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# Nutritional Benefits of Fostering: Evidence from Longitudinal Data in South Africa<sup>\*</sup>

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## Abstract

In sub-Saharan Africa, child fostering—a widespread practice in which a child moves out of the household of her biological parents—can have significant implications for a child’s overall well-being. Using longitudinal data from South Africa that includes individual tracking, we employ double machine learning techniques to evaluate the impact of fostering on nutrition, addressing biases related to selection into treatment and endogenous attrition, two common challenges in the literature. Our findings reveal that fostering reduces the probability of being stunted by 6.8 percentage points, corresponding to a 37 percent reduction compared to the mean prevalence. This improvement appears to be driven by foster children relocating to smaller, rural households, often including retired individuals, typically grandparents, who receive a pension. Furthermore, we find that it not only enhances the nutritional status of foster children but also benefits the nutrition of other children from sending households, suggesting that fostering can be mutually beneficial for both groups.

**Keywords:** Child Fostering, Nutrition, Machine Learning, South Africa

**JEL codes:** I15, J12, J13, O15, C14.

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# 1 Introduction

Although migration is one of the most studied adaptation mechanisms, child fostering – a specific form of migration in which children change their place of residence without their parents – has received less attention, despite its high prevalence. In sub-Saharan Africa (SSA), an estimated 14% of children live away from their mothers, despite their mothers being alive, for economic or cultural reasons.<sup>1</sup> This figure highlights the importance of understanding the consequences of this living arrangement for the development prospects of these children. Co-residence with parents is often perceived as crucial for both immediate well-being and long-term trajectories of individuals. However, the extended family, which usually hosts the foster child, traditionally plays an essential role in child care in low- and middle-income countries (Cox and Fafchamps, 2007; Fafchamps and Quisumbing, 2007). This challenges the idea that living apart from parents is necessarily detrimental. In this paper, we analyze the nutritional impact of fostering, defined as being sent away from the living parents. Although nutrition represents only one dimension of child well-being, focusing on this aspect is legitimate due to its role as a key determinant of welfare, long-term health and human capital development (Case et al., 2005; Hoddinott et al., 2013; McGovern et al., 2017). Furthermore, even in the short term, food security is positively associated with children’s overall health (Gundersen and Kreider, 2009; Gallegos et al., 2021).

Establishing the causal impact of child fostering on nutrition requires addressing key methodological challenges. Indeed, child fostering is not a random practice; it is shaped by various factors, including parental marital status (Grant and Yeatman, 2014), social norms (Marazyan, 2015), the number of siblings (Cotton, 2024), as well as intra-household allocation strategies, which could create endogeneity issues. Almost no cross-sectional, large-scale, representative surveys provide information on household and child characteristics before placement. As a result, most existing studies rely on data that capture the situation of the child only after they have been integrated into the host household, or using retrospective questions (Akresh, 2004; Beck et al., 2015). Our first contribution lies in accounting for the pre-fostering characteristics of the children and their sending household. We use the National Income Dynamics Study (NIDS), a nationally representative longitudinal dataset collected in South Africa. The survey includes child tracking, enabling us to observe child out-fostering and examine the characteristics of children both before and after placement.

Using a longitudinal survey raises an attrition issue, which we solve by taking advantage of the NIDS-specific survey design. The survey design requires that a specific subset of individuals be followed across waves, even if they change residence. Tracking efforts are therefore concentrated on this group, resulting in a particularly low attrition rate among them. However, as the proportion of such individuals increases in a given area, tracking becomes more complex, which in turn lowers the probability of successfully re-interviewing any respondent. These two exogenous sources of variation, the survey-imposed requirement to follow specific individuals and their proportion in an

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<sup>1</sup>This figure is based on the Demographic and Health Surveys, obtained from Cotton (2021).

area, affect the likelihood of being re-interviewed and can be used as instruments for attrition.

To obtain the causal impact of fostering on children’s nutritional status, we combine the instrumental strategy for attrition with a machine learning (ML) approach for conditioning on a large set of pre-treatment covariates. This is achieved through a Double Machine Learning (DML) estimator developed by [Bia et al. \(2023\)](#). ML techniques, which efficiently handle high-dimensional covariates and capture complex interactions among variables, minimize the risk of misspecification and strengthen the validity of the approach. Moreover, the estimator is doubly robust as it combines different ways of accounting for the covariates ([Chernozhukov et al., 2018](#)).

In addition to this main analysis, we provide complementary results. First, we provide estimates obtained from more standard methods to assess the robustness of our results. Second, we use the same method but change the outcome variables to evaluate the change in other dimensions of children’s lives as potential mediators of the effect on nutrition. Third, we expand the scope of the study by assessing the impact of fostering on other children: those who were living in sending households and are indirectly affected by a reduction in the number of siblings present at home. This paper, therefore, provides a complete assessment of the changes associated with child fostering. Finally, we implement placebo tests to evaluate the validity of our methodology.

South Africa offers a compelling context for exploring the relationship between child fostering and health outcomes. On the one hand, child fostering is widespread, with 26% of mothers reporting at least one child under 15 living outside their household in 2016, which is very similar to the average in SSA countries ([Cotton, 2021](#)). On the other hand, the country faces persistent malnutrition and displays interesting specificities. Approximately one in four children under the age of five is stunted, a prevalence that has remained essentially unchanged over the past decades and which is slightly below the SSA average (32%). Concurrently, rates of overweight and obesity are rising, confronting the country with the double burden of malnutrition ([Shekar and Popkin, 2020](#); [Azomahou et al., 2022](#)), that the whole continent is increasingly exposed to ([Popkin et al., 2020](#)). South Africa is, therefore, both an interesting case study and a country at the forefront of the challenges faced by the African continent in terms of malnutrition.

Our findings indicate that fostering has a positive impact on the nutritional status of children under 15 years old. Foster children experience a 6.8 percentage point reduction in the likelihood of being stunted over a 2-3 year period, equivalent to a 37 percent decrease in the probability of stunting. We also find improvements in nutrition when using alternative measurements, such as having an adequate BMI and not being overweight. The results for the stunting variable are very robust, while those based on indicators derived from BMI-for-age variables are more sensitive to specification choices. The placebo tests do not reject the methodology. In exploring the underlying mechanisms, we find that foster children are often sent to rural areas, typically to smaller households with old-age pension recipients, and placed under the care of the household head or spouse, often the grandparent. These factors may help explain the positive effects on their nutritional status. We also find that siblings of these children also benefit from the fostering, as their risk of stunting

also decreases, possibly due to a reduction in household size. Thus, fostering appears to create a win-win situation, benefiting both children who leave the household and their siblings.<sup>2</sup> While our definition of foster children is restricted to those who change household, children of migrant parents may also experience significant changes in their household of origin. Last, we show that the positive effects are limited to children who migrate out of their household and that children who are left behind by migrant parents do not obtain any nutritional advantage.

This paper belongs to the literature on child fostering, which has identified some stylized facts; we build on them to determine the set of relevant factors explaining the choice of implementing this living arrangement. Child fostering results from several strategic decisions made by both sending and receiving households (whether to send/receive a child, which child to send and where); the arrangement must be mutually acceptable to both households, impacting not only the foster child but also all households involved. In particular, when a child changes households, this has direct consequences in terms of available resources per person for both households. Fostering may also be used to mitigate the effects of adverse events, such as income shocks (Akresh, 2009), climatic shocks (Ronnkvist et al., 2023), excess fertility (Cotton, 2024), or parental divorce (Grant and Yeatman, 2014). In addition, the change in household composition can be compensated (in part) by inter-household transfers (Marazyan, 2011; Gautrain, 2023).

If children are sent to households with more resources, this presumably has positive consequences in terms of nutrition. However, additional effects could be at play. First, they change their living environment, most often arriving in an urban area (Eloundou-Enyegue and Shapiro, 2004; Serra, 2009), where they may be sheltered from food access variability but exposed to processed food of lower quality (Popkin, 1999; Hawkes, 2008). Second, the intra-household allocation in the receiving household may not be in their favor when they compete for resources with biological children (Hamilton, 1964; Prall and Scelza, 2017; Madhavan and Townsend, 2007). Ultimately, it depends on the inner motivations of the receiving household, who may or may not see the benefits of treating the foster child well. The predominant finding is that negative nutritional outcomes are identified (Bledsoe et al., 1988; Castle, 1995, 1996; Madhavan and Townsend, 2007; Scelza and Silk, 2014; Prall and Scelza, 2017). However, this body of literature relies primarily on cross-sectional data, often based on small or highly specific samples.

From a more methodological viewpoint, one has to turn to the literature on the effect of fostering on education to see attempts to identify a causal effect. Household fixed effect strategies have been implemented (Beck et al., 2015), which is interesting from a descriptive point of view but may not provide the causal impact of fostering if children of the sending and/or receiving household are indirectly affected by the arrangement. Some scholars have opted for a child fixed effects strategy (Akresh, 2004; Cichello, 2003; Bose-Duker et al., 2021), but differ from our approach: Bose-Duker et al. (2021) assess the effect of being left-behind (parents leave the house) on children’s education,

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<sup>2</sup>Due to the small number of children from host families, we cannot assess potential effects on those living in receiving households with foster children; however, for the same reason, any spillover effects are likely limited.

Akresh (2004) matches sending with receiving households and relies on retrospective information on education. Lastly, Badaoui and Mangiavacchi (2022) implement an IV strategy for fostering, exploiting differences in the historical level of community integration of the child’s ethnicity and the relative importance of the ethnicity in the neighborhood. Our approach is not only novel in terms of methodology, it also clarifies the type of fostering that we study. Compared to cross-sectional studies, in which the non co-residence of children with their parents may result from several mechanisms (such as parental death, parental migration, or child migration), we focus on children who have moved out of their origin household and have living parents. In addition, only children living with their mother during the first study period are considered, which means that fostering has lasted for a maximum of 2-3 years at the time of assessment.

The remainder of the paper is structured as follows. Section 2 introduces the data, sample characteristics, and key variables, complemented by descriptive statistics. In Section 3, the empirical strategy is presented. Section 4 discusses the results, including robustness checks and sensitivity analyses, and examines the underlying mechanisms. Section 5 presents the results for children in the sending households as well as for the left-behind children. Finally, Section 6 concludes.

## 2 Data

### 2.1 National Income Dynamics Study (NIDS)

We use data from the National Income Dynamics Study (NIDS), collected by the Southern Africa Labour and Development Research Unit (SALDRU) at the University of Cape Town.<sup>3</sup> NIDS explores the living conditions of individuals, addressing aspects such as economic activity, poverty, labor market participation, education, and health. This longitudinal survey has been conducted every two to three years since 2008, with the most recent wave covering 2017. It tracks 28,000 individuals residing in 7,305 households as of the first round. The sample was representative of the national population in 2008, and a two-stage cluster sampling design was employed.

The survey encompasses two types of interviewees: Continuing Sample Members (CSMs) and Temporary Sample Members (TSMs). For the CSMs, the survey design induces individual tracking regardless of whether they change households between two survey waves. The CSMs are either from the initial surveyed sample in 2008 or children born to female CSMs in subsequent waves. When such a person moves outside her household within the boundaries of South Africa, she must be followed to her new household, in which all members will be surveyed. TSMs are co-residents of a CSM during a survey wave. The individual tracking does not apply to these individuals, and they are only surveyed if they still reside in a household with a CSM in subsequent waves (Brophy et al., 2018).

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<sup>3</sup>The NIDS portal is available at: <https://www.datafirst.uct.ac.za/dataportal/index.php/catalog/NIDS>. Last accessed: 26 June 2025.

This tracking system is a notable feature of the NIDS, which is particularly valuable in the context of child fostering and rare in longitudinal household surveys.<sup>4</sup> Although not all CSMs are successfully followed, the distinction between CSMs and TSMs in tracking is clear, and we leverage this feature of the survey in our empirical framework.<sup>5</sup>

## 2.2 Sample

We focus on the last two survey rounds of the NIDS, Wave 4 (2014/2015) and Wave 5 (2017), but Wave 3 will also be used for placebo tests. This decision is based on several considerations. First, these rounds provide the most recent perspective on child fostering in South Africa. The second reason concerns data constraints related to tracking and missing data. The pair of Waves 4 and 5 has a high re-interview rate, along with the lowest rate of missing anthropometric data.<sup>6</sup> Finally, the prevalence of nutritional statuses, particularly stunting rates among children under five, confirms that the sample from Waves 4 and 5 aligns with external data sources.<sup>7</sup>

In our analysis, we focus on children who were aged 0 to 12 years during Wave 4. This age range is selected to ensure that these individuals are still considered children in Wave 5 (under age 15).

## 2.3 Children’s nutritional status

To assess the impact of fostering on children’s nutritional outcomes, we rely on anthropometric indicators, specifically z-scores derived from NIDS data. Using BMI-for-age and height-for-age z-scores, we define three nutritional statuses (WHO, 2006):

- Stunted, defined as height-for-age  $\leq -2$  SD;
- Overweight/Obese, defined as BMI-for-age  $\geq +1$  SD;
- Adequate BMI, defined as BMI-for-age between -2 standard deviation (SD) and +1 SD.

For each status, we define binary outcomes equal to 1 if the child falls into the status and 0 otherwise. Stunting reflects chronic malnutrition, capturing medium- to long-term effects of under-

<sup>4</sup>This feature also exists in the Indonesian Family Life Survey and the Mexican Family Life Survey.

<sup>5</sup>Among individuals surveyed in Wave 4 but not interviewed in Wave 5, 70.45% are TSMs and 29.55% are CSMs. Conversely, 8.28% of CSMs interviewed in Wave 4 are not interviewed in Wave 5, whereas 41.01% of TSMs interviewed in Wave 4 are not interviewed in Wave 5.

<sup>6</sup>Re-interview rates are the following: 78.86% of individuals between Waves 1 and 2, 82.77% between Waves 2 and 3, 81.50% between Waves 3 and 4, and 79.56% between Waves 4 and 5. Anthropometric data (height-for-age for children) are missing at the following rates: 25.03% in Wave 1, 45.93% in Wave 2, 20.02% in Wave 3, 12.64% in Wave 4, and 15.81% in Wave 5.

<sup>7</sup>In Wave 4 (2014-2015), 21.71% of children were stunted, and in Wave 5 (2017), 20.46%. These figures align with external data; UNICEF, WHO, and WB Joint Child Malnutrition Estimates reported 22.3% in 2015 and 22.4% in 2017. Data: [https://www.who.int/data/gho/data/indicators/indicator-details/GHO/gho-jme-country-children-aged-5-years-stunted-\(-height-for-age--2-sd\)](https://www.who.int/data/gho/data/indicators/indicator-details/GHO/gho-jme-country-children-aged-5-years-stunted-(-height-for-age--2-sd)). Last accessed: 26 June 2025.



nutrition. However, children initially classified as stunted can transition out of this status through catch-up growth, which occurs when their height velocity exceeds the expected growth rate for their age (Hirvonen, 2014). Such catch-up or substantial improvements in linear growth can occur during childhood, especially around pre-pubertal and pubertal growth spurts (Prentice et al., 2013), or through child adoption, migration or following rapid economic and social development (de Onis and Branca, 2016). Therefore, observing a change from stunted to non-stunted status between two survey waves is possible.

The other categories, overweight/obese and adequate BMI, derived from BMI-for-age z-scores, provide insights into dietary or lifestyle changes, which may, for instance, occur when a child moves to a household with different eating habits. Note that the adequate BMI variable is not the exact counterpart to the overweight/obese variable, as underweight children are coded as zero for both variables. Studying the underweight status would have been interesting, but we have too few cases in this category. Overall, these three measures are complementary in assessing the impacts of fostering on children’s nutrition.

## 2.4 Defining fostering

While there is no explicit, universally agreed-upon definition of fostering, we define child fostering as moving out from the household of the parents. This notion is reflected in the frequent use of terms in the literature that emphasize a child’s movement, such as “relocation,” “movement,” “sent to live,” “sending households,” “receiving households,” or “fostering-out” (see, for instance, Isiugo-Abanihe, 1985; Bledsoe and Isiugo-Abanihe, 1989; Castle, 1995; Ainsworth, 1996; Zimmerman, 2003; Akresh, 2004, 2005, 2009; Serra, 2009; Alber, 2013; Hampshire et al., 2015; Beck et al., 2015). These terms emphasize the transfer of a child between household units, framing fostering as a change in the child’s living arrangement and environment. Thus, all children left behind by migrant parents in their household of origin are excluded from the sample (both for treatment and control groups).<sup>8</sup> However, we also produce estimations using left-behind children, with results discussed in Section 5.3. We also restrict our attention to children who still have a parent alive. Fostering will therefore be interpreted as the decision made by one parent to send away the child, rather than a constrained situation of orphanhood. We also exclude children who become orphans during the period.

Our sample comprises children who are not fostered in Wave 4. Since it is rare for children to live solely with their father if the mother is alive, we focus on children living with their mother in Wave 4 to determine whether the child is included in the sample.<sup>9</sup> As a result, our sample is composed of children who live with their mother in Wave 4 and have at least one parent alive in

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<sup>8</sup>Fostering is sometimes interpreted more broadly and encompasses left-behind children (Démurger, 2015; Bose-Duker et al., 2021).

<sup>9</sup>In Wave 4, 3.13% of children aged 0-12 co-reside only with their father while their mother is alive, consistent with other national estimates (e.g., <http://childrencount.uct.ac.za/indicator.php?domain=1&indicator=2>, last accessed 26 June 2025).



Wave 5. We then define the treatment group (foster children), the indirectly treated group (siblings of fostered children) and the control group (children in non-fostering households).<sup>10</sup> We will start with the analysis of the effect of fostering (comparison between treated and control children) and then offer a similar analysis comparing the indirectly treated to the control group to evaluate more broadly the consequences of the fostering practice.

Since we use longitudinal data, some individuals interviewed in Wave 4 may not have been followed up in Wave 5. Consequently, each group is further divided into two subgroups based on whether the children were surveyed in Wave 5 or not.

- **Treated group** For those surveyed in Wave 5, we identify foster children as those who change household units and who no longer live with a parent in Wave 5 (and at least one parent is still alive).

The other subgroup of foster children includes those not surveyed in Wave 5, while their mothers stayed in the household of origin. Given that in South Africa, it is uncommon for children to live solely with their father, we consider that these children are fostered.

- **Indirectly treated group** We define siblings as those who lived in the original household unit from which at least one child has been defined as fostered, but who are not themselves fostered.

In Wave 5, among those who are surveyed, they are still living with their parents. For those not surveyed in Wave 5, we classify them as non-foster siblings if their mother is also not surveyed in Wave 5. If both of them are not observed, we assume that they have stayed together as it is the prevalent living arrangement.

- **Control group** The counterfactual group consists of non-foster children who are not in any fostering arrangement; i.e., they are neither residing in a sending household nor in a host household in Wave 5.

For those surveyed in Wave 5, non-foster children are those who continue to live with a parent. For those not surveyed in Wave 5, we also assume that they are not fostered if their mother is also not observed in the household.

Table 1 below summarizes the definitions of the treatment and control groups depending on whether children are surveyed or not in Wave 5.

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<sup>10</sup>Children in receiving households could also be considered as indirectly treated group, but we have very few such observations (1.39% of the total sample of children) and adding them to the siblings of fostered children would prevent a clear interpretation of the effects. As a result, they are completely excluded from the analysis. Note also that all children from sending households are considered, not just siblings. These are named siblings as they constitute 61.80% of that group.

Table 1: Description of treatment and control groups

	Surveyed in W5	Definition	Obs.
<b>Treated group: Foster children</b>	Yes	Co-resident with their mother in Wave 4 but no longer in Wave 5 and at least one parent still alive in Wave 5. The child moved to a different household unit.	201
	No	Co-resident with their mother in Wave 4 but not surveyed in Wave 5, while the mother remained in the original household unit.	83
<b>Indirectly treated group: Siblings</b>	Yes	Co-resident with their mother in Wave 4 in a sending household and still co-resident with at least one parent in Wave 5 in a non-receiving household.	260
	No	Co-resident with their mother in Wave 4 in a sending household, but neither the child nor the mother is surveyed in Wave 5.	48
<b>Control group</b>	Yes	Co-resident with their mother in Wave 4 in a non-sending household and co-resident with at least one parent in Wave 5 in a non-receiving household.	4,811
	No	Co-resident with their mother in Wave 4 in a non-sending household, but neither the child nor the mother is surveyed in Wave 5.	845

**Observability/Non-observability** For children included in the sample in Wave 4, there are two reasons why their outcomes may not be observed in Wave 5. First, this could be due to individuals not being re-surveyed in Wave 5, either by design or due to unsuccessful follow-up contact. Since the treatment definition involves changing housing units, child fostering is likely to be negatively correlated with the probability of being re-surveyed. Indeed, 29.23% of foster children were not surveyed in Wave 5, relative to 14.04% of non-foster children, and 15.58% of indirectly treated children. Second, even if an individual is re-surveyed in Wave 5, anthropometric outcomes in Wave 5 may be missing for various reasons, such as refusal to provide information, unavailability of the child at the time of the survey (for measurement and weighing), or postponement of the interview, leading to incomplete data collection.<sup>11</sup> However, nearly all cases of non-observability of the outcome are due to individuals not being surveyed in Wave 5 (89.21%), with only 10.79% attributable to missing data despite being surveyed. Therefore, non-observability related to missing data is a secondary concern. From this point onward, we refer to non-observability in Wave 5 without specifying the different circumstances leading to being unobserved.

<sup>11</sup>In 14 cases of children not re-interviewed in wave 5, indirect respondents reported them as deceased. We include these children in our analysis, as some deaths among attritors may also go unreported. It is also likely that these children were already in poorer health at baseline if they later died. Nonetheless, our main results remain robust when excluding them from the sample. Results are available upon request.

## 2.5 Descriptive statistics

We now describe the baseline characteristics (Wave 4) of children in our sample. As a first step, we examine how foster, siblings and children from the control group differ, to shed light on factors potentially driving treatment assignment. Next, we analyze the selection into observability for treated and control groups, i.e., the differences between the children observed in Wave 5 and those not.

**Selection into treatment** Table B.1 presents baseline differences by fostering status for children. Compared to both groups of non-foster children, foster children are more likely to have been born out of wedlock and less likely to live with their maternal grandparents. In terms of health, they are more frequently reported to be in fair health (indicating worse conditions) and are less often covered by medical aid relative to the control group. There is also evidence of negative intra-household selection on health: siblings of foster children are more likely to be reported in very good health. However, we do not observe significant differences in anthropometric outcomes. Relative to other children in sending household, foster children tend to have a lower birth order.

Regarding mother and household characteristics, Table B.2 shows that foster children tend to have younger mothers, live in smaller households, reside with younger household heads, and face poorer economic conditions. When foster children have siblings in their sending household, these households are more likely to be located in rural areas and to have an older household head.

**Selection into observability** In Tables B.3 and B.4 in the Appendix, we display the differences between the samples of observed and non-observed individuals, for children included in treated and control groups. Non-observability correlates with several baseline characteristics, particularly with baseline nutritional status and treatment. This suggests a selection bias in observability, which we address in our empirical strategy.

## 3 Empirical strategy

In this section, we describe our approach to estimate the causal effect of fostering on children’s nutritional status, using a model that simultaneously addresses selection into treatment and selection into observability, building on the framework of [Bia et al. \(2023\)](#).

### 3.1 Identification strategy

To describe our empirical strategy, we refer to the potential outcomes framework ([Rubin, 1974](#)). At first, we set aside the observability issue, which we incorporate later. Let  $D_i$  be the treatment variable for individual  $i$ , taking the values 0 (non-foster child) and 1 (foster child). Under the stable unit treatment value assumption,<sup>12</sup>  $Y_i$  represents the nutritional outcome in Wave 5 for

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<sup>12</sup>Child  $i$ ’s nutrition is only influenced by its own treatment value.

child  $i$ , with potential values  $Y_i(1)$  if the child is fostered and  $Y_i(0)$  if the child is not fostered. The observed outcome is  $Y_i = Y_i(0) + [Y_i(1) - Y_i(0)] \cdot D_i$ . The causal effect of fostering for individual  $i$  is  $\Delta_i = Y_i(1) - Y_i(0)$ . We aim to estimate the average treatment effect (ATE)  $E(\Delta)$ .

Under the assumption of conditional independence of the treatment, various methods have been proposed to estimate the ATE. This assumption posits that the covariates in the data are comprehensive enough to control for the influence of any confounder.<sup>13</sup> One method involves conditioning on the covariates to derive the conditional average treatment effect (CATE):

$$\Delta_x = E[Y(1)|X = x] - E[Y(0)|X = x] = \mu_1(x) - \mu_0(x). \quad (1)$$

Subsequently, averaging across the values  $x$  appearing in the population yields the ATE:

$$\Delta = E[\mu_1(x) - \mu_0(x)]. \quad (2)$$

Alternatively, under the same assumption of conditional independence of the treatment, one may also predict the likelihood of being treated based on the covariates, obtain a propensity score for treatment  $p(X)$ , and then weigh observations according to this propensity score to ensure that the treatment and control groups have comparable characteristics. The Inverse Probability Weighting (IPW) approach is one of those methods and computes the sample analog to  $\Delta = E\left[\frac{Y \cdot D}{p(X)} - \frac{Y \cdot (1-D)}{1-p(X)}\right]$  to estimate the ATE.

Combining both approaches is more efficient (Chernozhukov et al., 2018). The ATE can be expressed as:

$$\begin{aligned} \Delta &= E[\phi(X)], \text{ where} \\ \phi(X) &= \mu_1(X) - \mu_0(X) + \frac{[Y - \mu_1(X)] \cdot D}{p(X)} - \frac{[Y - \mu_0(X)] \cdot (1-D)}{1-p(X)}. \end{aligned} \quad (3)$$

The estimator is the sample analog to the previous expression and is doubly robust, i.e., the resulting estimator maintains consistency under the correct specification of either the conditional mean outcomes or the propensity score model. A key advantage of this approach lies in its robustness to model misspecification: estimation errors in either component are attenuated through their interaction. In practice, combining a regression approach (to estimate conditional means outcomes) with a propensity score estimation (via logistic regression, for instance) yields more efficient estimates than either method alone, although both approaches rely on the same conditional independence assumption (but not the same specification). It has also been shown to have better properties in small samples (Rothe and Firpo, 2013). Part of the identification strategy consists of using ML non-parametric estimation to obtain the components of  $\phi$ . This data-driven approach allows us to incorporate a rich set of covariates, thereby strengthening the plausibility of the conditional independence assumption while reducing model specification concerns.

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<sup>13</sup>We discuss this assumption in more detail below.

We now add the block of the non-random selection into outcome observability. The causal framework depicted in Figure 1 corresponds to our estimation and accounts for this selection process (Bia et al., 2023). The arrows indicate a potential effect between variables, while the absence of an arrow indicates that such an effect is assumed to be negligible or non-existent. For instance, while unobserved variables ( $U$ ) affect nutritional status ( $Y$ ), they are assumed not to influence child fostering ( $D$ ). However, these unobserved characteristics are correlated with the ones ( $V$ ) affecting Observability  $S$ , which determines whether  $Y$  is observed. The identification strategy relies on instrumental variables ( $Z$ ) that correlate with Observability  $S$ , may correlate with the treatment  $D$ , and covariates  $X$ , but should not be affected by omitted factors ( $U, V$ ).

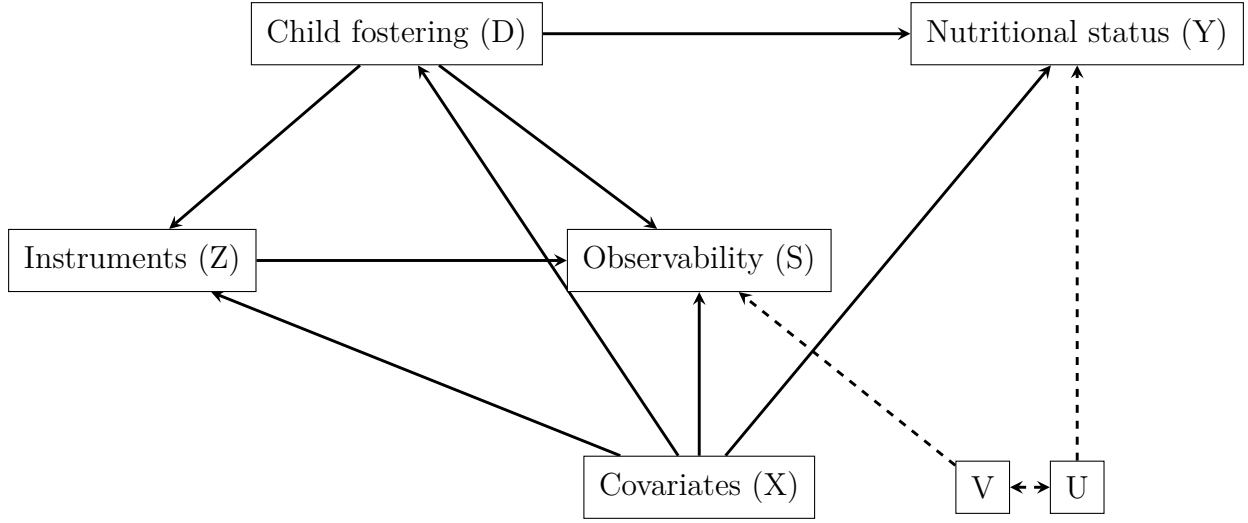


Figure 1: Hypotheses of the model

To address endogenous observability, we introduce propensity scores for the probability of being observed, noted as  $\pi_1(X, Z)$  for treated individuals and  $\pi_0(X, Z)$  for untreated individuals. The above notations need to be slightly adjusted to account for the joint determination of the likelihood of being treated and observed. Bia et al. (2023) demonstrate that, under the assumptions discussed below, the ATE in the total population is identified by:

$$\Delta = E[\phi_1 - \phi_0], \quad \text{with} \quad (4)$$

$$\phi_1 = \frac{D \cdot S \cdot [Y - \mu_1(S=1, X, \Pi)]}{p(X, \Pi) \cdot \pi_1(X, Z)} + \mu_1(S=1, X, \Pi),$$

$$\phi_0 = \frac{(1-D) \cdot S \cdot [Y - \mu_0(S=1, X, \Pi)]}{(1-p(X, \Pi)) \cdot \pi_0(X, Z)} + \mu_0(S=1, X, \Pi).$$

where  $p(X, \Pi)$  denotes the propensity score for treatment conditional on covariates and the propensity scores of observability, and  $\mu_1(S=1, X, \Pi)$  represents the average outcome among treated, observed individuals with covariates  $X$  and probability of being observed  $\Pi$ .

The identification of the ATE relies on a set of assumptions described in Appendix A.1. Under these assumptions, the expression in (4) is doubly robust, meaning that the estimator remains

consistent if either the model for the treatment (or selection) probabilities or the model for the outcome is correctly specified, even if one of them is misspecified. This property allows for robust estimation despite some model misspecifications, as long as at least one model is correct or both models are reasonably well specified. The expression can then be estimated by replacing the theoretical quantities in (4) with their estimated counterparts from the sample data.

## 3.2 Specification choices

Among our identifying assumptions, the conditional independence of the treatment requires particular attention. Its validity rests on two components: first, our ability to control for all relevant covariates that jointly determine fostering decisions and nutritional outcomes, and second, on our ability to use a well-specified model.

**Covariates** We employ a rich set of pre-treatment covariates, including child-level variables (gender, age, relationship to the head, ethnic group, etc.), parental characteristics (information on the father’s living conditions, co-residence with maternal/paternal grandparents, mother’s marital status, etc.), and household characteristics (area of residence, household size, characteristics of the household head, district dummies, wealth index, etc.). In line with Angrist and Pischke (2009), we do not use post-treatment variables to avoid introducing inappropriate controls that may have been affected by the treatment itself.

A crucial feature of our specification is the inclusion of baseline nutritional indicators (BMI-for-age and height-for-age z-scores in Wave 4). This allows our model to measure changes in nutritional status for foster children compared to non-foster ones. In essence, disregarding the aspect related to observability, this approach is similar to a value-added model, where fostering may either improve or worsen a child’s nutrition relative to their pre-treatment level. The exhaustive list of baseline covariates employed in our estimation procedure is detailed in Table A.2 in the Appendix.

**Model specification** Our setting potentially presents a methodological challenge: while we have access to a large and rich set of potential covariates, the number of treated children remains limited. ML methods are well-suited to deal with such situations, as they offer greater flexibility and choose the relevant covariates in a data-driven way rather than with ad hoc selection. We adopt a fully non-parametric method, which is detailed below. All the plug-ins that are necessary to estimate  $\phi_0$  and  $\phi_1$  ( $\mu_0(\cdot), \mu_1(\cdot), p(\cdot), \pi_0(\cdot), \pi_1(\cdot)$ ) will be obtained using this ML method.

**Instruments for selection** We exploit two instruments from the NIDS tracking design to address selection into observability. As mentioned previously, the survey design distinguishes between Continuing Sample Members (CSMs) and Temporary Sample Members (TSMs): biological children of CSM mothers are tracked across survey waves even if they move and change households, while children of TSM mothers are not. We leverage this specific survey feature to build two exogenous instruments.

The first instrument is a binary variable indicating whether a child has a CSM mother, which, by survey design, should increase the likelihood of being observed in Wave 5. The second instrument is a continuous variable representing the district-level share of children with CSM mothers in Wave 5. The intuition behind this instrument is that enumerators may face an increased burden in tracking individuals in districts with higher proportions of children with CSM mothers. In other words, it may pose more significant logistical challenges for enumerators conducting follow-up surveys. This increased difficulty likely results in higher attrition rates in the area, as the effort required to maintain consistent follow-up with these families becomes more demanding and resource-intensive. Interestingly, this second instrument likely affects the capacity to re-survey any individual, not only the CSMs. The survey design should thus generate exogenous variation in the probability to be observed in Wave 5.<sup>14</sup> We assess the instrument’s predictive power in Section 3.3.1.

**Threats to identification** The estimates could be biased if (i) we fail to account for variables that affect both the treatment and the potential outcomes or (ii) the instrument correlates with unobservables affecting the observability of the child or the fostering decision.

One potential threat falling in the (i) category is if parents have specific preferences favoring human capital that lead them to foster a specific child. For this reason, we control for birth order, which is known to predict different parental investments. We also control for several variables related to the mother’s health and education, as well as the child’s health (prior nutrition, serious illnesses, perceived health status, etc.). The ML algorithm we chose is non-parametric and, therefore, potentially selects any “interaction” between these variables. Thus, we assume that if the household has specific preferences, these would be captured by the available variables in Wave 4.

A second potential threat is the case where a shock impacts both the decision to foster and the child’s nutrition. While we control for adverse income shocks happening over the two years before Wave 4, a shock that happens after Wave 4 may trigger the fostering decision. To rule out this possibility, we assess whether fostering is associated with shocks happening between Wave 4 and Wave 5, and we show below that this is not the case.

Related to (ii), the instrument’s validity is questionable if  $Z$ , despite being determined by the survey design, correlates with characteristics that also explain the observability of the child or the fostering decision. TSM women, having joined the panel later, might exhibit less residential stability and, thus, a greater propensity for child fostering. We account for this by controlling for maternal marital status, a strong correlate of movement between households (Gautrain, 2023), as

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<sup>14</sup>The continuous instrument has missing values due to the absence of district of residence information in Wave 5. This affects 11.21% of the sample (710 out of 6,248 individuals) and only concerns the non-observed group, i.e., those for whom we cannot observe an outcome in Wave 5. They represent 64.90% of the non-observed children. Our strategy involves inputting these missing values with the mean value of this variable at the regional level, using the region of residence at Wave 4. In other words, we substitute each missing value with the regional mean of the share of children (by district) with CSM mothers. Regarding the distribution of the inputted missing values by fostering status, we proxy the continuous instrument for 10.73% of non-foster children (control and siblings) and 24.65% of foster children.



well as the number of rounds where the mother is present in the surveyed household unit of Wave 4. These controls capture systematic differences in women’s propensity to change households.

**Implementation** Among various ML techniques, we opt for the Random Forest (RF) procedure, a classifier that allows multiple decision trees to be drawn to classify observations based on covariates. Each tree looks at different random subsets of the data and variables. Therefore, RF can automatically explore and capture interactions between variables. For example, suppose two variables interact in a way that affects the outcome. In that case, the trees in the forest can capture this by making different splits based on various combinations of these variables. Combining all the trees, the RF can account for many potential interactions, even if they are complex or involve multiple variables. The RF procedure is also fully non-parametric and does not restrict the functional form between the outcomes and the explanatory variables nor between the observability, the treatment, and the covariates. This, therefore, limits the risk of misspecification, which would lead to a biased estimate of the causal effect.

We use the *treatselDML* command available in the *causalweight* package in *R* (Bodory and Huber, 2018), using 3-fold cross-fitting and the RF with default options of the *SuperLearner* package as machine learner.<sup>15</sup> We run estimations with 123 covariates measured at baseline. We exclude observations where the product of the estimated treatment and observability propensity scores is below a threshold of 0.01, in order to guarantee that the common support assumptions are satisfied.

### 3.3 Assessing the validity of the method

Before presenting the results, we perform key tests to assess the validity of our model. Specifically, we justify the relevance of our instruments and demonstrate the validity of the propensity score approach used in the DML.<sup>16</sup>

#### 3.3.1 Instrumentation

We first present the results analogous to a “first-stage” analysis, predicting observability based on the instrumental variables. Figure 2 shows the importance of the explanatory variables in predicting selection into observability. In this context, variable importance reflects the contribution of each variable to reducing prediction errors across the decision trees in the forest. Variables ranked higher have a more significant impact on the model’s predictive accuracy. The algorithm identifies the two instrumental variables as the most critical variables, ranking them first and second among all the covariates. This indicates that our instruments  $Z$  are highly effective in predicting observability.

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<sup>15</sup>We conducted a preliminary screening to remove highly correlated variables. In line with Bia et al. (2023), we have standardized all continuous variables to a mean of 0 and a standard deviation of 0.5 to enhance the ML based estimation of nuisance parameters.

<sup>16</sup>All reported tests refer to the DML specification estimating the impact of fostering on the stunted outcome, using the control group as reference. Results for other outcomes or specifications are available upon request.

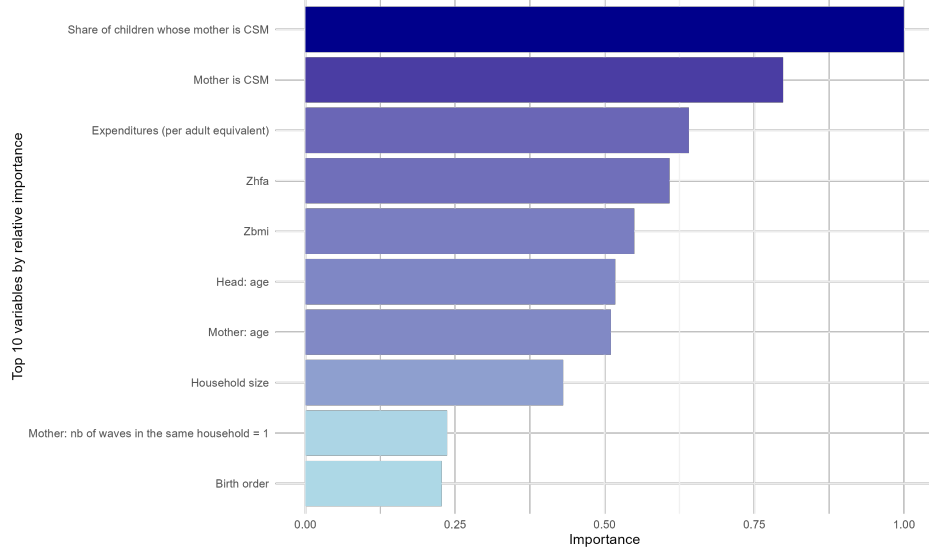


Figure 2: Standardized importance of the covariates explaining observability

Notes: Sample of children aged 0-12 at baseline (Wave 4). The share of children whose mother is CSM is measured at the district level. The graph shows the importance of covariates on an outcome variable, following a random forest with 3-fold cross-fitting. The outcome variable is a dummy equal to one if health outcomes are observable in Wave 5. Covariates are measured pre-treatment; the list is provided in Appendix A.2.

To further evaluate the instruments' validity with a more standard method, we run an OLS regression on “being observed in Wave 5”, including  $Z$ ,  $X$ , and  $D$  as regressors. Table 2 shows that having a CSM mother increases the likelihood of being observed in Wave 5 (column 1), while the share of CSM mothers in the district decreases this likelihood (column 2). Both effects remain significant when combined (column 3), justifying the use of both variables in our preferred specification. Although the method requires only one continuous instrument for identification, we include also the CSM variable to increase the predictive power. In column 3, the F-statistic is around 235, indicating strong predictive power when the instruments are used jointly.

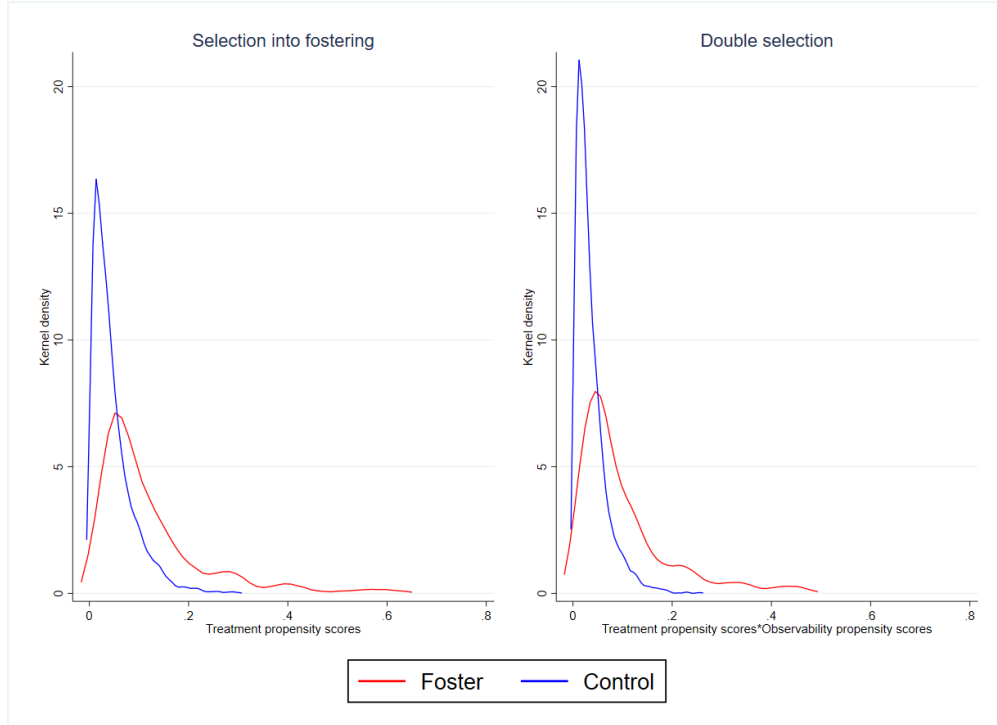
Table 2: First stage estimations of observability

	(1)	(2)	(3)
<b>Outcome: Being observed</b>			
Mother is CSM	0.255*** (0.012)		0.257*** (0.012)
Share of children whose mother is CSM		-0.418** (0.197)	-0.578*** (0.189)
Adjusted $R^2$	0.157	0.091	0.158
Observations	5,940	5,940	5,940
Average outcome	0.824	0.824	0.824
F	460.971	4.512	235.471

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. Share of children whose mother is CSM is measured at the district level. The outcome variable is a dummy equal to one if anthropometric measures are observed in Wave 5. Covariates are measured pre-treatment, they include the treatment (fostering) and the list provided in Appendix A.2. In column 3, the F-statistic for the continuous IV alone is 9.31. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

### 3.3.2 Common support assumptions

After applying the trimming rule, the propensity scores for treatment range from 0.010 to 0.630, and the propensity scores for observability are strictly positive (in accordance with Assumptions 3 and 4, detailed in Appendix A.1). Figure 3 displays two kernel density plots showing the distribution of propensity scores for foster and control children after the trimming. The left-hand plot shows an overlap in the treatment propensity scores. The right-hand plot incorporates treatment and observability propensity scores (reflecting the double selection) and demonstrates the overlap. These figures confirm the presence of a common support after the trimming.



Notes: Sample of children aged 0-12 at baseline (Wave 4). Propensity scores of treatment and observability are extracted from the DML: random forest with 3-fold cross-fitting and a trimming threshold of 0.01. Trimmed observations are excluded. The graph on double selection is drawn only for observed individuals. The outcome variable is being stunted and is measured in Wave 5. Covariates are measured pre-treatment; the list is provided in Appendix A.2.

Figure 3: Distribution of the propensity scores

### 3.3.3 Algorithm performance

In practice, the algorithm estimates three separate but interconnected ML specifications using RF classifiers with 500 trees. Each model solves a classification problem: predicting (i) the propensity score for observability, (ii) the propensity score for treatment, and (iii) the potential outcome as a binary variable, conditional on covariates and the observability score. For each specification, predictive performance is assessed using the accuracy rate, calculated as one minus the mean squared error expressed as a percentage. This measure provides an intuitive assessment of the model's

ability to correctly classify observations as observed, treated, or according to the binary outcome. The accuracy rates obtained are 86.47% for observability, 95.30% for treatment assignment, and 91.60% for the binary outcome, which indicate a satisfactory predictive performance in all cases.

In the treatment assignment model, the most important predictors include the observability score, anthropometric indicators, household expenditures, and the age of both the mother and the household head (see Appendix Figure B.1). These variables also emerge as key contributors in the estimation of the conditional outcome, suggesting that they play a central role both in selection into treatment and in shaping childrens outcomes (see Appendix Figure B.2).

## 4 Results

### 4.1 Impacts on foster children

Our main analysis consists of estimating the impact of fostering for foster children with a control group composed of children outside of fostering arrangement. Table 3 presents the DML estimates. We provide the results on the 0-12 years old sample (at baseline) in Panel A, and a restricted sample (2-12 years old), as the measurement error in outcomes tends to be larger for the very young children (WHO and UNICEF, 2019). In both panels, we find a significant improvement in all nutritional measures. In Panel A, stunting is reduced by 6.8 percentage points, overweight is reduced by 6.1 percentage points, and being of adequate BMI increases by 8.3 percentage points. These figures correspond to very sizable effects compared with the baseline prevalence: a 37% reduction in the likelihood of being stunted, 23% reduction in the likelihood of being overweight, and 12% increase in the likelihood of being in the normal range of BMI. The effects obtained in Panel B are slightly smaller, but still very sizable and significant at the 5% level.

Based on these results, there are two scenarios that could explain the positive impact of fostering on a child’s nutritional level, for example, stunting status: either children transition from being stunted to non-stunted, or they remain non-stunted. The nutritional status transition matrix provided in Table B.5 in the Appendix illustrates this. Among foster children initially stunted and tracked between the two waves, approximately 82.86% transitioned to non-stunted status by the following wave, which is much higher than the share of control children who changed status (50.29%, panel A). By comparison, there is a roughly similar proportion of children who maintained their non-stunted status, whether they belong to the treated or control group. Thus, fostering substantially enhances children’s nutritional outcomes, mostly by helping them transition out of stunting. The transition matrices for the other two outcomes (panels B and C) lead to the same conclusion: the effects are mostly driven by a reduction in the probability of remaining in poor nutritional status. Therefore, we conclude that fostering improves children’s nutrition.

Table 3: Impact of fostering on children’s nutrition

Outcome	ATE	Standard error	<i>p</i> -value	Baseline mean	Obs.	Obs. trimmed
<b>Panel A: Children aged 0 to 12</b>						
Stunted	-0.068***	0.020	0.001	0.156	5,940	930
Overweight/Obese	-0.061**	0.027	0.026	0.269	5,940	944
Adequate BMI	0.083***	0.029	0.004	0.698	5,940	944
<b>Panel B: Children aged 2 to 12</b>						
Stunted	-0.046**	0.022	0.037	0.133	5,096	808
Overweight/Obese	-0.059**	0.029	0.041	0.232	5,096	784
Adequate BMI	0.070**	0.034	0.036	0.738	5,096	784

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. The treatment is fostering. The outcome variables are nutritional statuses based on BMI-for-age *z*-scores (adequate BMI and overweight/obese) and height-for-age *z*-scores (stunted). Covariates are measured pre-treatment, and the list is provided in Appendix A.2. DML: random forest with 3-fold cross-fitting and a trimming threshold of 0.01. Standard errors are based on asymptotic approximations using the estimated variance of the efficient score functions. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

## 4.2 Placebo and robustness tests

We now provide placebo tests to evaluate the validity of the method and robustness checks.

### 4.2.1 Placebo tests

As a first placebo test, we estimate the effect of being treated in Wave 5 on the evolution of nutrition status between Wave 3 and 4. This analysis can only be done on the subsample of children who are also observed in Wave 3. We thus focus on children with complete information in Waves 3 and 4, aged 2-12 in Wave 4, and assign treatment as before.<sup>17</sup> As expected, the future fostering does not affect nutritional status in Wave 4, conditional on covariates from Wave 3 (Table B.6). This result rules out the possibility that fostering is a decision that is taken because the child’s nutritional status has deteriorated in Wave 4.

In a second placebo test, we simulate a random treatment assignment. Specifically, individuals are randomly assigned to either a treatment or a control group, with the proportion of treated individuals matching the percentage of foster children (4.78%). Appendix Table B.7 confirms that no significant ATEs are observed under random treatment assignment.

### 4.2.2 Stricter trimming rule

In the default version of the DML procedure, 1% of observations are discarded due to extreme propensity scores. This trimming rule is applied to avoid excessively small denominators in weight-

<sup>17</sup>In this sample, all children are observed in both waves, and the DML procedure addresses only the selection into treatment. This is done with the *treatDML* command from the *causalweight* package in *R* (Bodory and Huber, 2018).

ing by the inverse of the propensity scores. In a robustness analysis, we adjust this threshold to 2%. The results, available in the Appendix Table B.8, confirm that the main findings on stunting and adequate BMI remain robust under this alternative trimming rule (-6.4 percentage points and +5.8 percentage points, respectively). However, the effect on the likelihood of being overweight is not significant anymore, even at the 10% level.

### 4.2.3 Control for the lagged outcomes

In the main specification, we control for baseline nutrition using the continuous height-for-age and BMI-for-age z-scores in Wave 4. In an alternative specification, we replace the z-scores with the dummy variables adequate BMI and stunted at baseline for the estimation of adequate BMI and stunted outcomes. For the estimation with outcome overweight/obese, we replace the z-scores with the dummy variables overweight/obese and stunted at baseline. The findings on stunting and adequate BMI remain robust to this specification change (-6.5 percentage points and +7.4 percentage points, respectively), whereas the effect on overweight/obese does not (Appendix Table B.9). Together with previous results, this suggests that the estimated impact on overweight/obese is not robust to specification changes.

## 4.3 Results from other methods

Our findings conflict with previous studies in sub-Saharan Africa, which often report adverse effects of fostering on children’s nutrition (Ariyo et al., 2019). These discrepancies may be due to variations in treatment definition, such as excluding orphans or those who do not relocate, as well as the use of different methods. In this section, we provide the findings of other impact evaluation methods. Although these methods are not directly comparable with our empirical framework due to differing identification assumptions, we aim to evaluate to what extent our results rely on the specificity of our identification strategy.

Table 4 provides the results of a variety of estimations, ranging from small differences compared to our main specification to a very basic OLS estimation. Specification (1) shows the result when using a non-doubly robust estimator that relies solely on IPW, based on predicted observability and treatment scores. Specification (2), by contrast, estimates the outcome conditional on covariates and further applies IPW based on the observability propensity score. Both specifications can be interpreted as components of the main doubly robust estimator used in our DML framework. These two specifications lead similar estimates to our main specification. Specification (3) implements a DML strategy but uses a logit instead of a RF estimation. One can see that the capacity to predict well the observability and treatment through a logit is lost. Specification (4) shows that ignoring the attrition issues does not seem to make a large difference. Interestingly, specifications (5) to (8) also provide estimates in the same range.<sup>18</sup> These include difference-in-difference (DiD) estimations,

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<sup>18</sup>Only specification (8) provides a non-significant result, which we interpret as a difficulty in handling many

with various patches for differences between groups at baseline. One important aspect to note is that we keep our treatment defined as it was previously: we exploit the panel dimension of the data to identify who is fostered between Waves 4 and 5. This could not be done with repeated cross-sections, which is a standard context where DiD methodology is applied. Specifications (9) and (10) are IPW estimations, and also lead to very similar results (with the similar caveat that we do observe baseline characteristics to run the propensity score, and that we observe treatment between the two waves). Finally, we offer specification (11), which is a simple comparison of foster children with non-foster children, where fostering is defined using cross-section information (not living with their parents in Wave 5, while one of them is alive). This comparison, which is commonly used in non-causal analyses, yields no significant effect of fostering.

Table 4: Alternative methods to estimating the impact of fostering on stunting

	Estimator	ATE	Standard error	<i>p</i> -value	Obs.	Obs. on support
1	IPW observability and treatment (RF)	-0.061***	0.019	0.001	5,940	5,010
2	Conditional on X and IPW observability (RF)	-0.048**	0.020	0.019	5,940	5,010
3	DML (Logit)	0.023	0.097	0.812	5,940	2,155
4	DML not accounting for observability (RF)	-0.065**	0.029	0.025	4,897	3,075
5	DiD Conditional on X (OLS)	-0.062*	0.034	0.070	9,748	4,870
6	DiD FE individual (OLS)	-0.061**	0.025	0.015	9,748	4,870
7	DiD (OLS) IPW treatment (Logit)	-0.005	0.021	0.827	9,748	4,222
8	DiD (OLS) IPW treatment (RF)	-0.075***	0.014	0.000	9,748	4,674
9	IPW treatment (Logit)	-0.060***	0.008	0.000	4,870	4,212
10	IPW treatment (RF)	-0.051***	0.008	0.000	4,870	4,667
11	Cross section Wave 5 (OLS)	-0.011	0.031	0.888	8,136	8,136

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. The treatment is fostering, defined for estimators 110 as detailed in Section 2.4. For estimator 11, fostering is defined more broadly as children living without either parent while at least one parent is still alive in Wave 5. All estimators labeled *RF* rely on random forests with 3-fold cross-fitting. *IPW* refers to inverse probability weighting for the treatment (fostering), using propensity scores estimated from all covariates listed in Appendix A.2. Estimators (1) and (2) decompose our main identification strategy: (1) uses a combined *IPW* that incorporates both observability and treatment propensity scores; (2) applies *IPW* for observability only and adjusts for the outcome conditional on covariates. Estimators (3) to (11), do not account for observability thus the sample is restricted to observed children in Wave 5. For all estimators based on the *treatselDML* (1,2,4) or *treatsel* (3) commands, a trimming threshold of 0.01 is applied to ensure common support. For the remaining estimators, the sample is restricted to the strict common support of the estimated propensity scores. In estimator (11), the set of covariates is measured in Wave 5 and includes: age dummies, gender, birth order, born out of wedlock, African dummy, relationship to household head, illness/disability, medical aid coverage, father alive, rural dummy, household size, female head, heads age and education, wealth quintiles, total expenditure per adult equivalent, distance to nearest police station, and dummies for receiving pension, receiving/sending remittances, agricultural activity, and recent income shocks. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Tables B.10 and B.11 in Appendix provide the same analysis for the two other outcomes. We confirm that the effects on overweight/obese are not consistently supported across alternative estimation methods. We also find that in most specifications, the impact on adequate BMI vanishes, while it seemed robust in our previous variations: none of the DiD estimates are significantly

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covariates in a logit estimation.



different from zero and the coefficients are close to zero, specifications (1) and (4) provide large coefficients but only one is significantly different from zero. It is difficult to know whether the previously identified effect on adequate BMI should be considered valid and our method is superior to others in detecting it, or if we should take this result with caution. Given that the result on stunting was very robust to alternative estimations, we tend to trust the result on adequate BMI less, but still conclude that fostering has a positive impact on children’s nutrition.

## 4.4 Mechanisms

Our main findings show that fostering positively impacts children’s nutritional status, mainly by reducing the likelihood of stunting. To explore mechanisms, we apply the DML framework to post-treatment potential mediators, such as household and caregiver characteristics. These estimations provide the effect of fostering on household characteristics, conditional on baseline characteristics. The effects can therefore be interpreted as the change in household characteristics due to fostering. The rationale is to identify pathways through which the positive impact on nutrition might occur. Table 5 provides the ATEs on the (potential) mediators but also the standardized ATEs, which allows for the comparison of the effect size across the different continuous dependent variables.

Table 5: Impact of potential mediators for foster children

Outcome	ATE	Stand. ATE	Standard error	<i>p</i> -value	Baseline mean	Obs.	Obs. trimmed
Household size	-0.756***	-0.229***	0.273	0.006	6.594	5,940	888
Nb of household members 0-2	-0.073	-0.093	0.060	0.229	0.680	5,940	888
Nb of household members 3-14	0.136	0.086	0.162	0.400	2.363	5,940	888
Nb of household members 15-64	-0.899***	-0.489***	0.148	0.000	3.264	5,940	888
Nb of household members 65	0.092*	0.205*	0.049	0.059	0.194	5,940	888
Wealth index	-0.045***	-0.242***	0.015	0.003	0.532	5,940	920
Food expenditures	-172.630***	-0.164***	59.807	0.004	1330.633	5,940	912
Food expenditures (per capita)	17.342	0.086	20.424	0.396	236.193	5,940	912
Food expenditures (per adult equivalent)	38.857	0.101	41.892	0.354	479.364	5,940	912
Food expenditures (per calories adult equivalent)	37.322	0.148	25.633	0.145	293.784	5,940	912
Expenditures	-2414.822***	-0.341***	339.967	0.000	5095.321	5,940	912
Expenditures (per capita)	-324.901***	-0.198***	81.765	0.000	961.372	5,940	912
Expenditures (per adult equivalent)	-655.813***	-0.211***	154.617	0.000	1920.868	5,940	912
Household with retired pension	0.150***		0.047	0.001	0.252	5,940	1,034
Rural	0.249***		0.041	0.000	0.484	5,940	912
Negative household income shock last 2 years	-0.005		0.021	0.822	0.107	5,940	929
Cared by head or head’s spouse	0.258***		0.032	0.000	0.576	5,940	856
Grandchild of the head	0.290***		0.048	0.000	0.329	5,940	1,075
Household received remittances	0.031		0.048	0.518	0.276	5,940	1,041
Household sent remittances	-0.012		0.027	0.661	0.129	5,940	1,041

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. The treatment is fostering. Covariates are measured pre-treatment, and the list is provided in Appendix A.2. DML: random forest with 3-fold cross-fitting and a trimming threshold of 0.01. Standard errors are based on asymptotic approximations using the estimated variance of the efficient score functions. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Change in household characteristics** First, the improvement in children’s nutritional status may be attributed to a change in the living conditions of the child. A key change appears to be the one on household composition. Children who are fostered typically join smaller households with fewer working-age adults compared to their origin households. In addition, foster children tend to integrate into households with a significantly higher proportion of adults 65 years and older, who are often pension beneficiaries. This result is consistent with documented evidence that it benefits human capital investments in children (Duflo, 2003; Hamoudi and Thomas, 2014). When it comes to nutrition, the difference in household income sources may be advantageous for foster children, as pension income is a more stable and predictable resource, less vulnerable to idiosyncratic economic shocks.

However, receiving households have a lower wealth index, and lower total expenditures than origin households, but there is no significant difference in food expenditures at the household level.<sup>19</sup> The improved nutrition therefore does not seem to be explained by a relocation to a wealthier household. Interestingly, we also find that, in our context, child fostering is not associated with remittances, as the coefficient is not significant.

Table 5 also shows that, in South Africa, foster children are likely to move to rural areas. This could also explain the improvement in nutritional status: rural households are likely to engage in subsistence agricultural activities, which can enhance access to diverse and nutritious foods (Govender et al., 2017). Unfortunately, our methodology does not allow us to test whether living in a rural area is indeed a factor improving the child’s nutrition.

**Changing caregivers** Fostering, by definition, involves having a new caregiver, which may also explain the positive impacts on children’s well-being. The analysis in Table 5 indicates that foster children are significantly more likely to be under the direct care of the head of the household or the head’s spouse in their host households,<sup>20</sup> and grandchild of the head, as in Schrijner and Smits (2018). This change in care arrangements is notable, as it suggests that foster children are placed under the direct care of the primary decision-makers in host households. Such a position could enhance these children’s access to resources through more favorable intra-household allocation decisions. This would reconcile the observed improvement in nutritional outcomes with the absence of change in expenditures for the household as a whole, aligning with previous evidence on the role of proximity to the household head (Brown et al., 2019; De Vreyer and Lambert, 2021).

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<sup>19</sup>We use different ways to account for household composition, and this result is robust.

<sup>20</sup>86.6% of the foster children are under the care of the household head or their spouse.

## 5 Results for other groups of children

### 5.1 Impact of fostering on siblings

With our data, we can also explore the effects of fostering on other children from sending households. Using the same DML model, we estimate the effect of fostering, with the treated group composed of siblings of fostered children. The control group remains the same and is composed of all children not involved in fostering.

As shown in Table 6, the results indicate that fostering has a negative and significant effect on the probability of being stunted for children from sending households. In other words, fostering also reduces the likelihood of stunting among siblings staying with their parents, compared to non-foster children. The effect is, however, smaller, with a reduction in stunting by 4.4 percentage points (-28.5%), compared to a reduction of 6.8 percentage points for foster children (Table 3). The effect is somewhat smaller for children aged 2-12 years old, but remains significant at the 10% level. For the other outcomes, the coefficients are not statistically significant.

Table 6: Impact of fostering on siblings' nutrition

Outcome	ATE	Standard error	<i>p</i> -value	Baseline mean	Obs.	Obs. trimmed
<b>Panel A: Children aged 0 to 12</b>						
Stunted	-0.044**	0.021	0.035	0.154	5,964	494
Overweight/Obese	0.036	0.034	0.292	0.267	5,964	503
Adequate BMI	-0.036	0.037	0.321	0.700	5,964	503
<b>Panel B: Children aged 2 to 12</b>						
Stunted	-0.034*	0.019	0.078	0.132	5,126	394
Overweight/Obese	0.024	0.034	0.481	0.232	5,126	405
Adequate BMI	-0.008	0.035	0.822	0.738	5,126	405

Notes: Sample of children aged 0-12 years at baseline (Wave 4). The treated group consists of children from the sending households, and the control group consists of non-foster children (excluding host siblings of foster children). The outcome variables are nutritional statuses based on BMI-for-age *z*-scores (adequate BMI and overweight/obese) and height-for-age *z*-scores (stunted), measured in Wave 5. Covariates are measured pre-treatment, and the list is provided in Appendix A.2. DML: random forest with 3-fold cross-fitting and a trimming threshold of 0.01. Standard errors are based on asymptotic approximations using the estimated variance of the efficient score functions.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

The key takeaway from these results, along with those for foster children, is that although the indirect effect of fostering on children remaining in the household of origin is smaller, fostering appears to create a potentially mutually beneficial arrangement for both groups of children involved in the practice of fostering, particularly in terms of long-term nutritional outcomes.

## 5.2 Mechanisms for siblings

We now try to understand what explains the improvement in siblings' nutrition. To do so, we estimate the effect of fostering on the living conditions of the children staying in the sending household (Table 7). As expected, a primary channel operates through changes in household composition: fostering leads to a reduction in both household size and the number of children aged 3-14. However, the effect sizes are inconsistent, and it is difficult to give them coherence. By lowering the number of dependents, fostering reduces the number of mouths to feed and alleviates resource constraints, potentially freeing up resources. As a result, food expenditures remain stable, indicating that resources allocated to food may be preserved. However, we observe a decline in the wealth index. The 2014-2017 period covered by our analysis corresponds to a recession, which may explain this decline in households that remained unchanged.<sup>21</sup> Note that we do not observe any effect on shocks happening in the last two years, which suggests that, conditional on all covariates, fostering does not correlate with shocks taking place during the period. We may also note another change in household composition as children are less likely to be grandchildren of the head. One possible explanation is that some of the foster children are moving out of the household with their grandparent, therefore changing the structure of the origin household. Finally, looking at the sending households, we also confirm that fostering is not associated with remittances sent by the origin household.

## 5.3 Estimating the effect of being left behind

We finally assess whether children who are left behind also benefit from a change in their living arrangement. We define a last treatment variable which is equal to one when the children live with one of their parents in Wave 4, but not in Wave 5. In our paper, as they do not relocate, they were excluded from the analyses. However, part of the literature considers these children as fostered, as they stop co-residing with either parent, while a parent is still alive.<sup>22</sup> In cross-sectional studies, it is difficult to identify whether children who do not co-reside with their parents have been fostered or left-behind by migrant parents. We have already seen that (i) none of the nutrition effects survive a definition of fostering based on cross-sectional data, even the stunting variable (specification 11 of Table 4) and (ii) that most of the literature tends to find adverse nutritional effects of fostering. It is therefore interesting to apply our methodology to a treatment defined as being left behind. However, we face limitations in this exercise as left behind children may face attrition, which we cannot address (Bertoli et al., 2023).

The results for this alternative definition are shown in Table B.12. They indicate that being left behind has no effect on the probability of being stunted, overweight/obese, or having an ade-

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<sup>21</sup>The GDP per capita growth was -0.3 in 2014, -0.7 in 2015, -0.3 in 2016 and 0.5 in 2017 (<https://data.worldbank.org/indicator/NY.GDP.PCAP.KD.ZG?locations=ZA>, last accessed 26 June 2025.)

<sup>22</sup>Among others, Gosselin-Pali (2025) and Bai et al. (2022) find, for instance, that such situations negatively affect children's nutrition.

Table 7: Impact of potential mediators for siblings

Outcome	ATE	Stand. ATE	Standard error	p-value	Baseline mean	Obs.	Obs. trimmed
Household size	-0.396*	-0.117*	0.209	0.058	6.718	5,964	490
Nb of household members 0-2	0.023	0.028	0.055	0.674	0.696	5,964	490
Nb of household members 3-14	-0.306***	-0.189***	0.107	0.004	2.415	5,964	490
Nb of household members 15-64	-0.266**	-0.142**	0.129	0.040	3.319	5,964	490
Nb of household members 65	-0.002	-0.003	0.025	0.951	0.195	5,964	490
Wealth index	-0.032***	-0.176***	0.011	0.003	0.534	5,964	499
Food expenditures	-38.576	-0.037	125.641	0.759	1340.490	5,964	490
Food expenditures (per capita)	8.480	0.042	24.475	0.729	234.466	5,964	490
Food expenditures (per adult equivalent)	12.639	0.033	50.379	0.802	477.776	5,964	490
Food expenditures (per calories adult equivalent)	25.594	0.102	31.371	0.415	291.145	5,964	490
Expenditures	-868.825	-0.123	621.479	0.162	5103.980	5,964	490
Expenditures (per capita)	-153.137	-0.094	118.627	0.197	953.529	5,964	490
Expenditures (per adult equivalent)	-275.858	-0.089	247.848	0.266	1910.787	5,964	490
Household with retired pension	-0.026		0.026	0.304	0.252	5,964	490
Rural	0.006		0.024	0.798	0.488	5,964	490
Negative household income shock last 2 years	0.028		0.026	0.286	0.108	5,964	490
Cared by head or head's spouse	0.039		0.026	0.140	0.577	5,964	514
Grandchild of the head	-0.101***		0.028	0.000	0.330	5,964	479
Household received remittances	-0.053		0.036	0.149	0.278	5,964	518
Household sent remittances	0.054		0.034	0.113	0.126	5,964	518

Notes: Sample of children aged 0-12 years at baseline (Wave 4). The treated group consists of children from the sending households and the control group consists of non-foster children (excluding host siblings of foster children). Covariates are measured pre-treatment, and the list is provided in Appendix A.2. DML: random forest with 3-fold cross-fitting and a trimming threshold of 0.01. Standard errors are based on asymptotic approximations using the estimated variance of the efficient score functions. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

quate BMI. This suggests that the change in environment may be key in improving nutrition (for instance, because the foster children are often relocated in rural areas). A more detailed analysis comparing the foster and the left-behind children would be necessary to understand why foster children benefit while left-behind children do not, but this is outside the scope of this paper. In our data, we observe 360 left-behind children against 284 foster children, which highlights that parental migration leaves a significant share of children without their parents and calls for further analysis. Importantly, our results suggest that distinguishing between these two phenomena is crucial to identifying populations at risk, the living arrangements that drive long-term poverty, as well as those that improve welfare.

## 6 Conclusion

This paper investigates the impact of child fostering on children's nutritional outcomes in South Africa. We leverage the unique characteristics of the NIDS longitudinal dataset and employ advanced machine learning techniques. The analysis takes advantage of two unique features of the NIDS: the ability to track individuals over time and a survey design that intensively follows specific predefined individuals, mitigating non-observability issues post-treatment. The capacity to define well the treatment (i.e., that a child has left the household) differentiates our contribution from most papers in the literature. Additionally, by employing a double machine learning approach,

which accounts for selection into fostering and outcome observability, our research addresses critical limitations in the existing literature.

Our findings provide evidence that child fostering through relocation significantly improves children’s nutritional outcomes in South Africa, particularly by reducing stunting. This improvement in mid to long-term health outcomes is achieved through two main mechanisms: changes in living environments and caregiving arrangements. Foster children are often placed in smaller, rural households with older caregivers, frequently grandparents, who are more likely to receive stable old-age pension income. Furthermore, foster children are often directly cared for by household heads, which may play a role in the intra-household resource allocation. Importantly, fostering appears to create a mutually beneficial dynamic: other children from household of origin also experience improved nutritional outcomes, primarily due to the capacity of the household to compensate for births taking place via fostering.

These findings highlight fostering as a strategy with the potential to enhance child health in both sending and receiving households. Such living arrangements should therefore not be opposed by regulations. The South African government has made provisions to ensure that caregivers, even if they are not the biological parents, can obtain benefits for the children in their care, for instance through the Foster Child Grant ([Republic of South Africa, 2024](#)). This step is crucial in allowing an informal, traditional arrangement to survive in a society that becomes increasingly formal, as the role of the state in providing a socio-fiscal system expands.

However, our contribution also raises some important questions: Given that there are no remittances between households, the receiving household does not experience an increase in the available resources. If there is another mouth to feed, what is the impact on caregivers who host foster children? In other words, might caregivers be sacrificing their own well-being to prioritize the nutrition of foster children? To provide a thorough welfare assessment of the fostering practice, we would need to take these changes into account, which is not achievable given the dataset.

We have also found that children who are left-behind by their parents do not obtain a similar nutritional improvement. This result shows the importance of separating these two different types of living arrangements and understanding why they lead to different outcomes for children. These questions present intriguing avenues for future research.

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# A Appendix: Method

## A.1 Assumptions for identifying the ATE

The identification of the ATE relies on the following set of assumptions:

Assumption 1 : Conditional independence of the treatment.

$$Y(0), Y(1) \perp D | X = x \quad \text{for all } x \text{ in the support of } X$$

Assumption 2 : Instrument for selection into observability. The instrument  $Z$ , which may depend on  $D$ , is correlated with  $S$  but orthogonal to the outcome  $Y$ , conditional on the treatment and covariates.<sup>23</sup>

Assumption 3 : Common support for the treatment.

$$0 < p(x, \pi) < 1, \quad \text{for all } x, z \text{ in the support of } X, Z.$$

Assumption 4 : Common support for selection.

$$\pi_0(x, z) > 0 \quad \text{and} \quad \pi_1(x, z) > 0 \quad \text{for all } x, z \text{ in the support of } X, Z.$$

Assumption 5 : Conditional effect homogeneity.

$$E[Y(1) - Y(0) | S = 1, X = x, V = v] = E[Y(1) - Y(0) | X = x, V = v]$$

for all  $x, v$  in the support of  $X, V$ .

A sufficient condition for effect homogeneity is the separability of observed and unobserved components in the outcome equation, that is  $Y = \eta(D, X) + \nu(U)$  with  $\eta$  and  $\nu$  general functions (Huber, 2014).

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<sup>23</sup>The formal assumption 2 is the following:

- (a) The instrument  $Z$  (which may be a function of  $D$ ,  $Z = Z(D)$ ) is conditionally correlated with  $S$ , that is:  $E[Z \cdot S | D, X] \neq 0$ , and satisfies (i)  $Y(d, z) = Y(d)$  and (ii)  $Y \perp Z | D = d, X = x$  for all  $d \in \{0, 1\}$  and  $x$  in the support of  $X$ .
- (b)  $S = I\{V \leq \chi(D, X, Z)\}$ , where  $V$  is a scalar (index of) unobservables and  $\chi$  is a general function with a strictly monotonic cumulative distribution function conditional on  $X$ .
- (c)  $V \perp (D, Z) | X$ .

## A.2 Step-by-step estimation algorithm

Table A.1: Procedure for estimating ATE with DML for sample selection models

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<b>Step 1</b>	<b>Estimate propensity scores using machine learning</b> a. Split the dataset into two parts: a training set (2/3) and an estimation set (1/3) to implement cross-fitting and prevent overfitting. b. Estimate the propensity scores for observability, where observability is modeled as a function $\pi(d, X, Z)$ , predicting the probability of being observed. c. Estimate the propensity scores for treatment, conditional on the covariates $X$ and the estimated propensity scores for observability (which indirectly reflect $Z$ ). d. Using observations where the outcome is available, train a model to predict potential outcomes in the test set, based on both the covariates and treatment status.
<b>Step 2</b>	<b>Trim observations below threshold</b> Discard observations where the product of the propensity scores for observability and treatment falls below a specified threshold.
<b>Step 3</b>	<b>Estimate outcomes using machine learning</b> For the remaining observations, estimate the outcomes for both treated and control groups, weighting them by their respective propensity scores. Apply machine learning techniques to adjust for covariates in this estimation.
<b>Step 4</b>	<b>Calculate the average treatment effect (ATE)</b> Compute the ATE as the difference between the weighted outcomes of the treated and control groups.

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### A.3 List of covariates

Table A.2: List of pre-treatment covariates used in the Double Machine Learning framework

Child's characteristics	Gender (female)
	Age (dummy variables for each age from 0 to 12 years)
	Child of the head
	Born out of wedlock
	Ethnic group (African)
	Birth order
	Perceived health status (dummy variables from 1 to 5)
	Child already had serious illnesses or disabilities
	Child is covered by medical aid
	Body Mass Index z-score
	Height-for-age z-score
Parents' characteristics	Alive father
	Co-residence with father
	Co-residence with maternal grandparents
	Co-residence with paternal grandparents
	Co-residence with father and alive paternal grandparent
	Co-residence with mother and alive maternal grandparent
	Mother's age
	Mother's marital status (dummy variables for being a widow, or divorced/separated)
	Mother's number of years of union
	Mother's education level (dummy variables: uneducated, primary educated, secondary educated, tertiary educated)
	Mother's perceived health status (dummy variables from 1 to 5)
Household characteristics	Mother diagnosed with diabetes
	Mother diagnosed with HIV
	The mother is in charge of decisions about household expenditures
	Number of survey waves the mother spent in the same household as of Wave 4 (dummy variables from 1 to 4)
	Household size
	Household: Rural
	Household: Female head
	Household: Head's age
	Household: Wealth index (dummy variables for each quintile of a standardized wealth index)
	Household: Total expenditures (food and non-food) per adult equivalent
	Household: District of residence (dummy variables for each district from 1 to 52)
	Household: Region
	Household: Head's education level (dummy variables: uneducated, primary educated, secondary educated, tertiary educated)
	Household with a pensioner receiving the old age grant
	Agricultural household
	Household: Distance to the police station
	Household has experienced a negative shock over 2 years
	Remittances recipient household (internal/intra-province/abroad, family/non-family, head/other members)
	Remittances sending household (internal/intra-province/abroad, family/non-family, head/other members)

## B Appendix: Additional tables and figures

Table B.1: Children's baseline characteristics across fostering status

	(1)	(2)	(3)	(1)-(3)	(1)-(2)	(2)-(3)
	Foster	Sibling	Control			
Female	0.553 (0.030)	0.487 (0.029)	0.508 (0.007)	0.045 (0.030)	0.066 (0.041)	-0.021 (0.029)
Age	5.546 (0.217)	5.828 (0.202)	5.848 (0.047)	-0.302 (0.217)	-0.282 (0.296)	-0.020 (0.209)
Birth order	2.201 (0.100)	2.818 (0.099)	2.352 (0.021)	-0.151 (0.098)	-0.617*** (0.141)	0.466*** (0.094)
Born out of wedlock	0.683 (0.028)	0.610 (0.028)	0.574 (0.007)	0.109*** (0.030)	0.073* (0.039)	0.036 (0.029)
Ethnicity: African	0.880 (0.019)	0.896 (0.017)	0.826 (0.005)	0.055** (0.023)	-0.016 (0.026)	0.070*** (0.022)
Child of the head	0.553 (0.030)	0.555 (0.028)	0.544 (0.007)	0.009 (0.030)	-0.002 (0.041)	0.012 (0.029)
Zbmi	0.328 (0.085)	0.231 (0.075)	0.303 (0.017)	0.026 (0.079)	0.097 (0.113)	-0.071 (0.076)
Overweight/Obese	0.271 (0.026)	0.234 (0.024)	0.269 (0.006)	0.002 (0.027)	0.037 (0.036)	-0.036 (0.026)
Adequate BMI	0.687 (0.028)	0.727 (0.025)	0.698 (0.006)	-0.012 (0.028)	-0.041 (0.037)	0.029 (0.027)
Zhfa	-0.873 (0.085)	-0.935 (0.071)	-0.836 (0.017)	-0.037 (0.076)	0.061 (0.110)	-0.098 (0.073)
Stunted	0.187 (0.023)	0.143 (0.020)	0.155 (0.005)	0.032 (0.022)	0.044 (0.030)	-0.012 (0.021)
Ill/disable	0.056 (0.014)	0.032 (0.010)	0.038 (0.003)	0.019 (0.012)	0.024 (0.017)	-0.005 (0.011)
Covered by medical aid	0.053 (0.013)	0.045 (0.012)	0.092 (0.004)	-0.039** (0.017)	0.007 (0.018)	-0.047*** (0.017)
Poor subjective health status	0.007 (0.005)	0.000 (0.000)	0.003 (0.001)	0.004 (0.003)	0.007 (0.005)	-0.003 (0.003)
Fair subjective health status	0.035 (0.011)	0.013 (0.006)	0.018 (0.002)	0.017** (0.008)	0.022* (0.012)	-0.005 (0.008)
Good subjective health status	0.148 (0.021)	0.179 (0.022)	0.155 (0.005)	-0.007 (0.022)	-0.031 (0.030)	0.024 (0.021)
Very good subjective health status	0.278 (0.027)	0.390 (0.028)	0.321 (0.006)	-0.043 (0.028)	-0.111*** (0.039)	0.069** (0.027)
Father alive	0.905 (0.017)	0.899 (0.017)	0.923 (0.004)	-0.018 (0.016)	0.006 (0.024)	-0.024 (0.016)
Father and paternal grandparent co-resident	0.032 (0.010)	0.068 (0.014)	0.036 (0.002)	-0.005 (0.011)	-0.036** (0.018)	0.032*** (0.011)
Paternal grandparents alive	0.099 (0.018)	0.107 (0.018)	0.127 (0.004)	-0.029 (0.020)	-0.009 (0.025)	-0.020 (0.019)
Mother and maternal grandparent co-resident	0.254 (0.026)	0.321 (0.027)	0.321 (0.006)	-0.068** (0.028)	-0.068* (0.037)	0.000 (0.027)
Maternal grandparents alive	0.391 (0.029)	0.429 (0.028)	0.399 (0.007)	-0.008 (0.030)	-0.038 (0.041)	0.030 (0.029)
Observations	284	308	5,656	5,940	592	5,964

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. Foster children are those who will live out from the parental household in Wave 5. Standard errors are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table B.2: Mother and household characteristics across fostering status

	(1)	(2)	(3)	(1)-(3)	(1)-(2)	(2)-(3)
	Foster	Sibling	Control			
Mother: age	30.356 (0.452)	32.747 (0.449)	32.640 (0.106)	-2.285*** (0.483)	-2.391*** (0.638)	0.107 (0.465)
Mother: nb years in union	1.106 (0.234)	0.692 (0.161)	1.129 (0.049)	-0.023 (0.225)	0.414 (0.280)	-0.437** (0.214)
Mother: separated/divorced	0.000 (0.000)	0.006 (0.005)	0.016 (0.002)	-0.016** (0.008)	-0.006 (0.005)	-0.010 (0.007)
Mother: widow	0.028 (0.010)	0.032 (0.010)	0.034 (0.002)	-0.006 (0.011)	-0.004 (0.014)	-0.002 (0.011)
Mother: primary educated	0.077 (0.016)	0.084 (0.016)	0.084 (0.004)	-0.006 (0.017)	-0.007 (0.022)	0.001 (0.016)
Mother: secondary educated	0.722 (0.027)	0.724 (0.026)	0.697 (0.006)	0.025 (0.028)	-0.002 (0.037)	0.027 (0.027)
Mother: tertiary educated	0.169 (0.022)	0.159 (0.021)	0.189 (0.005)	-0.020 (0.024)	0.010 (0.030)	-0.030 (0.023)
Mother: diabetic	0.004 (0.004)	0.003 (0.003)	0.005 (0.001)	-0.002 (0.004)	0.000 (0.005)	-0.002 (0.004)
Mother: hiv	0.053 (0.013)	0.039 (0.011)	0.040 (0.003)	0.013 (0.012)	0.014 (0.017)	-0.001 (0.011)
Mother: decides about expenses	0.511 (0.030)	0.490 (0.029)	0.507 (0.007)	0.004 (0.030)	0.020 (0.041)	-0.017 (0.029)
Mother: poor subjective health status	0.007 (0.005)	0.026 (0.009)	0.012 (0.001)	-0.005 (0.006)	-0.019* (0.011)	0.014** (0.006)
Mother: fair subjective health status	0.049 (0.013)	0.049 (0.012)	0.051 (0.003)	-0.002 (0.013)	0.001 (0.018)	-0.003 (0.013)
Mother: good subjective health status	0.306 (0.027)	0.357 (0.027)	0.279 (0.006)	0.027 (0.027)	-0.051 (0.039)	0.078*** (0.026)
Mother: very good subjective health status	0.306 (0.027)	0.338 (0.027)	0.334 (0.006)	-0.028 (0.029)	-0.031 (0.039)	0.003 (0.028)
Rural	0.486 (0.030)	0.562 (0.028)	0.484 (0.007)	0.002 (0.030)	-0.076* (0.041)	0.078*** (0.029)
Household size	6.151 (0.181)	8.581 (0.236)	6.616 (0.044)	-0.465** (0.201)	-2.430*** (0.301)	1.965*** (0.197)
Head: female	0.711 (0.027)	0.688 (0.026)	0.652 (0.006)	0.059** (0.029)	0.023 (0.038)	0.036 (0.028)
Head: age	43.042 (0.900)	45.721 (0.769)	45.779 (0.198)	-2.737*** (0.908)	-2.679** (1.179)	-0.059 (0.869)
Wealth index [0-1]	0.441 (0.011)	0.492 (0.010)	0.536 (0.002)	-0.096*** (0.011)	-0.052*** (0.015)	-0.044*** (0.011)
Total expenses per adult equivalent	1,444.534 (117.766)	1,286.445 (107.101)	1,944.786 (41.956)	-500.252*** (189.096)	158.089 (158.833)	-658.341*** (181.532)
Head: primary educated	0.162 (0.022)	0.166 (0.021)	0.175 (0.005)	-0.013 (0.023)	-0.004 (0.031)	-0.009 (0.022)
Head: secondary educated	0.553 (0.030)	0.571 (0.028)	0.531 (0.007)	0.022 (0.030)	-0.019 (0.041)	0.041 (0.029)
Head: tertiary educated	0.183 (0.023)	0.130 (0.019)	0.152 (0.005)	0.031 (0.022)	0.053* (0.030)	-0.022 (0.021)
Household with retired pension	0.229 (0.025)	0.240 (0.024)	0.253 (0.006)	-0.024 (0.026)	-0.011 (0.035)	-0.012 (0.025)
Agricultural household	0.236 (0.025)	0.282 (0.026)	0.229 (0.006)	0.007 (0.026)	-0.047 (0.036)	0.054** (0.025)
Household received remittances	0.236 (0.025)	0.279 (0.026)	0.278 (0.006)	-0.042 (0.027)	-0.043 (0.036)	0.001 (0.026)
Household sent remittances	0.162 (0.022)	0.104 (0.017)	0.127 (0.004)	0.035* (0.020)	0.058** (0.028)	-0.023 (0.019)
Distance to police station	2.574 (0.064)	2.747 (0.059)	2.623 (0.014)	-0.049 (0.066)	-0.173** (0.087)	0.124* (0.064)
Negative household income shock last 2 years	0.123 (0.020)	0.146 (0.020)	0.106 (0.004)	0.017 (0.019)	-0.023 (0.028)	0.040** (0.018)
Observations	284	308	5,656	5,940	592	5,964

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. Foster children are those who will live out from the parental household in Wave 5. Standard errors are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table B.3: Children characteristics across observability status

	(1)	(2)	(1)-(2)
	Observed	Non observed	
Foster	0.039 (0.003)	0.087 (0.009)	-0.048*** (0.007)
Female	0.507 (0.007)	0.525 (0.015)	-0.019 (0.017)
Age	5.951 (0.051)	5.284 (0.114)	0.667*** (0.122)
Birth order	2.401 (0.024)	2.080 (0.043)	0.322*** (0.055)
Born out of wedlock	0.580 (0.007)	0.577 (0.015)	0.002 (0.017)
Ethnicity: African	0.830 (0.005)	0.819 (0.012)	0.012 (0.013)
Child of the head	0.554 (0.007)	0.499 (0.015)	0.055*** (0.017)
Zbmi	0.291 (0.019)	0.364 (0.040)	-0.073 (0.044)
Overweight/Obese	0.264 (0.006)	0.295 (0.014)	-0.031** (0.015)
Adequate BMI	0.704 (0.007)	0.669 (0.015)	0.034** (0.016)
Zhfa	-0.823 (0.017)	-0.911 (0.044)	0.088** (0.043)
Stunted	0.149 (0.005)	0.192 (0.012)	-0.043*** (0.012)
Ill/disable	0.038 (0.003)	0.044 (0.006)	-0.007 (0.007)
Covered by medical aid	0.085 (0.004)	0.117 (0.010)	-0.032*** (0.010)
Poor subjective health status	0.003 (0.001)	0.004 (0.002)	-0.001 (0.002)
Fair subjective health status	0.018 (0.002)	0.023 (0.005)	-0.005 (0.005)
Good subjective health status	0.155 (0.005)	0.151 (0.011)	0.004 (0.012)
Very good subjective health status	0.321 (0.007)	0.311 (0.014)	0.010 (0.016)
Father alive	0.918 (0.004)	0.942 (0.007)	-0.025*** (0.009)
Father and paternal grandparent co-resident	0.034 (0.003)	0.047 (0.007)	-0.013** (0.006)
Paternal grandparents alive	0.129 (0.005)	0.111 (0.010)	0.018 (0.011)
Mother and maternal grandparent co-resident	0.324 (0.007)	0.290 (0.014)	0.034** (0.016)
Maternal grandparents alive	0.400 (0.007)	0.393 (0.015)	0.007 (0.017)
Observations	4,897	1,043	5,940

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. Observed children are those for whom we can identify their nutritional status in Wave 5. Standard errors are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table B.4: Mother and household characteristics across child observability status

	(1) Observed	(2) Non observed	(1)-(2)
Mother: age	32.853 (0.114)	31.021 (0.239)	1.831*** (0.270)
Mother: nb years in union	1.129 (0.054)	1.122 (0.108)	0.007 (0.126)
Mother: separated/divorced	0.015 (0.002)	0.018 (0.004)	-0.003 (0.004)
Mother: widow	0.037 (0.003)	0.018 (0.004)	0.019*** (0.006)
Mother: primary educated	0.091 (0.004)	0.046 (0.006)	0.045*** (0.009)
Mother: secondary educated	0.697 (0.007)	0.706 (0.014)	-0.009 (0.016)
Mother: tertiary educated	0.180 (0.005)	0.224 (0.013)	-0.044*** (0.013)
Mother: diabetic	0.006 (0.001)	0.002 (0.001)	0.004 (0.002)
Mother: hiv	0.041 (0.003)	0.038 (0.006)	0.002 (0.007)
Mother: decides about expenses	0.517 (0.007)	0.458 (0.015)	0.059*** (0.017)
Mother: poor subjective health status	0.013 (0.002)	0.006 (0.002)	0.007* (0.004)
Mother: fair subjective health status	0.053 (0.003)	0.042 (0.006)	0.011 (0.008)
Mother: good subjective health status	0.285 (0.006)	0.260 (0.014)	0.025* (0.015)
Mother: very good subjective health status	0.333 (0.007)	0.334 (0.015)	-0.001 (0.016)
Rural	0.489 (0.007)	0.461 (0.015)	0.028 (0.017)
Household size	6.537 (0.047)	6.859 (0.106)	-0.322*** (0.113)
Head: female	0.662 (0.007)	0.620 (0.015)	0.042** (0.016)
Head: age	45.785 (0.212)	45.008 (0.479)	0.777 (0.510)
Wealth index [0-1]	0.529 (0.003)	0.545 (0.006)	-0.016** (0.006)
Total expenses per adult equivalent	1,821.995 (37.782)	2,385.090 (145.439)	-563.096*** (105.855)
Head: primary educated	0.177 (0.005)	0.163 (0.011)	0.014 (0.013)
Head: secondary educated	0.535 (0.007)	0.518 (0.015)	0.017 (0.017)
Head: tertiary educated	0.148 (0.005)	0.177 (0.012)	-0.029** (0.012)
Household with retired pension	0.252 (0.006)	0.250 (0.013)	0.002 (0.015)
Agricultural household	0.229 (0.006)	0.231 (0.013)	-0.002 (0.014)
Household received remittances	0.275 (0.006)	0.283 (0.014)	-0.008 (0.015)
Household sent remittances	0.129 (0.005)	0.129 (0.010)	-0.001 (0.011)
Distance to police station	2.632 (0.016)	2.568 (0.033)	0.064* (0.037)
Negative household income shock last 2 years	0.106 (0.004)	0.112 (0.010)	-0.006 (0.011)
Observations	4,897	1,043	5,940

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. Observed children are those for whom we can identify their nutritional status in Wave 5. Standard errors are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

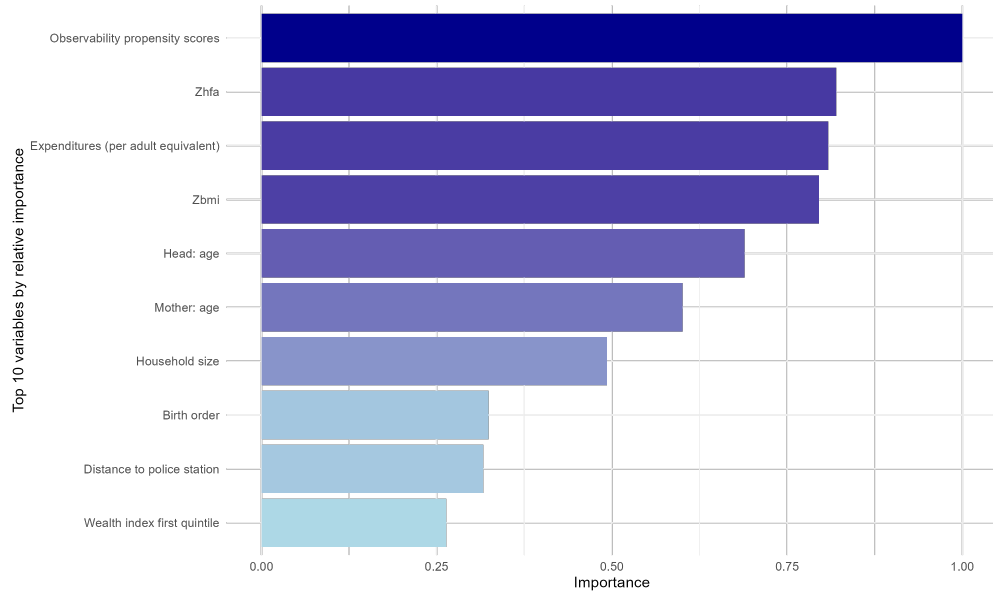


Figure B.1: Importance of covariates for explaining the treatment

Notes: Sample of children aged 0-12 at baseline (Wave 4). The graph shows the importance of covariates on an outcome variable, following a random forest with 3-fold cross-fitting. The outcome variable is a dummy equal to one if the child is fostered in Wave 5. Covariates are measured pre-treatment, the list is provided in Appendix A.2, and the propensity score of observability.

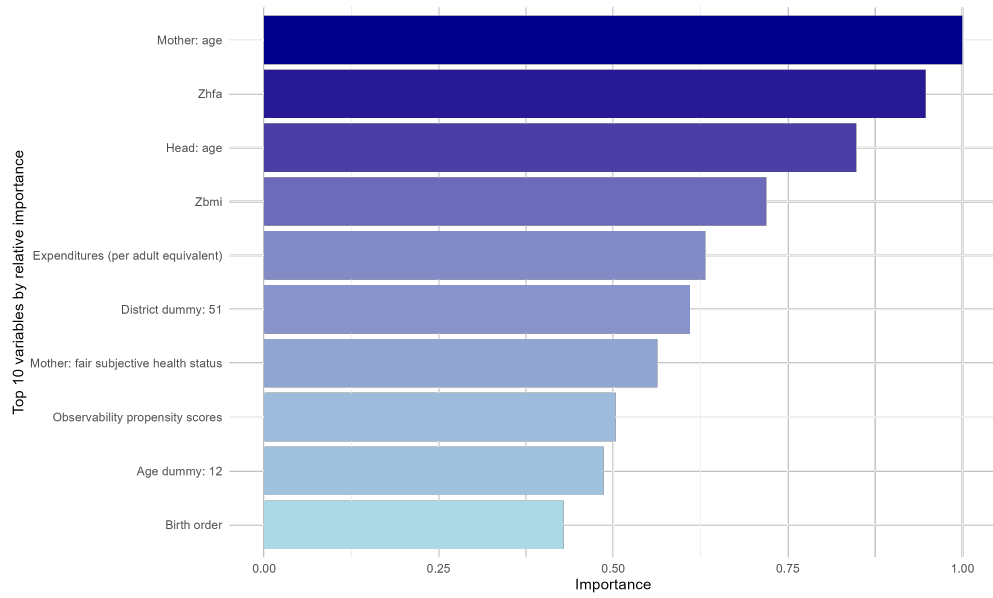


Figure B.2: Importance of covariates for explaining the Stunted outcome

Notes: Sample of children aged 0-12 at baseline (Wave 4). The graph shows the importance of covariates on an outcome variable, following a random forest with 3-fold cross-fitting. The outcome variable is a dummy equal to one if the child is fostered in Wave 5. Covariates are measured pre-treatment, the list is provided in Appendix A.2, and the propensity score of observability.

Table B.5: Nutritional status transition for foster and control children

Foster status	Status in Wave 4	Status in Wave 5		Total
Panel A : Stunted				
		Not stunted	Stunted	
Control	Not Stunted	95.14	4.86	100.00
	Stunted	50.29	49.71	100.00
Foster	Not Stunted	93.04	6.96	100.00
	Stunted	82.86	17.14	100.00
Panel B : Overweight/Obese				
		Not Overweight/Obese	Overweight/Obese	Total
Control	Not overweight/obese	92.31	7.69	100.00
	Overweight/Obese	45.92	54.08	100.00
Foster	Not Overweight/Obese	90.85	9.15	100.00
	Overweight/Obese	52.00	48.00	100.00
Panel C : Adequate BMI				
		Adequate BMI	Not Adequate BMI	Total
Control	Adequate BMI	89.30	10.70	100.00
	Not Adequate BMI	46.07	53.93	100.00
Foster	Adequate BMI	88.06	11.94	100.00
	Not Adequate BMI	55.17	44.83	100.00

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. The treatment is fostering. the sample is restricted to those observed in Wave 5.

Table B.6: Impact of fostering Waves 4-5 on children's nutrition in Wave 4

Outcome	ATE	Standard error	p-value	Baseline mean	Obs.	Obs. trimmed
Stunted	0.074	0.077	0.331	0.212	2,933	850
Overweight/Obese	-0.016	0.062	0.802	0.342	2,933	850
Adequate BMI	0.047	0.062	0.448	0.609	2,933	850

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. The treatment is fostering. The outcome variables are nutritional statuses based on BMI-for-age  $z$ -scores (adequate BMI and overweight/obese) and height-for-age  $z$ -scores (stunted), measured in Wave 4. Covariates are measured in Wave 3, and the list is provided in Appendix A.2, except lagged nutritional outcomes. DML: random forest with 3-fold cross-fitting and a trimming threshold of 0.01. Standard errors are based on asymptotic approximations using the estimated variance of the efficient score functions. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table B.7: Impact of a random treatment on children's nutrition

Outcome	ATE	Standard error	p-value	Baseline mean	Obs.	Obs. trimmed
Stunted	0.033	0.029	0.258	0.156	5,940	412
Overweight/Obese	0.005	0.033	0.877	0.269	5,940	398
Adequate BMI	-0.011	0.034	0.753	0.698	5,940	398

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. The treatment is assigned at random. The outcome variables are nutritional statuses based on BMI-for-age  $z$ -scores (adequate BMI and overweight/obese) and height-for-age  $z$ -scores (stunted), measured in Wave 4. Covariates are measured pre-treatment, and the list is provided in Appendix A.2. DML: random forest with 3-fold cross-fitting and a trimming threshold of 0.01. Standard errors are based on asymptotic approximations using the estimated variance of the efficient score functions. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table B.8: Impact of fostering on children's nutrition with a stricter trimming rule

Outcome	ATE	Standard error	p-value	Baseline mean	Obs.	Obs. trimmed
Stunted	-0.064***	0.023	0.006	0.156	5,940	2,124
Overweight/Obese	-0.033	0.031	0.280	0.269	5,940	2,108
Adequate BMI	0.058*	0.033	0.079	0.698	5,940	2,108

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. The treatment is fostering. The outcome variables are nutritional statuses based on BMI-for-age  $z$ -scores (adequate BMI and overweight/obese) and height-for-age  $z$ -scores (stunted), and are measured in Wave 5. Covariates are measured pre-treatment, and the list is provided in Appendix A.2. DML: random forest with 3-fold cross-fitting and a trimming threshold of 0.02. Standard errors are based on asymptotic approximations using the estimated variance of the estimated efficient score functions. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$



Table B.9: Impact of fostering on children’s nutrition, baseline outcome dummies

Outcome	ATE	Standard error	<i>p</i> -value	Baseline mean	Obs.	Obs. trimmed
Stunted	-0.065***	0.018	0.000	0.156	5,940	1,006
Overweight/Obese	-0.030	0.034	0.377	0.269	5,940	999
Adequate BMI	0.074**	0.030	0.014	0.698	5,940	998

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. The treatment is fostering. The outcome variables are nutritional statuses based on BMI-for-age *z*-scores (adequate BMI and overweight/obese) and height-for-age *z*-scores (stunted), and are measured in Wave 5. Covariates are measured pre-treatment, and the list is provided in Appendix A.2. In contrast to the main specification, the covariates *Zhfa*, *Zbmi* are replaced by the dummy variables Adequate BMI, Stunted for the outcomes Adequate BMI and Stunted or are replaced by the dummy variables Overweight/Obese and Stunted for the ATE estimate of Overweight/Obese. DML: random forest with 3-fold cross-fitting and a trimming threshold of 0.01. Standard errors are based on asymptotic approximations using the estimated variance of the estimated efficient score functions.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table B.10: Alternative methods to estimating the impact of fostering on overweight/obese

	Estimator	ATE	Standard error	<i>p</i> -value	Obs.	Obs. on support
1	IPW observability and treatment (RF)	-0.082***	0.027	0.003	5,940	4,972
2	Conditional on X and IPW observability (RF)	-0.013	0.028	0.646	5,940	4,972
3	DML (Logit)	0.033	0.090	0.715	5,940	2,124
4	DML not accounting for observability (RF)	-0.050	0.041	0.226	4,878	3,123
5	DiD Conditional on X (OLS)	-0.003	0.042	0.946	4,878	9,756
6	DiD FE individual (OLS)	-0.003	0.031	0.927	4,878	9,756
7	DiD (OLS) IPW treatment (Logit)	0.012	0.026	0.644	4,878	4,222
8	DiD (OLS) IPW treatment (RF)	0.029*	0.018	0.095	4,878	4,674
9	IPW treatment (Logit)	-0.054***	0.011	0.000	4,878	4,217
10	IPW treatment (RF)	0.018	0.012	0.141	4,878	4,675
11	Cross section Wave 5 (OLS)	-0.013	0.038	0.736	8,136	8,136

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. The treatment is fostering, defined for estimators 1-10 as detailed in Section 2.4. For estimator 11, fostering is defined more broadly as children living without either parent while at least one parent is still alive in Wave 5. All estimators labeled *RF* rely on random forests with 3-fold cross-fitting. *IPW* refers to inverse probability weighting for the treatment (fostering), using propensity scores estimated from all covariates listed in Appendix A.2. Estimators (1) and (2) decompose our main identification strategy: (1) uses a combined *IPW* that incorporates both observability and treatment propensity scores; (2) applies *IPW* for observability only and adjusts for the outcome conditional on covariates. Estimators (3) to (11), do not account for observability thus the sample is restricted to observed children in Wave 5. For all estimators based on the *treatselDML* (1,2,4) or *treatsel* (3) commands, a trimming threshold of 0.01 is applied to ensure common support. For the remaining estimators, the sample is restricted to the strict common support of the estimated propensity scores. In estimator (11), the set of covariates is measured in Wave 5 and includes: age dummies, gender, birth order, born out of wedlock, African dummy, relationship to household head, illness/disability, medical aid coverage, father alive, rural dummy, household size, female head, heads age and education, wealth quintiles, total expenditure per adult equivalent, distance to nearest police station, and dummies for receiving pension, receiving/sending remittances, agricultural activity, and recent income shocks. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table B.11: Alternative methods to estimating the impact of fostering on adequate BMI

	Estimator	ATE	Standard error	p-value	Obs.	Obs. on support
1	IPW observability and treatment (RF)	0.105	0.091	0.248	5,940	4,972
2	Conditional on X and IPW observability (RF)	0.025	0.030	0.419	5,940	4,972
3	DML (Logit)	-0.023	0.087	0.796	5,940	2,124
4	DML not accounting for observability (RF)	0.085**	0.043	0.050	4,878	3,123
5	DiD Conditional on X (OLS)	0.022	0.045	0.617	9,756	4,878
6	DiD FE individual (OLS)	0.022	0.034	0.508	9,756	4,878
7	DiD (OLS) IPW treatment (Logit)	0.013	0.027	0.633	9,756	4,222
8	DiD (OLS) IPW treatment (RF)	-0.003	0.018	0.878	9,756	4,674
9	IPW treatment(Logit)	0.073***	0.012	0.000	4,878	4,217
10	IPW treatment (RF)	0.008	0.012	0.510	4,878	4,674
11	Cross section Wave 5 (OLS)	0.015	0.041	0.729	8,136	8,136

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the treated and the control groups. The treatment is fostering, defined for estimators 1-10 as detailed in Section 2.4. For estimator 11, fostering is defined more broadly as children living without either parent while at least one parent is still alive in Wave 5. All estimators labeled *RF* rely on random forests with 3-fold cross-fitting. *IPW* refers to inverse probability weighting for the treatment (fostering), using propensity scores estimated from all covariates listed in Appendix A.2. Estimators (1) and (2) decompose our main identification strategy: (1) uses a combined *IPW* that incorporates both observability and treatment propensity scores; (2) applies *IPW* for observability only and adjusts for the outcome conditional on covariates. Estimators (3) to (11), do not account for observability thus the sample is restricted to observed children in Wave 5. For all estimators based on the *treatselDML* (1,2,4) or *treatsel* (3) commands, a trimming threshold of 0.01 is applied to ensure common support. For the remaining estimators, the sample is restricted to the strict common support of the estimated propensity scores. In estimator (11), the set of covariates is measured in Wave 5 and includes: age dummies, gender, birth order, born out of wedlock, African dummy, relationship to household head, illness/disability, medical aid coverage, father alive, rural dummy, household size, female head, heads age and education, wealth quintiles, total expenditure per adult equivalent, distance to nearest police station, and dummies for receiving pension, receiving/sending remittances, agricultural activity, and recent income shocks. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table B.12: Impact of being left behind on children's nutrition

Outcome	ATE	Standard error	p-value	Baseline mean	Obs.	Obs. trimmed
Stunted	0.026	0.026	0.312	0.157	6,016	1,087
Overweight/Obese	-0.021	0.028	0.460	0.268	6,016	1,090
Adequate BMI	0.022	0.031	0.478	0.700	6,016	1,090

Notes: Sample of children aged 0-12 years at baseline (Wave 4) who belong to the left behind children and the control groups. The treatment left-behind children, i.e. these are children that are not co-residing with any parent at Wave 5 and have at least a parent alive, while they were co-resident with at least one parent in Wave 4, disregarding of the change of household unit. The outcome variables are nutritional statuses based on BMI-for-age *z*-scores (adequate BMI and overweight/obese) and height-for-age *z*-scores (stunted), and are measured in Wave 5. Covariates are measured pre-treatment, and the list is provided in Appendix A.2. DML: random forest with 3-fold cross-fitting and a trimming threshold of 0.01. Standard errors are based on asymptotic approximations using the estimated variance of the efficient score functions. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$