AIDDATA
A Research Lab at William & Mary

WORKING PAPER 123

February 2023

Break it down: A disaggregated analysis of the effects of aid on stunting

Dick Durevall
Department of Economics, University of Gothenburg

Ann-Sofie Isaksson
Department of Economics, University of Gothenburg
The Institute for Futures Studies

Abstract

Motivated by a recent setback in the fight against child malnutrition, this study explores whether aid projects help to reduce stunting, or impaired growth, among children close to project sites. Focusing on Malawi, a country with very high stunting prevalence and for which we have access to geo-referenced data on aid projects from a broad range of donors, we geographically match spatial data on 778 aid project sites of 22 different donors with anthropometric and background data on 26,604 children under the age of 5. The detailed data allows for disaggregated analysis comparing aid impacts across sectors, donors, and locations. To identify the effect of aid, we rely on spatial and temporal variation in aid project coverage and survey rollout, coupled with variation in the child year of birth in relation to project start. The empirical results consistently indicate a positive impact of early-life aid exposure on child growth. The positive treatment effect, observed for children born 0-3 years after project start, is seemingly driven by multilateral aid and projects focusing on rural development, infrastructure, vulnerability and education.

JEL classifications: F35, I15, O12, O15

Keywords:, child health, foreign aid, malnutrition, Malawi, stunting

Author Information

Dick Durevall
Department of Economics, University of Gothenburg

Ann-Sofie Isaksson
Department of Economics, University of Gothenburg
The Institute for Futures Studies

1. Introduction

This study explores the capacity of foreign aid to protect children from the devastating consequences of stunting, or impaired growth. Stunting refers to being too short for one's age due to chronic malnutrition and repeated infections during early life. Stunted children are unlikely to reach their potential height and their brains are unlikely to reach their full capacity. They therefore perform less well in school and fall sick more often than other children do. As adults, they tend to display lower levels of human capital and productive capacity, and more often suffer from chronic diseases (WHO, 2015; UNICEF/WHO/World Bank Group, 2021). Stunting is thus a strong marker of general child health and child development, with important long-term consequences for the individual, and, in countries with high prevalence, for society at large.

Globally, close to 150 million children under age 5 suffered from stunting in 2020. This represented a 27 percent reduction compared with the year 2000 (UNICEF/WHO/World Bank Group, 2021). However, these figures are from before the Covid-19 pandemic and the Russian invasion of Ukraine, both of which have resulted in soaring energy prices and global shortages of grain and fertilizer. In addition, weather related shocks due to global warming increasingly disrupt food production. Thus, after decades of progress in fighting malnutrition and food insecurity, manifested by the "Zero Hunger" sustainable development goal (SDG2) in Agenda 2030 (UN, 2015), there has been a serious setback. Against this backdrop, the UN now warns of a global food crisis that could last for years and "tip tens of millions of people over the edge into food insecurity" (UN, 2022), and headlines speak of "the coming food catastrophe" (The Economist, 2022).

In view of these developments, investigating the capacity of foreign aid to prevent stunting in poor countries is key. This study focuses on Malawi, which is the country with the most complete geo-referenced record of aid projects from a broad range of donors, allowing for detailed and disaggregated analysis of local aid flows. Malawi is also one of the countries in the world with the highest prevalence of stunting (more on this in Section 3).

First, we ask whether aid projects, defined in a broad sense, help to reduce stunting in the local area. Next, we disaggregate the overall aid treatment and investigate when the potential treatment effects kick in and for how long they last, and how they vary depending on treatment intensity, type of project, and donor. Finally, we evaluate the robustness and heterogeneity of the estimated effects and explore possible mechanisms underlying the results.

To address these questions, we geographically match spatial data on aid projects in Malawi spanning the period 1998–2016, with respondents from four waves of the Demographic and Health Survey (DHS), conducted 2000–2016. Our benchmark sample consists of 26,604 children under the age of 5, matched with 778 aid project sites of 22 different donors.

Drawing on the anthropometric DHS data, we compute sex- and age-standardized height-for-age z-scores (HAZ scores), giving the number of standard deviations (SD) by which the observed child's height-for-age differs from the mean of a child of the same age in a reference population. We use three outcome variables: 1) stunting (HAZ<=-2), 2) extreme stunting (HAZ<=-3), and 3) the continuous HAZ score. The latter is important since studies have shown that associations between growth faltering and risk of death or poor cognitive outcomes exist along a HAZ continuum, without a notable inflection point at –2 SD (Perumal et al., 2018). Furthermore, overdispersion, or measurement error, would render comparisons of stunting rates based on specific cutoffs less reliable (Ghosh et al., 2020).

We want our main explanatory variable to capture aid exposure, or treatment, during a critical period during the child's early life. In the benchmark specification, we classify children as treated if they were born the same year as, or up to 3 years after, the start of a project located within 10 km of the survey cluster. In further estimations, we break down our treatment variable into multiple indicators depending on the number of years from project start to the birth of the child, the number of projects meeting the treatment criterion within the cutoff distance, the sectoral division of projects, and, finally, the donor in focus.

To identify the effect of aid, we rely on spatial and temporal variation in aid project coverage and survey rollout, coupled with variation in the year of birth of the child in relation to project start. In the main analysis, we start from the full sample, and then narrow down the control group in steps to ensure comparability with our treatment group. In the full sample estimations, we compare treated and untreated children within districts and within 55x55 km grid cells. In a next step, we restrict the sample to include only "ever-treated" clusters, consisting of survey clusters with a past, present, or future aid project within 10 km at the time of the survey, thus comparing only children living in areas that donors and the government have, at some point, deemed suitable for aid project localization. Finally, we restrict the sample to children born 0–3 years after project start (treated) and children born 2–4 years prior to project start (untreated) in ever-treated clusters.

In additional estimations we rely on sub-samples with variation in treatment status within clusters and across siblings within households, including cluster-by-year fixed effects and mother fixed effects, respectively. Furthermore, to ensure that treated and un-treated children are balanced on key covariates, we use coarsened exact matching (CEM) and run estimations based on a matched sample.

The empirical results consistently indicate a positive impact of early life aid exposure on child growth. The more we narrow down the comparison group to account for unobserved variation across time and space, the more pronounced the estimated treatment effect generally becomes. Treated children are around 2 percentage points less likely to be stunted in the least restrictive specification, compared with around 6 percentage points less likely to be stunted (4 in the case of severe stunting) in the most restrictive specification. Considering the continuous HAZ score, the corresponding effect sizes range from around 4 to 16 percent of a standard deviation. While no complete game changer, these effects are clearly not negligible.

As expected, there is significant treatment effect heterogeneity. First, we note that the positive treatment effects of aid projects on child growth materialize already for children born in the early project implementation phase, but do not remain for children born 4–5 years after project start. For children born within the treatment window, however, aid may help to protect against irreversible consequences of stunting that would otherwise have lasted a lifetime. With respect to treatment intensity, the results suggest no simple linear effect of the number of projects on our outcome variables of interest, but they nonetheless indicate that living near three or four projects fitting the treatment criteria has a stronger effect than living near one project. In terms of sectoral focus, we observe positive treatment effects for projects in the areas of rural development, infrastructure, vulnerability, and education, but somewhat surprisingly not for projects focusing on health, agriculture, and water and sanitation projects. Considering donor heterogeneity in the results, the treatment effects seem to be driven primarily by multilateral aid. This even though the bilateral donors – based on a key word search in the project activity descriptions provided by AidData – to a greater extent focus on more proximate determinants of stunting.

Our study contributes to the literature on the relationship between aid and health outcomes. To our knowledge, it is the first to use broad-based geocoded multi-donor aid data allowing for disaggregated analysis of the local effects of aid on impaired child growth in Africa.

Several earlier studies analyze the relationship between aid and various health outcomes (e.g., infant mortality, maternal mortality, life expectancy) at the country level. Some report a positive effect of aid (Arndt et al., 2015; Chauvet et al., 2013; Feeney and Ouattara, 2013; Gormanee et al., 2005; Gyimah-Brempong, 2015; Mishra and Newhouse, 2009; Pickbourn and Ndikumana, 2019; Taylor et al., 2013; Yogo and Mallaye, 2015), some find no relationship (Kizhakethalackal et al., 2013; Kosack and Tobin, 2006; Mukherjee and Kizhakkethalackal, 2013; Williamson, 2008; Wilson, 2011), and yet others find that the relationship depends on policy environment (Farag et al., 2013; Fielding, 2011). Two recent country-level studies estimate the impact of nutrition-related aid and agricultural aid on stunting. Khalid et al. (2019) find that interventions addressing immediate determinants of fetal and child nutrition reduce stunting, whereas no such treatment effects are observed for interventions influencing the underlying determinants of nutrition (such as water, sanitation and schooling). Mary et al. (2020) find moderate treatment effects of agricultural aid and larger effects of food aid.

While useful for uncovering broad patterns, the macro literature on aid effectiveness faces important challenges. First, it is difficult to establish causality. Receiving aid is associated with a multitude of country characteristics – known and unknown – that will tend to influence the estimates when seeking to establish the causal impact of aid (see, e.g., Bräutigam and Knack, 2004). Second, it is common to aggregate over aid flows that are provided for different purposes and thus should have different effects (see the discussion in Clemens et al., 2012 and Bourguignon and Gunning, 2016). Furthermore, the cross-country literature is not able to account for heterogeneity within countries. Many development projects target local development, arguably suggesting that they should be judged against location-specific outcomes (Findley et al., 2011). While (specific forms of) aid may have effects in targeted areas, these effects may not be sufficiently large to be measurable at country level or they may be obscured by omitted variable bias (Dreher and Lohmann, 2015). Against this background, we arguably need a finer lens when studying the effect of aid on child health outcomes.

At the micro level, there is a large literature evaluating the impact of specific interventions in a broad range of different areas on the nutritional status of children, with mixed findings. These include projects on nutritional supplements, feeding and/or behavioral change (e.g., Attanasio et al., 2014; Das et al., 2019; Attanasio et al., 2022a), conditional cash transfers (e.g., Cahyadi et al., 2020), nutrition-sensitive agriculture (for a review see Sharma et al., 2021) and antenatal care, water and sanitation and prevention and treatment of infectious diseases (for a recent overview see Vaivada et al., 2022).

Unlike impact evaluations, which focus on establishing the causal impact of specific interventions, we investigate the average impact of broad-based aid and aid broken down by sector and donor type. As illustrated by the so called ‘micro-macro paradox’ (Mosley, 1987), impacts of individual projects do not necessarily hold at a more aggregate level because of expenditure switching within the public sector, indirect effects on the private sector, or binding constraints (Rodrik, 2010). Sub-national analysis of geocoded aid and outcome data provides an intermediate perspective that can help bridge the micro-macro divide. Specifically, rather than estimating country-wide impacts of total aid, or analyzing the impact of specific interventions, it enables us to systematically estimate whether a multitude of aid projects have effects in the targeted areas on average, as well as to break down the analysis by donors and type of projects.

Our study contributes to the emerging literature evaluating sub-national effects of aid using geocoded aid and outcome data (e.g., Brazys et al., 2017; Civelli, et al., 2018; Isaksson and Kotsadam, 2018a,b; Dreher et al., 2019; Isaksson 2020; Isaksson and Durevall 2022). A few studies in this strand of literature focus on health outcomes. Odokonyero et al. (2018) find that health aid reduces the number of reported sick days of people living close to aid projects in Uganda. Two studies focus on Malawi. De and Becker (2015) find that health aid reduces workdays lost to illness and that water aid reduces the incidence of diarrhea. Marty et al. (2017) find that aid focusing on health infrastructure and parasitic disease control reduces malaria prevalence and improves self-reported healthcare quality. The above studies have in common that they primarily focus on health outcomes among adults.

To date, the literature evaluating sub-national effects of aid using geocoded aid and outcome data has seen relatively few attempts to explore the effects of aid on child health. Three papers find that aid helps to reduce infant mortality in the local area (Kotsadam et al., 2018, focusing on foreign aid to Nigeria; Wayoro and Ndikumana, 2020, focusing on World Bank aid to the Ivory Coast; and Widmer and Zurlinden, 2021, focusing on World Bank aid in a multi-country African sample). Rustad et al. (2020) study wasting, i.e., children being too thin for their height. Their results, based on a sample consisting of respondents from 16 African countries, suggest that aid helps reduce weight loss due to drought, but has little effect during normal meteorological conditions. Although both wasting and stunting are forms of malnutrition, they capture different conditions. Wasting is a marker of acute undernutrition, often indicating recent and severe weight loss. Stunting, on the other hand, captures linear growth faltering resulting from chronic or recurrent undernutrition due to inadequate dietary intakes and disease-related nutrient loss (Wright et al., 2021).[1] Hence, unlike Rustad et al., who consider whether aid helps to mitigate acute weight loss due to shocks, we investigate whether aid can help prevent the largely irreversible consequences of impaired child growth due to prolonged poor dietary and health conditions.

2. Stunting and aid: determinants and pathways to impact

Several studies have attempted to determine the drivers of stunting (see Vaivada et al., 2020, 2022; and Victora et al., 2021 for recent reviews of the literature). The fact that proximal determinants, such as maternal nutrition, postnatal diet, disease, and breastfeeding, might be affected by more distal factors, such as the socioeconomic status of the household, as well as by intermediate determinants such as access to clean water, makes the relative importance of variables difficult to ascertain. Some factors, however, undeniably stand out as important.

To begin with, there is ample evidence that the incidence of stunting (i.e., the rate at which new cases occur) varies with the age of the child (Magagula et al., 2021; Victora et al., 2021: Wright et al., 2021). The first 1,000 days of life, referring to the time spanning roughly between conception and the child's second birthday, are generally viewed as especially critical (UNICEF, 2013; WHO, 2015).

A first takeaway regarding the potential of aid to reduce stunting is thus the importance of reaching children early. This point is in line with recent literature emphasizing the importance of early childhood conditions and interventions for later human capital development more generally (Attanasio et al., 2022b). Judging from the 1000-day critical window from conception discussed above, pre-natal interventions and interventions reaching children in their first two years of life are likely to have a stronger impact than those targeting older children.

There is also strong evidence that stunting increases with: the mother having a low BMI (<18.5), teenage motherhood, a small birth size (< 2,500 grams), inappropriate complementary feeding, and diarrhea and other diseases. Likewise, there is evidence that stunting decreases with: appropriate antenatal care, exclusive breastfeeding during the first 6 months, birth spacing, use of malaria bed nets, the caregiver's educational attainment, household wealth, and access

[1] Indeed, recent evidence suggests that most stunted children have never suffered wasting, and thus that the two conditions may have different causes (Wright et al., 2021). In line with this, Ngwira et al. (2017) find no association between wasting and stunting among Malawian children surveyed in 2010.

to piped water and improved sanitation (Vaivada et al., 2020, 2022; Victora et al., 2021). Although nutrition is obviously important, the results of studies focusing on the impact of nutritional interventions are mixed (Attanasio et al., 2014; Khalid, Gill and Fox, 2019; Christian et al., 2020; Hurley et al., 2021; Vaivada et al., 2022).

With respect to pathways to impact, aid could potentially reduce stunting via specific interventions targeted directly at its proximal determinants, such as nutrition and diarrhea. As noted, however, proximal drivers of stunting are affected by more distal factors, such as the socioeconomic status of the household. This implies that more general forms of aid, affecting the living standard of households, may well be equally important.

Moreover, the mixed findings suggest that improving one factor, such as nutrition, might not affect stunting if other factors relating to common health risks are not addressed (Brown et al., 2019; Prado et al., 2019). For instance, a recent study found that improved early-life nutrition interventions affected stunting when the mother had appropriate antenatal care, but that the impact did not last after three years of age unless the child was participating in a nutrition enhancement program (Mwale, et al., 2022).

Considering the many determinants of stunting, and that interactions between proximate and distal determinants imply a multitude of possible causal pathways, we take an agnostic stance a priori and start by investigating local effects of aid broadly defined. We then disaggregate the aid flows and consider aid by sector and donor, as well as by the number of projects satisfying the treatment criterion, and when the child was born in relation to project start. Finally, we explore robustness, heterogeneity, and possible mechanisms.

3. Stunting in Malawi

Malawi is one of the poorest countries in the world, and with an estimated 35 percent of children under 5 suffering from stunting (UNICEF, 2022), it is also one of the world's countries most hard hit by child malnutrition. Yet, the last couple of decades have seen large improvements, with prevalence falling rapidly from over 60 percent in the late 1990s to 37 percent in 2015 (UNICEF 2022), after which progress has slowed. While a downward trend in stunting prevalence is visible in several African countries, the reasons for the decline are not all clear. Using data from Malawi, Chilinda et al. (2021), Kumchulesi (2021), and Magagula et al. (2021) find that improvements in household wealth and mother's education seem to be key factors, but several intermediate and proximate factors also matter.

The government of Malawi has initiated several policies to improve nutrition, but there is only limited evidence that these have worked (Malawi Government, 2006; Ruel-Bergeron et al., 2019). For instance, a large-scale community-based nutrition program was launched in 2014, The Right Foods at the Right Time (SUN, 2022). Christian et al. (2020) provide some evidence that the program reduced wasting, but find no effect on stunting, while Hurley et al. (2021), who focus on a sub-sample for a longer period, found a positive effect among children 6-23 months of age. The government also implemented a farm input subsidy program between 2005 and 2020, providing fertilizers and seeds cheaply to about 1,5 million small holder farmers. The main aim was to increase agricultural production. The program increased diet diversity (Matita et al., 2022), but had no direct effect on height-for-age (Mwale et al., 2022).

The aggregate numbers on stunting in Malawi hide substantial heterogeneity. As may be expected, there are large differences across socioeconomic groups: in 2015–16, 46 percent of children in households in the lowest wealth quintile were stunted, compared with 24 percent in the highest wealth quintile (DHS 2015–16). Furthermore, there is substantial regional variation, with prevalence being considerably higher in rural (39 percent) than in urban (25 percent) areas. The differences are even larger at the local level, with some rural districts having a prevalence of over 50 percent (Christian et al., 2020).

4. Data and empirical strategy

We geographically match spatial data on aid projects spanning the period 1998–2016,[2] with respondents from four DHSs (2000, 2004–5, 2010, 2015–16). The aid project database is from AidData and includes information from 30 donor agencies, representing about 80 percent of the total external assistance to Malawi reported to the government 2000–2011 (Peratsakis et al., 2012). It contains latitude and longitude project coordinates, and information about the precision of the location identified (AidData Research and Evaluation Unit, 2017). Restricting the sample to projects with recorded locations coded as corresponding to an exact location or as "near an

[2] Referring to the earliest recorded start year and the latest recorded end year among the projects in our sample. The dataset is from 2012, so end dates after that refer to planned date of completion.

exact location",[3] and with information on project start date, we cover projects from 22 different donors, spread across 778 aid project sites (Table A1 in Appendix).

We use the coordinates of the DHS clusters[4] to match surveyed children to aid project sites. Specifically, we measure the distance from the cluster center points to the aid project sites and identify the clusters located within a cutoff distance – in the benchmark setup 10 km – of at least one project site. Figure 1 maps the aid projects and the DHS clusters across the country.

The DHS children's data focuses on the children of interviewed women, born in the five years preceding the survey. Our benchmark sample thus consists of 26,604 children under the age of five (0–59 months). The DHS data contains a wealth of information related to the child's prenatal and postnatal care, immunizations, and health, as well as information on their mothers. Most relevant for our purposes, it contains detailed anthropometric, i.e., height and weight, data. This allows us to compute sex- and age-standardized height-for-age z-scores (HAZ scores), giving the number of standard deviations by which the observed child's height-for-age differs from the mean in a reference population, here the WHO Child Growth Standards.[5] The HAZ score enables comparison of an individual child with a growth standard derived from a healthy population living under optimal growth conditions (for variable definitions and summary statistics, see Tables A2 and A3 in the Appendix).

Our measure of stunting is a dummy variable indicating whether the child has a height-for-age that is more than two standard deviations below the mean of the reference population (HAZ<=-2). To get a sense of the variation among the most vulnerable children, we also consider a measure of extreme stunting, defined as having a height-for-age that is more than three standard deviations below the reference mean (HAZ<=-3). Finally, to capture variation along the full child growth spectrum as opposed to merely around the above cutoffs, we also consider the continuous HAZ score. This is important, considering that any specific cutoff point, while facilitating comparisons, will inevitably be arbitrary. In practice, the risk of undesirable

[3] Precision categories 1 and 2 in Strandow et al. (2011).
[4] The primary sampling unit in the DHS (usually based on census enumeration areas), often villages in rural areas or city blocks in urban areas (Burgert et al., 2013). In order to ensure respondent confidentiality, the DHS randomly displaces the GPS positions for all surveys, so that urban clusters contain a minimum of 0 and a maximum of 2 kilometers of error and rural clusters contain a minimum of 0 and a maximum of 5 kilometers of positional error, with a further 1% of the rural clusters displaced by a minimum of 0 and a maximum of 10 kilometers (DHS, 2023). This should attenuate but not bias our estimates.
[5] We use the Stata function Zanthro (Vidmar et al., 2004 and 2013) to compute HAZ scores, based on the WHO Child Growth Standards. The WHO Child Growth Standards are in turn based on the WHO Multicentre Growth Reference Study (MGRS), which collected primary growth data from approximately 8500 children from widely different ethnic backgrounds and cultural settings (Brazil, Ghana, India, Norway, Oman and the USA) between 1997 and 2003. The study was designed to provide data describing how children should grow, i.e., prescriptive standards for normal growth, as opposed to simply descriptive references, and thus included certain recommended health behaviors (breastfeeding, adherence to MGRS feeding recommendations, absence of maternal smoking etc.) as selection criteria. For more information about the MGRS, see de Onis et al. (2004).

outcomes for the child does not change drastically when crossing the cutoff point. There are not two distinct populations – one well-nourished and the other malnourished. On the contrary, it has been shown that associations between linear growth faltering and risk of death or poor cognitive outcomes exist along a HAZ continuum, without a notable inflection point at –2 SD (see Perumal et al. (2018) for a review of the evidence).

Trying different outcome measures is also useful for reliability purposes. In the reference population, the HAZ score is by construction normally distributed, with a mean of zero and a standard deviation of one. In our sample, the HAZ score variable has a mean of -1.76 and a standard deviation of 1.6 (see Table A3). In a developing country context, a lower mean and a higher standard deviation is to be expected due to widespread poverty and socio-economic inequalities. A high standard deviation could, however, reflect overdispersion, or measurement error, making country comparisons of stunting rates based on specific cutoffs less reliable (Ghosh et al., 2020).[6] While the focus in this paper is on within-country variation, it nonetheless seems reasonable to assess the sensitivity of results to using various stunting thresholds as well as to measuring the deficit in height-for-age along a continuous scale.

We intend our main explanatory variable to capture aid exposure, or treatment, during a critical period in the child's early life. In the benchmark specification, we classify children as treated if they were born in the same year or up to 3 years after the start of a project located within 10 km of the survey cluster, which applies to 26 percent of our sample (see Table A3). Thus, we classify the remaining 74 percent – children born before a project started, 4 or more years after a project started, or in an area without a project – as untreated. How to define treatment is not obvious, however, and involves making assumptions about effect onset and duration as well as the geographical reach of the potential effect. With respect to the former, stunting often begins in utero. As mentioned, the first 1,000 days of life – the time spanning roughly between conception and the child's second birthday – are generally viewed as especially critical (UNICEF, 2013; WHO, 2015). Although accelerated growth can take place at later stages in life, this time span has been identified as the most crucial window of opportunity for interventions (Georgiadis and Penny, 2017). Against this background, we ideally want to define treatment as aid exposure from conception until age 2. This, however, requires information about when the benefits of a project set in; the recorded project start dates capture the start of project implementation, not the actual start of service or infrastructure delivery. Moreover, the relevant time lag from the start of project implementation to when project benefits

[6] To capture extreme data entry errors, any HAZ scores with absolute values equal to or greater than 5 (that is, 5 standard deviations or more away from the mean) are by default set to missing.

reach children and pregnant women, as well as for how long project benefits last, is likely to differ considerably across different types of projects.

Considering these unknowns, we take care to evaluate effect onset and duration (see Section 5.2). Thus, in addition to estimations using our benchmark treatment dummy for being born 0–3 years after project start, we run estimations where we include separate dummies for being born in the same year as project start, as well as 1, 2, 3, 4, and 5 years thereafter, respectively.

Similarly, we are admittedly agnostic when it comes to the geographical reach of aid project benefits. These should vary considerably depending on the type of aid project. The appropriate cutoff distance from a project – within which we classify a child as treated – is a trade-off between noise and size of the treatment group. With a too short cutoff distance, we get a small sample of children linked to project sites. In contrast, a too large cutoff distance would include too many untreated children in the treatment group, leading to attenuation bias. We treat this too as an empirical question and test distances between 5 and 50 km (see Section 5.6). Due to the density of aid projects in Malawi, implying that nearly all surveyed children were exposed to (a multitude of) ongoing aid projects within the cutoff distance when using wider cutoffs, we are not able to explore distances beyond that.[7]

In the benchmark setup, our treatment variable captures exposure to any aid project starting 0–3 years before the child was born. In other estimations, we break down our treatment dummy into multiple indicators depending on 1) the number of years from project start to the birth of the child, 2) the number of projects satisfying the treatment criterion within the cutoff distance, 3) the sectoral division of projects, and 4) the donor in focus. To explore what drives heterogeneity across sectors and donors and identify projects targeting more proximal determinants of stunting, we conduct a key word search in the project activity descriptions provided by AidData.

4.1 Identification

Just like the distribution of aid across countries, the distribution of aid within countries is not random, implying that some individuals and sub-national areas, with certain characteristics, will be more likely than others to receive aid. In the best of worlds, donors may allocate aid to

[7] Sixty-seven percent of the surveyed children live within 50 km of a project that started 0–3 years prior to their birth, and 92 percent are exposed to an ongoing project within the same cutoff distance at the time of the survey. If considering, say, a 75 km cutoff distance, the corresponding figures are 69 and 98 percent, respectively.

subnational areas in particular need, i.e., with higher malnutrition and worse health conditions to begin with, in which case our estimated aid parameter would be biased downward. Another possibility is that donors allocate aid to areas that are easier to reach in terms of pre-existing infrastructures. If children have more favorable living conditions in these areas, this will bias our estimated aid parameter upward. In sum, we need to account for selection.

To identify the effect of aid, we rely on spatial and temporal variation in aid project coverage and survey rollout, coupled with variation in birth years in relation to project start. We start from the full sample and then narrow down the control group in steps to ensure comparability with our treatment group. Our benchmark specification takes the form:

$$Y_{ivt} = \beta Child\ Treated_{ivt} + +\alpha_{at} + \gamma \cdot \boldsymbol{X}_{ivt} + \varepsilon_{ivt} \qquad (1)$$

where the child growth outcome *Y* (stunting, severe stunting or the continuous HAZ-score) of child *i* in cluster *v* at year *t* is regressed on the treatment dummy $Child\ Treated$ indicating if the child was born within 0-3 years of the start of a project within 10 km of the survey cluster; α is area-by-survey-year fixed effect (146 district-by-survey-year or 272 grid-cell-by-survey-year fixed effects); and $\mathbf{X}$ is a vector of control variables (dummies for the child's age in years, for being a girl, for being a twin and for living in an urban area, and dummies for the ethnic group and religious affiliation of the mother). The error term ε_{iat} is clustered at the geographical survey clusters.

Cluster-level variation in treatment depends on whether, and when, the area was selected as an aid project site, and at what point in time relative to project start the area was surveyed by the DHS. In addition, child-level variation in treatment status depends on when the child was born in relation to project start.[8] Given these sources of variation, and comparing within relatively small area units, we argue that treatment should be near exogenous, and thus that we are in a fairly good position to interpret our estimated treatment effect causally.

[8] To illustrate, consider a cluster surveyed in 2010, covering children born 2006–2010. If there was a project that started in 2002 or earlier within 10 km, none of the children in the cluster were treated, according to our definition. Likewise, if the cluster lacked exposure to a project at the time of the survey, but we know that a project was going to start later, none of the children were treated. If, on the other hand, there was a project that started in 2006 within 10 km, children born 2006–2009 were treated and children born in 2010 were untreated. If the cluster instead was exposed to a project that started in 2008, then children born 2008–2010 were classified as treated, whereas children born 2006–2007 were untreated. If the cluster was exposed to multiple projects, say one starting in 2003 and one starting in 2009, children born in 2006, 2009, and 2010 were treated, whereas children born 2007–2008 were untreated.

Hence, in a first step we run full sample estimations, relying on 146 district-by-year or 272 grid-cell-by-year fixed effects to capture unobserved factors giving differences in average child growth as well as in child growth trends across area units. Considering that Malawi is a relatively small country in terms of area, and that it is divided into 26 district units, the district-by-year fixed effects should arguably do quite a good job at controlling for unobserved factors giving systematic sub-national variation in aid and child growth levels and trends. However, while some unobserved determinants of child nutritional status are likely to vary across sub-national administrative units (consider factors to do with local institutions, policy, and infrastructure), others (consider, e.g., local weather conditions and how conducive local lands are to farming) will not necessarily depend on administrative borders. To control for systematic sub-national variation at a greater level of detail, and independently of administrative borders, we next control for 272 55x55 km grid-cell-by-survey-year fixed effects.

In a next step, we restrict the sample to include only "ever-treated" clusters, consisting of survey clusters with a past, present, or future aid project within 10 km at the time of the survey. Here, we utilize the fact that many clusters surveyed in the early waves were not exposed to any projects at the time, but we know that a project would start in the area later, and that other clusters have had aid projects implemented earlier but too far back in time to be relevant for treatment according to our definition. Hence, to minimize bias from systematic selection of aid project sites, we compare only children living in areas that donors and the government, at one point or another, have deemed suitable for aid project localization.

To further enhance comparability of the treatment and control groups, we restrict the sample to children born within a specified time window before or after project start. Considering that Malawi is a small and aid-dependent country, limiting the sample to children living in areas targeted by aid projects at one point or another is not very restrictive. In fact, it applies to 76 percent of the full estimation sample (see Table A3). Within this sub-sample there are children born up to roughly 15 years before or after the start of the first nearby project. Hence, in these estimations we trim the sample even further to include only our treatment group – i.e., children born 0–3 years after project start – and a comparison group consisting of children instead born 2–4 years prior to project start.[9] We choose this particular time span to minimize contamination of the control group while maintaining comparability across the two groups. Considering that

[9] Since some children have more than one project within the cutoff distance, and this exercise requires us to tie children to the start of a specific aid project, these estimations focus on being born before and after the first aid project starting in the area.

children born just before project start will to some extent also be exposed to the aid project (albeit at a later, less critical age), we do not include children born one year ahead of project start in the comparison group. Since growth impairment during the first 1,000 days of life is commonly suggested to be particularly critical for long-term development (WHO, 2015), we instead focus on children born 2–4 years before projects start.[10] Furthermore, to ensure common support across the two groups in terms of birth years, we restrict the sample to children born 1998–2008.

Next, we carry out additional estimations relying on sub-samples with variation in treatment status within clusters and across siblings within households, including cluster-by-year fixed effects and mother fixed effects, respectively. Finally, to ensure that treated and un-treated children are balanced on key covariates, we use coarsened exact matching (CEM) and run estimations based on a matched sample.

5. Results

In this section, we present the results of our empirical estimations to investigate the local effects of aid on child growth in Malawi. We begin with our main findings, focusing on stunting, severe stunting, and the continuous HAZ score, respectively. Next, we examine when the observed treatment effect kicks in and for how long it lasts, how it varies with treatment intensity, what types of aid projects stand out as more (and less) important for child growth, and heterogeneity across donors. Finally, we evaluate the robustness and heterogeneity of the estimated effects, as well as possible mechanisms underlying the results.

5.1 Main results

The results consistently suggest that aid exposure reduces the child's probability of impaired growth. Table 1 presents the results of estimations focusing on the impact of treatment – i.e., the child being born 0–3 years after the start of an aid project within 10 km – on stunting (HAZ score<=-2 SD, see panel A) and severe stunting (HAZ score<=-3 SD, see panel B). The estimation in column 1 is included merely as a point of reference. It includes only individual controls and survey year fixed effects and hence does not adequately control for systematic

[10] Using a comparison group consisting of children born 3–4 years before project start gives very similar results.

selection of aid project sites. Nonetheless, we can note that the coefficient on our treatment variable is negative and statistically significant, implying that children exposed to aid in their first years of life are less likely to be stunted. Accounting for unobserved variation across time and space (columns 2–3), the estimated effect of aid exposure remains stable. Panel A in Table 1 shows that in the full sample, a child born 0–3 years after the start of the local project is close to 2 percentage points less likely to be stunted.

In columns 4–5, we present the results of estimations based on a sample consisting only of survey clusters with a past, present, or future aid project within 10 km at the time of the survey. The estimated treatment effect, if anything, becomes more pronounced. Furthermore, the estimated parameter again remains stable to using the more detailed grid-cell-by-survey-year fixed effects. Hence, even if limiting the comparison to children in areas that have been selected for aid projects and controlling for sub-national variation in average levels of impaired growth over time, children born within 3 years of the start of a local aid project are significantly less likely to be stunted.

In columns 6 and 7, we trim the sample even further to include only our treatment group – i.e., children born 0–3 years after project start – and a comparison group consisting of children born 2–4 years prior to project start. Hence, in the interest of comparability, we do not restrict the sample only to areas targeted by aid projects, but also to children born within a specified time window before or after project start. The resulting treatment effects are approximately three times the size of those observed for the full sample; treated children are around 6 percentage points less likely to be stunted.

The results for severe stunting (Panel B) are very similar: aid exposure involves an approximate 2-4 percentage point lower probability of being severely stunted. While no very large, these effects are clearly not negligible.

Stunting is a common and easy-to-interpret measure of child growth impairment. However, classifying a child as stunted if their HAZ score falls below a certain critical value inevitably involves some arbitrariness. Moreover, the resulting dichotomous variable hides substantial variation. In Table 2, we instead focus on the continuous HAZ score variable, thus capturing child growth variation along the whole spectrum.

The results correspond to those for stunting, consistently indicating a positive impact of aid exposure on child growth. The estimated effect size ranges from around 4 to 16 percent of a standard deviation. Again, accounting for unobserved variation across time and space, and restricting the sample to aid targeted areas and to children born within a specified time window ahead or after project start makes the estimated effect more pronounced.

The fact that the more we narrow down the comparison group and account for unobserved variation across time and space the more pronounced the estimated coefficient on aid exposure generally becomes suggests that selection effects work against rather than inflate the treatment effect. Or for that matter, that noise attenuates the effect in full sample estimations without appropriate controls for unobserved variation.

5.2 Effect onset and duration

Next, we explore when the observed treatment effect kicks in and for how long it lasts. For the sake of brevity, we present only the results for the continuous HAZ score variable in Figure 2, and the corresponding results for stunting and severe stunting in the appendix (Figures A1 and A2). The estimates are based on a regression (equivalent to column 3 in Tables 1–2) where, instead of a dummy for being born 0–3 years after project start, we include separate dummies for being born in the same year as project start, as well as 1, 2, 3, 4, and 5 years after project start. For comparison, we also include a dummy for being born 2–4 years before project start. As expected, this dummy is not statistically different from zero for any of the outcome variables, highlighting the relevance of the critical treatment window discussed in Section 2.

Focusing on the continuous HAZ score (Figure 2), where we have most variation, the positive treatment effect is visible already at the year of project start, but it becomes more pronounced 1–3 years after project start. Four to five years after project start, the effect fades away. For stunting (Figure A1), we get a statistically significant treatment effect for children born 1–3 years after project start, and for severe stunting (Figure A2) for children born 2 years after project start.

Taken together, the positive treatment effects of local aid projects on child growth thus materialize already for children born in the early project implementation phase and remain for children born up until around 3 years after project start. After that, however, we do not observe any statistically significant aid effects. This could indicate that the observed treatment effects are not permanent at the local area, but it could also reflect that the further away from a project start we get, the more the noise obscures the potential effect.

5.3 Treatment intensity: number of projects

Our benchmark treatment variable is a dichotomous indicator that simply captures exposure to an aid project that started 0–3 years prior to a child's birth within a 10 km cutoff distance. Some children, however, have been exposed to multiple projects during early life (see Table A3). The estimation in Figure 3, focusing on the HAZ score, breaks our treatment into five categories, including dummies for having 1, 2, 3, 4, and, finally, 5 or more projects fitting the treatment criteria. Again, we present the corresponding results for stunting and severe stunting, which are very similar, in the appendix (Figures A3 and A4).

We can note that the estimated effect of living near three or four projects fitting the treatment criteria is larger than that of living near one. The treatment effect of the former is more than three times the size of the latter (0.21–0.22 vs. 0.06).[11] That being said, the results suggest no simple linear effect of the number of projects on our outcome variables of interest. Specifically, we observe no statistically significant effect of living within the cutoff distance of two projects fitting the treatment criteria or of living near many (five or more) such projects. The latter is likely a result of it being difficult to identify the impact of aid exposure in areas with a multitude of past and present aid projects.

5.4 Breaking down aid projects by sector

In a next step, we explore what types of aid projects drive the observed treatment effect. Table A4 summarizes the number of project sites by sector. The estimation results in Figure 4, again focusing on the HAZ score, come from a regression where we break down our treatment variable into nine dummies for being exposed to aid from specific sectors, focusing on rural areas.[12] We can note that the estimated effects of rural development and infrastructure projects come out statistically significant. For stunting (Figure A5), none of the sector specific treatment variables have statistically significant parameters (infrastructure is nearly so). Focusing on severe stunting (Figure A6), however, rural development, vulnerability, infrastructure, and education projects all bring lower stunting levels (statistically significant at conventional levels).

Although the results are not clear cut, they provide some evidence of a heterogenous effect. To the extent that rural development and vulnerability projects target the poorest and involve social protection systems to help the poor meet basic consumption needs, these too should have an important role to play. With respect to education projects, a possible mechanism could be school-feeding.[13] School meals may benefit other members of the household when the

---

[11] The difference between being exposed to three and being exposed to one project is statistically significant at conventional levels and the difference between four and one is statistically significant at the 10 percent level.

[12] Because of the great number of closely located projects often found in urban areas, making it difficult to single out the impact of specific projects, these estimations focus on rural areas alone.

[13] Since 1999 the Government of Malawi and the World Food Program implement a school feeding program in the most food insecure districts of Malawi (WFP, 2021).

food provided is shared or when the school-aged child's intake at home is reduced (Ruel and Alderman, 2013). A randomized controlled trial of a school feeding program in Burkina Faso showed effects on the weight of younger siblings of beneficiaries (Kazianga et al., 2009). Infrastructure, finally, connects people with markets, thus facilitating both employment and dietary diversity (Usman et al., 2022).

On the other hand, we observe no statistically significant effects of health, water and sanitation, and agricultural projects (for the latter, the estimated effect on the HAZ score is, if anything, negative). Considering that infectious diseases are important determinants of stunting (WHO, 2018), the role of maternal care and child health services for early child development, and the role of agriculture for food security, this is somewhat surprising.

To identify projects targeting more proximal determinants of stunting, we conduct a key word search in the project activity descriptions provided by AidData. Specifically, we identify projects mentioning at least one of the following words: child, infant, nutrition, food, feeding or natal (as in prenatal, neonatal, and postnatal). Out of our 778 sample projects, 185 projects (24 percent) meet this definition. Interestingly, neither infrastructure nor rural development – the sectors for which we observe the clearest treatment effects – include any such projects (see Table A4). On the other hand, in health, water and sanitation, and agriculture, i.e., the sectors which for which we were most surprised not to find a treatment effect, a considerable share of the projects mention the key words. Indeed, running estimations where we break down our treatment variable into being treated by a project mentioning the child/nutrition keywords and by a project not doing so (Table A5), the estimated effect of the latter is more precisely estimated and for severe stunting significantly larger.

A possible interpretation of this somewhat puzzling finding is that distal determinants of stunting are important for addressing child malnutrition. However, it may also be the case that more efficient donors are over-represented in certain sectors. Looking at Table A4, we can note that infrastructure and rural development, where we find clear treatment effects, are the sectors with the largest share of multilateral donors (96 and 86 percent, respectively). In the next section we compare the effects of multilateral and bilateral aid, and in section 5.7 we discuss possible mechanisms further.

5.5 Comparing bilateral and multilateral aid

So far, we have considered all aid projects, irrespective of donor. Reasonably, however, there is donor heterogeneity as well. In particular, multilateral aid is often characterized as being relatively more focused on supporting development outcomes, while bilateral aid is seen as more likely to be allocated based on donors' strategic interests (for an overview, see Biscaye et al., 2017).

The estimations in Table 3 compare the effects of being born 0–3 years after the start of a bilateral and a multilateral project. In line with the above characterization, the results suggest that multilateral aid drives our treatment effect. Whereas the parameter on the bilateral treatment variable is not statistically significant for any of the outcomes, the estimated effect of multilateral aid is statistically significant and larger than that of overall aid in the benchmark setup when focusing on severe stunting and the continuous HAZ score (compare with the estimation in column 3 in Tables 1–2).

Considering the sectoral division of projects in the respective donor groups (Table A4, panel B-C), though, it is the bilateral donors in our sample that focus on more proximate determinants of stunting. The by far largest share of bilateral projects (47 percent) goes to the health sector, followed by agriculture and rural development (both at 11 percent). In comparison, the largest share of the multilateral aid projects goes to the rural development sector (44 percent), followed by infrastructure (29 percent). A sectoral breakdown of projects obviously hides substantial variation in terms of project types. However, if we instead consider the share of projects in the respective donor groups that mention the child/nutrition-related keywords, we get a similar picture. Whereas 47 percent of the activity descriptions of the bilateral projects do so, the corresponding figure for the multilateral projects is only 10 percent (Table A4, panel B-C).[14]

It is worth emphasizing that the heterogeneity along sectoral and donor lines is difficult to interpret. As noted, we observe the clearest treatment effects among multilaterals and in the infrastructure and rural development sectors. Yet, given the dominance of multilaterals in these sectors (they account for 96 and 86 percent of projects), we do not have sufficient variation to explore if it is the type of project or the type of donor that drives these differences. Furthermore, multilateral projects, as well as projects in the infrastructure and rural development sectors, tend to be larger on average, so an important part of the heterogeneity is likely due to size. However,

[14] Comparing treatment effects across individual donors is somewhat problematic, since several donors may be present in the same area at the same time and the number of projects a donor is involved in differs considerably. In the appendix, we nonetheless present estimations where we break down our treatment indicator into separate dummies for being born 0–3 years after the start of projects of specific donors. In particular, we consider donors with at least 50 recorded project sites in Malawi (namely, the European Union, the World Bank and the African Development Bank among the multilateral donors, and Germany, Norway, and the UK among the bilateral donors, as well as an "other" category capturing the remaining donors, see Table A1). Bearing in mind the difficulty of attributing impacts to specific donors, Table A6 suggests that the positive impacts on child growth are primarily driven by the World Bank and EU projects.

since we do not have sufficiently precise information on how total project flows are divided across project sites, we are not in a good position to explore this statistically. What we can say, though, is that projects focusing on more proximate determinants of stunting do not seem to drive the observed treatment effect.

5.6 Robustness and heterogeneity

In the benchmark setup, treatment status depends on two sources of variation: cluster-level variation in aid coverage (if and when an area has been selected as an aid project site, as well as at what point in time, relative to project start, the area was surveyed by the DHS), and child-level variation in year of birth in relation to project start. In most survey clusters, there is no variation in treatment status. In 31 percent of the clusters, however, we have observations for both treated and untreated children, allowing us to focus on within-cluster variation.

In Table A7 we present estimations including 2,252 cluster-by-survey-year dummies, meaning that we rely exclusively on variation in child year of birth in relation to project start in clusters that currently are or recently have been exposed to a local aid project.[15] This specification is demanding. To begin with, we control for cluster-level variation that is likely to depend on local aid exposure. Moreover, the distinction between treated and untreated children within an aid-exposed cluster is inevitably somewhat fuzzy (children born, say, 4 years after or 1 year before a project start would also have been exposed to aid, albeit at a less critical age), which should drive the difference between the two groups toward zero. One could even argue that what we capture in this specification is variation in treatment intensity – whether the child is exposed to aid at a critical age – rather than a comparison between treated and untreated children. With this in mind, it is noteworthy that the estimated coefficients in this setup are in line with the benchmark results, and that we still find a statistically significant effect of treatment on severe stunting.

Indeed, even if we use mother fixed effects, meaning that we compare treated and untreated siblings within families, the treatment effect for stunting and severe stunting comes out nearly statistically significant at the 10 percent level ($p=0.10$ and $p=0.11$, respectively, see Table A8). This specification requires variation within families, i.e., that mothers have at least two children under age 5 with differing treatment status. Considering that this applies only to a small subsample (approximately 10 percent of the benchmark sample), and again taking into

[15] Considering the density of projects and likely contamination of the control group in urban areas, these estimations focus on the rural sample.

account the likely contamination of supposedly untreated children within the same family, which should bias the treatment effect toward zero, this is notable.

Furthermore, the treatment effect is robust to using coarsened exact matching (CEM) rather than relying and spatial and temporal variation in a regression framework (Table A9). The key goal of matching is to prune observations from the sample so that the remaining data have better balance between the treated and control groups. The basic idea of CEM, specifically, is to temporarily coarsen the data into substantively meaningful groups, then use exact matching on these coarsened data, and finally run the analysis on the original un-coarsened data for the matched sample (Iacus et al., 2012). While the approach is no magic bullet in terms of causal identification, it has the advantage that it makes the potential lack of common support, or overlap in terms of covariates, between treatment and control group explicit (Isaksson, 2017). Using matching, we thus avoid drawing conclusions based on unreasonable extrapolations.

In particular, the procedure allows us to match on both child age, child year of birth and survey year. While all these variables likely carry substantive information, collinearity prevents us from controlling for them jointly in our standard regression framework. Reassuringly, comparing treatment and control cases that are matched exactly on district, urban residence, the child's sex, age in years, year of birth, and on interview years coarsened by survey wave,[16] the treatment effect remains unchanged.

In the benchmark setup, we define children born 0–3 years after the start of an aid project within 10 km as treated. The geographical cutoff is admittedly somewhat arbitrary. How far from project sites that children will experience its potential rewards is essentially an empirical question. Figures A7–A9 summarize the results of estimations using alternative distance cutoffs to define treatment. We can note that the results remain stable when using a 10–50 km cutoff, and that the estimated effect is similar, but less precisely estimated, when using a 5 km cutoff. Due to the density of aid projects in Malawi, implying that nearly all surveyed children have (a multitude of) ongoing aid projects within the cutoff distance when using wider cutoffs, we are not able to explore when the effect fades.

So far, we have considered an average treatment effect of aid on child growth, disregarding potential treatment effect variation across socio-economic groups. Reasonably, however, aid exposure matters more for children in vulnerable groups. Table A10 presents results of estimations where we let the treatment effect vary across urban/rural residence, and with the

[16] Ethnicity, religion, and whether the child is a twin we instead control for in the regression on the matched observations, where we also control for the un-coarsened interview year indicator.

socio-economic status and age of the mother.[17] The treatment effect comes out statistically significant in rural but not in urban areas.[18] This likely reflects that child malnutrition is more widespread in rural areas but could also be due to the density of ongoing and past projects and likely spillover effects in urban areas, leaving us without a clear comparison group in the urban setting.

Along the other dimensions considered, there is little indication of marked treatment effect heterogeneity; in most cases, the inclusion of an interaction term renders the parameters of both the treatment dummy and the concerned interaction term statistically insignificant. That said, considering parameter heterogeneity with respect to the age and level of education of the mother, there are some signs that the observed treatment effect is more pronounced in vulnerable groups. In particular, the observed treatment effect comes out statistically significant only among children of young mothers when focusing on stunting and the continuous HAZ score, and only among mothers with little or no education when focusing on severe stunting.

5.7 Mechanisms

In our baseline setup we include only control variables that can be judged as reasonably exogenous with respect to our treatment. That is, we do not control for variables that are themselves likely to be affected by aid exposure. Thus, to get a picture of the mechanisms underlying the observed results, we here include factors commonly suggested to affect stunting as right-hand-side variables (see the discussion in Section 2).[19] Table A11 in the appendix reports how the observed treatment effect reacts to the inclusion of possible mechanism candidates. For brevity, we focus on the continuous HAZ score and the ever-treated sample.[20] We use the same specification as in column 4 in Table 2, which includes baseline controls and

[17] In particular, we interact the treatment dummy with a rural dummy, with a dummy for belonging to the two poorest wealth quintiles, with a dummy for the mother having less than the median number of years of education (<4), and with a dummy for the mother being younger than 20 years old when she gave birth to the child in question.

[18] Focusing on severe stunting, the treatment effect actually has the opposite sign in urban areas. However, considering the density of ongoing and past projects and likely spillover effects in urban areas, these results should not be given too much weight.

[19] The selection of factors is based on indicators proposed in the reviews by Vaivada et al. (2020; 2022) and studies on stunting in Malawi (Espo et al., 2002; Kuchenbecker et al., 2015; Makoka and Masibo, 2015; Christian et al. 2020; Chilinda et al., 2021; Kumchulesi, 2015, 2021; and Magagula et al., 2021), and the data availability in the DHSs. Due to paucity of data, we do not evaluate inappropriate complementary feeding and optimal breastfeeding.

[20] Running equivalent estimations for the full sample or the sample focusing on children born just before and after project start, does not change the interpretation of the results. As in the baseline setup, the treatment effect is most pronounced and precisely estimated in the narrow comparison between children born before and after treatment. Hence, whereas the full sample treatment effect becomes somewhat smaller and less precisely estimated when including the mechanism candidates, the treatment effect in the narrow before/after sample remains stable and highly statistically significant.

district-by-year fixed effects.

To begin with, we note that all the included mechanism candidates are statistically significant and of the expected sign, suggesting that they are indeed relevant for child growth. Still, our estimated treatment effect remains stable to their inclusion, except in a couple of cases.

The mechanism candidate that stands out as most relevant is the variable indicating if the child slept under a bed-net last night (column 13). Controlling for bed-net use, the point estimate for the treatment effect is only 0.03 and no longer statistically significant (it remains statistically significant, but becomes smaller, 0.21 compared to 0.25, in the restricted sample focusing on children born just before and after project start). Hence, aid helping families to afford preventive measures against malaria is a possible mechanism underlying the observed treatment effect. This finding is consistent with Marty et al. (2017) who, using data on local aid in Malawi, reported a negative effect of aid on the prevalence of malaria.

Controlling for household wealth, the observed treatment effect shrinks from 0.085 to 0.060 and is less precisely estimated (columns 1 and 2). Hence, in line with Khomba and Trew (2022), who find that local aid in Malawi increases nighttime light – a proxy for growth in consumption – a candidate mechanism could be increased wealth in aid exposed households. While arguably surprising that wealth does not absorb the observed treatment effect to a larger extent, this is in fact in line with the results of Brown et al. (2019), who find that undernourished women and children are spread widely across socio-economic groups in Sub-Saharan Africa. Instead, they point to the importance of common health risks related to, for example, maternal health.

Access to piped water and toilet facilities, as well as the time to fetch water, have statistically significant parameters of the expected sign, suggesting that they are relevant determinants for child growth (column 3-5). Yet, the impact on the treatment effect is negligible even though all three variables are correlated with wealth.

While treatment – the child being born 0-3 years after project start – is unlikely to affect the education of the individual mother, aid may affect behavior, that is, when a girl or young woman gets pregnant. Thus, it may make girls postpone having children until they finish school, meaning that young mothers on average will be more educated. Even so, controlling for the education of the mother (column 6), the treatment effect remains relatively stable. The treatment effect is also only marginally affected by the inclusion of teenage births (column 7), and controlling for birth spacing (column 8), though it is less precisely estimated in the latter case. Completing at least four visits to antenatal clinics also reduces the risk of stunting but, if anything, seems to increase the positive effect of treatment (column 9).

Finally, we account for nutrition and health status variables. Controlling for low BMI of the mother and low birth weight of the child (column 10-11), the observed treatment effect again remains relatively stable and clearly significant. The same holds when accounting for the child having recent experiences with diarrhea (column 12).

6. Conclusions

After decades of progress, the fight against food insecurity has suffered a serious setback in recent years due to climate-related shocks, a pandemic, and a war disrupting global food supply chains. At this point, commentators warn of a global food crisis, clearly threatening progress toward the sustainable development goal of “zero hunger” (The Economist, 2022). In view of these developments, understanding the role of foreign aid in preventing impaired child growth due to chronic malnutrition is crucial.

The aid effectiveness literature has generally seen a divide between macro-level studies and studies investigating the causal impact of specific interventions. Sub-national analysis of geocoded aid and outcome data provides an intermediate perspective that can help bridge this divide. Specifically, rather than estimating country-wide impacts of total aid, or analyzing the impact of specific interventions, it enables us to systematically estimate whether a multitude of aid projects have effects in the targeted areas on average, as well as to break down the analysis by donors and type of projects.

This paper focuses on stunting in Malawi, a country with high prevalence of stunting, and for which we have access to geo-referenced data on aid projects from a broad range of donors. To investigate whether aid projects help to reduce impaired child growth in the local area, we geographically matched spatial data on 778 aid project sites of 22 different donors with anthropometric and background data on 26,604 children under age five. To identify the effect of aid, we relied on spatial and temporal variation in aid project coverage and survey rollout, coupled with variation in childbirth years in relation to project start. To the best of our knowledge, this is the first study to use geocoded multi-donor aid data allowing for disaggregated analysis of the local effects of aid on impaired child growth in Africa.

The empirical results consistently indicate a positive impact of early life aid exposure on child growth. While this result is robust across a broad range of specifications, there is significant treatment effect heterogeneity. In particular, we found that the positive treatment effects materialize already for children born in the early project implementation phase and last for children born up to 3 years after project start. Ideally, we would of course have liked to see effects lasting for children born later relative to project start. Worth emphasizing, however, for children born within the above time window, aid may have helped to protect against irreversible consequences of stunting that would otherwise have lasted a lifetime. For the cohorts of treated children, effects are thus likely to be long lasting.

Furthermore, the results suggest that treatment effects are driven primarily by multilateral aid and by projects in rural development and infrastructure (and to a lesser extent education and vulnerability). This even though the bilateral donors – based on a key word search in the project activity descriptions provided by AidData – to a greater extent focus on more proximate determinants of stunting. While we should acknowledge that projects with a multi-sector focus and the presence of multiple donors in the same area make it difficult to attribute effects to specific donors and sectors, these results nonetheless provide an indication of where to look for best practice examples.

With respect to mechanisms underlying the observed treatment effect, we present a set of estimations where we account for commonly suggested determinants of stunting that may also be affected by aid. The candidate that stands out as most relevant is child bed-net use, which is in line with previous findings suggesting that aid has helped to reduce malaria prevalence in Malawi (Marty et al., 2017). Furthermore, the results provide some indication that increased wealth in aid exposed households could help explain the reduction in stunting. Overall, however, accounting for variables related to socio-economic status does relatively little to explain the observed effects of aid on stunting. While arguably surprising, this is in line with results in previous literature suggesting that undernourished children are spread widely across socio-economic groups in Sub-Saharan Africa (Brown et al., 2019), presumably due to common distal factors such as access to markets for food, disease environment, and maternal health.

An interesting avenue for further research would be to analyze the interplay between type of aid, donor, and specific mechanisms on the outcome in question in more detail.

In sum, the results are encouraging in that they suggest that aid exposure reduces the risk that a child will suffer from stunting and its largely irreversible consequences. While effect sizes are relatively modest, they are consistent and become more pronounced the more we narrow down the comparison group to account for unobserved variation across time and space. For the analysis of aid effectiveness more broadly, our findings highlight that we can learn from using a disaggregated approach that compares aid impacts across sectors, donors, and locations.

References

AidData Research and Evaluation Unit (2017) “Geocoding Methodology, Version 2.0”, Williamsburg, VA: AidData at William and Mary. https://www.aiddata.org/publications/geocoding-methodology-version-2-0.

Attanasio, O., Baker-Henningham, H., Bernal, R., Meghir, C., Pineda, D., and Rubio-Codina, M. (2022a) ”Early Stimulation and Nutrition: The impacts of a scalable intervention”, *Journal of the European Economic Association*, 20(4), pp. 1395-1432.

Attanasio, O. P., Cattan, S., and Meghir, C. (2022b) ”Early Childhood Development, Human Capital, and Poverty”, *Annual Review of Economics*, 14, 853-892.

Attanasio, O. P., Fernández, C., Fitzsimons, E. O., Grantham-McGregor, S. M., Meghir, C., and Rubio-Codina, M. (2014). Using the infrastructure of a conditional cash transfer program to deliver a scalable integrated early child development program in Colombia: cluster randomized controlled trial. *BMJ*, 349.

Arndt, C., S. Jones, and F. Tarp (2015) “Assessing Foreign Aid’s Long Run Contribution to Growth and Development”, *World Development*, vol. 69, pp- 6–18.

Biscaye, P. E., Reynolds, T. W. and C. L. Anderson (2017) “Relative Effectiveness of Bilateral and Multilateral Aid on Development Outcomes”, *Review of Development Economics*, 21(4), pp. 1425–1447.

Bourguignon F. and J.W. Gunning (2016) “Foreign Aid and Governance: A Survey”, Economic and Development Institutions, available at: https://edi.opml.co.uk/resource/foreign-aid-governance-survey/ (accessed: 2020-02-13).

Brazys, S., Elkink, J. A. and G. Kelly (2017) “Bad Neighbors? How co-located Chinese and World Bank development projects impact local corruption in Tanzania”, *The Review of International Organizations*, 12(2), pp 227–253.

Bräutigam, D. and S. Knack (2004) “Foreign Aid, Institutions, and Governance in Sub-Saharan Africa”, *Economic Development and Cultural Change*, 52(2), pp. 255-285.

Brown, C., Ravallion, M and D. van de Walle (2019) "Most of Africa's Nutritionally Deprived Women and Children are Not Found in Poor Households," *The Review of Economics and Statistics,* 101(4), pp. 631-644.

Burgert, C.B., Zachary, B. and J. Colston (2013) “Incorporating geographic information into demographic and health surveys: A field guide to GPS data collection”, Demographic and Health Surveys Methodology, prepared for USAID by ICF International, available at: https://dhsprogram.com/pubs/pdf/DHSM9/DHS_GPS_Manual_English_A4_24May2013_DHSM9.pdf

Chauvet, L., Gubert, F. and S. Mesplé-Somps (2013) “Aid, Remittances, Medical Brain Drain and Child mortality: Evidence using inter and intra-country data”, *The Journal of Development Studies*, 49, pp. 801–818.

Chilinda, Z. B., Wahlqvist, M. L., Lee, M.-S., and Huang, Y.-C. (2021) “Higher maternal autonomy is associated with reduced child stunting in Malawi”, *Scientific reports*, 11(1), 1-12.

Clemens, M. A., Radelet, S., Bhavani, R. R. and S. Bazzi (2012) “Counting Chickens when they Hatch: Timing and the Effects of Aid on Growth”, *Economic Journal*, 122(561), pp. 590–617.

Civelli, A., Horowitz, A. and A. Teixeira (2018) “Foreign Aid and Growth: A Sp P-VAR Analysis Using Satellite Sub-national Data for Uganda”, *Journal of Development Economics*, vol. 134, pp. 50–67.

Christian, P., Hurley, K. M., Phuka, J., Kang, Y., Ruel-Bergeron, J., Buckland, A. J., . . . West Jr, K. P. (2020) “Impact evaluation of a comprehensive nutrition program for reducing stunting in children aged 6–23 months in rural Malawi”, *The Journal of Nutrition*, 150(11), pp. 3024-3032.

Das JK, Salam RA, Hadi YB, Sheikh SS, Bhutta AZ, Prinzo ZW, Bhutta ZA. Preventive lipid-based nutrient supplements given with complementary foods to infants and young children 6 to 23 months of age for health, nutrition, and developmental outcomes. Cochrane Database Syst Rev 2019;5:CD012611

De, R. and C. Becker (2015) “The Foreign Aid Effectiveness Debate: Evidence from Malawi”, Aid Data Working Paper 6, March 2015.

de Onis, M., Garza, C., Victora, C.G., Onyango, A. W., Frongillo, E. A. and J. Martines (2004) “The WHO Multicentre Growth Reference Study: Planning, study design, and methodology”, *Food and Nutrition Bulletin,* vol. 25, no. 1 (supplement 1), pp. S15-S26.

DHS (2015-16) National Statistical Office (NSO) [Malawi] and ICF. 2017. Malawi Demographic and Health Survey 2015-16. Zomba, Malawi, and Rockville, Maryland, USA. NSO and ICF.

DHS (2023) "GPS Data Collection", https://dhsprogram.com/Methodology/GPS-Data.cfm (accessed 2023-01-23).

Dreher, A., Fuchs, A., Hodler, R., Parks, B. C., Raschky, P. A., and M. J. Tierney (2019) "African Leaders and the Geography of China's Foreign Assistance", *Journal of Development Economics*, 140, pp. 44–71.

Dreher, A. and S. Lohmann (2015) "Aid and Growth at the Regional Level", *Oxford Review of Economic Policy*, 31(3-4), 420-446.

Espo, M., Kulmala, T., Maleta, K., Cullinan, T., Salin, M. L., and Ashorn, P. (2002) "Determinants of linear growth and predictors of severe stunting during infancy in rural Malawi", *Acta Paediatrica*, 91(12), 1364-1370.

Farag, M., Nandakumar, A., Wallack, S., Hodgkin, D., Gaumer, G. and C. Erbil (2013) "Health Expenditures, Health Outcomes and the Role of Good Governance", *International Journal of Health Care Finance and Economics*, 13(1), pp. 33–52.

Feeney, S. and B. Ouattara (2013) "The effects of health aid on child health promotion in developing countries: Cross country evidence", *Applied Economics*, 45(7), pp. 911–919.

Fielding, D. (2011) "Health Aid and Governance in Developing Countries", *Health Economics*, 20(7), pp. 757–769.

Findley, M.G., Powell, J., Strandow, D. and J. Tanner (2011) "The Localized Geography of Foreign Aid: A New Dataset and Application to Violent Armed Conflict", *World Development*, 39(11), pp. 1995-2009.

Georgiadis, A. and M. E. Penny (2017) "Child undernutrition: opportunities beyond the first 1000 days", *The Lancet Public Health*, 2(9), p. e399.

Ghosh, S., Shivakumar, N., Bandyopadhyay, S., Sachdev, H. S., Kurpad, A. V. and T. Thomas (2020) "An uncertainty estimate of the prevalence of stunting in national surveys: the need for better precision", *BMC Public Health*, 20(1634), pp. 1-10.

Gormanee, K., Girma, S. and O. Morrissey (2005) “Aid, Public Spending and Human Welfare: Evidence from Quantile Regressions”, *Journal of International Development*, 17(3), pp. 299–309.

Gyimah-Brempong, K. (2015) “Do African Countries Get Health from Health Aid?”, *Journal of African Development*, 17(2), pp. 83–114.

Hurley, K. M., Phuka, J., Kang, Y., Ruel-Bergeron, J., Buckland, A. J., Mitra, M., ... and Christian, P. (2021). A longitudinal impact evaluation of a comprehensive nutrition program for reducing stunting among children aged 6–23 months in rural Malawi. The *American Journal of Clinical Nutrition*, 114(1), 248-256.

Iacus, S. M., King, G. and G. Porro (2012) “Causal Inference Without Balance Checking: Coarsened Exact Matching.” *Political Analysis*, 20(1), pp. 1-24.

Isaksson, A. (2020) “Chinese aid and local ethnic identification”, *International Organization*, September 2020, 74(4), pp 833-852.

Isaksson, A. and D. Durevall (2022) “Aid and institutions: Local effects of World Bank aid on perceived institutional quality in Africa”, forthcoming in *Review of International Organization,* https://doi.org/10.1007/s11558-022-09478-w

Isaksson, A. and A. Kotsadam (2018a) “Chinese Aid and Local Corruption”, *Journal of Public Economics,* vol. 159, pp. 146-159.

Isaksson, A. and A. Kotsadam (2018b) “Racing to the Bottom? Chinese Development Projects and Trade Union Involvement in Africa”, *World Development*, vol. 106, pp. 284-298.

Kazianga, H., Walque, D. De and H. Alderman (2009) “Educational and health impact of two school feeding schemes: evidence from a randomized trial in rural Burkina Faso”, World Bank Policy Research Working Paper 4976, World Bank, Washington, DC (2009)

Khalid, H., Gill, S. and A. M. Fox (2019) “Global aid for nutrition-specific and nutrition-sensitive interventions and proportion of stunted children across low-and middle-income countries: does aid matter?”, *Health Policy and Planning*, 34(Supplement 2), ii18-ii27.

Khomba, D. C., and Trew, A. (2022) “Aid and Local Growth in Malawi”, *The Journal of Development Studies*, pp. 1-23.

Kizhakethalackal, E. T., Mukherjee, D., and E. Alvi (2013) “Quantile Regression Analysis of Health-aid and Infant Mortality: A Note”, *Applied Economics Letters*, 20(13), pp. 1197–1201.

Kosack, S., and Tobin, J. (2006) “Funding self-sustaining development: The role of aid, FDI and government in economic success”, *International Organization*, 60(1), pp. 205–243.

Kotsadam, A. Østby, G., Rustad, S., Tollefsen, A. and H. Urdal (2018) “Development Aid and Infant mortality. Micro-level Evidence from Nigeria”, *World Development*, vol. 105, pp. 59–69.

Kuchenbecker, J., Jordan, I., Reinbott, A., Herrmann, J., Jeremias, T., Kennedy, G., . . . Krawinkel, M. (2015) “Exclusive breastfeeding and its effect on growth of Malawian infants: results from a cross-sectional study”, *Paediatrics and International Child Health*, 35(1), 14-23.

Kumchulesi, G. (2018) “Persistence of Child Malnutrition in Malawi: Explanations From Demographic and Health Surveys”, *Journal of African Development*, 20(1), 69-75.

Kumchulesi, G. (2021) “Explaining the Decline in Child Stunting in Malawi between 2010 and 2015”, Working Paper no. 439, African Economic Research Consortium, Nairobi.

Magagula, M., Ramroop, S., and Habyarimana, F. (2021) “Significant Risk Factors Associated with Stunting for Children Under the Age of 5-years in Malawi: the Application of Proportional Odds Model Using DHS Datasets”, Preprint, Research Square, https://assets.researchsquare.com/files/rs-15157/v1_covered.pdf?c=1631863385

Makoka, D., and Masibo, P. K. (2015) “Is there a threshold level of maternal education sufficient to reduce child undernutrition? Evidence from Malawi, Tanzania and Zimbabwe”, *BMC Pediatrics*, pp. 15(1), 1-10.

Malawi Government (2006) “National Nutrition and Strategic Plan 2007-2012”, Lilongwe. Accessed 2022-06-22. Available at: https://cepa.rmportal.net/Library/government-publications/Malawi%20National%20Nutrition%20Policy%20and%20Strategic%20Plan.pdf

Marty, R., Dolan, C. B., Leu, M., and Runfola, D. (2017). Taking the health aid debate to the subnational level: the impact and allocation of foreign health aid in Malawi. *BMJ Global Health*, 2(1), e000129.

Mary, S., Shaw, K., Colen, L., and y Paloma, S. G. (2020) "Does Agricultural Aid Reduce Child Stunting?" *World Development*, 130, p. 104951.

Matita, M., Chiwaula, L., Chirwa, E. W., Mazalale, J., and Walls, H. (2022) "Subsidizing improved legume seeds for increased household dietary diversity: Evidence from Malawi's Farm Input Subsidy Programme with implications for addressing malnutrition in all its forms", *Food Policy*, 113, article 102309.

Mishra, P. and D. Newhouse (2009) "Does Health Aid Matter?", *Journal of Health Economics*, 28(4), pp. 855–872

Mosley, P. (1987) *Foreign Aid, its Defense and Reform*, University Press of Kentucky, Lexington, KY.

Mukherjee, D. and E. T. Kizhakkethalackal (2013) "Empirics of health aid, education and infant mortality: A semiparametric study", *Applied Economics*, 45(22), pp. 3137–3150.

Mwale, M., Smith, A., and von Fintel, D. (2022) "Child nutrition and farm input subsidies: The complementary role of early healthcare and nutrition programs in Malawi", *Food Policy*, 113, article 102340.

Ngwira, A., Munthali, E. C., and Vwalika, K. D. (2017) "Analysis on the association among stunting, wasting and underweight in Malawi: an application of a log-linear model for the three-way table", *Journal of Public Health in Africa*, 8(1).

Odokonyero, T., Ijjo, A., Marty, R., Muhumuza, T. and G. O. Moses (2018) "The Impact of Aid on Health Outcomes in Uganda", *Health Economics*. Vol. 27, pp. 733–745.

Peratsakis, C., Powell, J., Findley, M., Baker, J. and C. Weaver (2012) "Geocoded Activity-Level Data from the Government of Malawi's Aid Management Platform", Washington D.C. AidData and the Robert S. Strauss Center for International Security and Law.

Perumal, N., Bassani, D. G. and D. E. Roth (2018) "Use and Misuse of Stunting as a Measure of Child Health", *The Journal of Nutrition*, 148(3), pp. 311–315.

Pickbourn, L. and L. Ndikumana (2019) "Does Health Aid Reduce Infant and Child Mortality from Diarrhoea in Sub-Saharan Africa?", *The Journal of Development Studies*, 55(10), pp. 2212–2231.

Rodrik, D. (2010). Diagnostics Before Prescription. *Journal of Economic Perspectives*, 24(3), 33-44.

Ruel, M. T. and H. Alderman (2013) “Nutrition-sensitive interventions and programmes: how can they help to accelerate progress in improving maternal and child nutrition?”, *The Lancet*, 382(9891), pp. 536-551.

Ruel-Bergeron, J. C., Hurley, K. M., Kang, Y., Aburto, N., Farhikhtah, A., Dinucci, A., . . . Phuka, J. (2019) “Monitoring and evaluation design of Malawi’s Right Foods at the Right Time nutrition program”, *Evaluation and Program Planning*, 73, pp. 1-9.

Rustad, S. A., Rosvold, E. L. and H. Buhaug (2020) “Development Aid, Drought, and Coping Capacity”, *The Journal of Development Studies*, 56(8), pp. 1578-1593.

Sharma, I. K., Di Prima, S., Essink, D. and J. E. W. Broerse (2021) “Nutrition-Sensitive Agriculture: A Systematic Review of Impact Pathways to Nutrition Outcomes”, *Advances in Nutrition*, 12(1), pp. 251-275.

Strandow, D., Findley, M., Nielson, D. and J. Powell (2011) “The UCDP Aid Data codebook on Geo-referencing Foreign Aid. Version 1.1”, Uppsala Conflict Data Program, Paper no. 4, Uppsala University, available at: https://www.aiddata.org/publications/the-ucdp-and-aiddata-codebook-on-georeferencing-aid-version-1-1.

SUN (2022) “Scaling Up Nutrition (SUN)”. Malawi [Internet]. Washington (DC): SUN Movement; 2011. Accessed 2022-05-20. Available from: http://scalingupnutrition.org/sun-countries/malawi#tab-3.

Taylor, E. M., Hayman, R., Crawford, F., Jeffery, P. and J. Smith (2013) “The impact of official development aid on maternal and reproductive health outcomes: A systematic review”, *PLOS ONE*, 8(2), e5627, pp. 1-18.

The Economist (2022) “The coming food catastrophe. War is tipping a fragile world towards mass hunger. Fixing that is everyone’s business”, *The Economist*, May 19, available at: https://www.economist.com/leaders/2022/05/19/the-coming-food-catastrophe

UN (2015) “Transforming our world: the 2030 Agenda for Sustainable Development”, Resolution adopted by the General Assembly on 25 September 2015, available at: https://www.un.org/ga/search/view_doc.asp?symbol=A/RES/70/1andLang=E

UN (2022) “Secretary-General's remarks to the Global Food Security Call to Action Ministerial Meeting”, Secretary-General António Guterres, United Nations, 18 May 2022, available at: https://www.un.org/sg/en/content/sg/speeches/2022-05-18/secretary-generals-remarks-the-global-food-security-call-action-ministerial%C2%A0

UNICEF (2013) “Improving child nutrition: The achievable imperative for global progress”, United Nations Children’s Fund, April 2013, available at: https://www.unicef.cn/media/7451/file/IMPROVING%20CHILD%20NUTRITION.pdf

UNICEF (2022) “Malnutrition data”, Accessed 2022-05-30. Available at: https://data.unicef.org/resources/dataset/malnutrition-data/

UNICEF / WHO / World Bank Group (2021) “Levels and trends in child malnutrition”, UNICEF / WHO / World Bank Group Joint Child Malnutrition Estimates. Available at: https://www.who.int/publications/i/item/9789240025257

Usman, M. A., and Haile, M. G. (2022). Market access, household dietary diversity and food security: Evidence from Eastern Africa. *Food Policy*, 113, article 102374.

Vaivada, T., Akseer, N., Akseer, S., Somaskandan, A., Stefopulos, M., and Bhutta, Z. A. (2020) “Stunting in childhood: an overview of global burden, trends, determinants, and drivers of decline”, *The American Journal of Clinical Nutrition*, 112(Supplement_2), 777S-791S.

Vaivada, T., Lassi, Z. S., Irfan, O., Salam, R. A., Das, J. K., Oh, C., . . . Sharma, N. (2022) “What can work and how? An overview of evidence-based interventions and delivery strategies to support health and human development from before conception to 20 years”, *The Lancet*, 399(10337), pp. 1810-1829.

Victora, C. G., Christian, P., Vidaletti, L. P., Gatica-Domínguez, G., Menon, P., and Black, R. E. (2021) “Maternal and Child Undernutrition Progress 1 Revisiting maternal and child undernutrition in low-income and middle-income countries: variable progress towards an unfinished agenda. target, 2, 12”, *The Lancet*, 397(10282), pp. 1388-1399.

Vidmar, S., Carlin, J., Hesketh, K. and T. Cole (2004) “Standardizing Anthropometric Measures in Children and Adolescents with New Functions for Egen”, *The Stata Journal*, 4(1), pp. 50–55.

Vidmar, S., Cole, T.J. and H. Pan (2013) “Standardizing Anthropometric Measures in Children and Adolescents with Functions for Egen: Update”, *The Stata Journal,* 13(2), pp. 366-378.

Wayoro, D. and L: Ndikumana (2020) “Impact of development aid on infant mortality: Micro-level evidence from Côte d'Ivoire”, *African Development Review*. Vol. 32, p. 432–445.

Widmer, P. and N. Zurlinden (2021) "Born in the Right Place? Health Ministers, Foreign Aid and Infant Mortality”, Economics Working Paper Series 19-11, University of St. Gallen, School of Economics and Political Science (last updated April 2021).

Williamson, C. R. (2008) “Foreign aid and human development: The impact of foreign aid to the health sector”, *Southern Economic Journal*, 75(1), pp. 188–207.

Wilson, S. E. (2011) “Chasing Success: Health Sector Aid and Mortality”, *World Development*, 39(11), pp. 2032–2043

World Food Programme (2021) “School Feeding Programme Factsheet”, WFP Malawi, May 2021, accessed 2022-12-19 at: https://www.wfp.org/publications/2021-school-feeding-programme-factsheet-wfp-malawi-may-2021

WHO (2015) “Stunting in a nutshell”, Departmental news, 19 November 2015, World Health Organization, available at: https://www.who.int/news/item/19-11-2015-stunting-in-a-nutshell

Wright, C. M., Macpherson, J., Bland, R., Ashorn, P., Zaman, S., and Ho, F. K. (2021) “Wasting and stunting in infants and young children as risk factors for subsequent stunting or mortality: longitudinal analysis of data from Malawi, South Africa, and Pakistan”, *The Journal of Nutrition*, 151(7), pp. 2022-2028.

Yogo, U. T. and D. Mallaye (2015) “Health aid and health improvement in sub-Saharan Africa: Accounting for the heterogeneity between stable states and post-conflict states”, *Journal of International Development*, 7(7), pp. 1178–1196.

Figures and tables

Table 1: The impact of aid exposure on stunting among children aged 0–4. Treatment refers to child being born 0–3 years after project start.

VARIABLES	(1) Year FEs	(2) District-year Fes	(3) Grid-year FEs	(4) Ever-treated sample district-year FEs	(5) Ever-treated sample grid-year FEs	(6) Born before/after sample district-year FEs	(7) Born before/after sample grid-year FEs
Panel A: Dependent variable is Stunting (height-for-age z-score<=2):							
Child treated	-0.017**	-0.018**	-0.018**	-0.025***	-0.022**	-0.063***	-0.062***
	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.020)	(0.019)
Constant	0.416***	0.319***	0.378***	0.311***	0.382***	0.210***	0.168***
	(0.014)	(0.047)	(0.021)	(0.022)	(0.023)	(0.037)	(0.056)
Observations	26,604	26,604	26,604	20,251	20,251	6,543	6,543
R-squared	0.099	0.106	0.112	0.109	0.115	0.098	0.109
Panel B: Dependent variable is Severe stunting (height-for-age z-score<=3:							
Child treated	-0.016**	-0.015**	-0.017**	-0.019**	-0.016**	-0.044***	-0.035**
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.017)	(0.018)
Constant	0.258***	0.143***	0.190***	0.150***	0.191***	0.130***	0.202***
	(0.013)	(0.032)	(0.017)	(0.018)	(0.019)	(0.031)	(0.049)
Observations	26,604	26,604	26,604	20,251	20,251	6,543	6,543
R-squared	0.063	0.074	0.079	0.077	0.082	0.071	0.078

Robust standard errors (clustered at the survey cluster level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Child treated* refers to child being born 0–3 years after the start of an aid project within 10 km. All estimations include controls for the child's gender, age in year dummies, whether they are a twin, the ethnic and religious affiliations of the mother, and whether the household lives in an urban area. There are 6 survey years, 26 districts, and 55 grid cells, giving 146 district-by-survey-year FEs and 272 grid-cell-by-survey-year FEs. In columns 6-7, where we compare children born 0-3 years after project start to children born 2-4 years prior to project start, we restrict the sample to children born 1998–2008.

Table 2: The impact of aid exposure on height-for-age z-score among children aged 0–4. Treatment refers to child being born 0–3 years after project start.

VARIABLES	(1) Year FEs	(2) District-year Fes	(3) Grid-year FEs	(4) Ever-treated sample district-year FEs	(5) Ever-treated sample grid-year FEs	(6) Born before/after sample district-year FEs	(7) Born before/after sample grid-year FEs
Child treated	0.062**	0.060**	0.062**	0.085**	0.073**	0.252***	0.255***
	(0.029)	(0.030)	(0.030)	(0.034)	(0.034)	(0.070)	(0.068)
Constant	-1.364***	-0.818***	-1.108***	-0.812***	-1.104***	-0.400***	-0.282
	(0.050)	(0.131)	(0.066)	(0.070)	(0.075)	(0.116)	(0.239)
Observations	26,604	26,604	26,604	20,251	20,251	6,543	6,543
R-squared	0.136	0.149	0.154	0.149	0.154	0.147	0.159

Robust standard errors (clustered at the survey cluster level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Child treated* refers to child being born 0–3 years after the start of an aid project within 10 km. All estimations include controls for the child's gender, age in year dummies, whether they are a twin, the ethnic and religious affiliations of the mother, and whether the household lives in an urban area. There are 6 survey years, 26 districts, and 55 grid cells, giving 146 district-by-survey-year FEs and 272 grid-cell-by-survey-year FEs. in columns 6-7, where we compare children born 0-3 years after project start to children born 2-4 years prior to project start, we restrict the sample to children born 1998–2008.

Table 3: Comparing the effects of bilateral and multilateral aid

VARIABLES	(1) Stunting	(2) Severe stunting	(3) HAZ score
Child treated_bilateral aid	-0.011	0.005	-0.036
	(0.012)	(0.009)	(0.039)
Child treated_multilateral aid	-0.016	-0.026***	0.100***
	(0.010)	(0.008)	(0.034)
Constant	0.378***	0.190***	-1.106***
	(0.021)	(0.017)	(0.066)
Observations	26,604	26,604	26,604
R-squared	0.112	0.079	0.154

Robust standard errors (clustered at the survey cluster level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Child treated_bilateral aid* refers to child being born 0–3 years after the start of a bilateral aid project within 10 km. *Child treated_multilateral aid* refers to child being born 0–3 years after the start of a multilateral aid project within 10 km. All estimations include grid-cell-by-survey-year fixed effects and controls for the child's gender, age in year dummies, whether the child is a twin, the ethnic and religious affiliations of the mother, and whether the household lives in an urban area.

Figure 1: Malawi aid project sites and DHS survey clusters

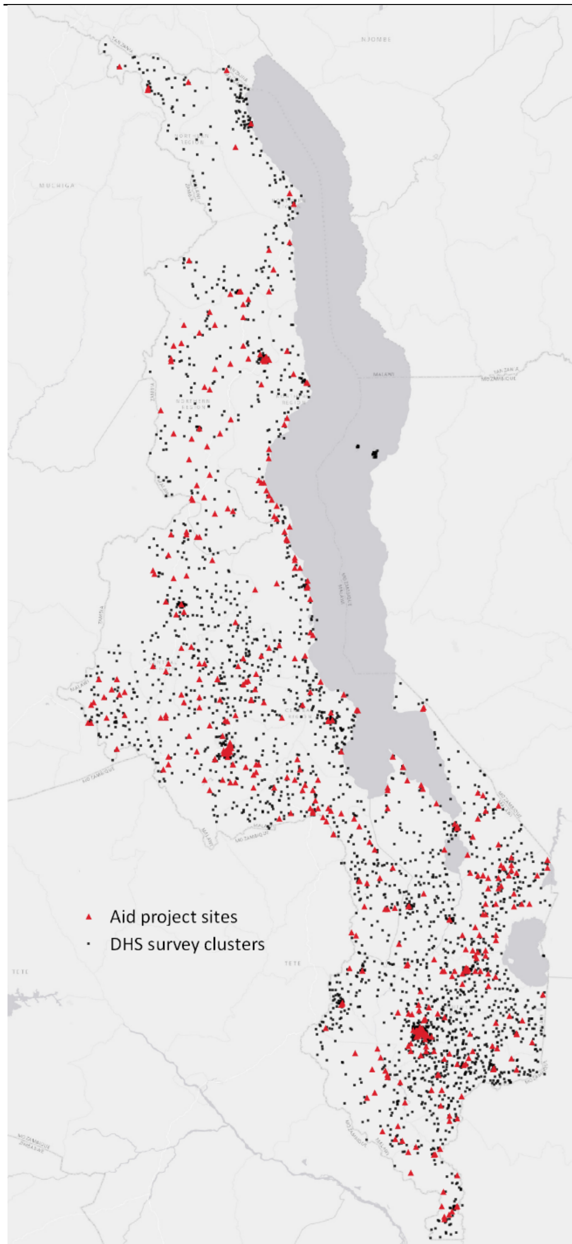
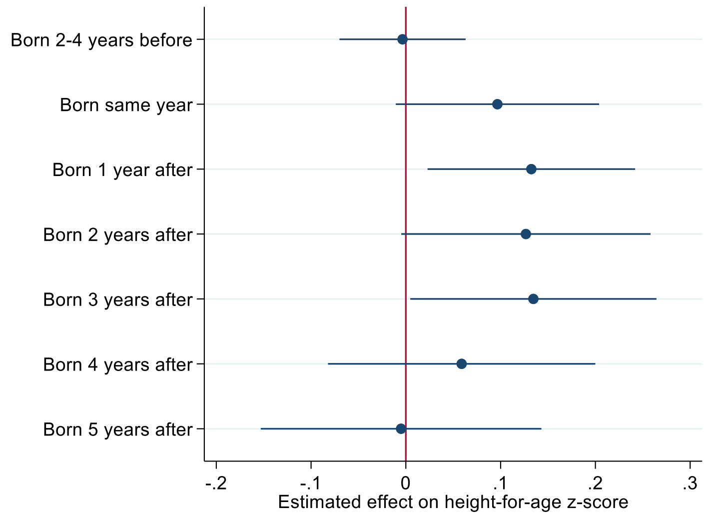
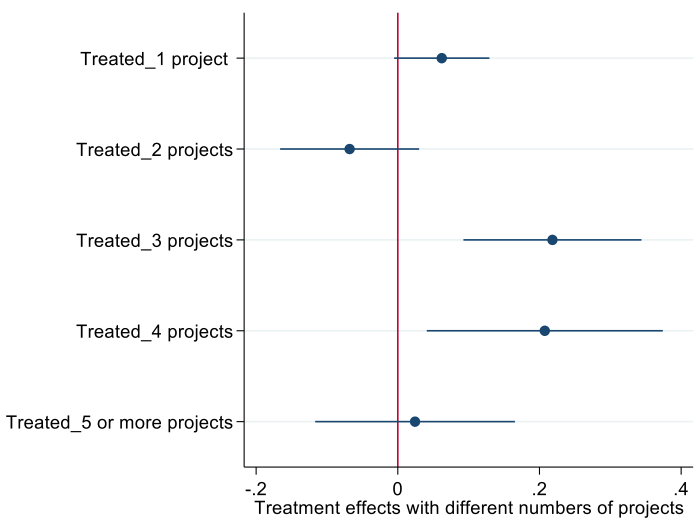


Figure 2: Estimated effect of birth year in relation to year of project start on HAZ score



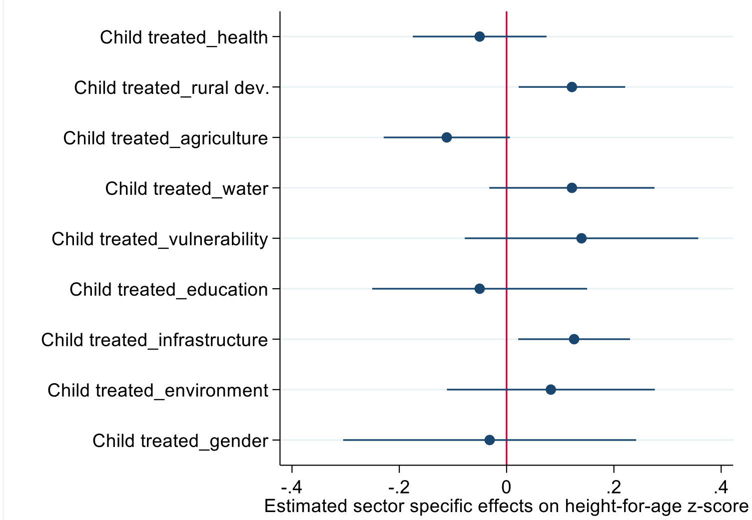
Notes: Point estimates with 95% confidence intervals. Based on full sample estimation with child and household controls and grid-cell-by-survey-year fixed effects. Treatment focuses on children born within X years of the first project starting in the area.

Figure 3: Comparing treatment effects with different numbers of projects starting 0–3 years prior to child's birth, dependent var. is HAZ score



Notes: Point estimates with 95% confidence intervals; based on estimations including five treatment dummies (for having 1, 2, 3, 4, or 5 or more projects starting 0–3 years prior to child's birth within the 10 km cut-off), and child and household controls and grid-cell-by-survey-year fixed effects.

Figure 4: Estimated sector specific effects on height-for-age z-score



Notes: Point estimates with 95% confidence intervals; based on rural sample estimation with child and household controls and grid-cell-by-survey-year fixed effects.

Appendix

Table A1: Donor composition of project sites

Donor	Freq.	Percent	Cum.
African Development Bank (AfDB)	79	10.15	10.15
Arab Bank for Economic Development in..	8	1.03	11.18
Australian Agency for International D..	7	0.90	12.08
European Union (EU)	313	40.23	52.31
Flemish International Cooperation Agency	4	0.51	52.83
German Agency for International Cooperation	53	6.81	59.64
Icelandic International Development A..	6	0.77	60.41
International Fund for Agricultural D..	3	0.39	60.80
Irish Aid	11	1.41	62.21
Japan International Cooperation Agency	1	0.13	62.34
KFW Bankengruppe	2	0.26	62.60
Kuwait Fund	5	0.64	63.24
Norwegian Agency for Development Cooperation	117	15.04	78.28
OPEC Fund	8	1.03	79.31
People's Republic of China	1	0.13	79.43
UK Department for International Development	69	8.87	88.30
UNDP	2	0.26	88.56
UNHCR	2	0.26	88.82
US Agency for International Development	8	1.03	89.85
US Center for Disease Control and Prevention	12	1.54	91.39
World Bank	66	8.48	99.87
World Food Program (WFP)	1	0.13	100.00
Total	778	100.00	

Table A2: Descriptions of key variables

Outcome variables

Height-for-age z-score: The number of standard deviations by which the observed child's height-for-age (in months) differs from the mean in the WHO Child Growth Standards reference population.

Stunted: Dummy variable equal to one if the child's height-for-age is more than two standard deviations below the mean of the reference population (HAZ<=-2), 0 otherwise.

Severely stunted: Dummy variable equal to one if the child's height-for-age is more than three standard deviations below the mean of the reference population (HAZ<=-3), 0 otherwise.

Variables relating to treatment

Child treated: Dummy variable=1 if the child is born 0–3 years after the start of any aid project within 10 km, 0 otherwise

Child treated_bilateral aid: Dummy variable=1 if the child is born 0–3 years after the start of a bilateral aid project within 10 km, 0 otherwise.

Child treated_multilateral aid: Dummy variable=1 if the child is born 0–3 years after the start of a multilateral aid project within 10 km, 0 otherwise.

Born same year as project start: Dummy variable=1 if the child is born in the same year as the start of an aid project within 10 km, 0 otherwise.

Born X years after project start: Dummy variable=1 if the child is born X years after the start of an aid project within 10 km, 0 otherwise. Separate dummies for being born 1, 2, 3, 4, and 5 years after project start.

Born 0-3 years after project start: Dummy variable=1 if the child is born 0–3 years after the start of an aid project within 10 km, 0 otherwise.

Born 2-4 years ahead of project start: Dummy variable=1 if the child is born 2–4 years before the start of an aid project within 10 km, 0 otherwise.

Evertreated cluster: Dummy variable=1 if child living in a survey cluster that has a past, current or future aid project located within 10 km, 0 otherwise.

Child treated_health: Dummy variable=1 if the child is born 0–3 years after the start of a "Health" aid project within 10 km, 0 otherwise.

Child treated_rural dev.: Dummy variable=1 if the child is born 0–3 years after the start of an "Integrated Rural Development" aid project within 10 km, 0 otherwise.

Child treated_agriculture: Dummy variable=1 if the child is born 0–3 years after the start of an "Agriculture" aid project within 10 km, 0 otherwise.

Child treated_water: Dummy variable=1 if the child is born 0–3 years after the start of a "Water, Sanitation and Irrigation" aid project within 10 km, 0 otherwise

Child treated_vulnerability: Dummy variable=1 if the child is born 0–3 years after the start of a "Vulnerability, Disaster and Risk Management" aid project within 10 km, 0 otherwise.

Child treated_gender: Dummy variable=1 if the child is born 0–3 years after the start of a "Gender, Youth Development and Sports" aid project within 10 km, 0 otherwise.

Child treated_environment: Dummy variable=1 if the child is born 0–3 years after the start of an "Environment, Lands and Natural Resources" aid project within 10 km, 0 otherwise.

Child treated_infrastructure: Dummy variable=1 if the child is born 0–3 years after the start of a "Roads, Public Works and Transport" aid project within 10 km, 0 otherwise.

Child treated_education: Dummy variable=1 if the child is born 0–3 years after the start of an "Education" aid project within 10 km, 0 otherwise.

Treated_X projects: Dummy variable=1 if the child has X projects within 10 km, 0 otherwise. Separate dummies for having 1, 2, 3, 4, or 5 or more projects within the 10 km cutoff.

Child treated by project mentioning child/nutrition keywords: Dummy variable=1 if the child is treated (born 0–3 years after the start of a project within 10 km) mentioning at least one of the following words: child, infant, nutrition, food, feeding or natal in the project activity descriptions provided by AidData, within the 10 km cutoff.

Child treated by project not mentioning child/nutrition keywords: Dummy variable=1 if the child is treated by a project not mentioning any of the above keywords.

Individual control variables

Girl: Dummy variable=1 if the child is a girl; 0 otherwise.

Age: Dummies for the child's age in years.

Twin: Dummy variable=1 if the child is a twin, 0 otherwise.

Urban: Dummy variable=1 if the child lives in an urban area; 0 otherwise.

Ethnic group of mother: Dummies for the ethnic group of the child's mother (Chewa, Tumbuka, Lomwe, Tonga, Yao, Sena, Nkhonde, Ngoni and 'other' category, respectively).

Religious affiliation of mother: Dummies for the religious affiliation of the child's mother (Catholic, CCAP, Anglican, Seventh Day Adventist/Baptist, "other Christian," Muslim, no religion, and "other," respectively).

Year and spatial fixed effects

Year FEs: Dummies for the 6 survey years (2000, 2004, 2005, 2010, 2015, 2016).

District-year FEs: 146 district by survey year dummies.

Grid-year FEs: 272 grid cell (55*55 km) by survey year dummies.

Table A3: Summary statistics

Variables	Obs.	Mean	Std dev.	Min	Max
Outcome variables					
Height-for-age z-score	26604	-1.76	1.6	-5	4.97
Stunted (z<=-2)	26604	0.47	0.5	0	1
Severely stunted (z<=-3)	26604	0.21	0.41	0	1
Variables relating to treatment					
Child treated	26604	0.26	0.44	0	1
Child treated_bilateral	26604	0.13	0.33	0	1
Child treated_multilateral	26604	0.2	0.4	0	1
Born same year as project start	26604	0.04	0.2	0	1
Born 1 year after project start	26604	0.04	0.2	0	1
Born 2 years after project start	26604	0.03	0.17	0	1
Born 3 years after project start	26604	0.03	0.18	0	1
Born 4 years after project start	26604	0.03	0.18	0	1
Born 5 years after project start	26604	0.03	0.16	0	1
Born 2-4 years before project start	26604	0.14	0.35	0	1
Born 0-3 years after project start	26604	0.15	0.36	0	1
Evertreated cluster	26604	0.76	0.43	0	1
Child treated_health	26604	0.09	0.28	0	1
Child treated_rural dev	26604	0.11	0.31	0	1
Child treated_agriculture	26604	0.05	0.21	0	1
Child treated_water	26604	0.05	0.21	0	1
Child treated_vulnerability	26604	0.01	0.09	0	1
Child treated_education	26604	0.02	0.14	0	1
Child treated_infrastructure	26604	0.11	0.31	0	1
Child treated_environment	26604	0.02	0.14	0	1
Child treated_gender	26604	0.01	0.1	0	1
Treated_1 project	26604	0.12	0.32	0	1
Treated_2 projects	26604	0.05	0.21	0	1
Treated_3 projects	26604	0.03	0.17	0	1
Treated_4 projects	26604	0.02	0.14	0	1
Treated_5 or more projects	26604	0.04	0.19	0	1
Child treated by project mentioning child/nutrition keywords	26604	0.12	0.31	0	1
Child treated by project ***not*** *mentioning child/nutrition keywords*	26604	0.15	0.35	0	1
Control variables					
Child age in years	26604	1.87	1.41	0	4
Child age in months	26604	27.91	17.08	0	59
Girl	26604	1.51	0.5	1	2
Twin	26604	0.03	0.17	0	1
Urban	26604	0.14	0.35	0	1

Table A4: Sectoral breakdown of aid projects

Sector	Freq.	Percent	Share of projects mentioning child / nutrition keywords	Multilateral share
Panel A: All projects (multilateral+bilateral)				
Agriculture	65	8.35	0.34	0.48
Democratic Governance	19	2.44	0.26	0.37
Economic Governance	9	1.16	0.00	0.00
Education	32	4.11	0.31	0.69
Environment, Lands and Natural Resources.	26	3.34	0.77	0.54
Gender, Youth Development and Sports	19	2.44	0.00	0.11
Health	148	19.02	0.69	0.06
Rural Development	248	31.88	0.00	0.86
Infrastructure	146	18.77	0.00	0.96
Tourism, Wildlife and Culture	4	0.51	0.25	0.00
Vulnerability, Disaster and Risk Management	10	1.29	0.90	0.10
Water, Sanitation, and Irrigation	52	6.68	0.31	0.81
Total	778	100.00	0.24	0.62
Panel B: Multilateral projects				
Agriculture	31	6.43	0.23	
Democratic Governance	7	1.45	0.14	
Education	22	4.56	0.36	
Environment, Lands and Natural Resour.	14	2.90	1.00	
Gender, Youth Development and Sports	2	0.41	0.00	
Health	9	1.87	0.00	
Rural Development	214	44.40	0.00	
Infrastructure	140	29.05	0.00	
Vulnerability, Disaster and Risk Mana.	1	0.21	1.00	
Water, Sanitation and Irrigation	42	8.71	0.38	
Total	482	100.00	9.75	
Panel C: Bilateral projects				
Agriculture	34	11.49	0,44	
Democratic Governance	12	4.05	0,33	
Economic Governance	9	3.04	0,00	
Education	10	3.38	0,20	
Environment, Lands and Natural Resours.	12	4.05	0,50	
Gender, Youth Development and Sports	17	5.74	0,00	
Health	139	46.96	0,73	
Rural Development	34	11.49	0,00	
Infrastructure	6	2.03	0,00	
Tourism, Wildlife and Culture	4	1.35	0,25	
Vulnerability, Disaster and Risk Mana.	9	3.04	0,89	
Water, Sanitation and Irrigation	10	3.38	0,00	
Total	296	100.00	46.62	

Notes: The share of projects mentioning child / nutrition keywords refers to mentioning at least one of the following words: child, infant, nutrition, food, feeding or natal (as in prenatal, neonatal and postnatal) in the project activity descriptions provided by AidData.

Table A5: Treatment effects when considering projects mentioning child/nutrition-related keywords

VARIABLES	(1) HAZ-score	(2) Stunting	(3) Severe stunting
*Child treated by project **not** mentioning child/nutrition keywords*	0.079**	-0.024**	-0.039***
	(0.040)	(0.012)	(0.009)
Child treated by project mentioning child/nutrition keywords	0.076*	-0.025*	-0.017
	(0.046)	(0.014)	(0.012)
Constant	-1.125***	0.363***	0.183***
	(0.072)	(0.023)	(0.019)
Observations	22,919	22,919	22,919
R-squared	0.160	0.117	0.084

Robust standard errors (clustered at the survey cluster level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Based on rural sample estimation. Treatment refers to a child being born 0–3 years after the start of an aid project within 10 km*,* either a project mentioning at least one of the following words: child, infant, nutrition, food, feeding or natal (as in prenatal, neonatal and postnatal) in the project activity descriptions provided by AidData, or a project not mentioning these keywords. All estimations include grid-cell-by-survey-year fixed effects and controls for the child's gender, age in year dummies, whether the child is a twin, the ethnic and religious affiliations of the mother, and whether the household lives in an urban area.

Table A6: Estimations breaking down treatment by individual donors

VARIABLES	(1) Stunting	(2) Severe stunting	(3) HAZ score
Child treated_AFDB	0.038	0.019	-0.047
	(0.023)	(0.018)	(0.077)
Child treated_EU	-0.012	-0.036***	0.079*
	(0.014)	(0.011)	(0.045)
Child treated_WB	-0.025*	-0.021*	0.135***
	(0.014)	(0.012)	(0.050)
Child treated_Germany	0.002	-0.014	0.002
	(0.016)	(0.012)	(0.058)
Child treated_Norway	-0.003	0.011	-0.027
	(0.018)	(0.013)	(0.051)
Child treated_UK	0.013	0.015	-0.082
	(0.019)	(0.013)	(0.055)
Child treated_Other	-0.032	-0.010	0.045
	(0.021)	(0.015)	(0.066)
Constant	0.378***	0.190***	-1.109***
	(0.021)	(0.017)	(0.066)
Observations	26,604	26,604	26,604
R-squared	0.112	0.080	0.154

Robust standard errors (clustered at the survey cluster level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Child treated_XX* refers to child being born 0–3 years after the start of an aid project of the specific donor within 10 km; We consider donors with at least 50 recorded project sites (see Table A1), as well as an "other" category capturing the remaining donors; All estimations include grid-cell-by-survey-year fixed effects and controls for the child's gender, age in year dummies, whether the child is a twin, the ethnic and religious affiliations of the mother, and whether the household lives in an urban area.

Table A7: Estimations with cluster-by-year fixed effects

VARIABLES	(1) Stunting	(2) Severe stunting	(3) HAZ score
Child treated	-0.021	-0.025**	0.043
	(0.015)	(0.012)	(0.052)
Constant	0.423***	0.316***	-1.496***
	(0.025)	(0.021)	(0.078)
Observations	22,919	22,919	22,919
R-squared	0.214	0.174	0.255

Robust standard errors (clustered at the survey cluster level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Child treated* refers to child being born 0–3 years after the start of an aid project within 10 km. All estimations include controls for interview year, the child's gender, age in year dummies, whether the child is a twin, the ethnic and religious affiliations of the mother, and whether the household lives in an urban area. All estimations include cluster-by-year fixed effects.

Table A8: Estimations with mother fixed effects

VARIABLES	(1) Stunting	(2) Severe stunting	(3) HAZ score
Child treated	-0.051	-0.037	0.062
	(0.031)	(0.023)	(0.103)
Constant	-0.263***	-0.040***	0.096
	(0.020)	(0.015)	(0.070)
Observations	1,959	1,959	1,959
R-squared	0.647	0.614	0.672

Robust standard errors (clustered at the survey cluster level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Child treated* refers to child being born 0-3 years after the start of an aid project within 10 km. All estimations include controls for interview year, the child's gender, age in year dummies, and whether the child is a twin. All estimations include mother fixed effects. We restrict the sample to kids in families with both treated and untreated children.

Table A9: Estimations using coarsened exact matching (CEM)

VARIABLES	(1) Stunting	(2) Severe stunting	(3) HAZ-score
Child treated	-0.030***	-0.025***	0.101***
	(0.010)	(0.007)	(0.032)
Constant	0.414***	0.197***	-1.344***
	(0.015)	(0.012)	(0.051)
Observations	20,871	20,871	20,871
R-squared	0.027	0.026	0.030

Robust standard errors (clustered at the survey cluster level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Child treated* refers to child being born 0–3 years after the start of an aid project within 10 km. We match exactly on district, urban residence, the child's sex, age in years, year of birth, and on interview years coarsened by survey wave in the full sample. The estimations are based on the resulting matched observations, and additionally control for ethnicity, religion, whether the child is a twin and the (un-coarsened) interview year dummies.

Table A10: Exploring treatment effect heterogeneity

VARIABLES	(1) Stunting	(2) Severe stunting	(3) z-score	(4) Stunting	(5) Severe stunting	(6) z-score	(7) Stunting	(8) Severe stunting	(9) z-score	(10) Stunting	(11) Severe stunting	(12) z-score
Child treated	0.011	0.031**	-0.034	-0.020*	-0.014*	0.077**	-0.016	-0.011	0.056	-0.010	-0.016**	0.046
	(0.018)	(0.015)	(0.062)	(0.011)	(0.008)	(0.036)	(0.011)	(0.008)	(0.036)	(0.009)	(0.008)	(0.032)
*Child treated*Rural*	-0.037*	-0.062***	0.123*									
	(0.020)	(0.016)	(0.069)									
*Child treated*Poorest*				0.014	-0.002	-0.069						
				(0.015)	(0.012)	(0.049)						
*Child treated*Little educ.*							-0.001	-0.014	0.004			
							(0.015)	(0.012)	(0.049)			
*Child treated*Young mother*										-0.047**	-0.011	0.100*
										(0.019)	(0.015)	(0.059)
Constant	0.255***	0.094***	-0.663***	0.361***	0.176***	-1.062***	0.351***	0.158***	-1.033***	0.375***	0.189***	-1.100***
	(0.025)	(0.021)	(0.084)	(0.020)	(0.017)	(0.065)	(0.021)	(0.017)	(0.067)	(0.021)	(0.017)	(0.066)
Observations	26,747	26,747	26,747	26,477	26,477	26,477	26,745	26,745	26,745	26,747	26,747	26,747
R-squared	0.113	0.080	0.155	0.117	0.083	0.158	0.114	0.082	0.156	0.114	0.080	0.156

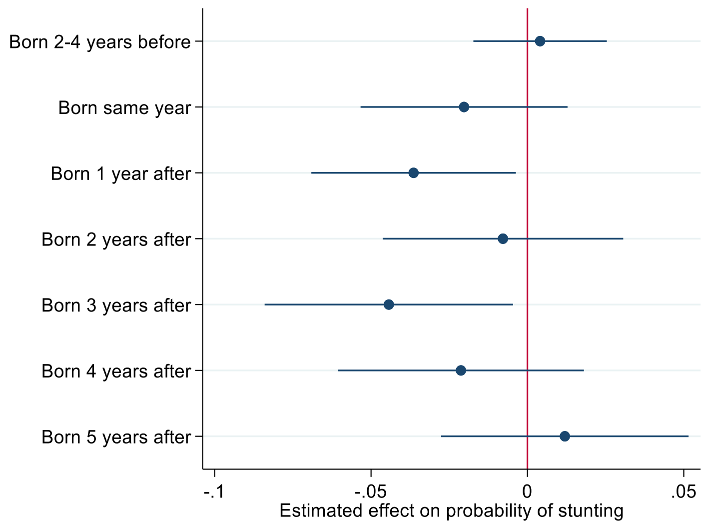
Robust standard errors (clustered at the survey cluster level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Child treated* refers to child being born 0–3 years after the start of at least one project within 10 km. All estimations include grid-cell-by-survey-year FEs, controls for the child's gender, age in year dummies, whether the child is a twin, the ethnic and religious affiliations of the mother, and whether the household lives in an urban area. *Child treated*Rural* interacts the treatment dummy with a rural dummy. *Child treated*Poorest* interacts the treatment dummy with a dummy for belonging to the two poorest wealth quintiles. *Child treated*Little educ.* interacts the treatment dummy with a dummy for the mother having less than the median number of years of education (<4). *Child treated*Young mother* interacts the treatment dummy with a dummy for the mother being below age 20 when she gave birth to the child in question. All estimations include the component variable (not presented) in the interaction term, i.e., *Poorest* in columns 4–6, *Little educ* in columns 7–9 and *Young mother* in columns 10–12.

Table A11: Exploring mechanisms. Dependent variable is the continuous HAZ score. Estimated on the 'ever-treated' sample.

VARIABLES	(1) Original model	(2) Wealth	(3) Tap water	(4) Toilet	(5) Fetch water	(6) Education	(7) Teenage birth	(8) Birth interval	(9) Antenatal visits	(10) Low BMI	(11) Low birth weight	(12) Diarrhea	(13) Child use bed net
Child treated	0.085**	0.060*	0.075**	0.075**	0.086**	0.076**	0.086**	0.070*	0.097***	0.084**	0.108***	0.085**	0.031
	(0.034)	(0.033)	(0.034)	(0.034)	(0.036)	(0.034)	(0.034)	(0.037)	(0.038)	(0.034)	(0.039)	(0.034)	(0.040)
Wealth index: Poorer		0.076**											
		(0.035)											
Wealth index: Middle		0.141***											
		(0.034)											
Wealth index: Richer		0.239***											
		(0.033)											
Wealth index: Richest		0.520***											
		(0.043)											
Tap water			0.098***										
			(0.034)										
Toilet				0.410***									
				(0.081)									
Time to fetch water					-0.002*								
					(0.001)								
Fetch water sq.					0.000*								
					(0.000)								
Years of education						0.027***							
						(0.004)							
Teenage birth							-0.14***						
							(0.028)						
Birth interv								0.001***					
								(0.000)					
Antenatal visit									0.060**				
									(0.027)				
BMI below 18.5										-0.188***			
										(0.048)			
Low birthweight											-0.252***		
											(0.050)		
Diarrhea												-0.095***	
												(0.014)	
Child use bed net													0.121***
													(0.032)
Constant	-0.812***	-0.959***	-0.834***	-0.822***	-0.794***	-1.006***	-0.792***	-0.710***	-0.919***	-0.801***	-0.808***	-0.769***	-1.34***
	(0.070)	(0.073)	(0.070)	(0.070)	(0.078)	(0.074)	(0.070)	(0.076)	(0.083)	(0.070)	(0.090)	(0.070)	(0.094)
Observations	20,251	20,035	19,947	19,941	18,581	20,249	20,103	20,251	14,791	13,147	11,984	20,223	15,654
R-squared	0.149	0.156	0.149	0.151	0.152	0.151	0.150	0.150	0.157	0.137	0.157	0.151	0.154

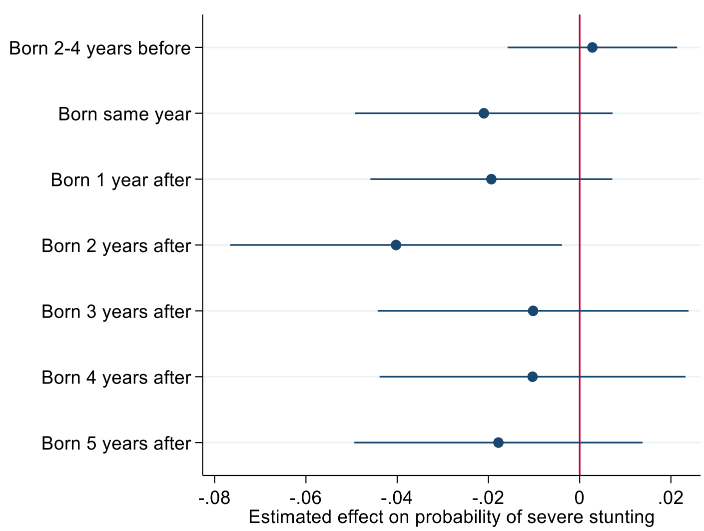
Note: Robust standard errors (clustered at the survey cluster level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All estimations include baseline controls and district-by-year fixed effects. Wealth index: index of assets grouped in quantiles; Tap water: access to piped water; Toilet: access to toilet with flush; Time fetch water in minutes; Education: years of schooling; Teenage birth; Birth spacing: at least three years since last birth; Antenatal visits: at least 4 visits; Low BMI: below 18.5: Low birth weight: below 2,500 grams; Diarrhea: child had diarrhea last two weeks; Child bed net: slept under any type of bed net last night

Figure A1: Estimated effect of birth year in relation to year of first project start on stunting



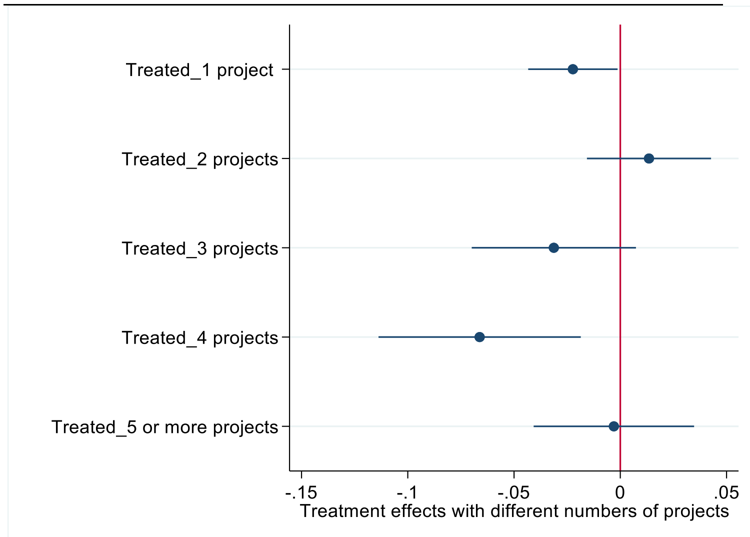
Notes: Point estimates with 95% confidence intervals. Based on full sample estimation with child and household controls grid-cell-by-survey-year fixed effects. Treatment focuses on children born within X years of the first project starting in the area.

Figure A2: Estimated effect of birth year in relation to year of first project start on severe stunting



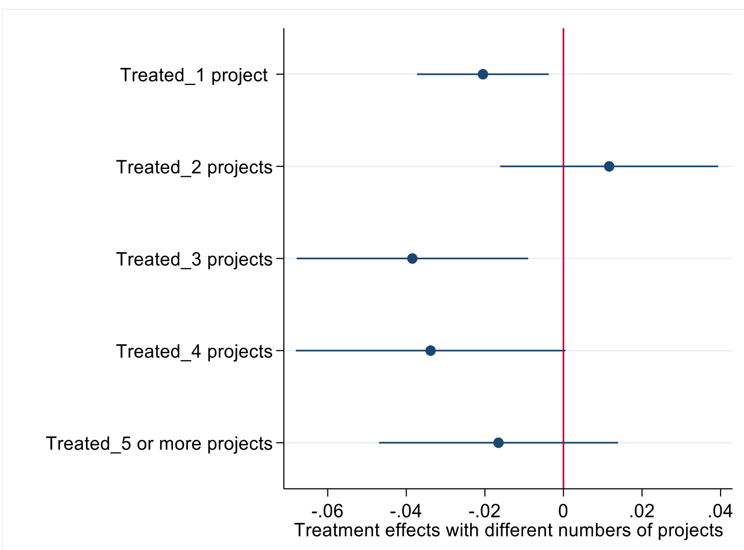
Notes: Point estimates with 95% confidence intervals. Based on full sample estimation with child and household controls and grid-cell-by-survey-year fixed effects. Treatment focuses on children born within X years of the first project starting in the area.

Figure A3: Comparing treatment effects with different numbers of project starting 0–3 years prior to child's birth, dependent var. is stunting



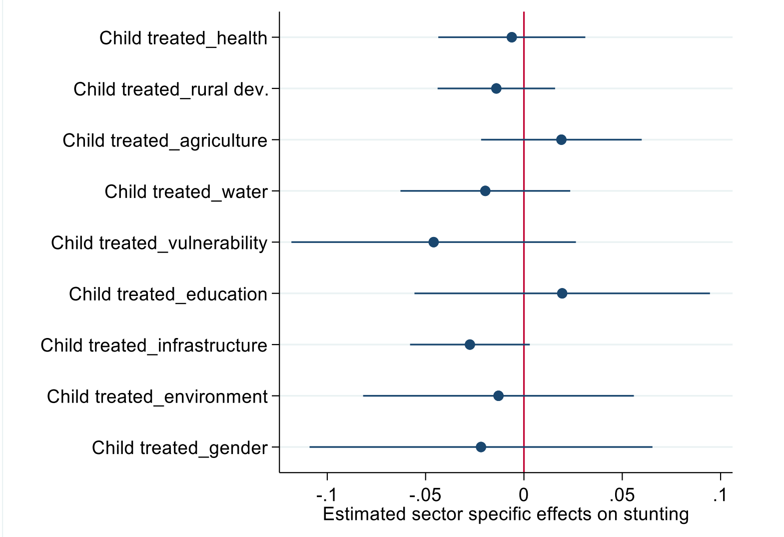
Notes: Point estimates with 95% confidence intervals. Based on estimations including five treatment dummies (for having 1, 2, 3, 4, or 5 or more projects starting 0–3 years prior to child's birth within the 10 km cut-off), and child and household controls and grid-cell-by-survey-year fixed effects.

Figure A4: Comparing treatment effects with different numbers of project starting 0–3 years prior to child's birth, dependent var. is severe stunting



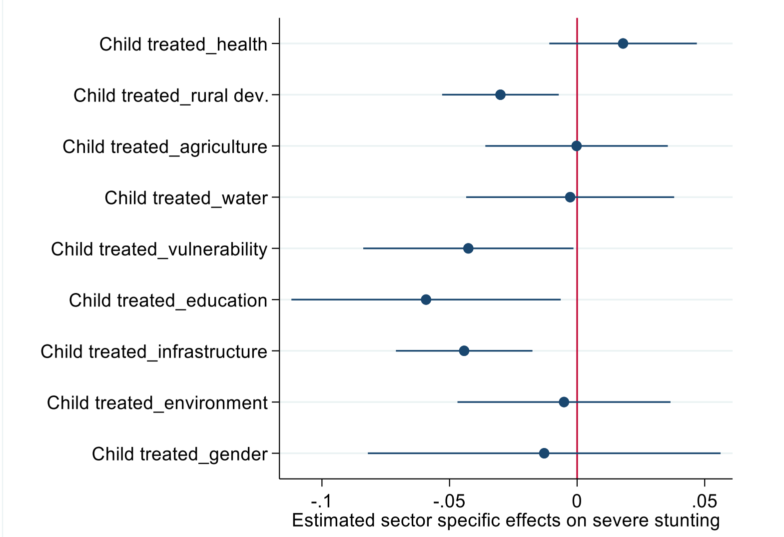
Notes: Point estimates with 95% confidence intervals. Based on estimations including five treatment dummies (for having 1, 2, 3, 4, or 5 or more projects starting 0–3 years prior to child's birth within the 10 km cut-off), and child and household controls and grid-cell-by-survey-year fixed effects.

Figure A5: Estimated sector specific effects on stunting



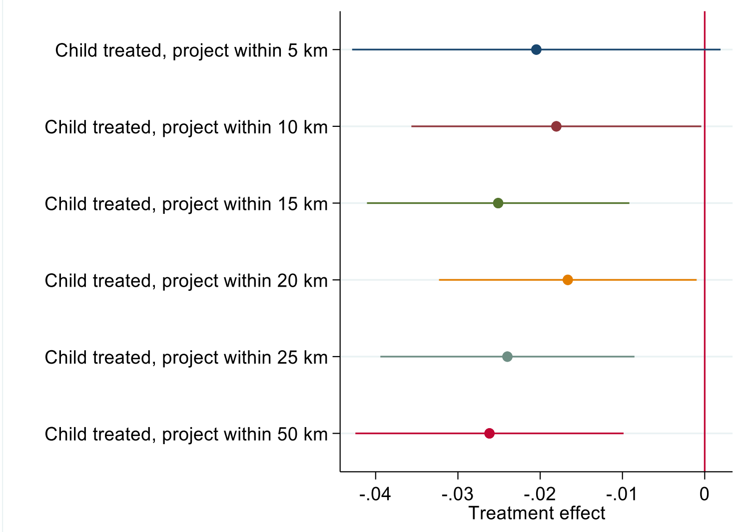
Notes: Point estimates with 95% confidence intervals. Based on rural sample estimation with child and household controls and grid-cell-by-survey-year fixed effects.

FigureA6: Estimated sector specific effects on severe stunting



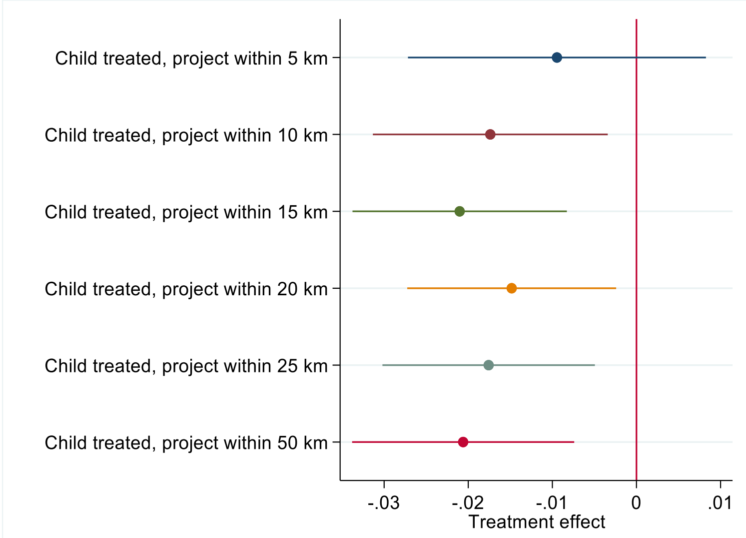
Notes: Point estimates with 95% confidence intervals. Based on rural sample estimation with child and household controls and grid-cell-by-survey-year fixed effects.

Figure A7: Estimated effects on stunting when using different geographical cut-offs



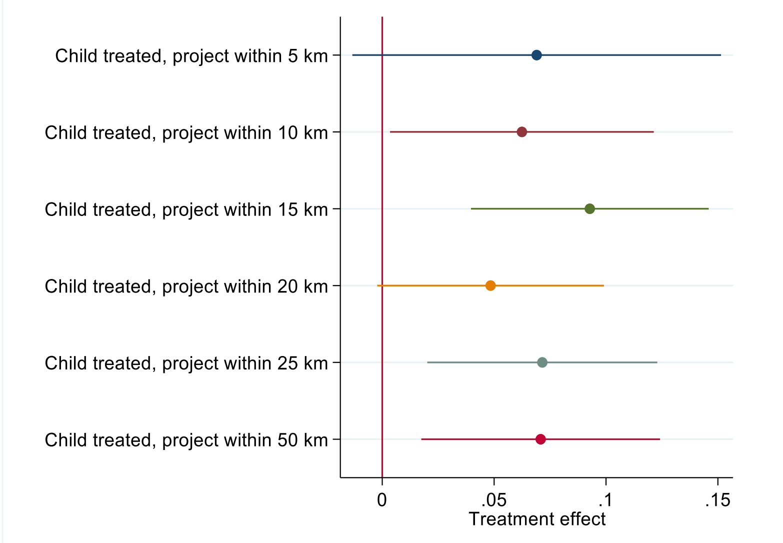
Notes: Point estimates with 95% confidence intervals. Coefficients from six separate estimations, each with child and household controls and grid-cell-by-survey-year fixed effects.

Figure A8: Estimated effects on severe stunting when using different geographical cutoffs



Notes: Point estimates with 95% confidence intervals. Coefficients from six separate estimations, each with child and household controls and grid-cell-by-survey-year fixed effects.

Figure A9: Estimated effects on height-for-age z-score when using different geographical cutoffs



Notes: Point estimates with 95% confidence intervals. Coefficients from six separate estimations, each with child and household controls and grid-cell-by-survey-year fixed effects.