nutrition, livelihoods, population movement, infrastructure and other aggravating factors. These districts were then ranked in terms of the adversity they faced, with aid provided, according to available budget, to those with the worst observable characteristics. In the first round of programming (essentially corresponding to our baseline survey), 70 priority districts were targeted. Due to a decreasing budget, however, only 37 districts were targeted in the next phase. This means that, between baseline and endline, assistance effectively ended in 33 districts in Niger. This implies that the

33 districts which lost assistance are likely to be those of the initial 70 that were the least vulnerable.

This 'phase out' of some areas from receipt of assistance is the backdrop to one of the major analytical processes we use in this research – although the nature of selection here implies the need for the use of bias-correction techniques. Given that the methodology used to define the WFP priority areas relies on the Nigerien Government's analysis, it is perhaps unsurprising that there is some overlap between these priority districts and those of the Government of Niger.

Within the WFP priority districts, the second stage aimed to target villages that were determined to be under particular stress. This approach was based on a list generated by WFP looking at deficits in food availability and weak local adjustment capacity, in turn based on an analysis of data collected by the Nigerien government. This included targeting areas where food security was over 30% from 2006 to 2011 and where GAM had been above the 15% emergency threshold at least twice. Indicators for school completion, prevalence of nomadism, agricultural potential and convergence of partners' activities were also integrated into this process.

At the village level, the final stage then selected specific households to receive assistance. This decision was jointly made by implementation partners, village committees and WFP. This approach aimed to define which households were 'very poor' and was implemented at the community level using the household economy approach. Given the involvement of a range of actors, including village authorities, certain questions were subsequently raised about whether assistance (always) made it to the households with the worst (un)observable conditions.

5. Data and Methods

A major barrier to the study of the performance of development and nutrition interventions in highly fragile environments is data quality (Puri et al., 2017). As per Deaton (2010), however, we note that a range of strategies exist to overcome these problems. Given how the functioning of more general theories of change might be affected in such environments, we note that the importance of conducting such work, from both an academic and policy perspective.

To evaluate the PRRO, we draw on bespoke and unique data from two WFP-supported household surveys.[6] Each survey includes typical household socioeconomic and demographic information, as well as anthropometric measurements for each child in the household. These measurements include a child's weight, height and mid-upper arm circumference (MUAC), for which we generate a z-score. We use this indicator to capture relatively short-term fluctuations in nutrition status. To capture more long-term impacts, we calculate weight-for-age and height-for-age z-scores.[7] We match our anthropometric measurements to questions in the household survey that ask heads whether or not they received certain forms of WFP assistance in the previous calendar year. Analyses then evaluate the relevant dynamic differences between baseline and endline, based on the combinations of assistance received, using a basic difference-in-differences model.

In this approach, at least under the assumption of parallel trends between the treatment and control groups, the impact of the intervention is isolated by an interaction between the treatment variable and a variable denoting the period after the treatment has been delivered.

We thus estimate:

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

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 endline observations and 0 for

[6] We note that both raw anthropometric data and the z-scores that can be derived from them are highly sensitive to measurement error. Enumerators, however, were fully trained in the precise use of highly specialised instruments for the collection of anthropometric data by WFP and INS. This work was overseen in the field by a member of the research team. We draw on the field medical expertise of WFP to ensure the accurate reporting of other information in the surveys and other field records.

[7] All z-scores using the National Centre for Health Statistics (NCHS) normalisation process and implemented using Stata's in-built software.

baseline observations; $Treat$ is a variable that defines the assistance receipt group into which a given child falls; $After * Treat$ is the interaction of these two variables, which isolates the programme impact; X is a vector of control variables; and ϵ the regression error term.

The first wave of our data (nominally the baseline) was collected in March 2014, before the current PRRO was rolled out and before the beginning of Niger's lean season. The endline was collected at the end of September 2017, at the end of the PRRO period and after the lean season. In both cases, the lean season qualification is important, because some forms of assistance in our analysis were available during this period only. This makes it important that the baseline was conducted before the lean season (to ensure our sample is not contaminated by short-term effects from these seasonal programmes) and that the endline was conducted afterwards, precisely to capture these effects. In principle, this allows us to capture the longer-term effects of the entire PRRO, while accounting for the short-term dynamics of some of the assistance typologies therein.

The baseline is a representative sample of households entitled to receive WFP assistance under the PRRO, comprising $m = 3{,}517$ households. Each child aged 0-59 months in those households was sampled, giving a sample of $n = 5{,}527$ children, sampled from 236 villages. As the sample is constructed based on entitlements to WFP assistance at the time of collection, the entirety of the baseline (within a small but acceptable error) has received FFA in the last calendar year but no other forms of assistance. The endline survey was designed to resample all households and children included in the baseline. As our empirical strategy relies on difference-in-differences as a workhorse model, a new sample frame was not drawn up due to the requirement of two observations for each child. The endline sampled $n = 3{,}482$ children[8] from $m = 1{,}694$ households in 200 villages.

[8] This figure includes all children aged 0-59 months at the time of baseline data collection and all children that were newly born to households in the baseline sample.

Households in the endline survey fall into one of three assistance groups. Due to an (exogenous) reduction in the WFP budget, just over half of endline households move from receiving FFA only in the baseline to receiving no assistance in the endline. Approximately a quarter continue to receive FFA and also receive some form of treatment and / or prevention assistance. The remainder lose access to FFA but receive at least one form of other assistance. We show these group splits in Table 1.

[TABLE 1 ABOUT HERE]

Due to intra-household movement of children, relatively high childhood mortality and children born between the two sampling periods, we have a (balanced) panel of children $n \times T = 2{,}804$. The reasons for this attrition are numerous but can broadly be split into 'exogenous' attrition and 'structural' attrition. In particular, two exogenous events took place during data collection that hampered our efforts to follow up. The first was a deteriorating security situation in Diffa, caused by Boko Haram activities near the border with north-east Nigeria. This meant that our enumeration teams could not visit the region. The second was that an enumeration team was attacked and robbed in Tillaberi, which resulted in a large loss of data in this region.[9] In both cases, as all observations within particular geographical areas were lost, we consider this attrition to be exogenous.[10] Furthermore, we note that, as per Nigerien custom, households have 'permanent' and 'temporary' children, due to child-swapping practices. These practices increase attrition at face value but also reflect coping strategies in the face of (nutritional) insecurity. Having said that, the remaining children are likely to be those of core

[9] Especially given this event, questions could be raised with regards to the ethics of our research. All research and data collection efforts were approved by internal WFP (and consequently) United Nations protocols. No assistance was withheld for the purpose of research.
[10] Put another way, we argue that these events do not affect the internal validity of our approach as all individuals and households interviewed at baseline within these regions are missing from the endline, not just those with particular observable characteristics.

concern to the households, leading our experience of attrition (in the sense of data loss) to be less severe than first feared.

As shown in Table 2, however, we see significant structural differences on a range of key variables, as measured at baseline, between the households that leave the sample and those that remain (excluding those lost in Tillaberi and Diffa). In particular, a number of these variables – such as the agroecological zone in which a household lives and household assets – are significantly worse for attritors than remainers and hold obvious ties to household nutrition outcomes.

[TABLE 2 ABOUT HERE]

In this regard, biases would likely arise in typical linear statistical analyses, including uncorrected difference-in-differences estimators, which could in turn lead to false inference being drawn from such analyses. To overcome this problem, we treat attrition as a selection problem (Heckman, 1979). That is, given the structural attrition in Table 2, we note that at endline, nutritional status is only observed for households with particular characteristics and not for the entire sample. With this in mind, we conduct a Heckman-style correction to account for potential biases arising from sample selection into the endline. To conduct this correction, we require a selection criterion that determines presence in the endline but that is not a determinant of our nutrition outcomes of interest.

We posit that the day a village was scheduled for enumeration is a valid criterion. Specifically, we show that if a household is in a village that was scheduled to be visited on a Friday, it is significantly less likely to remain in the endline than those visited on other days. Niger is an overwhelmingly Muslim country and a large percentage of the population observes Friday prayers. Friday, therefore, is de facto a half-day for most workers in the country including our National Statistical Agency enumeration teams. Thus, if a household is in a village that was randomly scheduled for enumeration on a Friday (based on a fieldwork plan drawn up in advance in Niamey), there was less time to collect data in that village than in other villages, resulting in correlation between the timing of the visit and the number of households from which data was collected.

Data collection took place in a formalised manner, with teams travelling in circles from the regional capital to each village and visiting one village per day. In turn, each village was visited in the order of its relative distance from the regional capital, meaning that the only determinant of whether or not a given village would be visited on a Friday was the day enumeration in a region started. Enumeration started immediately following completion of training and teams were randomly assigned a region (based on those still remaining after previous random draws). In this regard, there are no structural reasons why particular villages would be visited on a Friday, ensuring that the nutritional status in a village has no impact on whether or not it was scheduled to be visited on a Friday and vice versa.

We thus estimate the following selection equation by probit:

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

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

We present the results from Equation (2) in Table 3. As can be seen, households that live in villages that were scheduled to be visited on a Friday are significantly less likely to remain in the sample at endline than households visited on other days. Given the strong exogeneity of this criterion, we use this approach to generate the inverse Mills ratio, which we include directly in our main analysis to account for the effects of attrition.

[TABLE 3 ABOUT HERE]

Given local norms in Niger, such as 'child sharing' between households, we note that the level of attrition at the child level may be significantly greater, or at least structured differently, than that at the household level. Therefore, we adopt a second approach. Nutritional indicators are averaged at the household level, and analyses then conducted on these averaged outcomes (a so-called "pseudo-panel" approach).[11] This approach generates a balanced (household) panel $n \times T = 2{,}446$.[12] Alone, this approach does not fully account for attrition but, rather, for the complicated internal household dynamics at play in Niger. Therefore, as before, we still include the inverse Mills ratio from the Friday selection criterion.

A second source of endogeneity also arises due to the targeted nature of the PRRO. In this situation, major biases could influence the direction, scale and significance of the effect. For example, if the worst-off households receive the greatest levels of assistance, assistance may appear, incorrectly, to cause reductions in the nutritional status of recipient households. By contrast, if the worst-off households are excluded from receiving (certain forms of) assistance, the PRRO may appear to improve nutrition status without causally doing so. To investigate such potential biases, we conduct a simple test that compares the mean for each group of a range of key input variables compared to the pooled mean of the other groups. In these analyses, a purely random distribution of assistance would imply that there are no

[11] We note that, in effect, this could change the age composition of children in a household at a given time, especially given Nigerien social norms. In this scenario, we therefore use z-scores to normalise the underlying nutrition variables relative to age.

[12] Although this number is smaller than we would expect, given a simple doubling of all households at endline, this should not be taken to imply that there are households in the endline that were not present at baseline. Rather, it means that these are households that were in the baseline household survey but for whom we do not have baseline anthropometric measurements. In turn, including the 421 endline households for which we have anthropometric data in an analysis makes little sense, as we do not have corresponding baseline information. This could include, for example, households that were childless at baseline; or households for whom baseline anthropometric data was not collected for some other reason.

statistical differences between these means. We test the difference in the mean using a standard t-test. We present results from this simple analysis in Table 4.

[TABLE 4 ABOUT HERE]

Table 4 shows some structural determinants of the assistance group into which a household falls. Those in Group 1 tend to be larger than average, while those in Group 3 tend to be smaller. By contrast, however, households in Groups 2 and 3 have more children aged 0 - 5. Group 3 households are more likely to have a female household head and are less likely to be polygamous. Perhaps more importantly, we see important differences between the groups in terms of the livestock and assets indices. Households in Group 3 display higher than average access to livestock and assets but are also the group who receive the most forms of assistance. By contrast, Group 1, who receive nothing, score worst in the livestock index. *A priori*, the households with the greatest livestock or other assets are likely to be those with better access to nutrition.

We propose two strategies to overcome possible endogeneity. The first is an instrumental variables approach, where we include at least one ‘instrument’ that is correlated with each endogenous variable (in our case, the assistance group into which a household falls) but not with the regression error term. We note that in a basic difference-in-differences estimator, we have two endogenous variables; the treatment variable itself and the interaction of this treatment variable with time.

In this approach, we therefore first regress each endogenous variable on all exogenous regressors and its instrument, which we denote IV:

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

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 ϵ the regression error term.

We construct two instruments. The first is the spatial lag of violence in the period before the endline survey was collected[13]; the second, the interaction of this variable with the $After$ variable. In order for the instrument to be valid, two criteria must be satisfied. First, ϕ_2 must be correlated with $Treat$ but not with the regression error term. Put alternatively, $\phi_4 \neq 0$ and $COV(IV, \epsilon) = 0$. We show the correlations between our instruments and endogenous variables for each treatment group[14] in Table 5. As can be seen, ϕ_4 is significantly different from zero in all settings.[15] As we present a just identified analysis, however, we can only qualitatively justify the exclusion restriction. To this end, we note that we use the spatial lag of violence. In other words, this relates to violence that took place in communities other than that in which the household under observation is resident. In turn, there are no grounds to believe that such violence should directly influence nutrition in the community under study, as it did not take place there. Nothing that violence can be highly spatially autocorrelated, however, we note that local and distant violence might be correlated. To account for this, we control for violence that took place in the community of observation. After controlling for the level of violence in the community of observation, this spatial lag has no plausible impact on nutrition.

[TABLE 5 ABOUT HERE]

[13] See Ferguson and Michaelsen (2015) for baseline work using the spatial lag of conflict as an instrument and Brück and Ferguson (2018) for its use in an impact evaluation. In this case, we use the distance between a village and the nearest violent event to that village (other than those that took place within that village), as recorded by the UN's security team, in the three months before the endline was collected.

[14] We use three constructions of the variable Treat: Group 3 versus Group 1; Group 3 versus Group 2; and Group 2 versus Group 1.

[15] Comparison of these correlations against the Stock-Yogo thresholds suggests a maximum bias of less than 1% when analysing Group 3 versus Group 2, and Group 3 versus Group 1; and a maximum bias of about 12-13% when analysing Group 2 versus Group 1.

Our second approach involves the use of propensity-score matching across treatment groups (Rosenbaum and Rubin, 1983). In this approach, we account for the covariates that predict which treatment group an individual belongs to in order to mitigate the bias that arises from non-random receipt of assistance. We match on the full range of covariates in our analyses.

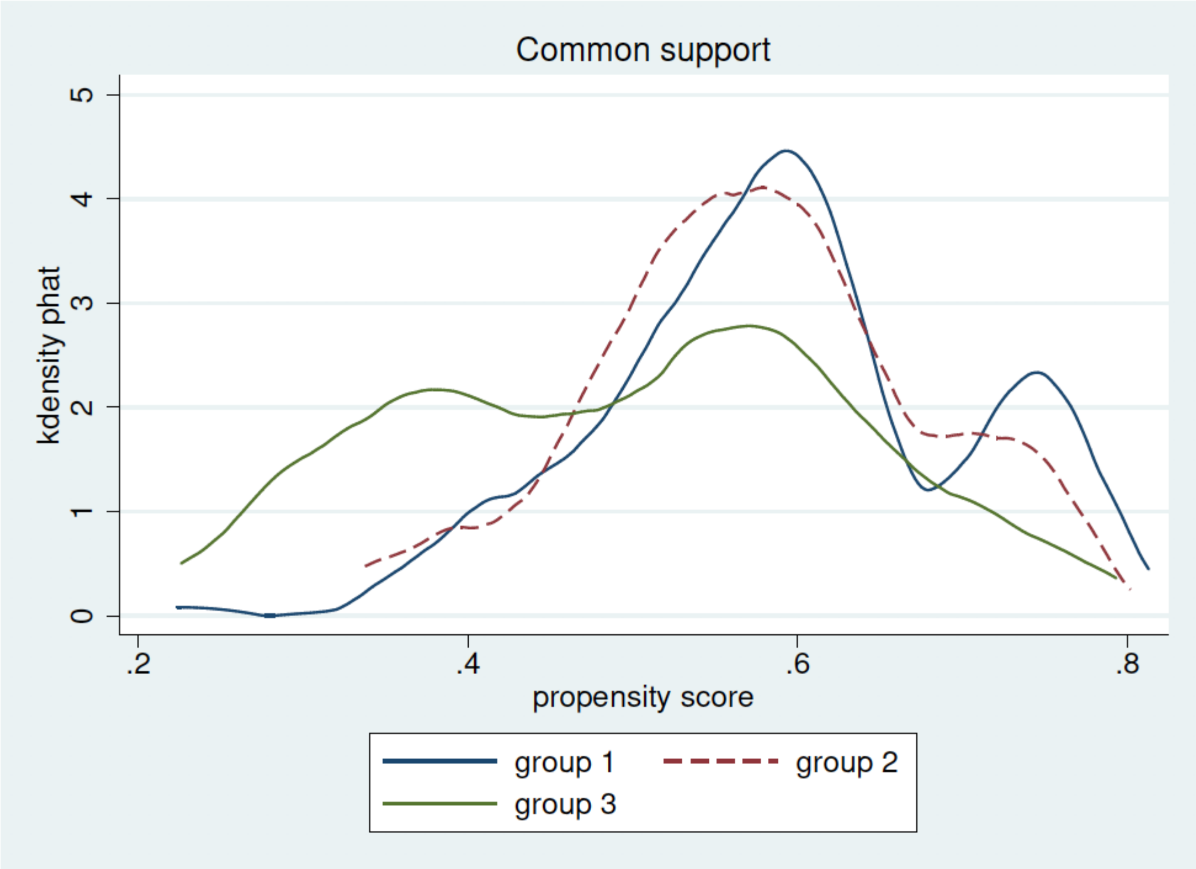
The objective of the propensity score matching is to correct in our estimations for the probability each observation has of being in a given treatment group. We fit a multinomial logit model using each household's endline treatment group as the dependent variable. We take the observable characteristics of that household at baseline that are likely to affect belonging to a particular group as the explanatory variables. We thus estimate:

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

Where the treatment status $Treat_{ihv2}$ of individual i in household h, village v and at period two, takes values 1, 2 or 3 indexed to the first period, X is the vector of exogenous demographic variables in the baseline that includes and indicator for the household level of assets. IV refers to the instrument described previously; ψ_j are regression coefficients; and ϵ_{ih2} the regression error term.

In this estimation, the actual values taken by the dependent variable are irrelevant as the multinomial logit ignores any natural order. Once we have predicted the predicted probability of an observation being in each of the treatment groups, we perform a Kernel-based Propensity Score Matching diff-in-diff, following Leuven and Sianesi (2018). In Figure 2, we show the common support of this approach in the context of our data.

Figure 2: Common Support in PSM across Treatment Groups



We thus conduct four main analyses that generate the estimates on which we base the discussion of our results. These are summed up in Equations (5) and (6):

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

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

And:

$$\underline{N}_{hvt} = \beta_1 After + \beta_2 Treat_{hv} + \beta_3 After * Treat_{hv} + \beta_4 X_{hvt} + \beta_5 M + \epsilon_{hvt} \quad (6)$$

where $\underline{N}$ is the average nutritional status of all children in household h in village v at time t; $Treat$ represents the treatment status of the household[16]; and the remainder of the

[16] As treatment is measured in the household survey, we note that in reality, there is no difference between this categorisation and that in Equations (1) and (5).

equation is as described for Equations (1) and (5). Subsequently, we estimate Equations (5) and (6) using the instrumental variables and propensity score matching approaches. X_{hivt} and X_{hvt} comprise the size of the household; the number of children under five years of age in the household; the gender of the household head; the number of wives present in the household; an index of household assets; and local security data, as per the requirements of the instrument.

What these equations really measure is the relative impact of belonging to a treatment group at endline. In that regard, we run each analysis three times. The first compares the impact of belonging to Group 3 versus Group 1. From this, we isolate the relative impact of receiving a combination of nutrition-specific and nutrition-sensitive programming at endline versus receiving no assistance. Second, we repeat this process to compare the impact of belonging to Group 2 versus Group 1 to isolate the relative impact of receiving only nutrition-specific programming against no assistance at endline. Finally, we compare Group 3 with Group 2 to test the relative effects of the two treatment combinations. In the latter case, however, it is important to note that we cannot precisely attribute the effect to nutrition-sensitive programming itself or to the combination of nutrition-sensitive and nutrition-specific forms of assistance. We do, however, note the important role nutrition-sensitive programming plays in either interpretation.

6. Results

We present results from our propensity score matching analyses as our preferred specification, due to the maximum extent of bias that might arise in instrumenting Group 2 assistance status. Results from these analyses are shown in Tables 6.1 - 6.3 for the child-level balanced panel and in Tables 7.1 to 7.3 for the household-level pseudo-panel. Results from the instrumental variables analyses are presented in Tables A1.1, A1.2, A1.3, A2.1, A2.2 and A2.3 as robustness checks and exhibit no major differences to those shown in Tables 6 and 7.

Tables 6.1, 6.2, 6.3, 7.1, 7.2 and 7.3 each contain four columns. In the first column, we present the results from our analyses on the MUAC z-score (MUACZ); in column two, the raw MUAC score; results from the weight-for-age z-score (WAZ) in column three; and the results for the height-for-age z-score (HAZ) in column 4. Columns 1 and 2 are designed to capture variations in short-term nutritional outcomes; with columns 3 and 4 focused on capturing longer-term impacts.

We present results from three analyses in each cluster of tables. First, we compare Group 3 to Group 1. Second, we compare Group 2 to Group 1. Finally, we compare Group 3 to Group 2. This allows us to test all three hypotheses mooted in Section 2. In other words, we compare the relative impacts of receipt of nutrition-sensitive and nutrition-specific assistance at endline against none (Group 3 versus Group 1); of the receipt of nutrition-specific programming at endline against none (Group 2 versus Group 1); and the relative impact of the combination of nutrition-specific and nutrition-sensitive against nutrition-specific only (Group 3 versus Group 2).

[TABLES 6.1, 6.2, 6.3, 7.1, 7.2 AND 7.3 ABOUT HERE]

At the child level, we see three key trends emerge. First of all, we have suggestive evidence that Group 3 are significantly better off than Group 1, certainly in terms of shorter-term nutrition indicators. That is, those children's MUAC scores have significantly improved. Although we do not see similar movements in weight-for-age, height-for-age is also positive and significant. The scale of the effect is also somewhat large. The relative impact of being in Group 3 over Group 1 is an increase in MUAC by 1.916mm.

Second, we see no material differences between Group 2 and Group 1. That is, there appears to be no impact from nutrition-specific programming shortly after the end of the assistance period. Given the timing of the endline survey, it is not clear if Group 2 assistance has no impact at all or if these impacts are lost in the period outside of immediate assistance provision. In turn, it is unclear if this finding relates to a general convergence in the nutritional status of Group 1 and Group 2; to the relative impacts of makeshift coping strategies and makeshift coping strategies forgone; or to an actual ineffectiveness of these programmes when provided in isolation. However, it does pose questions about the medium- and long-term effectiveness of nutrition-specific programming in chronic malnutrition situations and in fragile scenarios.

Finally, we see strong suggestive evidence that children in Group 3 are significantly better off than those in Group 2 across all nutritional indicators. Although this follows logically from the previous two findings, it is still important because it reinforces the importance of FFA (nutrition-sensitive programming) and, more so, it suggests these effects might also be traced to longer-term indicators. In turn, this provides some grounds to believe that the impact of FFA-based programming (in conjunction with nutrition-specific forms of assistance) may have effects over and above those related to seasonal nutritional fluctuations.

In Table 7, we show that these findings are, if anything, stronger when considered at the household level. Here, we see strong effects in both short-term and long-term indicators for Group 3 against Group 1 and Group 2; and, again, little (if any) difference between Groups 1 and 2. This is important for two reasons. First, it suggests that the child-level gains witnessed for those in Group 3 are not the result of intra-household decision-making but, rather, real gains based either on better use of food or increased food availability. Second, it provides good grounds to believe that FFA has impacts on longer-term nutritional trends, as well as on more immediate ones. Given the form of FFA, this implies that households who receive assistance in order to boost assets also see a measurable (and unintended) boost in nutrition. A second implication is that these households continue to make better investments in nutrition compared to those who receive only other forms of assistance, or those who receive no assistance at all. That such findings are traceable to the individual level as well implies that these household benefits trickle down to all members of the household, suggesting that intra-household decisions remain positive in the context of nutrition-sensitive assistance.

Despite the strength of these findings, however, we must be realistic about precisely what is captured. Although these findings point to the strong role played by FFA-based forms of assistance, we must more precisely consider attribution. What we can be reasonably certain about is that, at worst, FFA plays a very strong role in boosting the performance of other forms of assistance, given the findings of Group 3 relative to Group 2. However, we cannot ascertain if FFA, by itself, delivers these positive impacts; if it is the impact of the interaction of FFA with other forms of assistance that drives the impacts; or some combination of the two. For example, it is possible that FFA boosts nutrition; that FFA helps to sustain the one-time boost provided by treatment / prevention assistance; or that both occur simultaneously. Future work may like to consider the precise attribution of this effect more specifically. More generally, however, we note the positive role that FFA played, which is all the more impressive given that the purpose of this assistance was not, specifically, to boost nutrition.

We thus show that nutrition-specific programming, although it might have resulted in a temporary improvement in nutrition indicators during the project period that is not captured because of the timing of our follow-up survey, has no effect outside of this period. Or, in other words, Group 1 = Group 2. Second, we show that the combination of nutrition-sensitive and nutrition-specific assistance has a pronounced positive impact on a range of nutritional outcomes, vis-a-vis receipt of no assistance (Group 3 > Group 1). Finally, we show that the combination of nutrition-specific and nutrition-sensitive assistance outperforms receipt of nutrition-specific only assistance (Group 3 > Group 2). This combination of results confirms our hypothesised relationships and supports the notion that typical theories of change from nutrition programming are complicated in situations of chronic malnutrition and / or fragility.

7. Discussion and Conclusions

While there is good reason to believe that aid interventions can have positive impacts on nutrition (Maxwell and Singer, 1979; Barrett and Maxwell, 2007), literature tends towards three major trends. First, analyses have tended to focus on recovery from shocks and emergencies, rather than on protracted and chronic malnutrition. Second, there has been a tendency to focus on nutrition-specific forms of intervention, despite the fact that there are good grounds to believe that nutrition-sensitive programmes, especially those that reduce long-term income constraints, could be more suitable to perform in protracted crisis situations. Third, there is a general lack of work focussing on situations that are not ideal for nutrition-specific programming to work - such as highly fragile scenarios. In this article, we test the impact of the World Food Programme's PRRO interventions and examine the relevance of these results in the context of such knowledge gaps.

At the headline level, we find no impact, shortly after the end of the intervention period, of nutrition-specific programming (compared to a "control" group who received no assistance during the previous 12 months) on children's anthropometric outcomes. By contrast, however, we find positive impacts of a combined nutrition-specific and nutrition-sensitive assistance combination across a range of typical nutrition indicators. From this, we note that nutrition-sensitive interventions play a very important role in the delivery of positive nutritional impacts in a highly-complicated environment, where food insecurity is entrenched and exacerbated by fragility and other violent threats. At the same time, more direct provision of nutrition programming does not appear to have any sustained impacts in the same environment.

Despite a longer causal chain and a more complicated theory of change stemming from these outcomes, we note that the first set of outcomes are largely intuitive. In a range of scenarios, food insecurity and malnutrition relate as much to income constraints as knowledge ones (Baird et al., 2014). Supporting this hypothesis, a range of programmes using unconditional cash transfers have shown positive nutritional outcomes (Akresh et al., 2016; Porter and Goyal, 2015; Hidrobo et al., 2014), and recent macro-level evidence (Guel et al., 2013; Mary et al., 2018) supports the positive outcomes of nutrition-sensitive programming.

At the same time, whilst intuitive, these results are important for a number of reasons. First, they show that programmes that boost income appear to be more suited at delivering impacts in complicated, fragile environments with chronic food security concerns than nutrition-specific. Second, at least for as long as any impacts on asset accumulation endure, these results suggest that FFA is well-placed to deliver on-going nutritional benefits to recipients. This stands in contrast, even to UCTs, which might not necessarily permanently raise household income. At least in cases where malnutrition is an income constraint, therefore, it is plausible to believe that FFA is optimally placed to deliver on-going impacts.

Second of all, more generally little is known about the performance of food aid programmes in challenging fragile and conflict-affected environments. In part, this absence of academic studies likely relates to the data quality issues we faced in this project. However, despite these setbacks, a series of methodological innovations help to overcome these concerns and deliver a set of theoretically valid, empirically confirmed and highly robust findings. Not only do they suggest that at least certain forms of food aid boosts nutrition in challenging environments, but also that these improvements may endure into the medium term.

Third, our findings demonstrate that positive spillovers from food aid programmes specifically, and (both development and humanitarian) aid projects in general, exist. Put another way, they show that development interventions intended for one purpose (in the case of FFA, the development of household assets) can develop positive outcomes for another purpose (in this case, nutrition status). Although in part this fits with a wider literature (such as that which shows positive nutritional impacts from cash transfer programmes), the modality under investigation here is rather different. It suggests that the provision of a small amount of food assistance appears to induce further positive investments in nutrition in recipient households, which in turn combats malnutrition in children resident in those households.

Finally, our analysis reveals that there are potentially positive interactions between different forms of assistance, although we can only show this in theoretical terms. In most prior cases, efficacy has tended to be shown for one form of assistance or for different modalities of providing that assistance. In our case, we compare the outcomes of various bundles of assistance. In our setting, it is possible that the positive findings for Group 3 stem from a one-time boost in nutrition status from the provision of nutrition supplements that are, then, sustained into the longer term by improved income. In this regard, we conclude that while there are good grounds for optimism from food for assets programming and its interaction with other forms of assistance, more specific future research is needed to elucidate the precise attribution of these effects.

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Tables

Table 1: Defining the Assistance Receipt Groups

		Endline Status		
		No Assistance	No FFA but at Least One of TFA, BSF	FFA and at Least One of TFA, BSF
Baseline Status	FFA only	Group 1	Group 2	Group 3
Prop. Sample		52.28%	19.00%	25.31%

Table 2: Attrition Analysis - T-Test Comparison of Sample Means

VARIABLES	(1) Remainers	(2) Attritors	(3) Difference
poverty_status	1.172622	1.016892	0.1557***
agro_eco_zone	2.037867	1.172297	0.8656***
m_child_0_5	0.0583111	0.0709459	-0.0126
m_child_6_59	0.5788444	0.6182432	-0.0394
m_hh_size	3.018667	3.111486	-0.0928
f_child_0_5	0.0583111	0.0540541	0.0043
f_child_5_59	0.5488	0.4594595	0.0893**
f_hh_size	3.25	3.140444	0.109556
gender head	1.231289	1.25	-0.0187
marital head	1.630044	1.712838	-0.0828
age head	44	45.78258	-1.7826**
educ head	1.5984	1.658784	-0.0604
job head	4.264356	4.358108	-0.0938
income head	1.440356	1.442568	-0.0022
water	1.703289	1.719595	-0.0163
toilet	4.029867	4.135135	-0.1053
energy	2.874667	2.959459	-0.08479***
hh_tenure	1.646044	1.307432	0.3386***
chair	1.049422	1.084459	-0.0350***
carpet	0.1701333	0.1351351	0.0350*
table	0.8312889	0.4527027	0.3786***
bed	0.0311111	0.0202703	0.0108
mat	0.6792889	0.4358108	0.2435***
jewellery	0.9308444	0.9831081	-0.0523***
iron	0.1139556	0.0472973	0.0667***
sew_machine	0.0186667	0.0202703	-0.0016
telephone	0.0083556	0.0067568	0.0016
tv	0.4243556	0.3614865	0.0629**
radio	0.0060444	0.0168919	-0.0108**
hoe	0.2007111	0.1824324	0.0183
plough	0.8954667	0.847973	0.0475***
motorbike	0.1431111	0.1047297	0.0384**
bike	0.0344889	0.027027	0.0075
lamp	0.0177778	0.0168919	0.0009
other	0.9367111	0.8614865	0.0752***
animals	0.0307556	0.9831081	-0.9524
cows	0.6449778	0.3716216	0.2734***
sheep	0.5847111	0.2837838	0.3009***
goats	1.314667	0.4560811	0.8586***
camels	2.128356	0.902027	1.2263***
donkeys	0.0698667	0.0067568	0.0631**
horses	0.5363556	0.1047297	0.4316***

Note: *** p<0.01, **p<0.05, *p<0.1

Table 3: Testing the Correlation Between Friday Selection Criterion and Attrition

Non-random Selecition - Probit	
	(1)
	1 child is in both waves
VARIABLES	0 only baseline
Cluster was visited on friday 2016	-0.268***
	(0.0599)
Constant	-0.179**
	(0.0718)
Observations	3,447

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Group-Based Mean Differences

	(1)	(2)	(3)
VARIABLES	Group 1	Group 2	Group 3
HH Size	0.294*	0.269	-0.552***
	(2.47)	(1.68)	(-4.17)
Child 0-5	0.0216	0.0621**	0.0700***
	(1.39)	(2.98)	(-4.03)
Female HH Head	-0.0132	-0.0362*	0.0415**
	(-1.09)	(-2.22)	(3.07)
No. 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 MAM	0.00573	0.00662	-0.0117
	(0.75)	(0.64)	(-1.37)
Child MUAC	-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)

Note: Table shows t-test comparisons of endline means of key variables according to group status. Column (1) shows the comparison of the mean for Group 1, compared to pooled mean of Groups 2 and 3; Column (2) for Group 2, compared to Groups 1 and 2; and

Column (3) for Group 3, compared to Groups 1 and 2. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. T-statistics in parenthesis.

Table 5: Testing the Correlation Between Group Belonging and the Instrumental Variable

VARIABLES	(1) G2vG1	(2) G3vG1	(3) G3vG2
km_insecure	-0.000233***	0.000924***	0.00122***
	(5.75e-05)	(5.94e-05)	(8.11e-05)
Observations	3,646	3,631	3,631

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6.1: Group 3 versus Group 1 Outcomes – Child-level Balanced Panel Using Propensity Score Matching and Friday Selection Criterion

VARIABLES	(1) MUACZ	(2) MUAC	(3) WAZ	(4) HAZ
_diff	0.250***	1.916**	0.0999	0.341***
	(0.0879)	(0.910)	(0.0841)	(0.123)
mills_friday_2016	0.110	3.601	0.347	-0.954
	(0.447)	(5.378)	(0.421)	(0.610)
size_hh	0.00162	0.119	0.00213	0.0201
	(0.0117)	(0.138)	(0.0126)	(0.0147)
nb_0_5m	-0.108	0.248	-0.00592	0.00790
	(0.0831)	(0.590)	(0.0404)	(0.0521)
women_hh_head	0.0268	0.817	0.169*	0.519***
	(0.0993)	(1.120)	(0.0980)	(0.128)
education_hh_head	0.0240	0.178	-0.00914	-0.0946***
	(0.0225)	(0.239)	(0.0236)	(0.0315)
n_wifes	-0.0667	-0.959*	-0.0287	0.0363
	(0.0494)	(0.513)	(0.0555)	(0.0779)
assets_pc1	0.0632***	0.672***	0.0444*	0.0520
	(0.0200)	(0.211)	(0.0232)	(0.0345)
security	0.0532	1.052	0.0419	0.361***
	(0.0691)	(0.795)	(0.0759)	(0.108)
Observations	1,600	2,091	2,087	2,088
R-squared	0.021	0.142	0.012	0.060
Mean control t(0)	-1.227	136.4	-1.910	-0.877
Mean treated t(0)	-1.381	134.6	-2.014	-1.324
Diff t(0)	-0.154	-1.792	-0.104	-0.447
Mean control t(1)	-1.353	143.2	-2.067	-0.963
Mean treated t(1)	-1.257	143.3	-2.071	-1.069
Diff t(1)	0.0957	0.124	-0.00408	-0.106

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6.2: Group 2 versus Group 1 Outcomes – Child-level Balanced Panel Using Propensity Score Matching and Friday Selection Criterion

VARIABLES	(1) MUACZ	(2) MUAC	(3) WAZ	(4) HAZ
_diff	0.104	-0.356	-0.0725	0.0698
	(0.107)	(0.884)	(0.0812)	(0.114)
mills_friday_2016	0.165	2.399	-0.701	-2.083**
	(0.536)	(6.030)	(0.704)	(0.822)
size_hh	0.00611	0.275	0.0390**	0.0516***
	(0.0160)	(0.219)	(0.0178)	(0.0193)
nb_0_5m	0.141	1.339	0.0800	0.194*
	(0.0939)	(0.928)	(0.0818)	(0.106)
women_hh_head	0.0487	0.875	0.301**	0.555***
	(0.114)	(1.103)	(0.121)	(0.155)
education_hh_head	0.00518	-0.274	0.0206	0.0409
	(0.0290)	(0.311)	(0.0302)	(0.0402)
n_wifes	-0.0120	-0.161	0.0676	0.188
	(0.0687)	(0.788)	(0.0828)	(0.138)
assets_pc1	0.0520**	0.438*	0.0310	0.0247
	(0.0260)	(0.250)	(0.0254)	(0.0322)
security	0.101	1.163	0.132*	0.419***
	(0.0886)	(0.913)	(0.0776)	(0.117)
Observations	1,483	1,924	1,918	1,920
R-squared	0.016	0.114	0.033	0.057
Mean control t(0)	-1.380	136.8	-1.220	-0.325
Mean treated t(0)	-1.486	135.9	-1.309	-0.420
Diff t(0)	-0.105	-0.893	-0.0894	-0.0946
Mean control t(1)	-1.537	142.9	-1.529	-0.502
Mean treated t(1)	-1.539	141.6	-1.690	-0.527
Diff t(1)	-0.00167	-1.249	-0.162	-0.0248

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 6.3: Group 3 versus Group 2 Outcomes – Child-level Balanced Panel Using Propensity Score Matching and Friday Selection Criterion

VARIABLES	(1) MUACZ	(2) MUAC	(3) WAZ	(4) HAZ
_diff	0.218*	2.687**	0.201**	0.299**
	(0.119)	(1.047)	(0.0965)	(0.134)
mills_friday_2016	-0.0565	-2.596	-0.613	-1.737**
	(0.448)	(5.131)	(0.741)	(0.873)
size_hh	0.00137	0.333	0.0252	0.0330
	(0.0163)	(0.220)	(0.0183)	(0.0200)
nb_0_5m	-0.0752	0.265	-0.0225	0.0466
	(0.126)	(0.669)	(0.0496)	(0.0684)
women_hh_head	0.0524	1.735	0.362**	0.704***
	(0.116)	(1.149)	(0.143)	(0.193)
education_hh_head	0.00524	-0.143	-0.0139	-0.0461
	(0.0271)	(0.301)	(0.0287)	(0.0456)
n_wifes	-0.0691	-1.071	-0.0319	0.0645
	(0.0653)	(0.777)	(0.0739)	(0.114)
assets_pc1	0.0316	0.243	0.0284	0.0580
	(0.0273)	(0.297)	(0.0291)	(0.0427)
security	0.286***	3.048***	0.204**	0.521***
	(0.0813)	(0.884)	(0.0889)	(0.153)
Observations	841	1,103	1,099	1,096
R-squared	0.036	0.160	0.035	0.091
Mean control t(0)	-1.231	140.8	-1.214	-0.482
Mean treated t(0)	-1.262	139.9	-1.207	-0.806
Diff t(0)	-0.0314	-0.829	0.00703	-0.324
Mean control t(1)	-1.324	145.8	-1.580	-0.579
Mean treated t(1)	-1.137	147.6	-1.371	-0.604
Diff t(1)	0.187	1.859	0.208	-0.0253

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7.1: Group 3 versus Group 1 Outcomes – Household-level Balanced Panel Using Propensity Score Matching and Friday Selection Criterion

VARIABLES	(1) MUACZ	(2) MUAC	(3) WAZ	(4) HAZ
_diff	0.237**	2.456**	0.242***	0.351**
	(0.0981)	(1.173)	(0.0909)	(0.154)
mills_friday_2016	-0.354	-0.176	-0.708**	-1.855***
	(0.356)	(3.654)	(0.300)	(0.460)
size_hh	0.00178	0.0974	0.0117	0.0164
	(0.00998)	(0.118)	(0.0125)	(0.0185)
nb_0_5m	0.0285	0.893	0.0105	0.0200
	(0.0747)	(0.747)	(0.0517)	(0.0680)
women_hh_head	0.170*	2.047*	0.325***	0.556***
	(0.0912)	(1.067)	(0.0841)	(0.129)
education_hh_head	-0.00301	0.0918	-0.0160	-0.0694**
	(0.0234)	(0.265)	(0.0260)	(0.0344)
n_wifes	0.000379	-0.646	0.0183	0.0611
	(0.0542)	(0.516)	(0.0403)	(0.0622)
assets_pc1	0.0611***	0.606**	0.0126	0.0288
	(0.0213)	(0.265)	(0.0218)	(0.0314)
security	0.189***	2.316***	0.0347	0.382***
	(0.0651)	(0.737)	(0.0754)	(0.109)
Observations	2,905	3,531	3,520	3,522
R-squared	0.041	0.074	0.014	0.042
Mean control t(0)	-1	138.9	-0.920	-0.125
Mean treated t(0)	-1.100	137.1	-1.107	-0.546
Diff t(0)	-0.100	-1.755	-0.187	-0.421
Mean control t(1)	-0.899	142.8	-1.198	-0.160
Mean treated t(1)	-0.762	143.5	-1.143	-0.230
Diff t(1)	0.137	0.701	0.0553	-0.0697

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7.2: Group 2 versus Group 1 Outcomes – Household-level Balanced Panel Using Propensity Score Matching and Friday Selection Criterion

VARIABLES	(1) MUACZ	(2) MUAC	(3) WAZ	(4) HAZ
_diff	0.0211	-0.769	-0.128	-0.0375
	(0.0822)	(0.942)	(0.0778)	(0.118)
mills_friday_2016	-0.226	-2.230	-1.101**	-1.619**
	(0.506)	(5.148)	(0.553)	(0.634)
size_hh	-0.00561	0.0639	0.0235	0.0327**
	(0.0147)	(0.191)	(0.0162)	(0.0152)
nb_0_5m	0.163*	2.572***	0.179**	0.223**
	(0.0831)	(0.858)	(0.0745)	(0.0862)
women_hh_head	0.0930	0.905	0.312***	0.406***
	(0.107)	(0.985)	(0.103)	(0.129)
education_hh_head	0.0214	-0.270	0.0236	0.0148
	(0.0266)	(0.319)	(0.0272)	(0.0296)
n_wifes	-0.0201	-0.517	0.0132	0.0178
	(0.0634)	(0.702)	(0.0693)	(0.105)
assets_pc1	0.0411**	0.303	0.0111	0.0280
	(0.0197)	(0.198)	(0.0222)	(0.0290)
security	0.0485	0.720	0.0856	0.341***
	(0.0793)	(0.854)	(0.0773)	(0.0994)
Observations	2,593	3,157	3,146	3,147
R-squared	0.013	0.039	0.030	0.031
Mean control t(0)	-0.984	142.5	-0.763	-0.504
Mean treated t(0)	-1.001	142.6	-0.739	-0.471
Diff t(0)	-0.0175	0.145	0.0243	0.0338
Mean control t(1)	-0.908	146.7	-1.032	-0.720
Mean treated t(1)	-0.904	146	-1.136	-0.724
Diff t(1)	0.00359	-0.625	-0.104	-0.00374

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7.3: Group 3 versus Group 2 Outcomes – Household-level Balanced Panel Using Propensity Score Matching and Friday Selection Criterion

VARIABLES	(1) MUACZ	(2) MUAC	(3) WAZ	(4) HAZ
_diff	0.256***	3.572***	0.414***	0.137
	(0.0906)	(0.887)	(0.110)	(0.250)
mills_friday_2016	-0.310	-3.366	-0.209	0.0498
	(0.409)	(4.564)	(0.659)	(0.726)
size_hh	0.00506	0.266	0.0246*	-0.00323
	(0.0130)	(0.164)	(0.0133)	(0.0175)
nb_0_5m	0.107	1.300*	0.0677	0.141*
	(0.0830)	(0.687)	(0.0435)	(0.0828)
women_hh_head	0.0612	1.687	0.216	0.314*
	(0.109)	(1.143)	(0.133)	(0.179)
education_hh_head	-0.00540	-0.0191	0.00657	-0.0330
	(0.0250)	(0.280)	(0.0303)	(0.0377)
n_wifes	-0.0782	-1.497***	-0.118**	-0.0957
	(0.0522)	(0.567)	(0.0535)	(0.0771)
assets_pc1	0.0504**	0.0934	-0.000345	0.0553*
	(0.0229)	(0.238)	(0.0210)	(0.0307)
security	0.279***	3.838***	0.113	0.414***
	(0.0778)	(0.859)	(0.0901)	(0.124)
Observations	1,531	1,843	1,835	1,832
R-squared	0.062	0.097	0.035	0.063
Mean control t(0)	-0.957	142.1	-1.343	-2.070
Mean treated t(0)	-1.083	140.4	-1.555	-2.250
Diff t(0)	-0.126	-1.677	-0.212	-0.180
Mean control t(1)	-0.889	144.2	-1.846	-1.803
Mean treated t(1)	-0.758	146.1	-1.644	-1.847
Diff t(1)	0.131	1.895	0.202	-0.0436

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A1.1: Group 3 versus Group 1 Outcomes – Child-level Balanced Panel Using IV and Friday Selection Criterion

VARIABLES	(1) MUACZ	(2) MUAC	(3) WAZ	(4) HAZ
group3vs1	-0.725***	-6.168***	-0.109	-0.593**
	(0.167)	(2.035)	(0.194)	(0.261)
time_group3vs1	0.925***	5.591	0.216	0.493
	(0.356)	(3.492)	(0.332)	(0.445)
time	-0.303**	6.174***	-0.183	-0.126
	(0.119)	(1.229)	(0.118)	(0.157)
mills_friday_2016	0.310	7.128**	0.361	-0.951**
	(0.335)	(3.557)	(0.341)	(0.452)
size_hh	0.00131	0.0580	0.00233	0.0258*
	(0.0111)	(0.113)	(0.0108)	(0.0143)
nb_0_5m	0.00257	0.600	0.0144	0.0318
	(0.0651)	(0.565)	(0.0542)	(0.0718)
women_hh_head	0.0141	0.346	0.140*	0.439***
	(0.0821)	(0.871)	(0.0834)	(0.111)
n_wifes	-0.0231	-0.395	-0.000952	0.0648
	(0.0491)	(0.528)	(0.0507)	(0.0671)
assets_pc1	0.0640***	0.713***	0.0518***	0.0449*
	(0.0184)	(0.201)	(0.0193)	(0.0255)
security	-0.0242	0.0360	0.0221	0.322***
	(0.0550)	(0.587)	(0.0564)	(0.0745)
Observations	1,579	2,062	2,057	2,059
R-squared		0.117	0.008	0.042

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A1.2: Group 2 versus Group 1 Outcomes – Child-level Balanced Panel Using IV and Friday Selection Criterion

VARIABLES	(1) MUACZ	(2) MUAC	(3) WAZ	(4) HAZ
group2vs1	2.258	14.68	-1.060	-0.00742
	(1.442)	(11.77)	(1.279)	(1.354)
time_group2vs1	-6.387*	-31.22	-1.804	-1.373
	(3.792)	(20.85)	(2.315)	(2.474)
time	1.583	14.90***	0.295	0.268
	(1.030)	(5.398)	(0.600)	(0.642)
mills_friday_2016	0.748	6.203	0.133	-1.464***
	(0.835)	(4.515)	(0.482)	(0.517)
size_hh	-0.0179	0.0626	-0.00130	0.0327
	(0.0345)	(0.171)	(0.0185)	(0.0204)
nb_0_5m	-0.00612	0.375	-0.0816	0.0685
	(0.169)	(1.285)	(0.140)	(0.150)
women_hh_head	-0.0228	0.214	0.183*	0.431***
	(0.146)	(0.993)	(0.107)	(0.114)
n_wifes	0.0463	0.337	0.139*	0.190**
	(0.101)	(0.726)	(0.0785)	(0.0833)
assets_pc1	0.0666	0.486*	0.0506*	0.0247
	(0.0429)	(0.260)	(0.0284)	(0.0303)
security	0.0301	0.411	0.0852	0.372***
	(0.0918)	(0.693)	(0.0756)	(0.0812)
Observations	1,463	1,897	1,890	1,893

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A1.3: Group 3 versus Group 2 Outcomes – Child-level Balanced Panel Using IV and Friday Selection Criterion

VARIABLES	(1) MUACZ	(2) MUAC	(3) WAZ	(4) HAZ
group3vs2	-0.248	-3.183	-0.0494	-1.033***
	(0.166)	(2.001)	(0.195)	(0.259)
time_group3vs2	1.066***	8.311**	0.637*	1.225***
	(0.349)	(3.479)	(0.338)	(0.451)
time	-0.525***	2.040	-0.559***	-0.606**
	(0.196)	(2.049)	(0.199)	(0.265)
mills_friday_2016	-0.0432	-2.481	-0.405	-1.525***
	(0.413)	(4.468)	(0.435)	(0.574)
size_hh	-0.0116	0.260*	0.0112	0.0294
	(0.0159)	(0.152)	(0.0148)	(0.0198)
nb_0_5m	-0.124	0.0815	-0.0334	0.0443
	(0.0806)	(0.641)	(0.0623)	(0.0823)
women_hh_head	0.0190	1.548	0.305***	0.700***
	(0.112)	(1.180)	(0.115)	(0.152)
n_wifes	-0.0636	-1.120*	-0.0453	0.0737
	(0.0588)	(0.626)	(0.0610)	(0.0806)
assets_pc1	0.0332	0.242	0.0290	0.0428
	(0.0241)	(0.258)	(0.0251)	(0.0332)
security	0.345***	3.501***	0.256***	0.546***
	(0.0738)	(0.805)	(0.0785)	(0.104)
Observations	840	1,101	1,099	1,096
R-squared		0.150	0.017	0.058

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2.1: Group 3 versus Group 1 Outcomes – Household-level Balanced Panel Using IV and Friday Selection Criterion

VARIABLES	(1) MUACZ	(2) MUAC	(3) WAZ	(4) HAZ
group3vs1	-0.885***	-8.163***	-0.148	-0.825***
	(0.150)	(1.917)	(0.176)	(0.238)
time_group3vs1	1.147***	8.227***	0.636**	1.258***
	(0.251)	(2.978)	(0.274)	(0.368)
time	-0.166*	3.156***	-0.295***	-0.364***
	(0.0914)	(1.061)	(0.0979)	(0.131)
mills_friday_2016	-0.198	-0.238	-0.488*	-1.440***
	(0.248)	(2.848)	(0.263)	(0.351)
size_hh	-0.00579	-0.0589	-0.00459	0.0103
	(0.00786)	(0.0883)	(0.00816)	(0.0109)
nb_0_5m	0.0606	1.301***	0.0457	0.0390
	(0.0402)	(0.437)	(0.0404)	(0.0540)
women_hh_head	0.107*	1.035	0.209***	0.368***
	(0.0637)	(0.734)	(0.0677)	(0.0904)
n_wifes	0.0219	-0.181	0.00429	0.0260
	(0.0373)	(0.436)	(0.0402)	(0.0537)
assets_pc1	0.0442***	0.443***	0.0188	0.0211
	(0.0144)	(0.167)	(0.0154)	(0.0206)
security	0.0539	0.713	0.0504	0.358***
	(0.0436)	(0.494)	(0.0458)	(0.0611)
Observations	2,911	3,538	3,525	3,529
R-squared		0.037		0.014

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2.2: Group 2 versus Group 1 Outcomes – Household-level Balanced Panel Using IV and Friday Selection Criterion

VARIABLES	(1) MUACZ	(2) MUAC	(3) WAZ	(4) HAZ
group2vs1	2.031***	16.64**	-0.921	0.563
	(0.690)	(7.632)	(0.769)	(0.935)
time_group2vs1	-2.870***	-20.22**	-1.169	-2.272*
	(1.011)	(10.05)	(1.028)	(1.256)
time	0.802***	9.697***	0.0578	0.369
	(0.250)	(2.445)	(0.250)	(0.305)
mills_friday_2016	-0.162	-0.0704	-0.959***	-1.976***
	(0.316)	(3.245)	(0.325)	(0.395)
size_hh	-0.00598	-0.0515	-0.000415	0.0213*
	(0.0108)	(0.104)	(0.0104)	(0.0127)
nb_0_5m	0.125*	2.418***	0.0916	0.0967
	(0.0665)	(0.718)	(0.0718)	(0.0874)
women_hh_head	0.101	0.624	0.341***	0.455***
	(0.0805)	(0.835)	(0.0834)	(0.101)
n_wifes	0.0375	-0.0312	0.0740	0.0785
	(0.0489)	(0.511)	(0.0513)	(0.0622)
assets_pc1	0.0267	0.271	0.0434**	0.0230
	(0.0242)	(0.219)	(0.0221)	(0.0268)
security	0.0134	0.206	0.114*	0.364***
	(0.0558)	(0.589)	(0.0587)	(0.0718)
Observations	2,626	3,192	3,179	3,182

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2.3: Group 3 versus Group 2 Outcomes – Household-level Balanced Panel Using IV and Friday Selection Criterion

VARIABLES	(1) MUACZ	(2) MUAC	(3) WAZ	(4) HAZ
group3vs2	-0.497***	-5.560***	-0.229	-1.104***
	(0.136)	(1.728)	(0.163)	(0.216)
time_group3vs2	0.848***	7.615***	0.890***	1.652***
	(0.227)	(2.660)	(0.251)	(0.334)
time	-0.300**	-0.301	-0.731***	-0.870***
	(0.146)	(1.696)	(0.160)	(0.212)
mills_friday_2016	-0.525	-7.188*	-0.875**	-1.050**
	(0.328)	(3.824)	(0.360)	(0.476)
size_hh	0.00278	0.272**	0.0162	0.0142
	(0.0101)	(0.114)	(0.0107)	(0.0142)
nb_0_5m	0.0388	0.810*	0.0328	0.0584
	(0.0440)	(0.473)	(0.0446)	(0.0590)
women_hh_head	0.0886	2.049**	0.324***	0.462***
	(0.0876)	(0.994)	(0.0938)	(0.124)
n_wifes	-0.0501	-0.983*	-0.0632	-0.0175
	(0.0465)	(0.541)	(0.0510)	(0.0674)
assets_pc1	0.0386**	0.137	0.0195	0.0404
	(0.0184)	(0.213)	(0.0201)	(0.0266)
security	0.255***	2.915***	0.199***	0.498***
	(0.0595)	(0.696)	(0.0656)	(0.0870)
Observations	1,535	1,848	1,844	1,841
R-squared	0.026	0.074	0.002	0.020

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1