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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

The evidence concerning the impact of school feeding programmes on education is mixed. In this paper, I set out to investigate one of the potential reasons behind this disagreement. 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 the extensive and intensive margins of education, i.e., the percentage of children of primary school age who are in school and the percentage of primary school enrollees who have not dropped out. I focus on villages with overlapping characteristics to avoid confounding the impact of school feeding with factors that are specific to treated villages. School feeding has increased the extensive margin of education by 7 percentage points on average, but the impact on the intensive margin is relatively limited. When I distinguish between food-secure and food-insecure areas, not only do I find a larger impact on the extensive margin of schooling in food-insecure areas, but I also uncover a significant increase of 2 percentage points in the intensive margin of education in these same areas. I conclude that school feeding programmes bear an impact on education as long as they also intervene to relax a binding food constraint.

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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). The effectiveness of school feeding programmes appears to be highly dependent on national contexts. In addition, I argue that the impact of school feeding can also exhibit within-country variation. My hypothesis is that programme evaluations can lead to very different conclusions depending on when and where the intervention is observed and evaluated. I postulate that the food security situation at the time *and* place of the evaluation can be one of the reasons behind this discrepancy in the literature.

Thus, I set out in this paper to assess the magnitude and heterogeneity of school feeding impacts on the extensive and intensive margins of education, i.e., enrolment and retention rates. The main 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. A similar hypothesis has been put forward by Singh et al. (2014) 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 that is not the result of households' own assessment. No such investigation has been previously performed for the case of school feeding and educational outcomes. Finally, I complement the main analysis with secondary investigations into the impact of school feeding on age at enrolment, the grade-for-age gap and household food security.

I use Malawi's Third and Fourth Integrated Household Surveys. I complement these surveys with localized information on food insecurity from the Famine Early Warning Systems Network (FEWS-NET). I implement an instrumental variable approach at the individual and household levels 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 as good as random conditional on the inclusion of covariates that inform the targetting of villages. Similar strategies that rely on geographical targeting have been implemented by Ravallion & Wodon (2000), Sparrow (2007) and Islam & Choe (2013). 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.

I control for district and time effects, individual, household and village characteristics, and find that school feeding has chiefly attracted new enrolments. However, its impact on dropouts has been more nuanced. 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, 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 which have similar, overlapping characteristics. I refer to these communities as the *overlap sample*. Buttenheim et al. (2011) also adopt a similar strategy to rid the sample of dissimilar communities.

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 0.5–0.9 percentage points. As for the intensive margin of education, children in food-insecure and treated villages are more likely to remain in school compared to control villages by approx. 1 percentage point. The coefficients of the reduced-form specification are robust to omitted variables (Altonji et al., 2005; Oster, 2019). The magnitude is smaller here than in the aforementioned set of results because this is an intention to treat effect, which estimates the average impact of being exposed to school feeding, as opposed to benefiting from the programme directly. In addition to the individual-level analysis, I aggregate the information and employ propensity score matching at the community level. Results confirm previous findings. If all food-insecure communities had received school feeding, then the extensive margin of schooling would have been 2–3 percentage points greater than in a scenario whereby no community was treated. Lastly, I run a household analysis and find that treated households experienced 0.5 fewer days in which household members have had to skip at least one meal in the seven days prior to the survey interview. The magnitude is increased to 1 fewer days in food-insecure areas.

I conclude that school feeding improves enrolment and retention rates as long as there is a binding food constraint to relax. Increases in enrolment appear to be more sensitive to school feeding than are retention rates. This raises the question 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. School feeding appears to keep children in school during episodes of food insecurity, but does little toward improving enrolment and retention rates during periods of standard food availability. Ultimately, school feeding is a safety net rather than an educational intervention. 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. For instance, there is evidence that school feeding is associated with improved cognitive and nutritional outcomes for children in Grade 1 in Malawi (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 households. The scaling up of school feeding to cover everyone regardless of their socio-economic situation is likely to lead to a reduced, watered-down average impact. Nevertheless, this does not deny the fact that school feeding has the potential to be very effective in certain vulnerable areas or at times when food insecurity is rife, such as in the aftermath of failed harvests. Importantly, if the target is a moving one, e.g., communities oscillate between food security and insecurity, then casting a wider net can ensure preparedness against spells of food insecurity.

The findings of this article contribute to the existing 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 an average intention to treat effect on enrolment rates of 4 percentage points. Moreover, Azomahou et al. (2019) use experimental data from Senegal and find that school meals have had no impact on enrolment rates, but they triggered a reduction of 7 percentage points in dropouts. This reduction nevertheless disappears after accounting for non-random attrition and treatment compliance issues. Furthermore, using experimental data from Uganda, Alderman et al. (2012) find that food-for-education interventions have not impacted mean enrolment rates. However, after restricting the sample to children who were not enrolled at baseline, the authors do find a positive and significant average intention to treat of 9 percentage points. Outside of the African continent, Ravallion & Wodon (2000) and Meng & Ryan (2010) evaluate the impact of a food-for-education programme on school participation in Bangladesh and find a positive impact of 15–21 percentage points. Finally, Tan et al. (1999) and Buttenheim et al. (2011) find no impact on enrolment or dropout rates for the Philippines or Laos. My paper brings all these studies together by identifying one important reason why the same programme can lead to both significant and insignificant results, namely the food security situation at the time and place of the programme evaluation.[1]

2 Motivation

2.1 Context

In Malawi, primary education starts at age 6 and lasts for 8 years. Primary school fees have been abolished since 1994. Country-wide, the net enrolment rate among children aged 6 to 14 years old has increased from 84 percent in 2010 to 90 percent in 2016 against the background of an also increasing population of primary-school-aged children (National Statistical Office, 2017). The number of classrooms, too, has increased, but insufficiently so, such that the average number of primary school students per classroom rose from 105 in 2011 to 116 in 2015 and 121 in 2017 (Ministry of Education, Science and Technology, 2014; UNICEF, 2018, 2019). At least, the availability of qualified teachers has improved. The average number of students per qualified teacher has gone down from 91 in 2010 to 80 in 2015 and 77 in 2017 (Ministry of Education, Science and Technology, 2014; UNICEF, 2018, 2019). These seemingly contrasting trends can be explained by the increasingly common practice of running double shifts, i.e., morning and afternoon classes. Enrolment statistics are encouraging, but there is still room for improvement — especially after seeing how enrolment rates decreased from 90 percent in 2016 to 88 percent in 2017 in the aftermath of the flooding and drought events of 2016 (UNICEF, 2019). Besides enrolment rates, students' low performance is also a pressing issue. However, due to data limitations, I study the quantitative side of education. I do briefly explore the grade-for-age gap, which incorporates information on grade repetition, and thus partially reflects school performance.

Educational outcomes vary along various dimensions. One of the most significant dimensions is the urban-rural divide. The difference between primary school *completion* rates in urban and rural Malawi is of 34 percentage points (Ministry of Education, Science and Technology, 2014). Schools and households in rural and urban areas face different obstacles, and rural areas have more catching up to do. To keep a homogenous group of households, I restrict this study to rural Malawi. Despite the focus on rural populations, not much of the sample is lost, as more than 80 percent of Malawians live in rural areas (National Statistical Office, 2019). Moreover, rural areas are also the most pertinent setting to discussing educational outcomes and school feeding programmes against the background of food insecurity that is brought on by natural causes, such as droughts and floods. Malawi's rural population is overly-reliant on small-scale, rain-fed agriculture; therefore, households are vulnerable to climate variability and change.

[1] This paper also adds to the literature that evaluates the impact of other types of interventions on education in Malawi. Baird et al. (2011) find strong evidence of a positive effect of both conditional and unconditional cash transfers on enrolment and attendance rates, albeit the conditional transfers were significantly more cost-effective. In contrast, Covarrubias et al. (2012) only find weak, positive evidence concerning the impact of a social cash transfer on attendance. Finally, Hazarika & Sarangi (2008) find that the work of children, which is positively related to microcredit access, did not substitute education, but that it was subtracted from children's leisure time. However, Shimamura & Lastarria-Cornhiel (2010) evaluate the same intervention and find that in the case of girls, credit uptake did reduce school attendance.

It does not help that Malawi is prone to floods and droughts or that the frequency and spread of droughts have intensified in the past four decades. The impact of such disasters is also likely to be aggravated by future population growth and continued environmental degradation (Government of Malawi, 2016).

2.2 Benefits and Limitations of School Feeding

School feeding programmes ensure the provision of in-school meals to enrolled children, and they reward regular school attendance. The expected impact of school feeding programmes is threefold. First, they improve attendance and attract out-of-school children. Second, if children suffer from nutritional problems, then school feeding helps by providing a steady, fortified meal. Lastly, the third impact is a by-product of improved attendance and nutrition. It consists of better school performance.

School feeding programmes are not immune to problems. In absence of classroom-building or teacher-hiring campaigns, school infrastructure can become overcrowded, as more children enrol and attend school following the implementation of school feeding. This can reduce the quality of education as well as the performance of children (Kremer & Vermeersch, 2005). In addition, there is also the risk that the time used to serve meals is deducted out of teaching time (Kazianga et al., 2009, 2012). Finally, although there is only limited evidence that this channel applies, households could also reallocate food from the children receiving school meals toward other household members (Chakraborty & Jayaraman, 2019).

Diluted nutritional effects, overcrowded classrooms, insufficient books and reduced teaching time can all explain why the literature finds mixed evidence linking school feeding to educational outcomes, particularly school performance. Other sources of heterogeneity have also been explored: children's baseline nutritional status (Jacoby et al., 1998; Powell et al., 1998; Chakraborty & Jayaraman, 2019), age (Powell et al., 1998; Alderman et al., 2012), gender (Ravallion & Wodon, 2000; Kremer & Vermeersch, 2005; Afridi, 2011; Alderman et al., 2012; Chakraborty & Jayaraman, 2019), school quality (Kremer & Vermeersch, 2005; Alderman et al., 2012), labour constraints (Kazianga et al., 2009), the length of exposure to the programme and socio-economic status (Chakraborty & Jayaraman, 2019). However, all these heterogeneities are not consistently explaining why the impact of school feeding is sometimes limited.

I hypothesize that for school feeding to have an impact in terms of educational outcomes, the programme must be implemented in an area that is facing 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. Children's time might be better employed to run household chores or perform agricultural tasks. Without binding food constraints, school feeding programmes do very little in terms of changing the incentive system. They offer a solution, i.e., complementary meals, to a non-existent problem. In this context, the prevailing local food security situation can explain why the impact of school feeding on enrolment and attendance is sometimes limited.

2.3 The 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. Since the focus of this study falls on enrolment and retention rates, then the classification of school feeding 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 is efficient to only target the most vulnerable of households.

2.4 School Feeding Programmes in Malawi

According to the National Education Sector Plan for 2008–17, the Government's first priority in terms of primary education has been to improve the quality of its services (Ministry of Education, Science and Technology, 2008). Increased and equal access came in second. Among the tools and means listed to achieve its second priority, the Sector Plan noted the role of school feeding. Moreover, the provision of school meals has also been listed in the 2006–10 and 2011–16 Growth and Development Strategies as a key intervention to improve educational outcomes, albeit emphasis on school feeding was significantly stronger in the more recent document (Ministry of Finance and Development Planning, 2005, 2010).

In 2007, the Department of School Health and Nutrition was established to provide guidance on health and nutrition-related interventions, such as the provision of school meals. The Department operates across various ministries and at multiple levels: national, district-level, educational zones and communities. A series of guidelines were issued to support the execution of the School Health and Nutrition Strategy for the period 2009–18 (Government of Malawi, 2009). The Government recommends priority areas and districts based on data that proxies for regional and local vulnerabilities. Then, schools within these districts are tentatively selected, sensitization meetings take place, and the final sample of schools is identified. The Government of Malawi (2009) has put forward criteria to choose beneficiary schools.

These criteria include: the number of classrooms and teachers, the accessibility of schools for the delivery of supplies, enrolment rates, number and quality of latrines, water availability, storage facilities, security and willingness of community members to help with food preparation, serving and cleaning.

Malawi's experience with school feeding programmes pre-dates the Government's efforts to regulate these interventions. School feeding dates back to the late 1990s, when the World Food Programme first implemented a pilot project in the country (Government of Malawi, 2009).[2] They have been a continuous presence ever since. The other important entity implementing school feeding in Malawi is Mary's Meals.[3] 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. Mary's Meals catered to 635 schools in 20 districts as of 2016 (McMahon, 2016). Malawi has 28 districts. The implementing agencies follow the Government-issued guidelines. They consult with the Government to target communities and areas that are vulnerable and lagging behind in terms of education. The mapping of these communities is usually done with the support of the district-level School Health and Nutrition Committees.

3 Data

3.1 Household and Community Data

I use Malawi's Third and Fourth Integrated Household Surveys (2010–11 and 2016–17). These surveys have been implemented by the Government of Malawi through the National Statistical Office with support from the World Bank. They are cross-sectional datasets representative at the national, district, urban and rural levels. The information they collect is from individuals, households and communities. The analysis sample is limited to rural communities and includes all boys and girls that are of primary school age, which is 6 to 14 years old.[4] On average, 16 percent of these children have received school meals at some point during the 12 months prior to the interview taking place, while 34 percent of communities were being targeted by school feeding at the time of the interview.[5] Not everyone from a targeted community

[2] Currently, the World Food Programme has two projects: McGovern Dole and Purchase from Africans for Africa (PAA), which started in 2010 and 2012, respectively. The former is vastly larger than the latter, as PAA is a pilot. Moreover, while the Mc-Govern Dole programme is using imported food, PAA is only using locally sourced food (Webb et al., 2018). The McGovern Dole programme provides one meal a day comprising of 100g of an enriched corn-soya blend, which provides roughly 400 kcal or 22 percent of the daily energy requirements for school-aged children. The meals provided by the PAA project are a mix of cereals, pulses, fruits, vegetables, dairy products and meat. Galloway et al. (2009) estimate that the cost of school feeding per child and per academic year was USD 23 in 2005. A more recent estimation, which is unfortunately limited to only four districts in Malawi, puts the cost per child per year at USD 17 in 2020 (World Food Programme, 2019).

[3] Similarly to the Purchase from Africans for Africa programme, the approach of Mary's Meals is also to source food locally whenever possible (McMahon, 2016). Mary's Meals serves children the same corn-soya blend dish as the Mc-Govern Dole Programme. The difference is that Mary's Meals is prioritizing local suppliers. Mary's Meals operates in several countries, but Malawi is their biggest programme. As of 2019–20, out of a total of 1.6 million beneficiaries globally, 1 million were in Malawi. Mary's Meals reports a global average cost of USD 21 per child and academic year. (https://www.marysmeals.org/who-we-are/news-and-blogs/16-million-and-counting, accessed April 20, 2020.)

[4] I pay attention not to include children who were six at the time of the interview but who turned six after the start of the school year, and who were thus ineligible to start school during the current academic year. I have used the following rule to set the lower bound of the age interval. I have included those born in or before August of 2003 and 2009 if the survey interview took place between April and August of 2010 and 2016, respectively; and those born in or before August of 2004 and 2010 for interviews conducted between September 2010 and April 2011, and between September 2016 and April 2017, respectively. The upper bound was set at 14 years of age.

[5] Summary statistics seem to 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). This is visible in Table 1, as the percentage of treated villages has gone up from 25 to 43 percent. However, there can be a lag between announcing the introduction of school feeding and actually supplying the meals — especially during the trying circumstances of 2016–17. 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

was necessarily treated. Because the concept of a community often overlaps with the boundaries of a village, I will use references to communities and villages interchangeably.

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
At least one parent has at least PSLC	0.259	0.257	0.258	0.438	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						
Worried about food in the past week	0.378	0.675	0.532	0.499	0	1
Nr. days with skipped meals in the past week	0.618	1.803	1.234	2.035	0	7
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
Head has PSLC	0.093	0.097	0.095	0.293	0	1
Head has JCE	0.067	0.079	0.073	0.261	0	1
Head has MSCE	0.036	0.041	0.039	0.193	0	1
Head has post-secondary diploma	0.010	0.015	0.012	0.110	0	1

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. It is likely that only the utmost vulnerable among schools were targeted. This contrasts with the previous approach to implementation, 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 stood at 35.

	2010	2016	Pooled			
Variables	**Mean**	**Mean**	**Mean**	σ	**Min**	**Max**
HOUSEHOLD CHARACTERISTICS (continued)						
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
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						
Perc. in school, extensive margin (aged 6-14)	88.705	91.921	90.374	11.524	9	100
Perc. in school, intensive margin (aged 6-14)	97.442	97.541	97.493	4.973	56	100
Community 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. islam	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
Dist. gov. secondary school (km)	29.28	24.629	26.866	26.438	0	200
Dist. health clinic (km)	7.559	6.200	6.853	8.797	0	90
Dist. 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
Dist. 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
Nr. private primary schools	0.101	0.114	0.108	0.552	0	10
Nr. 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**		

The reference categories are the following: 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, head has no education, 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). The grade-for-age gap is computed such that an increase in the variable denotes a worsening of educational outcomes, as children are in a lower grade than the one expected given their age.

Children are classified as *in school* if they have replied positively to the following 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 given by the grade in which they should have been given their age. For early achievers, the gap is zero (Islam & Choe, 2013). The sample for age at enrolment is limited to current and previous enrollees.

Table 1 presents the summary statistics.[6] 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. However, note 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. Nevertheless, I pool all children regardless of their age, as long as they are between 6 and 14 years old. Thus, summary statistics are underestimating the true dropout rates, or otherwise overestimating the intensive margin of education. For instance, the dropout rate among fourteen-year-olds is 8.1 percent versus 2.3 percent for the sample that includes all age groups. Moreover, among the out-of-school children, 23 percent had dropped out and 77 percent had never enrolled in primary education. Thus, if school feeding is to impact educational outcomes, then it would presumably have more room to improve the extensive margin of schooling, while the magnitude of the impact on the intensive margin of schooling would likely be much smaller. Furthermore, some slight improvement has also been registered in terms of the grade-for-age gap, and the average age at enrolment has gone down and is getting closer to 6. This could mean that children are increasingly enrolling at the recommended age, although there was also a slight increase in the number of children who started school earlier than the recommended age, which can also bring down the average. This tendency of sending five-year-olds to school may be reflective of the coping strategies that households adopt to mitigate constraints.

3.2 Food Security Data

To assess the heterogeneity of the impact of school feeding with respect to the prevailing food security situation, I merge the aforementioned household and community surveys point-to-polygon with information from the Famine Early Warning Systems Network (FEWS-NET). The points are household GPS coordinates and the polygons are the areas defined according to the FEWS-NET food security classification. The time match between survey interviews and the food security information is almost to the yearly quarter. Thus, very relevant. FEWS-NET is a tool of the United States Agency for International Development and it relies on the input of several other organisations. FEWS-NET mainly offers projections of food insecurity to act as early warnings. However, 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

[6] Corrections were necessary to address some outliers. A few outliers were present due to blatant 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. In the case of the former, the data clerk has imputed kilometres where metres must have been the correct metric. Thus, I made the correction, but only after I have checked that the amended numbers are 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 that are not greater than 25. However, these two rural communities reported 350 and 401. I have set them to 3 and 4, respectively. They, too, are in line with the numbers of neighbouring villages. Furthermore, there were some additional outliers in terms of the pupils-to-teacher variable. In this case, the reason behind the outliers was not apparent, and thus I have excluded these observations from the analysis. Very few individuals were dropped due to this reason. Lastly, the community-level population numbers were off for 3 communities. They exceeded the population of the districts to which they belonged. I assume the error was due to the erroneous imputation of extra zeroes. Thus, I cut two zeroes to bring the numbers down from some hundred thousands to thousands of people.

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).[7] The two scales are very similar. Nevertheless, to avoid any misalignment, I proceed by collapsing the five categories of food insecurity (minimal, stressed, crisis, emergency and famine) into two (minimal vs. some stress).

The FEWS-NET classification is based on a convergence of available data and evidence, including indicators related to food consumption, livelihoods, malnutrition and mortality. For instance, minimal stress means that households are able to meet essential food and non-food needs without engaging in atypical and unsustainable strategies to access food and income. 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 only meeting their minimum food needs by depleting or even 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. This insight is corroborated by the survey data: In 2010, 38 percent of households reported to have been worried about food during the week prior to the interview. In 2016, their proportion increased to 68. Moreover, in 2010, households reported that they skipped meals on 0.6 days during the 7 days prior to the interview. In 2016, the average was 1.8 days. Therefore, it is clear that 2016 was generally affected by food insecurity, while 2010 was rather normal, although pockets of food insecurity still existed.

4 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. School feeding programmes in Malawi are no exception. For instance, the agencies implementing school feeding use school-selection criteria that draw heavily on local knowledge. In this case, researchers cannot claim to observe or be able to proxy for these criteria. Implementing agencies know about the willingness of schools and communities to engage with school feeding, and target them accordingly. In turn, a higher willingness to implement school feeding may also be associated with the schools' more general willingness to support students, who will thus perform better. This creates a positive omitted variable bias. Moreover, further positive bias is likely because of the nature of the data and the type of analysis that is implemented. Specifically, 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. Lastly, a negative bias due to omitted variables is also possible if one considers the fact that school feeding targets the most vulnerable of children who are also more likely to have relatively lower educational performance.

Despite the fact that the programme's implementation design is a source of endogeneity, the same design also puts forward the solution. The intervention in Malawi has followed a two-step targeting of beneficiaries. To begin with, 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

[7] https://fews.net/IPC

as well. Thereafter, once these areas are set, the implementing 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 solution. I argue that treatment at the community level can be used as an instrument for treatment at the individual and school levels provided that I am able to control for the observables that determine treatment at the community level, e.g., the variables that the implementing agencies and government departments are likely to consult during the first stage of the programme implementation process. Briefly, this is a selection-on-observables type of problem, and the exogeneity of village treatment relies on my ability to control for the variables that explain both treatment at the village level and the outcomes of interest. Similar strategies that rely on geographical targeting have been implemented by Ravallion & Wodon (2000), Sparrow (2007) and Islam & Choe (2013).

4.1 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 covariates that define community characteristics. Q is a set of district dummies. Finally, T is simply a dummy which equals 1 if the year of data collection is 2016.

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

$$D = \alpha_0 \iota_N + \alpha_1 Z + X\alpha_2 + V\alpha_3 + Q\alpha_4 + \alpha_5 T + \nu \tag{2}$$

The coefficient of interest is γ_1. Equation 3 shows that it represents the ratio between the causal effect of Z on Y and the causal effect of Z on D (Angrist & Pischke, 2008). Where $i = \overline{1...N}$ and N is the number of individual observations. γ_1 identifies the impact of school feeding if several assumptions are met (Wooldridge, 2010; Cerulli, 2015; Angrist & Pischke, 2008; Imbens & Angrist, 1994).

$$\hat{\gamma}_1 = LATE = \frac{cov(Z_i, Y_i \mid X, V, Q, T)}{cov(Z_i, D_i \mid X, V, Q, T)} = \frac{E(Y_i \mid Z_i = 1, X, V, Q, T) - E(Y_i \mid Z_i = 0, X, V, Q, T)}{E(D_i \mid Z_i = 1, X, V, Q, T) - E(D_i \mid Z_i = 0, X, V, Q, T)} \tag{3}$$

The first assumption is *conditional mean independence*. In the context of selection on observables, this assumption argues that treatment at the community level is 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.[8] 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 chiefly treated if their village was also targeted. First-stage regressions are strong and confirm this claim.[9]

[8] In a community-level linear regression of Z on Q, T and aggregated values of X and V, the *F-stat* is 21 and R^2 is 0.36 (not reported). In a non-linear specification, χ^2 is 442 and the pseudo-R^2 is 0.31 (not reported).

[9] See Appendix A.1.

The third assumption is that of *sufficient overlap*. For each village or community v, the condition for identification is that $0 < p(Z_v = 1 \mid X, V, Q, T) < 1$. This is the equivalent of saying that 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*. Regardless of the sample, results tell the same story.[10]

Fourth and last, I assume the *instrument is monotonous*. This means that the sample is assumed only to consist of compliers, i.e., individuals who respond as expected to the instrument being switched on. Defiers are assumed not to exist. If defiers were numerous, then the estimates would be biased downwards and make my hypothesis easier to reject.[11] Following the assumption of monotonicity, the average treatment effect (ATE) is estimated for compliers. As compliers are a sub-sample of the population, this becomes a local average treatment effect (LATE) (Imbens & Angrist, 1994; Angrist & Pischke, 2008).

Lastly, I employ the reduced-form specification in Equation 4. Now, this is purely a selection-on-observables type of problem. Treatment at the community level is as good as random if I control for the factors that inform village treatment.

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

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

In contrast to the two-stage least squares estimator, which identified LATE, β_1 identifies an average intention to treat effect (AIT). 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. Thus, this measure estimates the average impact of school feeding on eligible individuals; that is, the impact of being exposed to school feeding programmes but not necessarily being treated by one.

4.2 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. Technically, the estimation is of an ATE. However, since the treatment variable is targeting by school feeding at the village level, then the estimation will be of an AIT. The effect is the average of the difference between the observed and imputed potential outcomes computed for each community. The treatment effect is identified under the assumptions of conditional mean independence and sufficient overlap (Wooldridge, 2010).

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

[10] The graphs plotting the extent of overlap between communities are in Appendices A.4 and A.6. The overlap is limited in absence of any 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, then a sample with a better overlap will increase my chances of finding the hypothesized positive link between school feeding and educational outcomes. For this reason, the amount of post-correction overlap is acceptable.

[11] Defiers who seek treatment are likely to exist. Chances are their numbers are limited. In resource-constrained environments, it can be difficult to move to other schools to receive school feeding. This is made even more difficult by the Government's policy to target clusters of communities, as opposed to singletons, to limit the migration of children (Government of Malawi, 2009).

The *hat* notation is for imputed values: $\hat{Y}_{i,Z=0}$ must be imputed for treated communities, and so does $\hat{Y}_{i,Z=1}$ for control villages. In turn, 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.

5 Individual-Level Results

5.1 Local Average Treatment Effect

Table 2 suggests that the impact of school feeding has chiefly consisted of attracting out-of-school-children into primary education for the first time. The impact on the extensive margin is positive and significant in the pooled 2010–16 sample. The IV coefficient puts the magnitude of the impact at roughly 5.8 percentage points. However, in the case of the intensive margin of schooling, the impact is not significant. This may suggest that children enrol in school in times of need to benefit from school feeding, but that school feeding is not sufficient to dissuade dropout. The use of the 2SLS estimator, as opposed to the OLS estimator, is motivated by the robust score test. First stage regressions are reported in Appendix A.1.

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 as a consequence of strong El Niño conditions. Erratic rains and prolonged dry spells have greatly affected the 2015–16 agricultural output, such that the Government of Malawi declared a state of disaster in April 2016 (Government of Malawi, 2016). There is a clear difference between children's response in 2010 versus the drought year of 2016. I find no impact of school feeding 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 a 3–4 percentage-point increase in the intensive margin.[12] For 2016, the χ^2 statistic of the robust score test fails to reject the null of exogeneity of school feeding. Thus, the OLS coefficient should take precedence over the IV estimate, as the former is more efficient.[13]

To identify the treatment effect correctly, there must be sufficient overlap between the control and treated communities in terms of their characteristics. Otherwise, I run the risk of confounding the impact of school feeding with that of other unobservable factors that set the treated communities apart from the control communities. Thus, in order to mitigate this risk, I proceed by limiting the sample to similar communities. This is done by excluding the communities for which the propensity score of their treatment status is

[12] I have also explored the heterogeneity of results in terms of gender. There is little difference between boys and girls. School feeding has not had any significant impact on the schooling of either gender in 2010. The drought sample of 2016 does uncover some limited heterogeneity. In the case of both the extensive and intensive margins of schooling, the OLS coefficient is greater for boys than it is for girls: 12.1 vs. 7.4 percent in the case of enrolment rates, and 4 vs. 2.5 percent in the case of retention rates (not reported). Nevertheless, the unconditional probabilities of being enrolled and staying in school do seem to suggest that there is more room for improvement in the case of boys. The estimation results are partly reflective of this situation.

[13] 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 drought and against the background of the amended programme implementation strategy.

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 (0.005)*** [0.008]***	0.058 (0.018)*** [0.026]**	0.159 (0.008)*** [0.016]***	-0.002 (0.024) [0.038]	0.098 (0.007)*** [0.009]***	0.084 (0.033)*** [0.038]**	0.032 (0.003)*** [0.004]***	0.007 (0.009) [0.011]	0.035 (0.005)*** [0.006]***	-0.011 (0.012) [0.014]	0.032 (0.004)*** [0.006]***	0.037 (0.018)** [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 (0.005)*** [0.010]***	0.072 (0.018)*** [0.027]***	0.169 (0.009)*** [0.018]***	0.006 (0.025) [0.036]	0.082 (0.008)*** [0.012]***	0.084 (0.034)** [0.039]**	0.030 (0.003)*** [0.004]***	0.015 (0.009) [0.010]	0.040 (0.006)*** [0.008]***	-0.008 (0.013) [0.015]	0.027 (0.005)*** [0.007]***	0.048 (0.018)*** [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 and presented in square brackets. F, χ^2 and the exogeneity 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. Treatment at village level is regressed on the usual aggregated individual-, household- and village-level variables plus district and time dummies. The extensive margin considers the proportion of all children of primary school age who are in school. The intensive margin of schooling measures the proportion of children of primary school age who are still in school given that they have enrolled in school at some point since they turned 6 years of age. Only the variable of interest has been reported. 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.

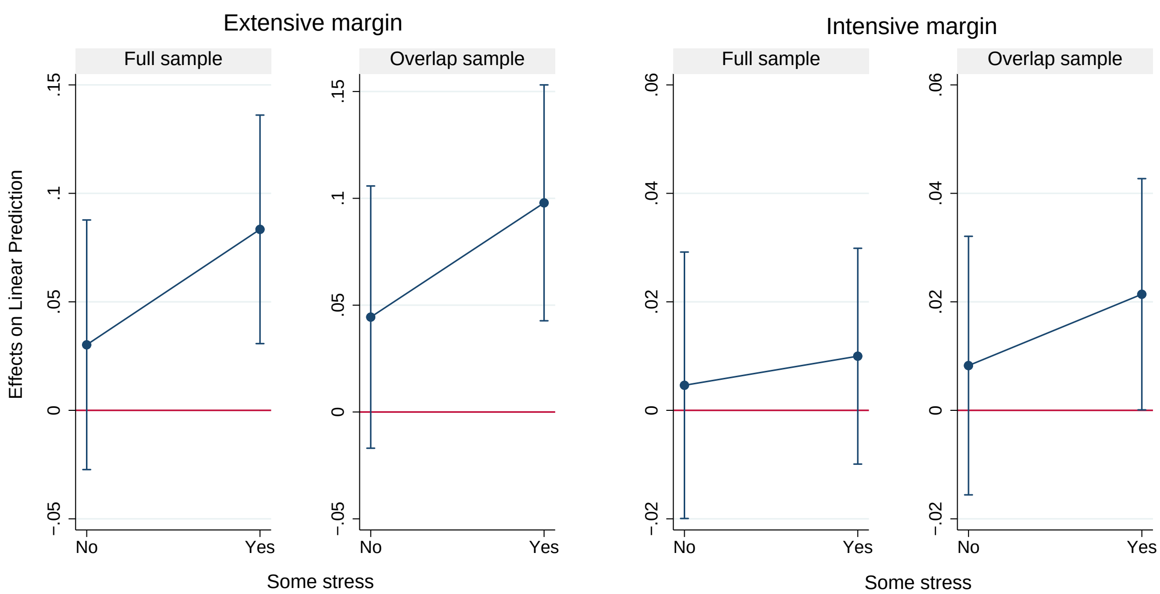
either lower than 0.10 or greater than 0.90. Appendix A.4 shows graphically how this sample restriction improves the amount of overlap between the control and treated villages. I find that previous results are robust to the new sample of communities. Panels A and B of Table 2 lead to the same conclusions. However, the robust score test fails to reject the null of exogeneity for the case of the intensive margin of education 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. Appendix A.2 shows that school feeding does not impact the false dependent variable in any consistent way. Thus, my analysis passes the falsification test.

5.2 Impact Heterogeneity

In this section, I explore another avenue to estimate 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 generalized drought, some areas will, nevertheless, be minimally affected by food insecurity. Figure 1 presents the marginal effects of school feeding on the extensive and intensive margins of schooling given two levels of localized food insecurity: minimal versus some food-related stress or crisis. The plots are the result of the same regressions presented in Table 2, plus one additional explanatory variable: the interaction between treatment by school feeding and the FEWS-NET variable assessing food insecurity. This is done for both the full and the overlap samples, the latter of which is limited to similar communities. First stage regressions are presented in Appendix A.3.

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



Clustered errors are employed. IV estimations on the full and overlap samples. The covariates from Table 2 apply, plus an interaction term between school feeding and the prevailing food security situation. The graph shows the impact of school feeding on enrolment and retention rates given the prevailing food security situation (minimal stress versus some stress). The overlap sample consists of communities that are judged similar, as their treatment propensity scores $\in (0.1, 0.9)$, where propensity scores are predicted based on logit regressions at the community level.

Figure 1 confirms that the impact of school feeding on enrolment rates is heterogeneous in terms of 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.

Regarding the intensive margin of education, the interaction term between school feeding and food insecurity brings new insights. For the overlap sample, Figure 1 points to stronger conclusions in terms of impact heterogeneity. I find that school feeding has impacted the intensive margin of schooling. In food-insecure areas in the overlap sample, the magnitude of the effect is approx. 2 percentage points.

5.3 Average Intention to Treat Effect

To further investigate the robustness of my findings, I estimate a reduced-form specification. I regress individual-level schooling outcomes on treatment at the community level plus the regular covariates. I use the methodology proposed by Altonji et al. (2005) and Oster (2019) to test the robustness of the reduced-form estimations to the bias created by unobservables. The main argument in Altonji et al. (2005) and Oster (2019) is 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. These papers propose a method to recover the relationship between treatment and unobservables based on the relationship between treatment and observables. The intuition is that the more stable a coefficient is when relevant R^2-increasing observables are included, the smaller the potential bias from unobservables will be. 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}). Results are robust if coefficients keep their sign despite various imputed values for δ and R_{max}. Second, one can compute the maximum level of δ such that the β coefficient is brought to zero while R^2 is set to R_{max}. In this case, the researcher identifies how many times over must the importance of unobservables be relative to observables such that the impact of interest becomes zero given R_{max}. Lastly, the method can also help identify the maximum value of R_{max} for which the β coefficient is still positive and δ is of a set value. Oster (2019) recommends $\delta = \pm 1$, which means that the unobservables are as important as the observables, and $R_{max} = 1.3\hat{R}^2$, where $\hat{R}^2$ is the estimated R^2.[14] Finally, it is important to note that since the reduced-form regression estimates an average intention to treat effect, as opposed to an average treatment effect, then the coefficient is already one that is conservative. Thus, the robustness of reduced-form results will be all the more encouraging.

Table 3 shows that the impact of school feeding on the extensive and intensive margins of education is positive and significant, and it 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, whereby 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 schooling. The AIT 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 1 percentage point in food-insecure areas.

[14] The 1.3 value was recommended by Oster (2019) such that 90 percent of the results she replicated from a sample of randomized control trial studies survived the test of robustness to omitted variable bias.

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	0.021	0.007	0.022	0.031	0.006	0.002	0.008	-0.002	0.005	0.010	-0.002
	(0.005)***	(0.008)**	(0.007)	(0.006)***	(0.009)***	(0.008)	(0.003)	(0.004)*	(0.004)	(0.003)	(0.005)**	(0.004)
	[0.007]**	[0.011]*	[0.010]	[0.008]***	[0.011]***	[0.011]	[0.003]	[0.005]*	[0.005]	[0.003]	[0.005]*	[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. Treatment at the community level is regressed on the usual aggregated individual-, household- and village-level variables plus district and time dummies.

5.4 Mechanisms

In this subsection, I briefly explore two other dependent variables to discuss the mechanisms that connect school feeding to improved enrolment and retention rates. First, I look at age at enrolment conditional on enrolment, and second, I explore children's grade-for-age gap. I assume that exposure to treatment has been lengthy, i.e., villages have been treated for a while before they were interviewed at the time of the Integrated Household Surveys. Otherwise, there would be no impact on either age at enrolment or the grade-for-age gap, or I would run the risk of observing a higher average age at enrolment in treated communities, as overaged children enrol in school for the first time following the implementation of school feeding. Lastly, because I am eliciting the impact of school feeding over longer periods of time, then a food security analysis is no longer appropriate since the food security data is punctual not historical.

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

	AGE AT ENROLMENT							
	Children enrolled at 6 or above				Children enrolled at 5 or above			
	Full sample		***Overlap sample***		***Full sample***		***Overlap sample***	
Explanatory variable	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
	(0.018)	(0.050)*	(0.021)	(0.052)*	(0.019)	(0.053)***	(0.021)	(0.054)***
	[0.025]	[0.073]	[0.027]	[0.070]	[0.026]	[0.077]***	[0.029]	[0.073]***
R^2	0.15	0.15	0.13	0.13	0.14	0.14	0.13	0.12
F	22	-	12	-	26	-	16	-
χ^2	-	2122	-	1144	-	2521	-	1452
Robust score test	-	4.02	-	4.81	-	19.92	-	21.04
P-value test	-	0.13	-	0.09	-	0.00	-	0.00
N	15,527	15,527	10,069	10,069	18,549	18,549	11,979	11,979

	GRADE-FOR-AGE GAP							
	All children 6–14				In-school children			
	Full sample		***Overlap sample***		***Full sample***		***Overlap sample***	
Explanatory variable	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Beneficiary school feeding	-0.194	-0.202	-0.215	-0.224	0.020	-0.096	0.012	-0.098
	(0.023)***	(0.073)***	(0.025)***	(0.074)***	(0.021)	(0.062)	(0.023)	(0.062)
	[0.030]***	[0.104]*	[0.034]***	[0.101]**	[0.026]	[0.081]	[0.029]	[0.079]
R^2	0.45	0.45	0.45	0.45	0.47	0.47	0.47	0.47
F	176	-	120	-	180	-	124	-
χ^2	-	16816	-	10988	-	17208	-	11329
Robust score test	-	0.01	-	0.01	-	4.08	-	3.66
P-value test	-	0.99	-	0.99	-	0.13	-	0.16
N	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. 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.

Primary school starts at age 6 in Malawi, but early-age enrolment is not uncommon. There are many students who report having started school earlier than age 6. They might have enrolled in pre-school first and then proceeded to primary education at age 6, or they are, in fact, early primary school enrollees. I have suggestive evidence that the latter explanation applies. For instance, among five-year-olds in rural Malawi at the time of the Integrated Household Surveys, only 4 percent reported being in pre-school, while 28 percent reported being in primary education. The remainder had not yet enrolled in school.

Table 4 suggests that school feeding has reduced the average age at enrolment, and that it has done so by attracting children in school earlier than the Government's advice. 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 are in Appendix A.5. Early enrolment is not necessarily good. Five-year-olds are unlikely to have developed the necessary capacity to absorb information the same way as children aged 6 or 7. Early enrolment can negatively impact the student's education down the line.

As for 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. This argument is based on the comparison between the two sample blocks in Table 4. 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.[15,16] In this case, the IV and OLS estimates are not statistically different from each other. The robust score test also fails to reject the null of exogeneity.

6 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 presents the results for the pooled sample, which includes both food-secure and food-insecure communities. Results point to a positive and significant impact of community-level targeting by school feeding on the extensive margin of schooling. 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 tolerance parameter for the sufficient overlap assumption. Significance is lost in the most stringent of specifications, when only communities with propensity scores between 0.20 and 0.80 are kept. Lastly, retention rates do not seem to respond to school feeding programmes in the pooled sample.[17]

[15] I have also run an analysis whereby I exclude early achievers (not reported). The implications are the same.

[16] The grade-for-age gap is underestimated because the sample includes children of all ages. The youngest will have been exposed to a smaller risk of dropping out of school compared to thirteen- or fourteen-year-olds. Therefore, the evidence can only be considered suggestive of the true relationship between school feeding programmes and children's grade-for-age gap.

[17] I also investigate the link between school feeding, age at enrolment and the grade-for-age gap (not reported). If all communities were targeted, then the average age at enrolment would be roughly one month lower than the average in a scenario with no treated communities. Moreover, the grade-for-age gap also seems to be reduced among the 6-to-14 age group. The magnitude, however, is small. The significance of the treatment variable is also changing across the various specifications. If the community-level grade-for-age average is only for children in school, then there is no significant impact of school feeding on the said grade-for-age gap. Note AIT is smaller compared to LATE. What is more, the relatively smaller sample of villages further impacts the efficiency of estimates. Thus, significance may be harder to achieve.

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 are reported in parenthesis. The dependent variables are community-level averages of the extensive and intensive margins of education. 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 as enumerated in Table 1, 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 ratio of pupils to teachers), as well as district and year dummies.

To explore the heterogeneity of the impact 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 tolerance in terms of the overlap assumption. As for the intensive margin of education, the impact of school feeding in food-insecure communities is consistently positive, but the magnitude is small and treatment is only occasionally significant. Thus, I am abstaining from linking school feeding to retention rates, as the evidence is not strong. Overlap and balance plots are in Appendices A.6 and A.7.

The evidence in this section agrees with the insights from the individual-level analysis to the extent that both put forward the argument that school feeding improves enrolment rates, and that the magnitude of the impact is highest where households are food insecure. Moreover, while the community-level analysis provides weak evidence that school feeding impacts the intensive margin of education, the individual-level analysis does, however, provide supportive evidence in this respect, albeit only for food-insecure areas when the comparison is done among similar communities. The reason for these seemingly different results boils down to the fact that the individual-level analysis estimates a LATE, while the community-level analysis estimates an AIT. In the latter case, the impact is watered down.

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

	AVG. IN SCHOOL, EXTENSIVE MARGIN									
	Minimal stress sample					***Some stress sample***				
	Level of tolerance overlap assumption					Level of tolerance overlap assumption				
Explanatory 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***				
	Level of tolerance overlap assumption					Level of tolerance overlap assumption				
Explanatory 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

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

Lastly, I run a falsification test. The extensive margin of primary schooling is swapped for the extensive margin of secondary schooling. 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 interlinked with that of sending their older siblings to secondary school, or if school feeding would have allowed more children to graduate from primary education and thus be eligible for secondary education. Nevertheless, results do not suggest a clear pattern between school feeding and the falsified outcome. My analysis passes the falsification test. See Appendix A.8.

7 Household-Level Food Security Results

Besides targeting educational outcomes, school feeding programmes also intervene to protect households during times of adversity. Therefore, I am also briefly exploring food security as an outcome. I focus on two indicators: whether the household reported worrying about food during the week prior to the survey interview, and whether the household has had to cut down on the number of meals for at least one member of the family during the same time. For this analysis, I collapse the database at the level of households.

Table 7 shows that school feeding does fulfil its role as a safety net intervention, as households with treated children report a lower rate of worrying about food availability, and they also experience fewer days in which they had to cut down on the number of meals. For instance, treated households are 6 to 8 percentage points less likely to report worrying about food, and they experience 0.5 fewer days whereby at least one household member has had to skip meals. First stage regressions are in Appendix A.9.

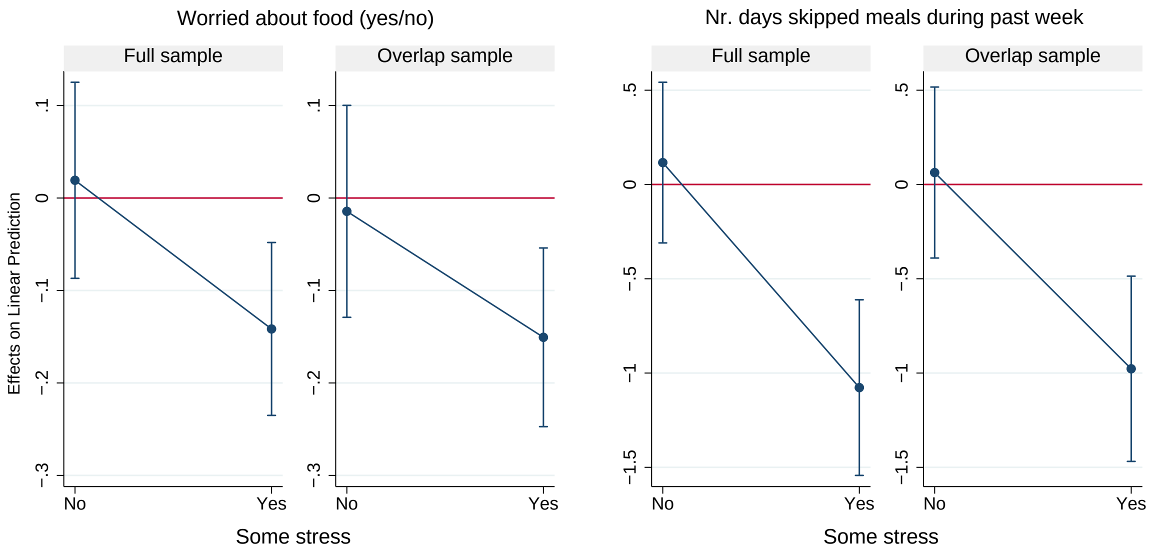
Table 7: Impact of School Feeding on Household Food Security

	WORRIED ABOUT FOOD (past week)				NR. DAYS SKIPPED MEALS (past week)			
	Full sample		***Overlap sample***		***Full sample***		***Overlap sample***	
Explanatory variable	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Beneficiary school feeding	0.011	-0.064	0.012	-0.084	0.047	-0.499	0.072	-0.471
	(0.013)	(0.038)*	(0.014)	(0.038)**	(0.054)	(0.164)***	(0.060)	(0.165)***
	[0.014]	[0.049]	[0.016]	[0.049]*	[0.063]	[0.218]**	[0.071]	[0.220]**
R^2	0.21	0.21	0.21	0.20	0.17	0.16	0.15	0.14
F	48	-	32	-	26	-	17	-
χ^2	-	4181	-	2666	-	2288	-	1410
Robust score test	-	4.33	-	7.20	-	12.40	-	12.27
P-value test	-	0.11	-	0.03	-	0.00	-	0.00
N	10,814	10,814	7,121	7,121	10,814	10,814	7,121	7,121

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are reported in parenthesis. Errors are clustered at the level of communities and presented in square brackets. F, χ^2 and the exogeneity statistics correspond to the specification with robust errors. The covariates from Table 2 apply. However, instead of mother's and father's education, I use the education of the household head. There may be children of different parents in the same household; therefore, the collapse of data at the household level would be problematic. The overlap sample consists of households from communities with treatment propensity scores $\in (0.1, 0.9)$, where propensity scores are predicted based on logit regressions at the village level.

Moreover, Figure 2 shows that if all households lived in food-insecure areas, then treatment would make these households 14 to 15 percentage points less likely to report worrying about food compared to control households. Similarly, they would also experience one fewer days in which they skipped at least one meal. In contrast, school feeding does not have a significant impact on the food security situation of treated households in food-secure areas. This is the same type of insight that the analysis of educational outcomes has put forward. Namely, school feeding programmes register a significant or greater impact as long as there is a binding constraint to relax.

Figure 2: Average Marginal Effects of School Feeding with 90% CIs and Varying Food Insecurity



Clustered errors are used. IV estimations on the full sample. The covariates from Table 2 apply plus the interaction term between school feeding and the FEWS-NET food security variable. Instead of mother's and father's education, I use the education of the household head. The overlap sample is limited to households from similar communities.

8 Conclusion

In this paper, I have shown that the significance and magnitude of the impact of school feeding programmes rely on the prevailing local food security situation. 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. This points to the fact that school feeding is more akin to safety net programmes than to educational interventions. Therefore, to improve educational outcomes, school feeding must be complemented by other programmes.

Importantly, this study does not rely on experimental or pseudo-experimental data. Thus, these results should be interpreted as causal only to the extent that the reader trusts the exogeneity of the instrumental variable, which depends on my capacity to control for certain factors linked to both village treatment and educational outcomes. To mitigate concerns, I run a battery of robustness checks. Results and conclusions hold. Second, the extent of overlap between communities is limited, but I show that results are nevertheless robust to various samples, and I argue that the dissimilarity between communities is creating a negative bias which makes results conservative rather than overly optimistic. Thus, I judge that the risk is of limited consequence. Third, the available data only allow me to study enrolment and retention rates. Therefore, given the policy background, per which the Government of Malawi is chiefly focusing on the quality side of education, my study only informs secondary items on the Government's agenda. Nevertheless, there is still room for improvement in terms of enrolment rates, and dropout rates are still exceedingly high. Thus, this study remains relevant. Moreover, because school feeding programmes are meant to improve attendance and nutrition, they will ultimately impact educational performance as well. Thus, school feeding can also speak to items that are of primary public interest. In fact, it has been shown in the literature that school feeding is associated with improved cognitive and nutritional outcomes for children in early grades. Finally, this paper was not limited to discussing the educational objectives of school feeding, but it has also researched the programme in general, its limitations and strengths in terms of its implementation design.

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Appendix

A.1 First Stage Regressions

	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 Falsification Test, Individual-Level Analysis

	AT LEAST ONE PARENT HAS AT LEAST PRIMARY EDUCATION											
	Pooled sample				2010 typical year				2016 drought year			
Explanatory	***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
Beneficiary	0.000	-0.001	0.000	0.017	-0.012	-0.038	0.007	0.014	0.011	0.061	0.023	0.111
school feeding	(0.009)	(0.027)	(0.010)	(0.027)	(0.013)	(0.033)	(0.015)	(0.035)	(0.012)	(0.052)	(0.015)	(0.054)**
	[0.013]	[0.042]	[0.014]	[0.040]	[0.018]	[0.048]	[0.018]	[0.047]	[0.018]	[0.081]	[0.022]	[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
Rob. score test	-	0.00	-	0.48	-	0.75	-	0.04	-	1.01	-	2.97
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

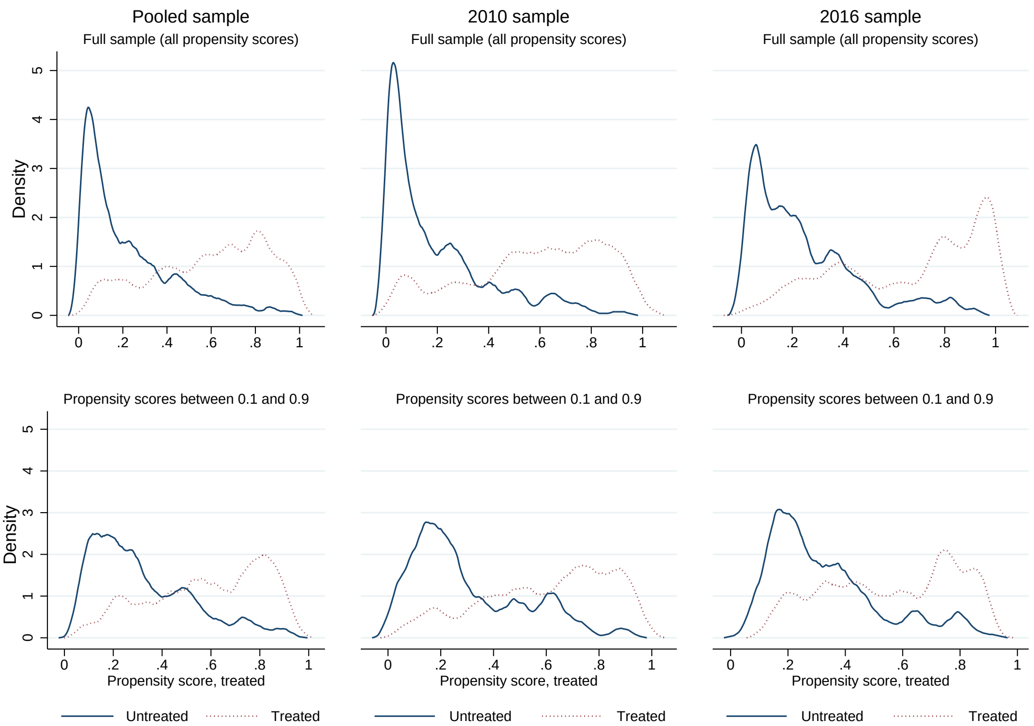
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is a dummy variable. F, χ^2 and the exogeneity statistics correspond to the specification with robust errors. The covariates from Table 2 apply, except for the education of parents, which is now the explained variable. The overlap sample 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.

A.3 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 insecurity.

A.4 Sufficient Overlap Assumption, Wave Samples



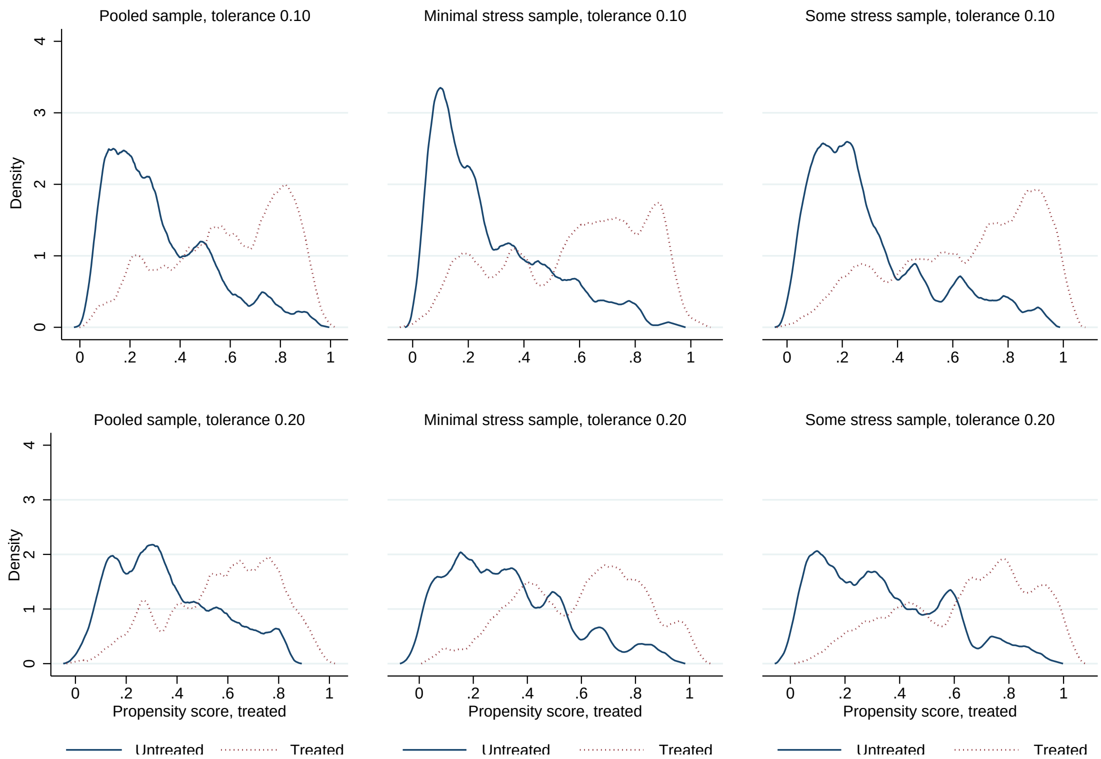
The figure plots the overlap between treated and untreated communities based on covariates. Propensity scores are predicted based on logit regressions at the community level. Treatment at the community level is regressed on the usual aggregated individual-, household- and village-level variables plus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

A.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

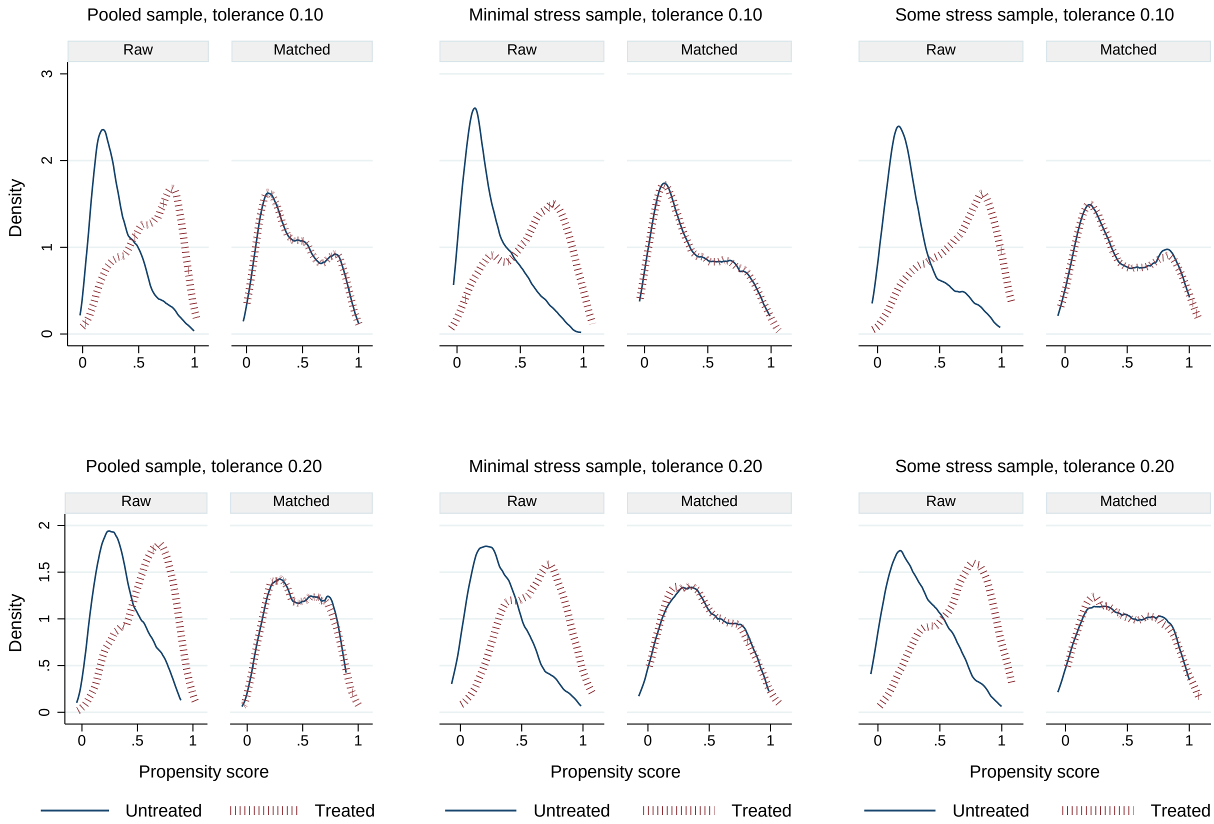
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The covariates from Table 4 apply.

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. Also see Appendix A.4.

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
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
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
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.

A.9 First Stage Regressions, Household-Level Analysis

	BENEFICIARY SCHOOL FEEDING				INTERACTION	
	No interaction		Interaction model		Interaction model	
Explanatory variable	***Full sample***	***Overlap sample***	***Full sample***	***Overlap sample***	***Full sample***	***Overlap sample***
Community targeted	0.292	0.313	0.274	0.287	-0.027	-0.026
by school feeding	(0.010)***	(0.010)***	(0.013)***	(0.015)***	(0.004)***	(0.004)***
	[0.018]***	[0.019]***	[0.024]***	[0.027]***	[0.008]***	[0.009]***
Community targeted			0.038	0.052	0.369	0.371
× Some stress			(0.018)**	(0.020)***	(0.013)***	(0.014)***
			[0.031]	[0.035]	[0.022]***	[0.025]***
R^2	0.32	0.29	0.32	0.30	0.43	0.38
F	44	35	44	35	19	15
N	10,814	7,121	10,814	7,121	10,814	7,121

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The covariates from Table 7 apply.