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SCHOOL FEEDING PROGRAMMES, EDUCATION AND FOOD SECURITY IN RURAL MALAWI

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School Feeding Programmes, Education and Food Security in Rural Malawi

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Abstract

Existing investigations of the impact of school feeding programmes on educational outcomes have provided mixed evidence of success. In this chapter, I investigate a potential explanation for this lack of consensus in the literature. I argue that the prevailing food security situation at the time and place of the programme's evaluation plays a major role. I study the case of rural Malawi. I use an instrumental variable approach and propensity score matching to estimate the impact of school feeding on primary school enrolment and retention rates. I focus on villages with overlapping characteristics. I estimate that school feeding has increased enrolments by 7 percentage points on average, but the impact on retention rates has been relatively limited. However, when I distinguish between food-secure and food-insecure areas, not only do I find a larger impact on enrolments in food-insecure areas, but I also uncover a significant increase of around 2 percentage points in the retention rate of students in these same areas. Across the board, impacts are not significant in food-secure areas. I conclude that school feeding programmes bear an impact on education as long as they also intervene to relax a binding food constraint.

JEL classification: I21, I38.

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1 Introduction

Evidence concerning the impact of school feeding programmes on educational outcomes is mixed. Some researchers find positive and significant effects for at least *some* indicators (Jacoby et al., 1998; Powell et al., 1998; Ravallion & Wodon, 2000; Kremer & Vermeersch, 2005; Kazianga et al., 2009; Meng & Ryan, 2010; Alderman et al., 2012; Kazianga et al., 2012; Nikiema, 2019), while others find no impact at all or very modest effects (Tan et al., 1999; Buttenheim et al., 2011; Adrogue & Orlicki, 2013; McEwan, 2013; Azomahou et al., 2019). I investigate how one dimension of within-country variation across time and space can impact the outcomes of school feeding evaluations and can thus help explain the lack of consensus in the literature.

Specifically, I assess the magnitude and heterogeneity of school feeding impacts on the extensive and intensive margins of education, i.e., enrolment and retention rates. My hypothesis is that the prevailing food security situation at the time of the evaluation, in the areas where school feeding is implemented can predict whether school feeding will boost or leave enrolment and retention rates unaffected. Singh et al. (2014) have put forward a similar hypothesis with respect to the impact of school feeding on nutrition. However, the authors look at drought from a historical perspective, and they rely on self-reported exposure to drought. In contrast, I use a contemporaneous measure of food insecurity, which is the result of institutional evaluations of food availability, to provide insights into the interactions between local food insecurity and the effectiveness of school feeding. I complement the main analysis with secondary investigations into age at enrolment and the grade-for-age gap.

I combine Malawi's Third and Fourth Integrated Household Surveys with localised information on food insecurity from the Famine Early Warning Systems Network (FEWS-NET). I implement an instrumental variable approach at the individual level to estimate the local average treatment effect of school feeding on educational outcomes. I instrument the endogenous individual-level treatment with village-level treatment, which is considered to be as good as random *conditional* on the inclusion of the covariates that inform the targeting of villages. In addition, I also run a reduced-form specification at the individual level and a propensity score matching analysis at the community level to estimate the average intention to treat effect.

Controlling for district and time effects as well as individual, household and village characteristics, I find that school feeding has chiefly attracted new enrolments. However, its impact on dropouts is less clear. School feeding has improved the extensive margin of schooling by 6–7 percentage points on average, but there is no significant impact on the intensive margin. When I distinguish between food-secure and food-insecure areas, however, not only do I find a larger impact on the extensive margin of schooling in food-insecure areas, but I also uncover a significant 2 percentage point increase in the intensive margin of schooling in food-insecure areas in a sample of communities with overlapping characteristics. I call this the *overlap sample.*

Reduced-form regressions confirm the above pattern. The likelihood of being in school is roughly 2 percentage points higher for children in treated villages than it is for children in control communities. This magnitude is further increased in food-insecure areas by 0.6–1.0 percentage points. As for the intensive margin of education, children in treated food-insecure villages are 1 percentage point more likely to remain in school when compared to control children. Moreover, propensity score matching estimations at the community level yield similar results. Had all food-insecure communities received school feeding, then the extensive margin of schooling would have been 2–3 percentage points greater than in a scenario where no community was treated. Across the board, there are no significant impacts in food-secure communities.

I conclude that school feeding improves enrolment and retention rates as long as there is a binding food constraint to relax. Moreover, increases in enrolment appear to be more sensitive to school feeding than are retention rates. This raises the question of whether children might not be enrolling in school only temporarily when food is scarce and droughts reduce the demand for their time in agriculture. I argue that school feeding is best implemented alongside a package of interventions to improve educational outcomes regardless of environmental factors. However, even if the impact of school feeding on enrolment and retention rates is occasionally limited, the programme can still improve nutrition for those children who would anyway attend school, which in turn can increase their school performance. There is evidence that school feeding is associated with improved cognitive and nutritional outcomes for children attending Grade 1 in Malawi during the academic year 2010–11, which was minimally affected by food insecurity (Nkhoma et al., 2013).

The impact heterogeneity that is documented in this study strengthens the argument that school feeding should be directed at the most vulnerable of areas in Malawi. The scaling up of school feeding to cover everyone regardless of their socio-economic situation is likely to lead to a watered-down average impact. However, if the target is a moving one, e.g., communities oscillate between food security and insecurity, then casting a wider net can ensure preparedness when food insecurity is widespread.

The findings of this article help to interpret the conflicting evidence on school feeding, enrolment and retention rates. For instance, Kazianga et al. (2012) evaluate a food-for-education experiment in Burkina Faso and estimate a 4 percentage point average intention to treat effect on enrolment rates. Similarly, Ravallion & Wodon (2000) and Meng & Ryan (2010) find a positive impact of 15–21 percentage points on school participation in Bangladesh. Finally, Alderman et al. (2012) estimate that food-for-education interventions in Uganda have chiefly led to the enrolment of children who were out of school at baseline. Among these children, those exposed to treatment were 9 percentage points more likely to be in school at endline. In contrast, Azomahou et al. (2019) find that school meals have not impacted enrolment or retention rates in Senegal. Similarly, Tan et al. (1999) and Buttenheim et al. (2011) also find no impact on enrolment or dropout rates in the case of the Philippines and Laos. This article identifies one important reason why the same programme can lead to both significant and insignificant results; namely, the prevailing food security situation at the time and place of the programme's evaluation.

2 Theory of School Feeding

School feeding programmes can be classified as an in-kind conditional welfare transfer whose aim is to incentivise households to invest in the education of their children, i.e., enrolling and keeping them in school. Thus, school feeding programmes can be ascribed to the category of demand-side interventions in education. However, if we move away from the premise that school feeding is mainly meant to increase attendance, enrolment and retention rates, and instead consider that school feeding can also improve performance, as well-nourished children are better able to learn and participate in school, then school feeding can also be regarded as a school input. This means that school feeding can be treated as a supply-side intervention. Nevertheless, since the focus of this study falls on enrolment and retention rates, then the classification of school feeding programmes as a demand-side intervention is in order.

As a demand side intervention, the impact of school feeding programmes can be analysed using a cost-benefit analysis (Becker, 1962; Machin & Stevens, 2004; Jimenez & Patrinos, 2008; Glewwe & Muralidharan, 2016), per which parents decide to invest in the education of their children if the present value of the benefits associated with better-educated children is greater than the current cost of sending children to school. Households incorporate individual benefits into their decision-making process; however, they are unlikely to also consider the social benefits of additional schooling. Thus, their decisions may not be optimal for societies as a whole (Machin & Stevens, 2004; Jimenez & Patrinos, 2008). Consequently, governments and their development partners have the incentive to intervene in this context. They can rely on two types of interventions to incentivise investments in education. They can either reduce the cost of schooling or increase the benefits of education.

Costs can be direct, such as fees, uniforms and supplies, or indirect, such as lost labour, whether paid or unremunerated chores and agricultural tasks. Benefits generally incorporate the present value of future income from skilled employment. Though smaller in magnitude, benefits can also be immediate if, for instance, regular school attendance is rewarded. School feeding is a case in point. The magnitude of the impact, however, is not the same for everyone. The provision of school meals can be invaluable to food-insecure, budget-constrained households. But it can also mean very little to households that were already effortlessly providing meals to their children. This is a consequence of the nature of the intervention, which is an in-kind transfer that implicitly assumes what the problem is and gives a one-size-fits-all type of solution. If the assumption is faulty and the household is *not* facing a binding food constraint, then an otherwise well-intended intervention can have limited impact. This is the main theoretical reason why it may be cost-effective to target only the most vulnerable of areas or schools against the background of limited public resources.

Consequently, I hypothesise that for school feeding to have an impact in terms of educational outcomes, the programme must be implemented in an area that is facing both binding food constraints *and* educational gaps. If there are no binding food constraints, then households have no additional incentive to send their children to school following the implementation of school feeding programmes. In this case, school feeding offers a solution, i.e., complementary meals, to a non-existent problem. Therefore, the prevailing local food security situation can explain why the impact of school feeding on enrolment and attendance can be limited on occasions.

3 Context

3.1 Education, Rural Malawi

The net enrolment rate among children aged 6 to 14 years old has increased from 84 percent in 2010 to 90 percent in 2016 in Malawi (National Statistical Office, 2017). These statistics are encouraging, but there is still room for improvement—especially after seeing how enrolment rates have decreased from 90 percent in 2016 to 88 percent in 2017, in the aftermath of the flooding and drought events of 2016 (UNICEF, 2019). Moreover, educational outcomes vary substantially between urban and rural Malawi. The difference between primary school *completion* rates in urban and rural Malawi was of 34 percentage points around the time when school feeding impacts are observed and evaluated in this study (Ministry of Education, Science and Technology, 2014).

I restrict this study to rural Malawi to keep a homogeneous sample of households. Nevertheless, only a small part of the data is lost as more than 80 percent of Malawians live in rural areas (National Statistical Office, 2019). Rural areas are also the most pertinent setting to discuss educational outcomes and school feeding against the background of food insecurity that is brought on by natural causes. Malawi's rural population is overly-reliant on small-scale, rain-fed agriculture; therefore, households are vulnerable to climate variability and change. It does not help that Malawi is prone to floods and droughts or that their frequency and spread have intensified in the past decades. Finally, population growth and environmental degradation will also continue to aggravate the impacts of the aforementioned natural disasters (Government of Malawi, 2016).

3.2 School Feeding Programmes in Malawi

Malawi's experience with school feeding programmes pre-dates the efforts of the government to regulate these interventions. For instance, the Department of School Health and Nutrition, which provides guidance on health and nutrition-related interventions, was established in 2007. However, school feeding in Malawi dates back to the late 1990s, when the World Food Programme first implemented a pilot project in the country (Government of Malawi, 2009). The other important entity implementing school feeding in Malawi is Mary's Meals. Their work started in 2002.

As of 2015, Malawi counted 5,864 primary schools (UNICEF, 2018). The World Food Programme worked with 783 of these schools in 13 districts (Webb et al., 2018). Mary's Meals catered to 635 additional schools in 20 districts as of 2016 (McMahon, 2016). Malawi has 28 districts. Both implementing agencies follow the guidelines issued by the government. The World Food Programme and Mary's Meals target communities and areas that are vulnerable and lagging behind in terms of educational outcomes.

4 Data

4.1 Household and Community Data

I use Malawi's Third and Fourth Integrated Household Surveys (2010–11; 2016–17). The result is a pooled cross-sectional dataset. These surveys have been implemented through the National Statistical Office with support from the World Bank. The analysis sample is limited to rural communities and includes all children that are of primary school age, 6 to 14 years old. On average, 16 percent of these children have received school meals at some point during the 12 months prior to the interview taking place. Moreover, 34 percent of communities were being targeted by school feeding at the time of the interview.[1] Not everyone within a targeted community was necessarily treated. As the concept of a community often overlaps with the boundaries of a village, I will use references to communities and villages interchangeably.

Children are classified as *in school* if they replied positively to the survey question: "Are you currently attending school or, if school is not in session now, did you attend school in the session just completed and plan to attend next session?" This variable measures the intensive margin of schooling, i.e., the proportion of children who are still in school conditional on past enrolment. I obtain the extensive margin of schooling by complementing the out-of-school sample per the above survey question with children who have never attended school but who were of primary school age at the time of the survey interview. Thus, the extensive margin measures the proportion of all children of primary school age who are in school, regardless of whether they have or have never enrolled in primary education. Besides the intensive and extensive measures of schooling, I explore two additional variables: the grade-for-age gap and age at the time of enrolment. For those who have never enrolled in school, the grade-for-age gap is the grade in which they should have been given their age. For early achievers, the gap is zero (Islam & Choe, 2013).

[1] Summary statistics suggest that between 2010 and 2016, the targeting of villages by school feeding has increased by more than the population of individual beneficiaries. There are two reasons that can explain and put this into context. First, the 2016–17 period has been hit hard by food insecurity due to floods and a prolonged drought. The Government of Malawi has listed the scaling up of school feeding programmes as a key strategy to limit the educational consequences of failed harvests (Government of Malawi, 2016). However, the implementation of school feeding has suffered, supplies became scarce, prices soared, and the budgets of schools became insufficient (Government of Malawi, 2016). It is likely that a significant number of the newly targeted villages had not actually started serving meals at the time of the survey, hence the seemingly smaller increase in the population of individual beneficiaries. Second, summary statistics also seem to suggest that the scaling up of school feeding has been particularly strategic during the drought year, such that the number of beneficiaries per village was smaller. Only the utmost vulnerable schools were targeted. This contrasts with the previous approach, which was one of cluster treatment to limit the migration of children from untreated to treated schools. In 2010, 53 percent of children in a targeted village were treated, while in 2016, the percentage was 35.

Table 1: Summary Statistics, Rural Malawi

	2010	2016	Pooled			
Variables	**Mean**	**Mean**	**Mean**	σ	**Min**	**Max**
INDIVIDUAL CHARACTERISTICS						
In school, extensive margin	0.887	0.918	0.903	0.297	0	1
Grade-for-age gap (early achievers have zero gap)	1.333	1.249	1.290	1.524	0	8
Beneficiary school feeding past year	0.157	0.171	0.164	0.371	0	1
Age	9.943	10.197	10.074	2.433	6	14
Gender (girls 0, boys 1)	0.498	0.491	0.495	0.500	0	1
Father has PSLC	0.104	0.080	0.092	0.289	0	1
Father has JCE	0.068	0.072	0.070	0.255	0	1
Father has MSCE	0.049	0.050	0.049	0.217	0	1
Father has post-secondary diploma	0.009	0.010	0.009	0.096	0	1
Mother has PSLC	0.061	0.070	0.065	0.247	0	1
Mother has JCE	0.033	0.040	0.036	0.187	0	1
Mother has MSCE	0.008	0.013	0.011	0.104	0	1
Mother has post-secondary diploma	0.001	0.002	0.002	0.041	0	1
Sample individuals aged 6-14	**10,038**	**10,573**		**20,611**		
In school, intensive margin	0.977	0.976	0.976	0.153	0	1
Sample 6-14 & enrolled in the past	**9,113**	**9,943**		**19,056**		
Age when first started school	6.340	6.045	6.186	0.916	5	14
Sample 6-14, incl. early start age of 5+	**8,888**	**9,661**		**18,549**		
Age when first started school	6.606	6.245	6.417	0.822	6	14
Sample 6-14, start age of 6+	**7,418**	**8,109**		**15,527**		
HOUSEHOLD CHARACTERISTICS						
Some stress i.t.o. food security (FEWS-NET)	0.230	0.589	0.417	0.493	0	1
Household size	5.698	5.357	5.521	1.840	2	19
Share of males between 6 and 9	8.554	8.432	8.490	11.501	0	67
Share of males between 10 and 18	13.683	14.594	14.156	15.335	0	100
Share of males between 19 and 40	10.045	9.279	9.647	10.803	0	67
Share of males over 40	6.540	7.300	6.935	9.637	0	50
Share of females between 6 and 9	8.878	8.700	8.786	11.859	0	67
Share of females between 10 and 18	13.107	14.708	13.939	15.097	0	100
Share of females between 19 and 40	13.613	13.721	13.669	10.700	0	67
Share of females over 40	8.243	9.093	8.685	12.524	0	75
Female household head	0.274	0.308	0.292	0.455	0	1
Head is married but spouse not present	0.024	0.040	0.032	0.177	0	1
Head is divorced, separated, widowed	0.247	0.260	0.254	0.435	0	1
Head is single	0.005	0.005	0.005	0.072	0	1
House made from permanent materials	0.247	0.231	0.238	0.426	0	1
House made from mix permanent and traditional	0.261	0.402	0.334	0.472	0	1
Owns basic furniture	0.495	0.494	0.495	0.500	0	1
Owns radio and/or TV	0.497	0.394	0.443	0.497	0	1
Size of garden (acres)	1.926	1.542	1.726	1.683	0	40
Time to water (hours)	0.238	0.221	0.229	0.284	0	8
Sample households	**5,195**	**5,619**		**10,814**		
HOUSEHOLD SAFETY NETS						
Free maize	0.024	0.260	0.147	0.354	0	1
Free food (not maize)	0.008	0.203	0.109	0.312	0	1
Public works programme	0.027	0.111	0.071	0.256	0	1
Inputs for work programme	0.002	0.003	0.003	0.052	0	1
Likuni Phala to children and mothers	0.005	0.020	0.013	0.111	0	1
Feeding for malnourished children	0.001	0.004	0.003	0.051	0	1
Bursaries for secondary education	0.003	0.005	0.004	0.066	0	1
Direct cash transfers from government	0.002	0.029	0.016	0.125	0	1
Direct cash transfers from others	0.005	0.022	0.014	0.117	0	1

	2010	2016	Pooled			
Variables	**Mean**	**Mean**	**Mean**	σ	**Min**	**Max**
HOUSEHOLD SAFETY NETS (continued)						
Other safety nets	0.003	0.063	0.034	0.182	0	1
Scholarships for tertiary education	0.001	0.001	0.001	0.025	0	1
Sample households	**5,195**	**5,619**	**10,814**			
COMMUNITY CHARACTERISTICS						
Ratio in school, extensive margin (aged 6-14)	0.887	0.919	0.904	0.115	0.09	1
Ratio in school, intensive margin (aged 6-14)	0.974	0.975	0.975	0.050	0.56	1
Comm. targeted by school feeding programmes	0.251	0.433	0.345	0.476	0	1
Perc. aged 24+ with at least PSLC	19.297	21.395	20.386	14.419	0	83
Perc. Muslim	11.399	12.122	11.774	22.420	0	100
Perc. Christian	78.811	82.067	80.501	25.207	0	100
Pop. community (thousands)	3.673	4.689	4.200	6.482	0	80
Most land in planned housing	0.024	0.014	0.019	0.135	0	1
Most land in squatter	0.018	0.027	0.023	0.150	0	1
Most land for industry	0.006	0.005	0.005	0.073	0	1
Most land for shops	0.002	0.003	0.003	0.051	0	1
Most land for other	0.007	0.010	0.009	0.094	0	1
Graded gravelled road	0.253	0.195	0.223	0.416	0	1
Dirt road	0.489	0.514	0.502	0.500	0	1
Dirt track	0.097	0.151	0.125	0.331	0	1
Distance gov. secondary school (km)	29.28	24.629	26.866	26.438	0	200
Distance health clinic (km)	7.559	6.200	6.853	8.797	0	90
Distance commercial bank (km)	30.991	29.858	30.403	24.562	0	168
Natural disaster in community 2 years before	0.227	0.299	0.264	0.441	0	1
Natural disaster in community 1 year before	0.502	0.514	0.508	0.500	0	1
Natural disaster in community current year	0.200	0.440	0.324	0.468	0	1
Irrigation scheme in community	0.203	0.250	0.227	0.419	0	1
Distance closest gov. primary school (km)	1.817	2.404	2.121	3.491	0	50
Classrooms are properly built	0.652	0.708	0.681	0.466	0	1
Electricity in primary school	0.035	0.080	0.058	0.234	0	1
Number private primary schools	0.101	0.114	0.108	0.552	0	10
Number religious primary schools	0.463	0.436	0.449	1.452	0	25
Pupils to teacher ratio	106.496	90.366	98.125	48.689	8	425
Sample communities	**546**	**589**	**1,135**			

Reference categories: father has no education, mother has no education, share of males younger than 6, share of females younger than 6, head is married and the spouse is present in the household, the house is made from traditional materials, percentage practising traditional beliefs or other religions, most community land is destined to agriculture, and the community is endowed with asphalt road(s). Abbreviations: Primary School Leaving Certificate (PSLC), Junior Certificate of Education (JCE) and Malawi School Certificate of Education (MSCE).

Table 1 presents the summary statistics.[2] This table shows that the extensive margin of schooling in rural areas has slightly gone up from 89 percent in 2010 to 92 percent in 2016. As for the intensive margin of education, the situation appears to have remained stable at a high level. Note, however, that exposure to dropout is increasing with age. A thirteen-year-old will have had more chances to drop out of school than an eight-year-old. For instance, the dropout rate among fourteen-year-olds is 8.1 percent versus 2.3 percent for the pooled sample. Moreover, among the out-of-school children, 23 percent dropped out and 77 percent have never enrolled in primary education. Consequently, school feeding has more room to improve the extensive margin of schooling. Furthermore, some slight improvement has also been registered in terms of the grade-for-age gap. Similarly, the average age at enrolment has gone down and is getting closer to 6. This could mean that children are increasingly enrolling in school at age 6. However, there was also an increase in the number of children starting school earlier than the recommended age, which can also bring down the average.

4.2 Food Security Data

To assess the impact heterogeneity of school feeding with respect to the prevailing food security situation, I merge the aforementioned surveys point-to-polygon with information from the Famine Early Warning Systems Network (FEWS-NET). The time match between survey interviews and the food security information is almost to the yearly quarter.

FEWS-NET is a tool of the United States Agency for International Development, and it relies on the input of several organisations. I use their historical, non-projection data to capture the food security situation on the ground during 2010–11 and 2016–17. FEWS-NET classifies areas into five categories of food insecurity, as per the Integrated Food Security Phase Classification Version 2 (2016–17) and the FEWS-NET-defined Food Insecurity Severity Scale (2010–11). The two scales are very similar, but to avoid any misalignment, I collapse the five categories of food insecurity (minimal, stressed, crisis, emergency and famine) into two (minimal vs. some stress).

Minimal stress means that households are able to meet essential food and non-food needs without engaging in atypical and unsustainable strategies. If there is some stress, then FEWS-NET documents the presence of any of the following signals: households not being able to afford some essential non-food expenditures without engaging in stress-coping strategies, households experiencing high or above-usual acute malnutrition, or households

[2] A few outliers were present due to entry errors in the case of two variables which measure distance to the closest primary school and the number of religious primary schools present in the community. Regarding the former, the data clerk has inputted kilometres where metres must have been the correct metric. Thus, I made the correction, but only after I checked that the amended numbers were similar to those of neighbouring communities. Moreover, in the case of religious primary schools, there where only two communities with very large numbers. All other communities report numbers smaller than 25. However, these two rural communities reported 350 and 401. I have set them to 3 and 4, respectively. Furthermore, there were additional outliers in terms of the pupils-to-teacher variable. In this case, the reason behind the outliers was not clear. I have excluded these observations from the analysis. Very few individuals were dropped. Lastly, population numbers were off for 3 communities. They exceeded the population of the districts to which they belonged. I cut two zeroes to bring the numbers down.

only meeting their minimum food needs by depleting or liquidating essential livelihood assets or through crisis-coping strategies. The FEWS-NET variable shows that 2016 was a drought year. In 2010, 23 percent of households lived in areas classified as food insecure, while as many as 59 percent of households lived in food-insecure areas in 2016. It is clear that 2016 was affected by food insecurity, while 2010 was a rather typical year.

5 Empirical Strategy

The literature studying interventions that offer food in exchange for school attendance is generally concerned that treatment at the individual or school level is endogenous. For instance, the agencies implementing school feeding use school-selection criteria that draw heavily on local knowledge. Researchers cannot claim to observe these criteria. Moreover, further bias is likely. For an individual to have received school feeding at some point in the past year, which is the treatment variable, s/he must have been in school to begin with. Thus, it is more likely that s/he is still in school at the time of the interview. This may lead to a positive bias. Lastly, a negative bias due to omitted variables is also possible because school feeding programmes target the most vulnerable of schools, which are also likely to have relatively lower educational performance.

While the implementation design of the programme is a source of endogeneity, it also puts forward the solution. The intervention in Malawi has followed a two-step targeting of beneficiaries. The implementing agencies consult with the government and with regional and district administration departments to decide on which areas to target. These decisions must be based on information that is observable to the relevant authorities, and thus, it is argued, observable to researchers as well. Thereafter, once these areas are set, the agencies use their local knowledge to target schools (Government of Malawi, 2009). The latter step is the source of endogeneity, while the former is the identification solution.

I argue that treatment at the community level can be used as an instrument for treatment at the individual level provided that I control for the observables that inform treatment at the community level, e.g., the decision-making factors used during the first phase of the implementation process. Therefore, I defend the exogeneity of the instrument in the context of selection on observables. Similar strategies have been implemented by Ravallion & Wodon (2000), Sparrow (2007) and Islam & Choe (2013).

5.1 Individual-Level Analysis

Equations 1 and 2 describe the instrumental variable approach. These equations are presented as stacked individual observations. Y denotes the outcomes of interest, e.g., whether a child is in school, his or her grade-for-age gap and age at enrolment. D and Z represent treatment at the individual and village level, respectively. X is a set of covariates that vary at the individual and household levels, and V is a set of controls that define community characteristics. Q consists of district dummies. Finally, T is simply a dummy which equals 1 if the year of data collection is 2016.

$$Y = \gamma_0 \iota_N + \gamma_1 \widehat{D} + X\gamma_2 + V\gamma_3 + Q\gamma_4 + \gamma_5 T + \varepsilon \quad (1)$$

$$D = \alpha_0 \iota_N + \alpha_1 Z + X\alpha_2 + V\alpha_3 + Q\alpha_4 + \alpha_5 T + \nu \quad (2)$$

The coefficient of interest is γ_1. Equation 3 shows that it represents the ratio between the effect of Z on Y and the effect of Z on D (Angrist & Pischke, 2008). Where $i = \overline{1...N}$ and N is the number of individuals. γ_1 identifies the impact of school feeding if several assumptions are met (Imbens & Angrist, 1994; Imbens, 2004; Angrist & Pischke, 2008).

$$\hat{\gamma}_1 = LATE = \frac{cov(Z_i, Y_i \mid X, V, Q, T)}{cov(Z_i, D_i \mid X, V, Q, T)}$$

$$= \frac{E(Y_i \mid Z_i = 1, X, V, Q, T) - E(Y_i \mid Z_i = 0, X, V, Q, T)}{E(D_i \mid Z_i = 1, X, V, Q, T) - E(D_i \mid Z_i = 0, X, V, Q, T)} \quad (3)$$

The first assumption is *conditional mean independence*. Treatment at the community level can be considered as good as random *if* the factors affecting selection into treatment are included: $E(Y_i^1 \mid Z, X, V, Q, T) = E(Y_i^1 \mid X, V, Q, T)$ and $E(Y_i^0 \mid Z, X, V, Q, T) = E(Y_i^0 \mid X, V, Q, T)$. I do my best to include a variety of such factors; however, I cannot claim that all relevant variables have been included. I do argue, though, that I have mitigated the risk to a satisfactory extent.[3] The assumption of conditional mean independence leads to $cov(\varepsilon, \nu) = 0$. That is, the *instrument is exogenous*, which is a necessary condition for the identification of γ_1.

The second assumption relates to the *relevance of the instrument*. The instrument Z must be strongly correlated with the endogenous variable. This is intuitively the case, as schools and individuals are treated if their village was targeted. First-stage regressions are indeed strong and can be consulted in the appendix.

The third assumption is *sufficient overlap*. For each village or community v, the condition for identification is that $0 < p(Z_v = 1 \mid X, V, Q, T) < 1$, i.e., there must be both treated and untreated villages among those with the same set of attributes. I compare and contrast results that are based on the full sample of villages as well as on a sample that is limited to communities with similar, overlapping characteristics, which I call the *overlap sample*.[4] Buttenheim et al. (2011) adopt a similar strategy, per which the authors keep the units of observation that have overlapping characteristics.

[3] In a community-level linear regression of Z on Q, T, V and aggregated values of X, the F-*stat* is 21 and R^2 is 0.36. In a non-linear specification, χ^2 is 442 and the pseudo-R^2 is 0.31.

[4] The graphs plotting the extent of overlap between communities are in Appendices A.2 and A.6. The overlap is limited in absence of corrections to the sample. After corrections, the overlap improves. However, ideally, the mass around the lowest and the highest values of the treatment propensity scores would have been further reduced. Nevertheless, since one expects that a more dissimilar pool of villages would negatively bias estimates, as treatment is targeted at the more vulnerable of villages with lower educational performance, then a sample with a better overlap will increase my chances of finding the hypothesised positive link between school feeding and educational outcomes. Thus, the amount of post-correction overlap is conservative and thus acceptable.

Fourth and last, I assume the *instrument is monotonous*, i.e., the sample is assumed only to consist of compliers. In this case, if defiers were numerous, then the estimates would be biased downwards and make my hypothesis easier to reject. Because of the assumption of monotonicity, the estimation is of a local average treatment effect (LATE) (Imbens & Angrist, 1994; Angrist & Pischke, 2008).

Finally, I employ the reduced-form specification presented in Equation 4. This is a classical problem of selection on observables.

$$Y = \beta_0 \iota_N + \beta_1 Z + X\beta_2 + V\beta_3 + Q\beta_4 + \beta_5 T + \epsilon \tag{4}$$

$$\hat{\beta}_1 = AIT = E(Y_i \mid Z_i = 1, X, V, Q, T) - E(Y_i \mid Z_i = 0, X, V, Q, T) \tag{5}$$

In contrast to the two-stage least squares estimator, which identified LATE, β_1 identifies an average intention to treat effect (AIT) in Equation 5. The identification assumptions are those of conditional mean independence and sufficient overlap. This is an intention to treat effect because not everyone in a targeted village is treated.

5.2 Village-Level Analysis

I also run a village-level analysis by collapsing the data accordingly. Equation 4 applies, but N denotes villages as opposed to individuals. I use propensity score matching. In this context, the treatment effect is the average of the difference between the observed and imputed potential outcomes computed for each community. See Equation 6. Identification is achieved under conditional mean independence and sufficient overlap.

$$AIT = E(\hat{Y}_i \mid Z_i = 1, X, V, Q, T) - E(\hat{Y}_i \mid Z_i = 0, X, V, Q, T) \tag{6}$$

$\hat{Y}_{i,Z=0}$ must be imputed for treated communities, and so does $\hat{Y}_{i,Z=1}$ for control villages. Meanwhile, the following are observed: $\hat{Y}_{i,Z=1} = Y_i$ for treated communities and $\hat{Y}_{i,Z=0} = Y_i$ for control communities. The imputation is done based on matching with the most similar village of the opposite treatment status. Similarity is ascertained based on treatment propensity scores.

6 Individual-Level Results

6.1 Local Average Treatment Effect

Table 2 shows that the impact of school feeding has chiefly consisted of attracting out-of-school children into primary education for the first time. The IV coefficient puts the magnitude of the impact on the extensive margin at roughly 5.8 percentage points in the pooled 2010–16 sample. In contrast, the impact for the intensive margin of schooling is not significant. This may suggest that children enrol in school during times of need, but that school feeding is not sufficient to dissuade dropout.[5]

[5]First stage regressions are strong. They are reported in Appendix A.1.

Table 2: Impact of School Feeding on the Extensive and Intensive Margins of Schooling

	IN SCHOOL, EXTENSIVE MARGIN						**IN SCHOOL, INTENSIVE MARGIN**					
	Pooled sample		2010 typical year		2016 drought year		Pooled sample		2010 typical year		2016 drought year	
Explanatory variable	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Panel A: Full sample												
Beneficiary school feeding	0.124	0.058	0.159	-0.002	0.098	0.084	0.032	0.007	0.035	-0.011	0.032	0.037
	(0.005)***	(0.018)***	(0.008)***	(0.024)	(0.007)***	(0.033)***	(0.003)***	(0.009)	(0.005)***	(0.012)	(0.004)***	(0.018)**
	[0.008]***	[0.026]**	[0.016]***	[0.038]	[0.009]***	[0.038]**	[0.004]***	[0.011]	[0.006]***	[0.014]	[0.006]***	[0.020]*
R^2	0.11	0.10	0.14	0.11	0.10	0.10	0.04	0.04	0.05	0.04	0.04	0.04
F	20	-	14	-	9	-	4	-	2	-	2	-
χ^2	-	1599	-	1084	-	737	-	414	-	211	-	223
Robust score test	-	13.75	-	48.09	-	0.17	-	6.24	-	13.81	-	0.05
P-value test	-	0.00	-	0.00	-	0.92	-	0.04	-	0.00	-	0.97
N	20,611	20,611	10,038	10,038	10,573	10,573	19,056	19,056	9,113	9,113	9,943	9,943
Panel B: Overlap sample												
Beneficiary school feeding	0.128	0.072	0.169	0.006	0.082	0.084	0.030	0.015	0.040	-0.008	0.027	0.048
	(0.005)***	(0.018)***	(0.009)***	(0.025)	(0.008)***	(0.034)**	(0.003)***	(0.009)	(0.006)***	(0.013)	(0.005)***	(0.018)***
	[0.010]***	[0.027]***	[0.018]***	[0.036]	[0.012]***	[0.039]**	[0.004]***	[0.010]	[0.008]***	[0.015]	[0.007]***	[0.019]**
R^2	0.11	0.11	0.14	0.10	0.10	0.10	0.04	0.04	0.07	0.05	0.05	0.05
F	14	-	7	-	6	-	3	-	1	-	2	-
χ^2	-	1062	-	451	-	467	-	270	-	102	-	149
Robust score test	-	9.53	-	44.95	-	0.00	-	2.35	-	12.79	-	1.24
P-value test	-	0.01	-	0.00	-	1.00	-	0.31	-	0.00	-	0.54
N	13,353	13,353	4,493	4,493	6,524	6,524	12,315	12,315	4,098	4,098	6,147	6,147

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are reported in parenthesis. Errors are clustered at the level of communities in square brackets. F, χ^2 and the exogeneity test statistics correspond to the specification with robust errors. Panel A includes all observations. Panel B only includes individuals from communities with treatment propensity scores $\in (0.1, 0.9)$, where propensity scores are predicted based on logit regressions at the community level. Other covariates include: *individual-level variables* (age, age squared, gender, father's education: no education vs. primary/ lower/ upper secondary and post-secondary, and mother's education), *household-level variables* (household size, household gender-age composition, whether the household benefits from safety nets other than school feeding and as enumerated in Table 1, female-headed household, marital status: married with spouse present vs. head is married but spouse is not present/ head is divorced, separated or widowed/ head is single, wall materials: traditional dwelling vs. walls made from permanent materials/ walls made from a mix of permanent and traditional materials, household owns basic furniture, household owns a radio or TV-set, average garden size, average time to water source, prevailing food security situation in the household's area: some stress vs. minimal stress), *village-level characteristics* (population, perc. Muslims, perc. Christians, main land use: agriculture vs. planned housing/ squatter/ industry/ shops and other, main access road: asphalt vs. gravelled road/ dirt road and dirt track, presence of irrigation schemes, whether there were any natural disasters during the current year, one or two years before, distances to chipatala, banks, secondary and primary government schools, whether schools are mainly made from permanent materials, whether they are electrified, the number of private primary schools, the number of religious primary schools, and the ratio of pupils to teachers), district and year dummies.

Moreover, I separate the pooled sample into its component years to tentatively investigate the heterogeneity of school feeding in terms of the prevailing food security situation. 2010 was a year of satisfactory agricultural output and minimal food insecurity. In contrast, 2016 was marked by food insecurity. I find no impact on the extensive or intensive margins of schooling in 2010, but the results for 2016 suggest a positive and significant increase of 8–9 percentage points in the extensive margin of schooling and 3–4 percentage points for the intensive margin.[6] For 2016, the χ^2 statistic of the robust score test fails to reject the null of exogeneity. Thus, the OLS coefficient can take precedence over the IV estimate.[7]

Furthermore, I limit the sample to similar communities to mitigate the risk of confounding the impact of school feeding with that of unobservable factors that set the treated communities apart from the control communities. This is done by compiling an *overlap sample* which excludes the communities whose treatment propensity scores are lower than 0.10 or greater than 0.90.[8] I find that previous results are robust to the overlap sample. Panels A and B of Table 2 lead to the same conclusions. However, the robust score test fails to reject the null of exogeneity for the case of retention rates in the pooled sample in Panel B. Thus, the more efficient OLS coefficient, which is significant with a magnitude of 3 percentage points, can be considered instead of the IV estimate.

Finally, I run a falsification test on the dependent variable. I replace the education of children with that of their parents. I measure the education of parents as a dummy variable taking the value of 1 if at least one parent has achieved at least primary education. In Appendix A.3, I show that school feeding does not impact the false dependent variable. Thus, my analysis survives the falsification test.

6.2 Impact Heterogeneity

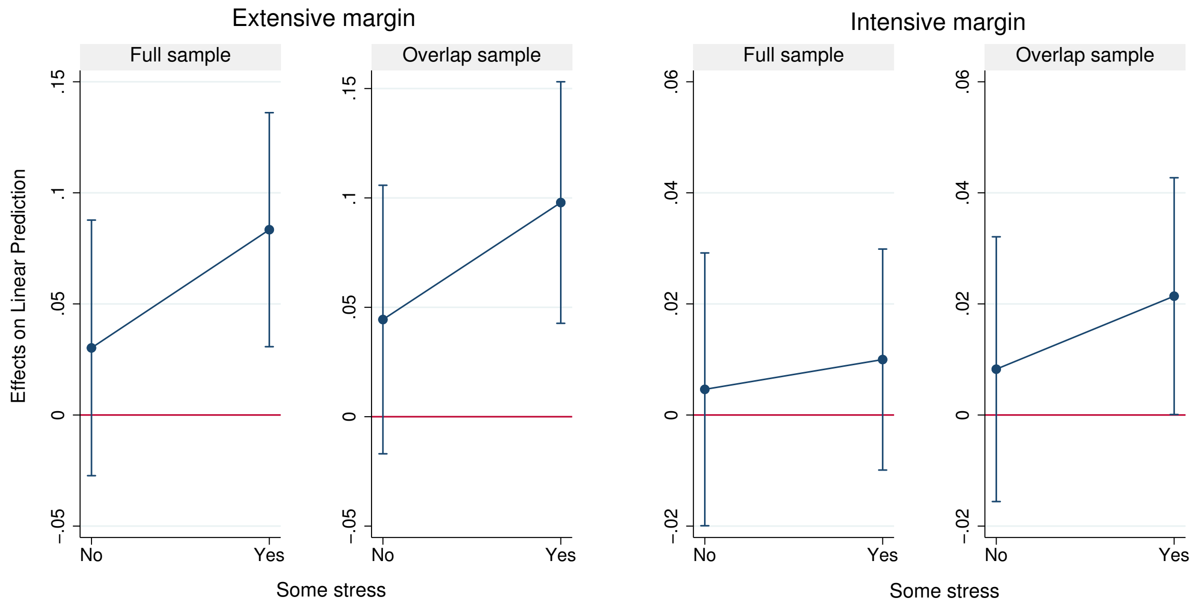
In this section, I explore a further avenue to assessing the impact heterogeneity of school feeding in terms of the prevailing food security situation. This is based on the observation that even during times of satisfactory harvests, pockets of food insecurity still exist. Similarly, even if the situation is one of generalised drought, some areas will, nevertheless, be minimally affected by food insecurity.

[6] I have also explored the heterogeneity of results in terms of gender. Differences between boys and girls are limited. School feeding has not impacted the education of either gender in 2010, and the drought sample of 2016 shows very limited heterogeneity.

[7] The 2010 and 2016 samples offer a snapshot of Malawi during two very different socio-economic contexts. This can explain why the robust score tests, which examine the presence of endogeneity, have led to different conclusions in the case of the 2010 and 2016 samples. On account of the drought experienced during the 2015–16 agricultural season, the implementation strategy of school feeding programmes was modified to mitigate the consequences of drought. The agencies implementing school feeding were asked to expand the coverage of school feeding (Government of Malawi, 2016). As a consequence of this expansion, the criteria that the implementing agencies were previously using to decide which schools to target have probably lost part of their relevance, as a wider and more diverse array of schools and students were now being targeted. These criteria were the source of endogeneity. Thus, the bias that was previously created by my inability to observe the school-selection criteria has likely been reduced in the aftermath of the 2016 drought and against the background of the amended programme implementation strategy.

[8] Appendix A.2 shows graphically how this sample restriction improves the amount of overlap between the control and treated villages.

Figure 1: Average Marginal Effects of School Feeding with 90% CIs and Varying Food Insecurity



IV coefficients, confidence intervals are based on clustered errors. The covariates from Table 2 apply, plus an interaction term between school feeding and the prevailing food security situation.

Figure 1 presents the marginal effects of school feeding on the extensive and intensive margins of schooling given two levels of localised food insecurity: minimal versus some food-related stress or crisis.[9] This figure confirms that the impact of school feeding is heterogeneous with respect to the prevailing food security situation. The impact of school feeding would not be significant if all children lived in food-secure areas. In contrast, if food insecurity were to prevail, then the impact on enrolment rates would become significant at 8.3 and 10 percentage points in the full and overlap sample, respectively. Similarly, retention rates would be 1–2 percentage points higher, although significance is only attained in the overlap sample.

6.3 Average Intention to Treat Effect

To investigate the strength of my findings further, I estimate a reduced-form specification. I use the methodology proposed by Altonji et al. (2005) and Oster (2019) to test the robustness of the estimates to the bias created by unobservables. The AIT is a conservative estimate compared to LATE. Therefore, the robustness of the reduced-form results with respect to unobservables will be even more encouraging.

Altonji et al. (2005) and Oster (2019) argue that one can tentatively evaluate the robustness of results to the omitted variable bias by observing coefficient and R^2 movements before and after the inclusion of controls. First, this method can give the bias-adjusted β coefficient conditional on two parameter inputs: the relative degree of selection on observed and unobserved variables (δ), and the R^2 value that one assumes corresponds to a scenario whereby all relevant variables are observed and included in the regression (R_{max}).

[9]First stage regressions are presented in Appendix A.4.

Second, one can compute the maximum level of δ such that the β coefficient is brought down to zero and R^2 is set to R_{max}. Lastly, the method can also help identify the maximum R^2 for which the β coefficient is still positive and δ is of a set value. Oster (2019) recommends $\delta = \pm 1$, which means that unobservables are as important as observables, and $R_{max} = 1.3\hat{R}^2$, where $\hat{R}^2$ is the estimated R^2 when observables are included in the estimation.

Table 3 shows that the impact of school feeding on the extensive and intensive margins of education is positive and significant, and that the effect is driven by the programme's effectiveness in food-insecure areas. Moreover, results are generally robust to omitted variables. The coefficients of interest are all keeping their sign after they are corrected for the omitted variable bias, where R_{max} is set to $1.3\hat{R}^2$ and unobservables are assumed as important as observables, i.e., $\delta = \pm 1$. Most results are robust to even more stringent specifications than the one recommended by Oster (2019). This allows me to state with some level of confidence that school feeding has had a positive and significant effect on educational outcomes. The effect ranges from 1.6 to 3.1 percentage points in the case of the extensive margin of schooling, while for the the intensive margin, the magnitude is of 1 percentage point in food-insecure areas.

6.4 Mechanisms

In this subsection, I look at age at enrolment conditional on enrolment and children's grade-for-age gap to discuss the mechanisms that connect school feeding to enrolment and retention rates. I assume that exposure to treatment has been lengthy, i.e., villages have been treated for several years before they were interviewed at the time of the Integrated Household Surveys. In this case, a food security analysis is no longer appropriate, as I am eliciting the impact of school feeding over longer periods of time and the food security data is punctual not historical.

Primary school starts at age 6 in Malawi, but late- or early-age enrolments are not uncommon. Among five-year-olds in rural Malawi at the time of the Integrated Household Surveys, 4 percent reported being in pre-school, 28 percent reported being in primary education, and the remainder had not yet enrolled in any form of education. Moreover, among the ever-enrolled sample of children aged 6 to 14 years old, 23 percent have reportedly enrolled at age 7 or above.

Table 4 suggests that school feeding has reduced the average age at enrolment, and that it has done so by attracting children into school earlier than the advice of the government. This statement is based on the observation that the impact of school feeding is weak for the sample of children whose age at enrolment is 6 or above, but it is negative and strongly significant when early enrollees are also allowed in the sample. The magnitude of the local average treatment effect is of 2–3 months. First stage regressions can be consulted in Appendix A.5.

Table 3: Reduced-Form Regressions

	IN SCHOOL, EXTENSIVE MARGIN						IN SCHOOL, INTENSIVE MARGIN					
	Full sample			***Overlap sample***			***Full sample***			***Overlap sample***		
Explanatory variable	Pooled	Food insecure	Food secure	Pooled	Food insecure	Food secure	Pooled	Food insecure	Food secure	Pooled	Food insecure	Food secure
Community targeted by school feeding	0.016 (0.005)*** [0.007]**	0.021 (0.008)** [0.011]*	0.007 (0.007) [0.010]	0.022 (0.006)*** [0.008]***	0.031 (0.009)*** [0.011]***	0.006 (0.008) [0.011]	0.002 (0.003) [0.003]	0.008 (0.004)* [0.005]*	-0.002 (0.004) [0.005]	0.005 (0.003) [0.003]	0.010 (0.005)** [0.005]*	-0.002 (0.004) [0.004]
Bias-adjusted β for:												
$R_{max} = 1.3\hat{R}^2$ and $\delta = \pm 1$	0.015	0.010	-	0.020	0.028	-	-	0.008	-	-	0.010	-
$R_{max} = 0.15$ and $\delta = \pm 1$	0.015	negative	-	0.018	0.022	-	-	0.006	-	-	0.009	-
$R_{max} = 0.50$ and $\delta = \pm 1$	0.012	negative	-	0.011	negative	-	-	0.006	-	-	0.009	-
δ for:												
$R_{max} = 1.3\hat{R}^2$ and $\beta = 0$	9.862	1.613	-	72.575	3.686	-	-	6.977	-	-	5.892	-
Max R_{max} for:												
$\beta > 0$ and $\delta = \pm 1$	1.000	0.130	-	1.000	0.190	-	-	1.000	-	-	1.000	-
R^2	0.09	0.09	0.11	0.09	0.09	0.12	0.04	0.04	0.04	0.04	0.05	0.04
F	17	8	11	11	7	7	4	2	3	3	2	2
N	20,611	8,431	12,180	13,353	6,650	6,703	19,056	7,706	11,350	12,315	6,086	6,229

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are reported in parenthesis. Errors are clustered at the level of communities and presented in square brackets. The covariates from Table 2 apply. The F statistic corresponds to the specification with robust errors. The overlap sample only includes individuals from communities with a treatment propensity score between 0.1 and 0.9. Propensity scores are predicted based on logit regressions at the community level.

Table 4: Impact of School Feeding on Age at Enrolment and Grade-for-Age Gaps

	AGE AT ENROLMENT								**GRADE-FOR-AGE GAP**							
	Children enrolled at 6 or above				Children enrolled at 5 or above				All children 6–14				In-school children			
Explanatory	***Full sample***		***Overlap sample***		***Full sample***		***Overlap sample***		***Full sample***		***Overlap sample***		***Full sample***		***Overlap sample***	
variable	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Beneficiary school feeding	0.003	-0.088	0.004	-0.098	-0.009	-0.227	-0.018	-0.241	-0.194	-0.202	-0.215	-0.224	0.020	-0.096	0.012	-0.098
	(0.018)	(0.050)*	(0.021)	(0.052)*	(0.019)	(0.053)***	(0.021)	(0.054)***	(0.023)***	(0.073)***	(0.025)***	(0.074)***	(0.021)	(0.062)	(0.023)	(0.062)
	[0.025]	[0.073]	[0.027]	[0.070]	[0.026]	[0.077]***	[0.029]	[0.073]***	[0.030]***	[0.104]*	[0.034]***	[0.101]**	[0.026]	[0.081]	[0.029]	[0.079]
R^2	0.15	0.15	0.13	0.13	0.14	0.14	0.13	0.12	0.45	0.45	0.45	0.45	0.47	0.47	0.47	0.47
F	22	-	12	-	26	-	16	-	176	-	120	-	180	-	124	-
χ^2	-	2122	-	1144	-	2521	-	1452	-	16816	-	10988	-	17208	-	11329
Rob. sc. test	-	4.02	-	4.81	-	19.92	-	21.04	-	0.01	-	0.01	-	4.08	-	3.66
P-value test	-	0.13	-	0.09	-	0.00	-	0.00	-	0.99	-	0.99	-	0.13	-	0.16
N	15,527	15,527	10,069	10,069	18,549	18,549	11,979	11,979	20,611	20,611	13,353	13,353	18,602	18,602	12,020	12,020

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are reported in parenthesis. Errors are clustered at the level of communities and presented in square brackets. The covariates from Table 2 apply. F, χ^2 and the exogeneity statistics correspond to the specification with robust errors. Age at enrolment is recalled for each interviewee. Children who have never enrolled are excluded. The grade-for-age gap is determined based on age, the grade a child is in and the grade that s/he should be in given her or his age. The gap is always zero or positive. Children who are ahead in school (early achievers) are considered with a zero gap. If a child has never been to school, then the gap is given by the grade the child should have been in given her or his age.

Regarding the grade-for-age gap analysis, Table 4 shows once again that school feeding appears to be improving educational outcomes by attracting out-of-school children into primary education. If out-of-school children are left out of the analysis, then school feeding does not have a strong impact on the grade-for-age gap of children who are currently attending school. In contrast, if the sample includes all children in the age group 6–14, then school feeding does reduce the grade-for-age gap by 2–3 months on average. In this case, the IV and OLS estimates are not statistically different from each other.

7 Community-Level Results

I use propensity score matching to estimate the average intention to treat effect of school feeding on community-level averages of the extensive and intensive margins of schooling.

Table 5: Impact of Community-Level Targeting on Average Schooling

	AVG. IN SCHOOL, EXTENSIVE MARGIN					
	# matches		Level of tolerance overlap assumption			
Explanatory variable	# 1	# 2	# 0.01	# 0.05	# 0.10	# 0.20
Pooled sample						
Community targeted	0.018	0.021	0.019	0.021	0.020	0.009
by school feeding	(0.006)***	(0.007)***	(0.006)***	(0.009)**	(0.009)**	(0.008)
	AVG. IN SCHOOL, INTENSIVE MARGIN					
	# matches		Level of tolerance overlap assumption			
Explanatory variable	# 1	# 2	# 0.01	# 0.05	# 0.10	# 0.20
Pooled sample						
Community targeted	0.005	0.002	0.005	0.002	-0.001	0.007
by school feeding	(0.004)	(0.006)	(0.004)	(0.004)	(0.004)	(0.002)***
Treated	384	384	381	360	326	240
Untreated	715	715	692	547	428	280
# matches	1	2	1	1	1	1

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors. Other covariates include: individual- and household-level variables aggregated at the level of communities (household size, perc. households who benefit from safety nets other than school feeding, perc. female-headed households, perc. households whose head is married but spouse is not present, perc. households whose head is divorced, separated or widowed, perc. households whose head is single, perc. households with walls made from permanent materials, perc. households with walls made from a mix of permanent and traditional materials, perc. households who own basic furniture, perc. households who own a radio/TV-set, perc. Muslims, perc. Christians, average education of individuals aged 24+, average garden size, average time to collect water from source and food insecurity prevalence) and village-level characteristics (population, main land use: agriculture vs. planned housing/ squatter/ industry/ shops and other, main access road: asphalt vs. gravelled road/ dirt road and dirt track, presence of irrigation schemes, history of natural disasters, distances to chipatala, banks, secondary and primary schools, whether schools are mainly made from permanent materials, whether they are electrified, the number of private primary schools, the number of religious primary schools and the pupils-to-teacher ratio), as well as district and year dummies.

Table 5 presents the results for the pooled sample, which includes both food-secure and food-insecure communities. If all communities were targeted by school feeding, then they would all see a 2 percentage point increase in enrolments compared to a no-treatment scenario. The coefficient is robust to various samples defined by the overlap tolerance parameter. Significance is lost only in the most stringent of specifications. In contrast, retention rates do not seem to respond to school feeding programmes in the pooled sample.

To explore the impact heterogeneity of school feeding, I divide the sample into food-secure and food-insecure communities. Table 6 confirms that results are heterogeneous in terms of the prevailing food security situation. School feeding has no impact on the extensive margin of schooling if communities face minimal food insecurity. However, school feeding does increase enrolment by 2–4 percentage points in food-insecure communities. Results are strongly significant and robust to various levels of the tolerance parameter. As for the intensive margin of education, the impact of school feeding in food-insecure communities is positive, but the magnitude is small and treatment is only occasionally significant.[10]

Lastly, I run a falsification test. The extensive margin of primary schooling is swapped for the extensive margin of secondary education. The latter is defined as the percentage of children aged 15–18 who are in secondary education. General equilibrium effects could impact this analysis insofar as parents' decision to enrol children in primary education is linked to their older siblings' enrolment in secondary school, or if school feeding would have allowed more children to graduate from primary school and thus be eligible for secondary education. Results do not suggest any clear link between school feeding and the falsified outcome. My analysis survives the falsification test. See Appendix A.8.

8 Conclusion

In this chapter, I have used an instrumental variable approach and propensity score matching to estimate the impact of school feeding on educational outcomes in rural Malawi. The evidence is causal to the extent that I manage to control for the factors that explain both village-level treatment and educational outcomes. My results and conclusions are shown to hold even after running a number of robustness checks.

I provide evidence that the significance and magnitude of the impact of school feeding programmes rely on the prevailing local food security situation at the time and place of the evaluation. As long as there is a food constraint to relax and an educational gap to fill, school feeding improves educational outcomes. However, school feeding appears to be more effective in attracting children into school for the first time than in keeping them in school. Enrolment rates have increased by roughly 8–10 percentage points in food-insecure areas. Targeted villages, which include both treated and control children, have experienced an increase of 2–3 percentage points in average enrolment in food-insecure areas. In contrast, the impact on retention rates is less robust. While the IV and reduced-form estimations point to a 1–2 percentage point improvement in food-insecure areas, my propensity score matching analysis fails to find a robust impact. Across the board, impacts are not significant in food-secure areas. My investigation into mechanisms further proves that the effect of school feeding has been at its strongest in enrolling out-of-school children.

[10]Overlap and balance plots are Appendices A.6 and A.7.

Table 6: Heterogeneous Impact of Community-Level Treatment on Average Schooling

	AVG. IN SCHOOL, EXTENSIVE MARGIN									
	Minimal stress sample					***Some stress sample***				
Explanatory	Level of tolerance overlap assumption					Level of tolerance overlap assumption				
variable	#0	# 0.01	# 0.05	# 0.10	# 0.20	#0	# 0.01	# 0.05	# 0.10	#0.20
Community targeted	-0.001	-0.001	-0.005	-0.007	-0.009	0.027	0.026	0.039	0.021	0.019
by school feeding	(0.024)	(0.021)	(0.005)	(0.006)	(0.008)	(0.005)***	(0.005)***	(0.016)**	(0.003)***	(0.012)*

	AVG. IN SCHOOL, INTENSIVE MARGIN									
	Minimal stress sample					***Some stress sample***				
Explanatory	Level of tolerance overlap assumption					Level of tolerance overlap assumption				
variable	#0	# 0.01	# 0.05	# 0.10	# 0.20	#0	# 0.01	# 0.05	# 0.10	#0.20
Community targeted	-0.018	-0.019	-0.000	0.002	0.002	0.001	0.001	0.007	0.008	0.001
by school feeding	(0.025)	(0.020)	(0.002)	(0.008)	(0.004)	(0.004)	(0.004)	(0.003)**	(0.002)***	(0.010)
Treated	161	160	152	142	99	199	184	163	143	106
Untreated	453	427	296	221	123	242	228	195	170	113
# matches	1	1	1	1	1	1	1	1	1	1

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are reported in parenthesis. The covariates from Table 5 apply.

My findings suggest that school feeding should be complemented by other educational programmes to improve educational outcomes in the long run. School feeding is also best targeted at the most vulnerable of schools or villages; however, if villages oscillate between food security and insecurity, then casting a wider net can be all-important when shocks to food availability are widespread. This is especially true of predominantly rural economies such as Malawi, which rely on rain-fed agriculture.

Finally, the impact heterogeneity that is documented in this chapter can help explain the often conflicting evidence with respect to the impact of school feeding on enrolment and retention rates. Therefore, this study can act as a bridge between studies such as Ravallion & Wodon (2000), Meng & Ryan (2010) and Kazianga et al. (2012), on the one hand, and articles such as Tan et al. (1999), Buttenheim et al. (2011) and Azomahou et al. (2019), on the other. Against this background, this study has also emphasised the caveats of not considering the interaction between programme design and the food security context at the time and place of programme observation, which has implications for the outcome of evaluations. Therefore, this study can provide valuable insights to inform the planning of future school feeding evaluations, as it underscores the importance of collecting the necessary information to ascertain whether food-related constraints are binding in the evaluators' samples of interest.

A Appendix

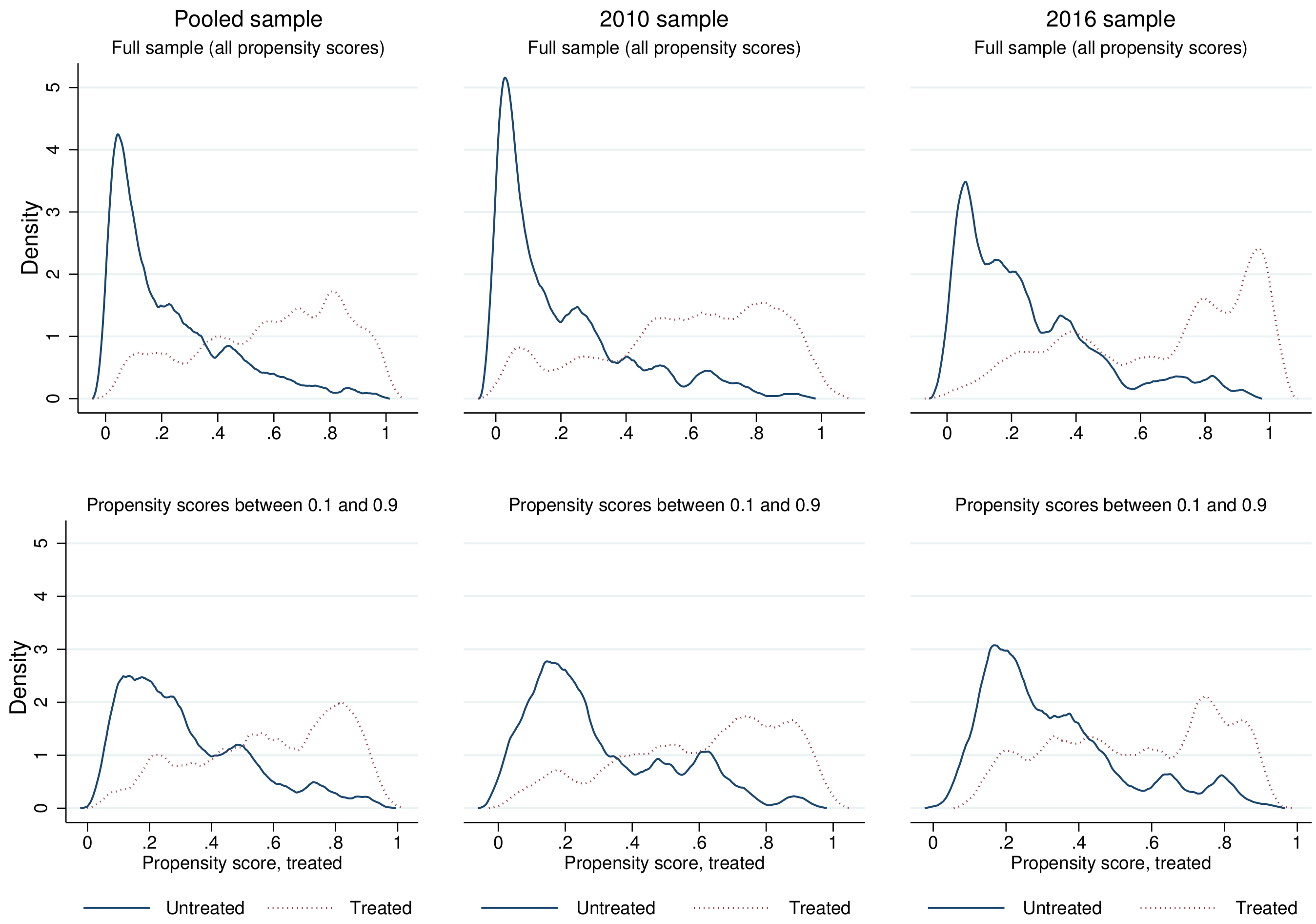
A.1 First Stage Regressions, Main Analysis

	BENEFICIARY SCHOOL FEEDING					
	Sample for extensive margin			Sample for intensive margin		
Explanatory variable	Pooled	2010	2016	Pooled	2010	2016
Full sample						
Community targeted	0.280	0.365	0.204	0.297	0.391	0.215
by school feeding	(0.007)***	(0.012)***	(0.009)***	(0.007)***	(0.013)***	(0.009)***
	[0.017]***	[0.032]***	[0.020]***	[0.019]***	[0.034]***	[0.020]***
R^2	0.31	0.39	0.28	0.33	0.44	0.30
F	66	44	36	77	57	40
N	20,611	10,038	10,573	19,056	9,113	9,943

	BENEFICIARY SCHOOL FEEDING					
	Sample for extensive margin			Sample for intensive margin		
Explanatory variable	Pooled	2010	2016	Pooled	2010	2016
Overlap sample						
Community targeted	0.303	0.398	0.209	0.322	0.426	0.221
by school feeding	(0.008)***	(0.014)***	(0.010)***	(0.008)***	(0.015)***	(0.010)***
	[0.018]***	[0.036]***	[0.021]***	[0.008]***	[0.038]***	[0.021]***
R^2	0.28	0.32	0.29	0.31	0.36	0.31
F	51	28	25	61	38	28
N	13,353	4,493	6,524	12,315	4,098	6,147

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The covariates from Table 2 apply.

A.2 Sufficient Overlap Assumption, Wave Samples



The figure plots the overlap between treated and untreated communities based on covariates. Propensity scores are predicted based on logit regressions at the community level. Treatment at the community level is regressed on the usual aggregated individual-, household- and village-level variables plus district dummies. For the pooled sample, a time dummy is also included.

A.3 Falsification Test, Individual-Level Analysis

	AT LEAST ONE PARENT HAS AT LEAST PRIMARY EDUCATION											
	Pooled sample				2010 typical year				2016 drought year			
	Full Sample		***Overlap Sample***		***Full Sample***		***Overlap Sample***		***Full Sample***		***Overlap Sample***	
Expl. variable	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Benef. school feeding	0.000 (0.009) [0.013]	-0.001 (0.027) [0.042]	0.000 (0.010) [0.014]	0.017 (0.027) [0.040]	-0.012 (0.013) [0.018]	-0.038 (0.033) [0.048]	0.007 (0.015) [0.018]	0.014 (0.035) [0.047]	0.011 (0.012) [0.018]	0.061 (0.052) [0.081]	0.023 (0.015) [0.022]	0.111 (0.054)** [0.073]
R^2	0.14	0.14	0.15	0.15	0.15	0.15	0.13	0.13	0.16	0.16	0.18	0.17
F	40	-	29	-	23	-	12	-	24	-	21	-
χ^2	-	3499	-	2429	-	2009	-	900	-	2053	-	1500
N	20,611	20,611	13,353	13,353	10,038	10,038	4,493	4,493	10,573	10,573	6,524	6,524

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is a dummy variable. The covariates from Table 2 apply. The education of the mother and father, which were covariates in Table 2, however, are no longer included in the specification.

A.4 First Stage Regressions, Interaction Model

	BENEFICIARY SCHOOL FEEDING		BENEFICIARY SCHOOL FEEDING× SOME STRESS	
Explanatory variables	Sample for ext. margin	Sample for int. margin	Sample for ext. margin	Sample for int. margin
Full sample				
Community targeted by school feeding	0.259 (0.009)*** [0.024]***	0.276 (0.010)*** [0.025]***	-0.023 (0.003)*** [0.007]***	-0.021 (0.003)*** [0.007]***
Community targeted × Some stress	0.046 (0.013)*** [0.029]	0.046 (0.013)*** [0.031]	0.355 (0.009)*** [0.021]***	0.373 (0.010)*** [0.022]***
R^2	0.31	0.33	0.40	0.44
F	65	76	47	57
N	20,611	19,056	20,611	19,056
Overlap sample				
Community targeted by school feeding	0.276 (0.010)*** [0.026]***	0.294 (0.011)*** [0.028]***	-0.023 (0.003)*** [0.008]***	-0.023 (0.003)*** [0.009]***
Community targeted × Some stress	0.056 (0.014)*** [0.033]*	0.055 (0.015)*** [0.036]	0.360 (0.010)*** [0.024]***	0.378 (0.011)*** [0.025]***
R^2	0.28	0.31	0.36	0.40
F	50	61	14	16
N	13,353	12,315	13,353	12,315

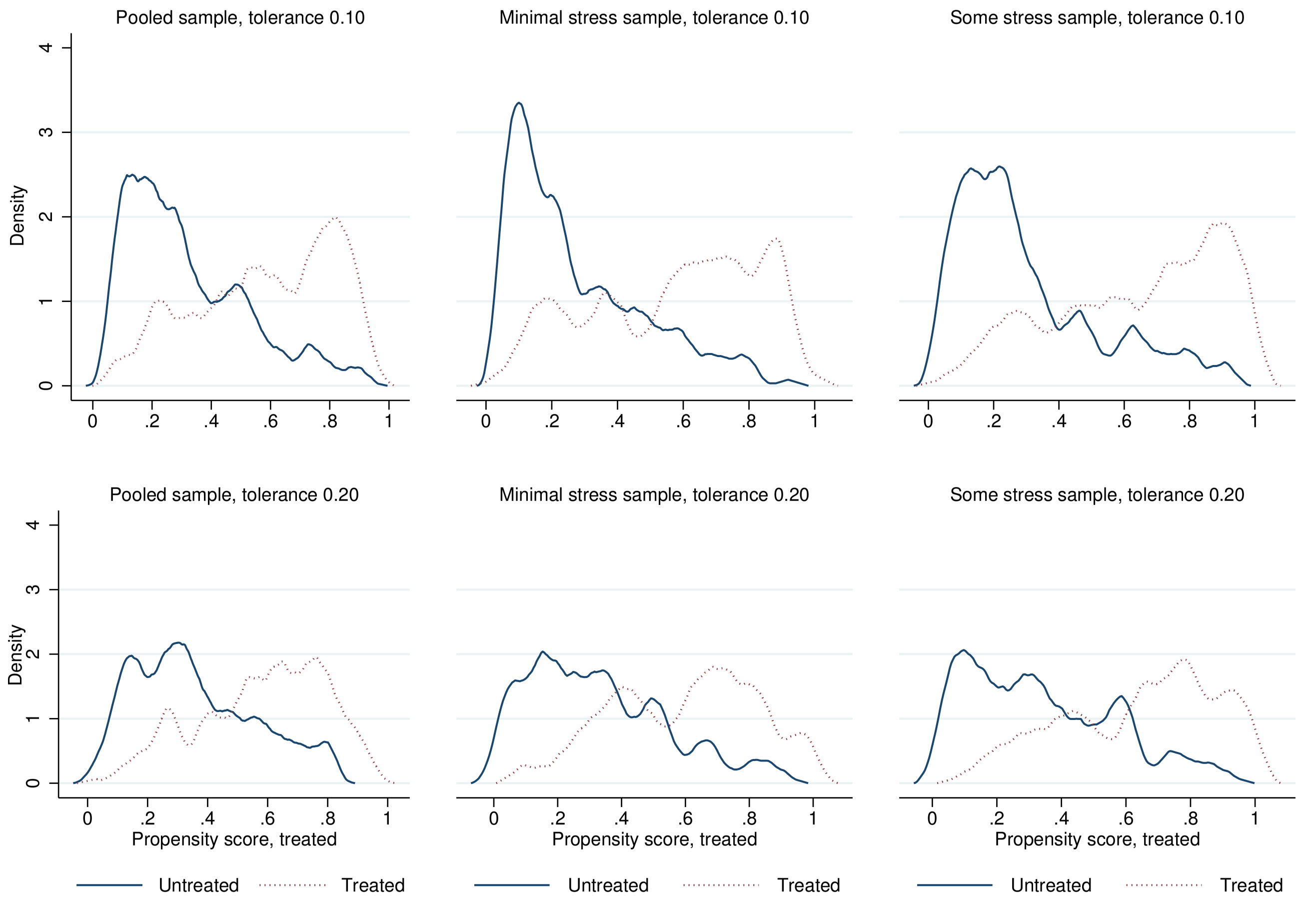
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The covariates from Table 2 apply, plus the interaction term: instrument × food security.

A.5 First Stage Regressions, Mechanisms

	BENEFICIARY SCHOOL FEEDING							
	6+ sample		5+ sample		Sample all children 6–14		Sample in-school children	
Explanatory variable	***Full sample***	***Overlap sample***	***Full sample***	***Overlap sample***	***Full sample***	***Overlap sample***	***Full sample***	***Overlap sample***
Community targeted by school feeding	0.303 (0.008)*** [0.019]***	0.325 (0.009)*** [0.020]***	0.297 (0.007)*** [0.019]***	0.322 (0.008)*** [0.020]***	0.280 (0.007)*** [0.017]***	0.303 (0.008)*** [0.018]***	0.303 (0.007)*** [0.019]***	0.328 (0.008)*** [0.020]***
R^2	0.34	0.32	0.33	0.31	0.31	0.28	0.34	0.32
F	66	51	74	59	66	51	82	65
N	15,527	10,069	18,549	11,979	20,611	13,353	18,602	12,020

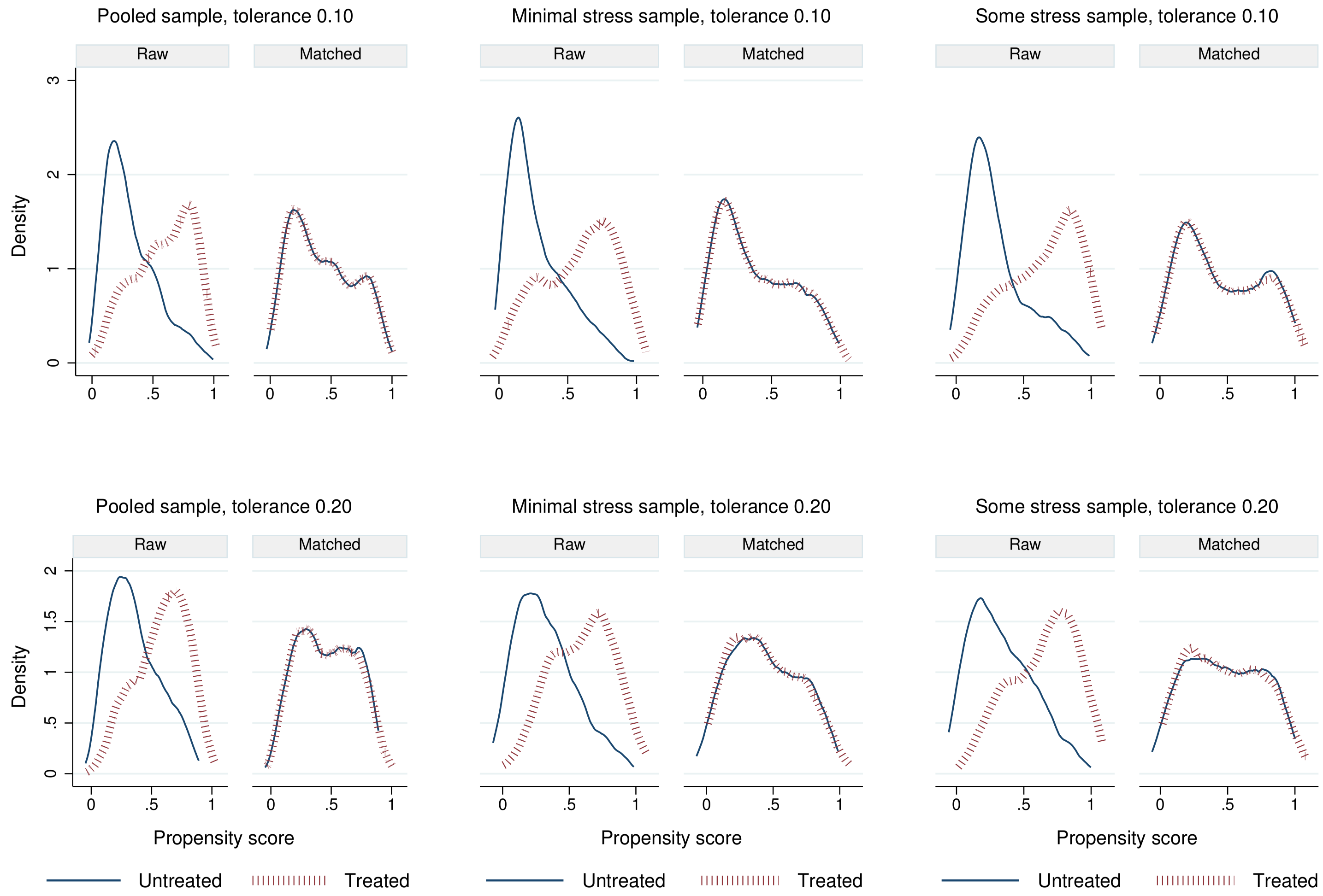
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The covariates from Table 4 apply.

A.6 Sufficient Overlap Assumption, Food Security Samples



The figure plots the overlap between treated and untreated communities based on covariates. The top row includes communities with a propensity score between 0.1 and 0.9. The bottom row only includes communities with a propensity score between 0.2 and 0.8. Note samples: pooled, food secure and food insecure.

A.7 Balance Plots, Food Security Samples



A.8 Falsification Test, Community-Level Analysis

	AVG. IN SECONDARY SCHOOL EXTENSIVE MARGIN					
	# matches		Level of tolerance overlap assumption			
Explanatory variable	# 1	# 2	# 0.01	# 0.05	# 0.10	# 0.20
Panel A: Pooled sample						
Community targeted	-0.004	-0.006	-0.004	-0.005	-0.010	-0.068
by school feeding	(0.013)	(0.011)	(0.014)	(0.014)	(0.011)	(0.020)***
Treated	382	382	379	358	324	238
Untreated	710	710	688	543	426	278
# matches	1	2	1	1	1	1
		Level of tolerance overlap assumption				
		#0	# 0.01	# 0.05	# 0.10	# 0.20
Panel B: Minimal stress sample						
Community targeted		-0.024	-0.027	0.023	0.026	-0.064
by school feeding		(0.030)	(0.031)	(0.042)	(0.015)*	(0.020)***
Treated		160	159	151	141	99
Untreated		448	423	292	219	122
		Level of tolerance overlap assumption				
		#0	# 0.01	# 0.05	# 0.10	# 0.20
Panel C: Some stress sample						
Community targeted		-0.031	-0.038	0.015	0.030	-0.003
by school feeding		(0.023)	(0.014)***	(0.040)	(0.034)	(0.058)
Treated		198	183	162	142	105
Untreated		242	228	195	170	113
# matches		1	1	1	1	1

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The covariates from Table 5 apply.

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