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Abstract

We combine data on Chinese development projects with data from Demographic and Health Surveys to study the impact of Chinese aid on household welfare in sub-Saharan Africa. We use a novel methodology to test the effect of Chinese aid on three important development outcomes: education, health, and nutrition. For each outcome, we use difference-in-difference estimations to compare household areas near Chinese project sites to control areas located farther away, before and after receiving Chinese aid. This empirical strategy rules out many confounding factors that can bias measuring the impact of Chinese aid on our outcome variables. First, we find that Chinese projects significantly improve education and child mortality in treatment areas, but do not significantly affect nutrition. Second, social sector projects have a larger effect on outcomes than economic projects. Third, we do not find significant effects for projects that ended more than five years before the post-treatment survey wave. Our results are robust to a host of robustness checks.

Keywords

Aid effectiveness; Chinese aid; household welfare; DHS; geocoded data

JEL classifications: F35, O19, R20

1. Introduction[*]

An "earth-shaking rise" is how the Diplomat, an international online news magazine, described China's evolution from a poor country to a global superpower within no less than 30 years.[1] The international community, however, is torn between admiration for the country's achievements and criticism for its authoritarian leadership style, human rights violations, and aggressive business practices. This also applies to China's new role as an international donor. Formerly an aid recipient itself, China is assumed to be the largest of the so-called emerging donors (Rudyak 2018). Speculation is rife about the effects of Chinese foreign assistance on recipient countries (see Naím 2007), but there is still little evidence on its actual consequences. This applies to sub-Saharan Africa especially, where China has ramped up its activities not long after the turn of the millennium.[2]

Assessing the allocation and effectiveness of Chinese aid is generally challenging: the Chinese government barely publishes information on its foreign assistance; but data availability has greatly improved since the AidData research lab first issued the *Chinese Official Finance Dataset* in 2017– giving rise to a new body of quantitative research on Chinese foreign assistance. A number of studies in the field focus on the drivers of Chinese aid allocation (see Strange et al. 2013, Dreher et al. 2014, Strange et al. 2014, Dreher et al. 2015, Dreher and Fuchs 2015, Strange et al. 2015, Kilama 2016). Another strand of literature explores the governance dimension of Chinese aid. A study by Isaksson and Kotsadam (2018) finds a positive relationship between Chinese aid and local corruption. Gehring, Wong, and Kaplan (2018) investigates the relationship between Chinese aid and conflict in Sub-Saharan Africa. They find that, if anything, Chinese aid tends to reduce conflict. Wegenast, Strüver, Giesen, and Krause (2017) finds that Chinese controlled mining stirs anti-Chinese sentiments on the one hand, but improves access to infrastructure (paved roads and piped water) on the other. Research on the effectiveness of China's aid is conducted too, but to a considerably lesser degree. Dreher et al. (2016), Dreher et al. (2017), and Bluhm et al. (2018) are the only studies we are aware of that do so and they all focus on economic outcomes. Dreher et al. (2016) and Dreher et al. (2017) finds a positive effect of Chinese aid on growth in recipient countries. A recent working paper by Bluhm et al. (2018) explores the effect of Chinese aid projects on the distribution of subnational economic activity in low- and middle-income countries. The findings indicate that infrastructure projects in particular reduce economic inequality (measured with nighttime light data) between subnational localities.

In view of China's growing influence as an international donor, we need to better understand its impact on a broader set of development outcomes, beyond pure economic effects. First, although China is mainly known for its focus on business and infrastructure projects in sub-Saharan Africa, it is active in other sectors as well, especially education and health (Rudyak 2018, Shajalal et al. 2017, Strange et al. 2014, King 2014, King 2010, Bräutigam 2009). Contrary to common perception, China's engagement in these areas goes beyond mere provision of infrastructure and includes activities such as training cooperation and scholarships (Shajalal et al. 2017, King 2010). For example, during the five year period between 2009 and 2013, China increased its health-related assistance in sub-Saharan African by 146.26% (Shajalal et al. 2017). Second, economic projects are likely to have indirect effects on other dimensions of well-being as well, including education and health (Rudyak 2018, King 2010).

[*] We thank Axel Dreher, Samuel Brazys, Brad Park and participants to a workshop at Heidelberg University, to 2nd UNU-MERIT Internal Conference in Maastricht,, to the Annual International Conference of the German Economic Association Research Group on Development Economics at the University of Zurich and to the ASSET Conference in Florence, for useful comments and discussions. We also thank Francesco Iacoella for excellent research assistance. This project has received funding from the European Union's Horizon 2020 research and innovation programme under grant agreement No 770680.

[1] https://thediplomat.com/2011/02/understanding-chinas-global-impact/

[2] See Kobayashi (2008) and Bräutigam (2009) for a discussion of the evolution of Chinese aid since the 1990s.

Improvements in health can result indirectly from projects with relevant spillover effects such as access to electricity, piped water, road infrastructure and improved household wealth (Kotsadam et al. 2018). Similarly, transportation infrastructure such as roads, which makes up at least 21% of Chinese aid, can facilitate access to education for a large number of pupils, and increased household wealth through better access to jobs can improve households' ability to cover school fees.

Against this background, this paper investigates the effectiveness of Chinese aid in sub-Saharan Africa (SSA): the poorest region in the world, the largest recipient of Chinese aid[3], and a major destination for other forms of economic cooperation from China (Brautigam 2009, Sanfilippo, 2010). For 13 sub-Saharan African countries, we analyze to what extent Chinese aid projects impact household welfare in three highly poverty relevant areas: education (average years of education and maximum educational attainment), health (child mortality), and nutrition (body mass index and weight for height). Moreover, we perform a sector level analysis where we simultaneously considering the impact of social and economic projects on each outcome variable. The purpose of this analysis is to test for heterogeneous effect across different sectors and unravel potential channels through which Chinese projects affect households' social welfare. This links to the point made before that Chinese aid, even if purely economic, can have important spillover effects on other dimensions of development. To the best of our knowledge, this study is the first to provide quantitative evidence on the effectiveness of Chinese project aid at the micro-level and beyond economic outcomes.

Our analysis relies on observational data that is prone to selection bias: Chinese aid projects are unlikely randomly allocated across countries or regions within countries. In order to obtain reliable estimates of the effect of Chinese aid, we apply a novel identification strategy that exploits the high level of geographic disaggregation of our data and the timing of Chinese aid in Africa (see also Section 2). First, we match georeferenced data on Chinese aid projects available from *AidData* to georeferenced household data in the Demographic and Health Survey (DHS) at two points in time: before and after the inflow of Chinese aid, our treatment variable. Second, using a difference-and-difference estimator, we compare the wellbeing of households located within 25 kilometers of at least one aid project – at any point in time between the two DHS survey waves - to the wellbeing of control households located farther away. Instead of comparing individual households, we aggregate all variables at the area level (see Section 2.3). That means, we compare the same household areas[4] before and after treatment. This greatly increases the efficiency of our estimates (Hsiao et al. 1995).[5] Third, we use propensity score matching, inverse probability weighting to be precise, to further reduce selection bias between treatment and control areas. Since data on the number of Chinese projects on the ground as well as project finance volumes is patchy, for our main analysis, we use a binary treatment dummy to indicate the presence of a project. However, we also want to consider treatment intensity and extend our main analysis, first, by replacing the treatment dummy with the number of projects and, second, by project finance volume. Moreover, we estimate a set of regressions that consider the temporal distance between the timing of treatment and post-treatment survey year. Treatment effects might take more or less time to materialize depending on the welfare dimension we look at, or they may entirely die down after a while.

Our research contributes to two important strands in the aid literature. First, it contributes to the growing body of cross-country research that uses project based data in order to study aid effectiveness at the sub-national level (see Kilby 2000, Dollar and Svensson 2000, Guillaumont and Laajaj 2006, Denizer et al. 2013, Bulman et al. 2015, Dreher and Lohmann 2015, Metzger and Günther 2015, Briggs

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[3] See the first China White Paper on Foreign Aid published in 2011: http://english.gov.cn/archive/white_paper/2014/09/09/content_281474986284620.htm

[4] We construct these areas based on their geographical proximity to a Chinese aid project. These areas are not identical to predefined DHS household clusters.

[5] Since the DHS survey is not a panel, we cannot compare the same households at two different points in time, namely before and after treatment. However, we can compare households located in *same area* at two different points in time, allowing us to achieve a high comparability between households across survey waves.

2018, Kotsadam et al. 2018). We add to the newer literature in this field which relies on georeferenced micro-level data and quasi-experimental methods in order to causally identify local effects of aid (Kotsadam and Tolonen 2016, Isaksson and Kotsadam 2018, Kotsadam et al. 2018).[6] Such studies have the important advantage that they can consider the influence of micro- and macro-level factors on development outcomes and, thus, bridge the gap between macro- and microeconomic research. Second, our study contributes to the small, but important, body of quantitative economic literature on the effectiveness of Chinese aid (see e.g., Dreher et al. 2016, Dreher et al. 2017; Bluhm et al. 2018). While evidence on the effectiveness of Chinese aid is scarce, some hints are available. On one hand, China's South-South cooperation philosophy[7] may lead to higher ownership, larger freedom of choice and, thus, better targeting of aid in recipient countries, rendering foreign assistance more effective. Moreover, China's emphasis on project aid as well as on technical cooperation and training can be an effective way to promote local economic development in the short-term (being mostly of the "early-impact" type of aid projects as discussed by Clemens et al. 2012). Also, China's comparatively strong focus on big scale economic cooperation may create opportunities for the local population directly, and lead to a better integration of national markets into the global economy in the longer-term. The positive relationship between Chinese aid and regional (Dreher et al. 2016) and national growth (Dreher et al. 2017) lends support to this view. On the other hand, China's non-interference and no-strings-attached loan policy can threaten debt sustainability (Kilama 2016), and seems to negatively affect good governance and foster corruption (Isaksson and Kotsadam 2018, Wegenast, Strüver, Giesen, and Krause 2017).

To what extent Chinese aid projects affect important development outcomes such as education, health, and nutrition is a widely understudied question that we explore in this study. Our main results consistently point to an overall positive development impact of Chinese project assistance on education and child mortality. Households in areas that receive Chinese projects tend to stay in school longer, have a higher educational attainment, and experience a reduction in child mortality. However, we do not detect a significant effect on nutrition-based outcomes. These findings are confirmed by the sector level analysis where we, in addition, find evidence for heterogeneous effects of aid: social sector projects have a somewhat larger effect on education and child mortality than economic projects. This finding suggests that a sectoral analysis is an interesting starting point to obtain more nuanced insights into different channels through which Chinese projects affect household welfare. Concerning the analysis on treatment timing, we find that treatment effects are not detectable for projects that occurred more than three to five years back in time. We also detect a very similar non-linear relationship between treatment intensity and education levels for both, the number of projects and financial volume. Lower level treatment intensity (below the median) has a significant positive effect on education outcomes. However, this effect disappears for higher levels of Chinese aid (above the median). There is no effect, whatsoever, for negative returns to increasing levels of Chinese aid on our outcome variables. Last, our results are robust to various cuts to the data and to a placebo exercise in which we exploit the available information on aid projects announced but never realized instead of completed ones.

2. Data and Methods

Our analysis is based on the combination of two datasets: (1) *Aid Data's Chinese Official Finance to Africa* dataset, and (2) the Demographic and Health Survey (DHS). Our sample consists of 13 African countries for which at least two DHS survey waves are available and for which the first survey wave corresponds to a period that is characterized by little or no Chinese aid activities (around the year 2000). These countries are: Benin, Cote d'Ivoire, Ethiopia, Ghana, Guinea, Kenya, Malawi, Namibia, Nigeria, Senegal, Togo, Uganda and Zimbabwe.

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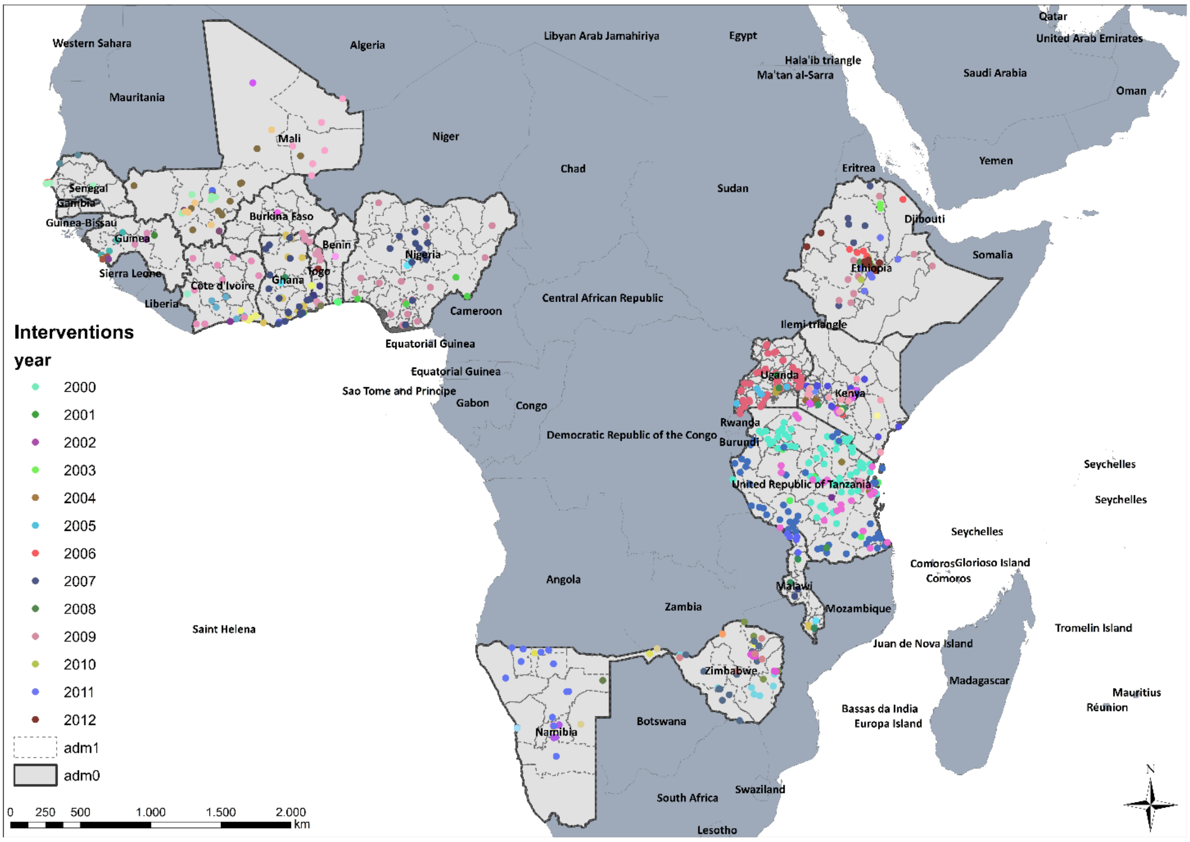
6 Benyishay et al. (2017) provides a review on how increasingly available disaggregated geospatial data allows for applying a variety of impact evaluation methods to assessing the effectiveness of aid, among others.

7 China's foreign assistance is guided by eight principles. For details see: http://www.china.org.cn/opinion/2011-11/29/content_24030234.htm

2.1 Aid Data

The *Chinese Official Finance to Africa* dataset comprises 1,955 geocoded projects in 50 African countries, spanning 3,545 locations and covering the years 2000 to 2012. The countries included in our sample account for 852 projects in 1,745 locations, which are widely distributed within countries and over time (see Figure 1).

Figure 1. Mapping of Chinese Aid Projects by Destination Countries



Source: Authors' elaboration on AidData

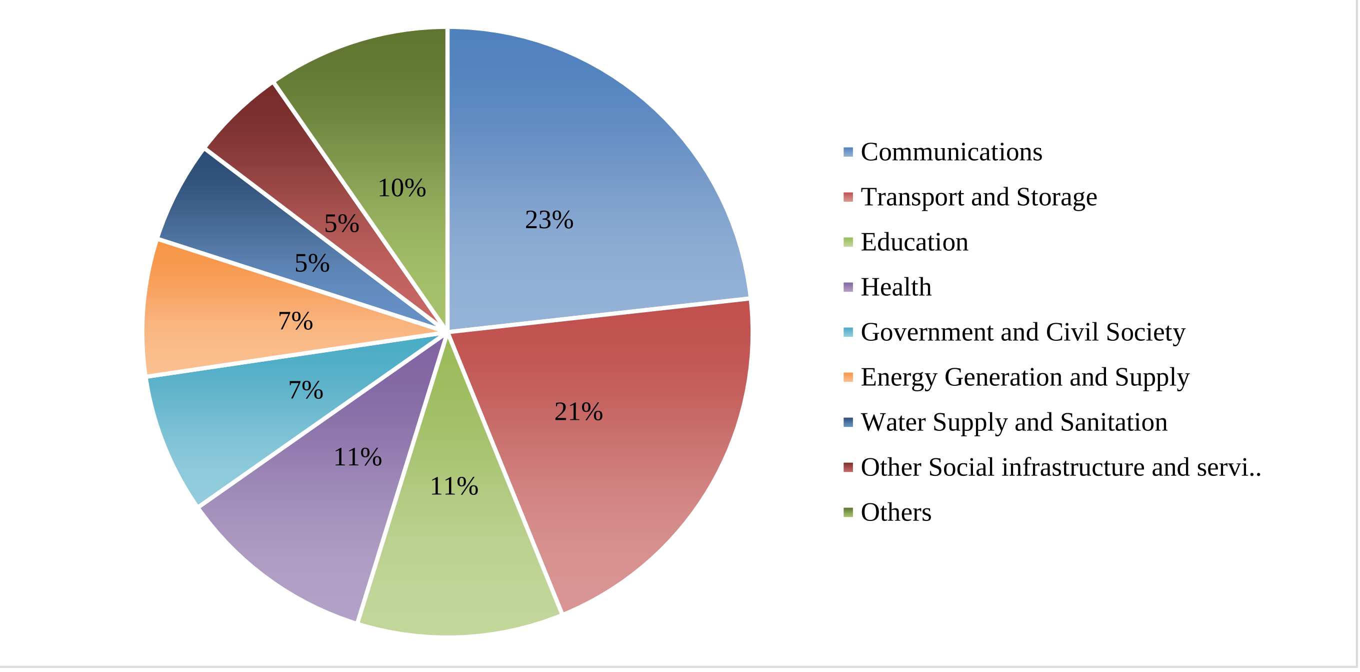
For each project, the database provides detailed information on its location, the sector it belongs to (classified following the OECD Creditor Report System (CSR) purpose codes), its financial volume, the type of flow[8] (e.g. ODA or other official flows, OOF) and its status (e.g. planned or implemented).

We make two main adjustments to the data. First, a large majority (67.4%) of projects are classified either as "completed" or "implemented", while the rest is "in the pipeline". Projects in the pipeline have not yet formally started; 11.3% of them are pledged only. Since their implementation is uncertain, we

[8] In the case of China, it is important to distinguish between official development assistance (ODA)-like flows and other official flows (OOF). According to the DAC definition, ODA is (a) provided by official agencies to developing countries, (b) aimed at promoting economic development and welfare, and (c) contains a grant element of at least 25 percent. OOF are also funded by government agencies, but not primarily aimed at development goals and/or not sufficiently concessional to categorise as ODA. Results from an empirical work by Dreher et al (2015) show that Chinese ODA to Africa are not significantly correlated with national political institutions, while OOF-like flows are more likely to go to countries with higher corruption levels.

exclude these projects from our analysis.[9] Second, we only consider projects that have a relatively precise location, since it is key to our identification strategy to define control and treatment areas as precisely as possible. The AidData data set provides a precision code for each georeferenced project, indicating the level of granularity of the GPS data.[10] Following common practice in the relevant literature (see e.g. Dreher et al., 2015; Briggs, 2018), we consider projects with a precision code of up to 4, meaning that the project location is – in the least granular case – analogous to a first order administrative division such as a province, state or governorate.[11] 67% of the projects in our sample fall into this category. Our final sample counts 878 projects. Overall, transport, communication and energy projects account for 44% of projects. Education and health projects account for about 22% of all projects (Figure 2). This is particularly true for Ghana, where education represents 26% of projects, as well as Guinea and Malawi.

Figure 2. Distribution of Chinese aid projects by sector



Source: Authors' elaboration on AidData

Most projects in our sample are categorized as ODA-like (53.2%); 41.8% are classified as Vague Official Finance, since information required for a more specific categorization is lacking. Only 4.3% of all projects are classified as other official flows (OOF): commercial activities mostly related to the communication sector and loans to build telecom infrastructure. Moreover, above 46% of project financing is provided in the form of loans.

There are some limitations to the AidData dataset that should be kept in mind for our analysis (Strange et al. 2013). First, China does not officially report data on its foreign assistance. Project-related data is compiled from various sources, including government reports, the media, the private sector, and civil society organizations. Consequently, the data might be not fully representative of Chinese assistance to sub-Saharan African countries. Second, a bias in China publicly reporting certain projects but not others cannot be ruled out (Strange et al. 2013). Third, since China's foreign assistance does not

9 We will use this information in the rest of the paper to conduct robustness checks (see Section 3.3.4).

10 http://docs.aiddata.org/ad4/files/geocoding-methodology-updated-2017-06.pdf

11 For more details see Aid Data's methodological notes: http://china.aiddata.org/content/methodology

adhere to the OCED-DAC Creditor Reporting System (CRS), it cannot be categorized as precisely as aid from OECD-DAC donors. This reduces the comparability between aid flows from China and OECD-DAC members. Finally, information on financial disbursements to projects is not available at the location level (i.e. the geographic project site level). Financial disbursement data is only available at the overall project level. In light of this, we do not use information on projects' financial flows in the main analysis. Instead, we indicate the presence of a project in any given geographical location with a dummy variable. This is the treatment dummy in our difference-in-difference estimation. However, in order to take treatment intensity into account, we repeat our main analysis by replacing the treatment dummy with (a) the number of projects going to certain areas (see Table 6), and (b) the project finance volume (see Table 7). Since financial disbursement information is not available at the local level, we, again, follow common practice in the relevant literature and divide the total financial disbursement to a project by the number of its locations (see e.g. Dreher et al. 2016).

2.2 Demographic and Health Survey (DHS)

The DHS is a large-scale survey program. Data are nationally representative and include information on a wide range of indicators of households' wellbeing, including assets, education, health, and nutrition. All DHS surveys are based on largely standardized questionnaires, which allows us to analyze and compare the effect of Chinese aid on household welfare across countries. For this study, we use DHS survey data on 13 African countries. We consider two data points for each country. We define the pre-treatment wave to be before or very close to the year 2000. Until 1999, China was a recipient of development assistance (Galiani et al. 2017) itself and not very active as a donor in Sub-Saharan Africa. This changed with the establishment of the Forum on China-Africa Cooperation (FOCAC) in 2000 and the financial pledges China made to African governments within this framework (Lum et al. 2009; Brautigam 2009).[12] Financial flows picked up from 2006 in particular, when China significantly increased its development assistance to sub-Saharan Africa and moreover installed the China-Africa Development Fund for 5 billion dollars (Rudyak 2018). Figure A1 in the Appendix illustrates this development: the provision of Chinese loans, a major component of China's overall aid flows, were nearly zero around 2000 for the continent as a whole and then increased significantly afterwards. Consequently, we define the post-treatment wave to start well after the year 2000. Pre- and post-treatment waves vary for every country in the sample. The countries and their corresponding survey years are listed in Table A1 in the Appendix. Taken together, the surveys include information on 297,459 households.

2.2.1 Choice of outcome variables

We assess the impact of Chinese aid projects on three highly poverty relevant outcomes: education, health and nutrition. Our specific choice of outcome variables within these domains is guided by methodological concerns. The internal validity of our difference-in-difference analysis crucially depends on the parallel trend assumption which requires that in the absence of any treatment, the difference between the treatment and control group be constant over time. We tested this assumption for various education, health and nutrition indicators and were able to confirm that it holds for five indicators:

(a) the average years of education. The average years of education in each household (counting only members over 25 years of age)

[12] Malawi represents an exception since it was not affected by the first FOCAC, as it has established diplomatic relations with China only in 2008. In this specific case, we can compare HH's living conditions in 2004 (a few years before relations with China normalized) with those in 2010 (i.e. a few years after).

(b) The average educational attainment in each household. Educational attainment consists of six categories that are coded as follows: (0) “none”, (1) “incomplete primary”, (2) “complete primary”, (3) “incomplete secondary”, (4) “complete secondary”, and (5) “higher education”.

(c) Child mortality. Child mortality refers to children dead between 1 and 5 years old over total number of children born. It is calculated as number of children died for a single woman per household over the total number of children born from that woman.

(d) Weight for Height Z-score. The weight for height z-score is our first anthropometric measure that simple compares weight and height. This is computed multiplying weight (in Kg) for standing height (in cm).

(e) BMI for age Z-score. The Body Mass Index (BMI) is a commonly used index of weight-to-height. It is calculated dividing weight (in Kg) over standing height (in cm). To make it more reliable for non-adults, age (in months) is added to the computation of the BMI.

Details on the parallel trend assumption analysis are provided in Figure A2 in the Appendix. All indicators are taken from the relevant DHS surveys. Summary statistics of all variables at the household and area level are provided in Table 1.

2.3 Empirical Strategy

In order to link household data from the DHS to project data in *AidData* we use georeferenced information. Household clusters surveyed in the DHS and Chinese aid projects in *AidData* both have point coordinates based on GPS; hence, we match household and project data via their GPS coordinates. The *AidData* data set moreover contains information on the point in time in which an aid project was established. Combining all this data enables us to compare the welfare of control and treatment household areas before and after the implementation of a Chinese aid project.[13] Our empirical strategy leans on recent studies that evaluate the impact of aid projects at the sub-national level by means of a difference-in-differences (DiD) approach (Kotsadam and Tolonen 2016, Isaksson and Kostadam 2018, Wegenast et al. 2016, Kotsadam et al. 2018). However, it differs from previous work in the following aspects.

Our estimation strategy, the DiD, grounds on the premise that both DHS waves (before and after treatment with a Chinese aid project) are based on the same underlying population. However, since the DHS is not a panel, different survey waves are based on different sets of clusters and households within clusters. The chance that the same households are interviewed in both survey waves is extremely low. In order to deal with this limitation, which can potentially bias our results, we follow three steps.

First, we move the analysis from the level of household clusters to the area level. Areas are defined as unique boundaries sharing the first decimal place for both latitude and longitude coordinates[14]. For each DHS survey wave, we identify the household clusters located within such an area. Using the first decimal is a good strategy to balance the number of clusters and the degree of homogeneity of the households included in each area. We then compare the same area with respect to our specified outcome variables before and after the treatment with a Chinese aid project. Figure 3 provides a graphical representation of this procedure, using Ethiopia as an example. Map 1 reports the geographic distribution of the newly constructed household areas in both survey waves. Map 2 in Figure 3 zooms in and shows the steps through which we move from household clusters to areas in more detail. Purple dots and green

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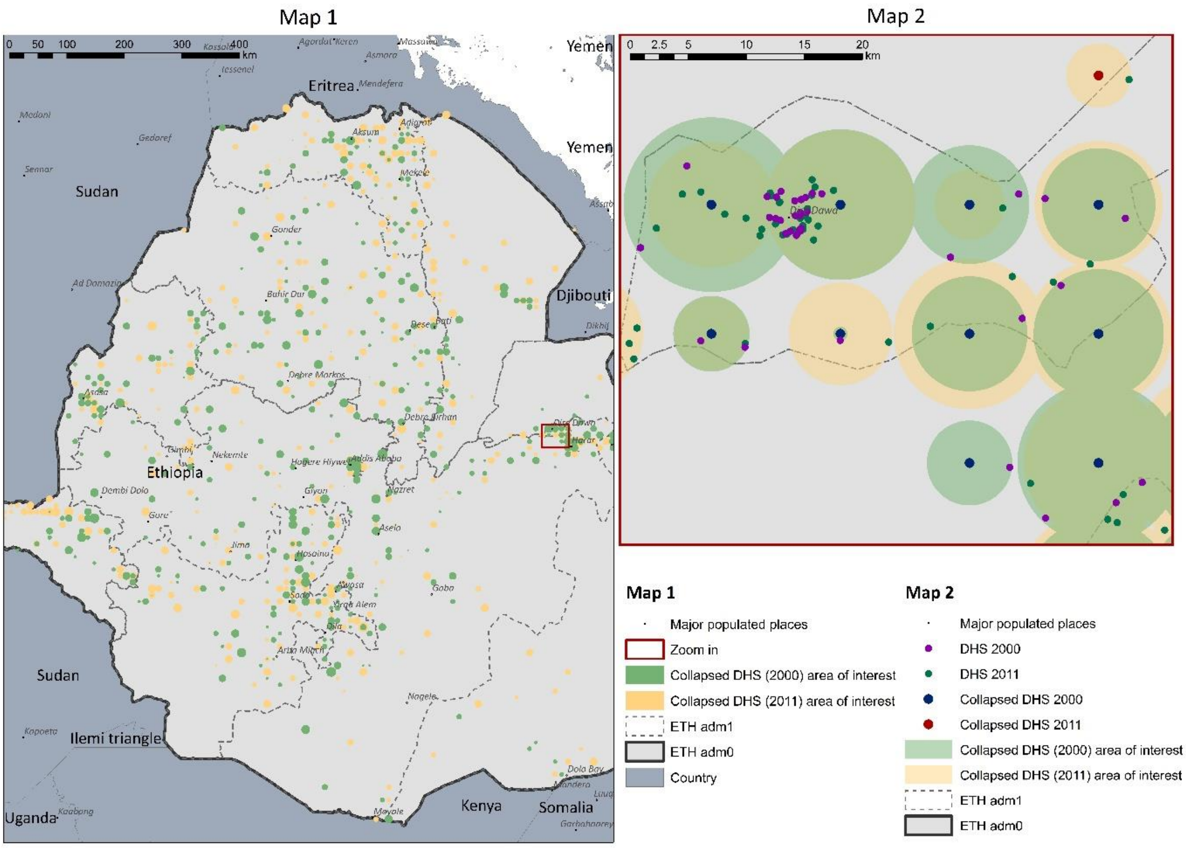
13 Each DHS cluster is randomly reassigned a GPS location that falls within a specified distance of the actual location (0-2 km for urban DHS clusters and 0-5 km for rural clusters, with one percent of clusters randomly selected to be displaced by up to 10km). This is done to preserve the anonymity of survey respondents. While we cannot control directly for this, our strategy to work with larger buffer zones (as detailed in the rest of the section) is in line with standard practice in the literature and should partially attenuate this problem (see Gollin et al. 2017).

14 Each decimal roughly corresponds to 11.1 km.

dots are the original clusters from each DHS survey wave - 2000 and 2011 respectively for Ethiopia. Blue and red dots are the centroid of each newly constructed household areas in 2000 and 2011, respectively. Green (2000 wave) and yellow (2011 wave) circles around those dots indicate the entire area. That means, each newly constructed area is comprised of several DHS household clusters. The number of clusters varies by area. For each of these areas, we compute the average for all household characteristics and outcome variables that we use in our analysis[15]. We do this for every survey wave and then match the same areas across the two waves. [16] For Ethiopia, there are 84 household areas for which we can match information from pre-treatment surveys to post-treatment surveys; these are the overlapping green and yellow circles. For all countries included in our final sample, we have 1229 household areas that we observe across two time periods (1229*2).[17]

Figure 3. Procedures to construct areas



Source: Authors' elaboration on AidData and DHS

Second, we use a non–experimental technique, the inverse probability weighting (IPW) to adjust for the potential selection bias that results from the fact that we are dealing with non-randomized, observational data (Horvitz and Thompson 1952, Fitzgerald et al., 1988, Wooldridge, 2007). IWP aims at constructing a credible counterfactual from the data in two stages by identifying and then matching quasi identical units in the treatment and control groups. Recent work by Busso et al. (2014) shows that reweighting

15 When doing this, we use weights provided by each DHS survey.

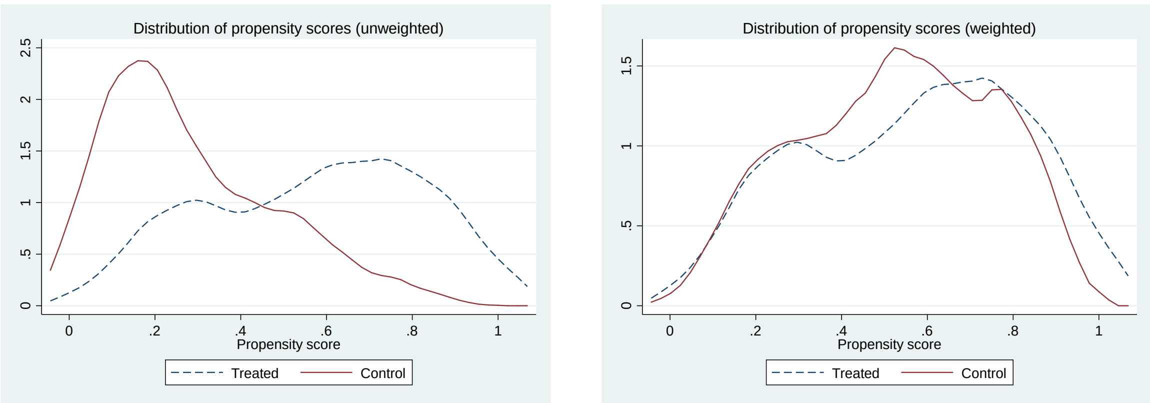
16 Wegenast et al. (2016) propose a similar method since they re-aggregate information from the clusters to the district level, computing mean values to get to a dataset that varies by district and year. They do not, however, provide the additional steps we make in our analysis to ensure a more precise comparability between treated and control areas.

17 Benin: 79 units; Cote d'Ivoire: 38; Ethiopia: 84; Ghana: 105; Guinea: 61; Kenya: 191; Malawi: 232; Nigeria: 96; Namibia; 87; Senegal: 61; Togo: 90; Uganda: 61; Zimbabwe: 44.

performs well, compared to other matching estimators, especially in those cases (like ours, see below) in which the overlap is good. For each area we estimate the probability of being a recipient of Chinese aid with a probit model. Variables in the model includes the demographic composition of the households living in the area: the share of female-headed households, average age of household members, average size of households, the number of male, the number of female, the number of children aged between 0 and 4, the number of children aged between 5 and 14, the number of children aged between 15 and 17, average dependency ratio. We also consider assets owned by the same group of households (percentage of households holding a car and a radio), other information as proxies for living conditions (e.g. floor living conditions), the share of households living in rural areas as well as region fixed effects and year dummies. Finally, we add the log number of households included in each area in order to control for population density and the log of night time light intensity.

All variables are measured in the baseline year and computed as average levels among all households belonging to the same area. The graph depicting the weighted propensity scores for control and treatment groups indicates the goodness of the overall balance and the overlap of the two distributions after matching (Figure 4, left panel). This is further confirmed by the Hotelling test: after matching, we do not observe a significant difference in means between control and treatment group for the variables that were used to calculate the propensity scores (Prob > F (90, 938) = 0.3685).

Figure 4. Distribution of propensity scores, before (left panel) and after (right panel) matching



Third, we apply a difference-in-difference model where we reweight the control sample by the IWP to make it as comparable as possible to the treatment sample. The model specification is:

$$Y_{xit} = \beta_1 T_{it} + \beta_2 D_{xi} + \beta_3 (T_{it} * D_{xi}) + \beta_4 Z_{xit} + \delta_x + \theta_{it} + \varepsilon \qquad (2)$$

Where Y_{xit} represents the outcome of interest (see Section 3.2) computed at the area level *x* in country *i* at time *t*. D is the treatment dummy which equals 1 if an area was treated with a Chinese aid project. T_{it} is a time dummy that equals 1 for the post-treatment DHS survey wave. Z_{xit} is a vector of time variant characteristics measured at the area level. These characteristics include (a) the demographic composition of the households - number of persons living in the household, household members' average age, the dependency ratio, and a dummy variable (=1) if the household head is female; (b) the assets owned by the household (i.e. car and radio), plus further proxy variables for living conditions such as access to electricity and flooring in the house; (c) a dummy variable (=1) for households living in rural areas and (d) the (log) number of households in each area. Furthermore, we add time varying country (θ_{it}) and area (δ_x) fixed effects to control for time trends and area-specific characteristics that may be spuriously correlated with our dependent and independent variables. This allows to control for confounding factors and omitted variables at a disaggregated geographic level. Additionally, time variant country fixed effects account for factors potentially affecting the treatment and outcome variables at the national level and over time. Standard errors are clustered at the country level.

Table 1 reports descriptive statistics on the main variables used in the regression analysis.

Table 1: Descriptive Statistics

Variables	Obs	Mean	Std. Dev.	Min	Max
Dependent Variables					
Average Years of Education	292,702	4.95	3.17	0.00	16.00
Average Educational Attainment	292,702	1.52	0.99	0.00	4.78
Child Mortality	297,394	0.03	0.03	0.00	0.33
Weight for Height Z-score	292,111	1.54	0.16	-0.58	2.24
BMI for age Z-score	292,142	1.56	0.16	-0.82	2.21
Treatment Variables					
Treated (D) with Project	297,459	0.31	0.46	0.00	1
Number of Projects	297,459	2.51	7.58	0.00	52
Total Amount in Millions (Financial Volume)	297,459	44.30	251.00	0.00	5490
Household Characteristics					
Female-Headed Household	297,459	0.27	0.15	0.00	1
Age of Household Members	297,459	44.65	5.30	27.04	68
Household Size	297,459	4.86	1.47	1.00	19
Dependency Ratio	297,459	0.51	0.11	0.17	1
Rural Area	297,459	0.66	0.44	0.00	1
No. of Households per area	2,484	3.62	0.75	1.61	7.39
Household Assets					
Radio	297,459	0.58	0.21	0.00	1
Electricity	297,459	0.33	0.38	0.00	1
Car	297,459	0.06	0.10	0.00	1
Type of Flooring	297,459	2.03	0.73	1.00	3

3. Results

First, we discuss the results of our main analysis: the effect of Chinese aid projects on the selected education, health and nutrition outcomes (see Section 3.1). In this analysis, we aggregate Chinese aid projects across all sectors (see Figure 2 for a detailed breakdown of Chinese aid by sector). Second, we conduct a sector level analysis where we simultaneously consider the effect of social and economic projects on each outcome variables (Section 3.2). The sector analysis serves to test for heterogeneous effect across different sectors and provide insights into potential channels through which Chinese projects affect household social welfare. Third, we consider the timing of treatment with Chinese project(s) between pre- and post-treatment survey waves (Section 3.3). This analysis takes into account that treatment effects might vary with the temporal distance between the time of treatment and the time when the outcome indicators were recorded, namely the second DHS survey wave: some project effects may take more (education) or less time (health) to materialize, or even die down after some time. Last, we test to what extent the distribution and scale of Chinese aid might affect our outcome variables

(Section 3.4). To this end, we replace our binary treatment dummy with (a) the number of projects within an area, and (b) the financial volume of the project. As discussed before, our main analysis is not based on these metrics since their reliability is limited.

3.1 Main analysis

Our main analysis, comprising the DiD regressions for each outcome variable, is reported in Table 2. The results indicate that Chinese project aid has a positive and significant effect on household areas' wellbeing. The analysis on education outcomes (Table 2, columns 1-2) shows that Chinese aid projects have a positive and significant effect on average years of education and on educational attainment, i.e. the level of acquired education. This is well in line with other studies that demonstrate positive effects of "traditional Western" foreign aid on education outcomes (Riddel and Niño-Zarazúa, 2016). Furthermore, we find that areas receiving Chinese aid experience an improvement in an important health-related outcome: a reduction in child mortality rates (Table 2, column 3). Similarly, this is in line with research which shows that foreign aid from OCED-DAC donors reduces infant mortality in Nigeria (Kotsadam et al., 2018). Thus, Chinese aid, on which no evidence has been available so far, does not seem to differ from other donors in these dimensions. As far as nutrition outcome are concerned, we do not find a significant effect of Chinese aid (Table 2, columns 4-5).

Considering the peculiarities of Chinese aid delivery modes, several arguments can be raised to explain these results. First, better educational and health outcomes can, obviously, be a direct result of effective Chinese projects in the education and health sector. After all, education and health related projects account for 11% each of the sectoral distribution of Chinese aid (see Figure 2). According to Shajalal et al. (2017), China is concerned with building an African public health system and currently concentrates activities on setting up infrastructure including hospitals, equipment, and medicine. Moreover, it trains health teams and makes efforts to combat malaria and maternal, neonatal, and child health. These activities may well help to reduce child mortality in targeted areas, which seems to be reflected in our data. However, Shajalal et al. (2017) underlines that China has paid limited attention to disease prevention, health promotion, awareness initiatives, and health education – factors which can be key to induce the behavior change necessary to improve nutrition outcomes for example. This could explain why we observe an effect of Chinese projects on child mortality, but not on nutrition.

Second, as Kotsadam et al. (2018) argues, improvements in infant mortality can result indirectly from projects with relevant spillover effects such as access to electricity, piped water, road infrastructure and improved household wealth. Similar arguments apply to educational outcomes. Transportation infrastructure such as roads, which makes up at least 21% of Chinese aid, can facilitate access to education for a large number of pupils, and increased household wealth through better access to jobs can improve households' ability to cover school fees. In the next section, we explore these possible mechanisms more closely.

Table 2: Main Results, OLS Weighted (IWP)

	1	2	3	4	5
	Average Years of Education	Average Educational Attainment	Child Mortality	Weight for Height Z-score	BMI for age Z-score
Treated*Survey Year (*D*T*)	0.382***	0.115***	-0.005*	-0.007	-0.005
	[0.095]	[0.037]	[0.003]	[0.012]	[0.017]
Most Recent Survey Year (*T*)	0.592***	0.162***	-0.004	0.184***	0.157***
	[0.129]	[0.043]	[0.003]	[0.013]	[0.017]
Female-Headed HH[1]	0.02	-0.035	0.013*	0.064	0.062
	[0.398]	[0.118]	[0.006]	[0.065]	[0.068]
Age Of HH members	-2.518***	-0.805***	0.028***	-0.045	-0.057
	[0.286]	[0.103]	[0.008]	[0.091]	[0.099]
HH Size	0.246	0.026	0	-0.009	-0.034
	[0.341]	[0.106]	[0.010]	[0.044]	[0.037]
Access To Electricity	1.558***	0.506***	-0.008	0.001	-0.005
	[0.431]	[0.135]	[0.006]	[0.023]	[0.033]
Radio	2.803***	0.844***	-0.012	0.085**	0.086
	[0.657]	[0.205]	[0.012]	[0.032]	[0.049]
Car	6.113***	1.994***	-0.019	0.044	0.032
	[1.362]	[0.395]	[0.012]	[0.121]	[0.106]
Type Of Flooring	0.407*	0.149	-0.007**	-0.01	-0.017
	[0.213]	[0.088]	[0.003]	[0.010]	[0.010]
Rural Area	-0.389*	-0.160**	-0.005	0.006	0.011
	[0.185]	[0.056]	[0.003]	[0.015]	[0.018]
Dependency Ratio	4.078***	1.317***	0.043***	0.081	0.085
	[1.172]	[0.394]	[0.014]	[0.158]	[0.143]
N. of HH	-0.074	-0.015	-0.002	-0.003	-0.001
	[0.043]	[0.015]	[0.002]	[0.011]	[0.013]
Constant	9.309***	2.955***	-0.071	1.523***	1.640***
	[1.769]	[0.738]	[0.041]	[0.465]	[0.468]
Observations	2,031	2,031	2,058	2,027	2,027
R-squared	0.705	0.701	0.252	0.121	0.132

*** p<0.01, ** p<0.05, * p<0.1; [1]HH: Household

Note: All the variables are calculated at the area-level. See Section 2.2 for a description of how geographic areas have been constructed. All regressions include area fixed effects and country-year fixed effects. Standard errors are clustered at the country level.

3.2 Sectoral Analysis

The sectoral analysis serves to more directly link Chinese projects to their intended outcomes. We group all projects into two big categories on the basis of their Creditor Report System (CRS) sectoral codes:

social sector projects and economic sector projects.[18] We then re-estimate the entire set of regressions from Table 2, but include two separate treatment dummies for social and economic projects. Including both sector treatment dummies in the same model enables us to compare the magnitude of both coefficients, complementing the previous analysis on treatment effects. Moreover, we exclude a potential omitted variable bias which might result from the fact that the decision to provide both types of projects is mutually dependent. As stated before, Chinese aid is known for the interlinkages it seeks to create between different sectors (King 2010). Results are reported in Table 3.

We observe a comparatively stronger impact of social sector projects on education and health outcomes. Moreover, we find that the treatment effect on child mortality seems to be mainly driven by social sector projects, not economic projects. Overall, the results presented in Table 3 are well in line with the findings shown in Table 2: Chinese aid projects improve education outcomes and reduce infant mortality at the household area level, but do not significantly affect nutrition outcomes. The findings are somewhat consistent with macro-level studies that find a positive relationship between social sector projects – an in particular education aid - provided by Western donors and educational outcomes in developing countries (see e.g. Michaelowa and Weber, 2007, Birchler and Michaelowa, 2016 as well as Heynemann and Lee, 2017 and Riddel and Niño-Zarazúa, 2016 for a review of findings on the effectiveness of education aid). Furthermore, it is consistent with micro- and project-level studies which also find a positive relationship between social interventions and outcomes (see Heynemann and Lee, 2017 and Riddel and Niño-Zarazúa, 2016)

[18] Economic projects include: Transport and Storage (210); Communications (220); Energy Generation and Supply (230); Banking and Financial Services (240); Business and Other Services (250); Agriculture, Forestry and Fishing (310); Industry, Mining, Construction (320); Trade and Tourism (330). Social projects include: Education (110); Health (120); Population Policies (130); Water Supply and Sanitation (140); Government and Civil Society (150); Other Social Infrastructure and Services (160); Women in Development (420); Developmental Food Aid (520); Non-Food Commodity Assistance (530). CRS codes are provided in parentheses.

Table 3. Main Results, OLS Weighted (IWP) – Sector Projects

	1	2	3	4	5
	Average Years of Education	Average Educational Attainment	Child Mortality	Weight for Height Z-score	BMI for age Z-score
Treated_Ec*Survey Year (*D*T*)	0.236**	0.071**	0.000	-0.004	-0.012
	[0.105]	[0.031]	[0.003]	[0.012]	[0.016]
Treated_Soc*Survey Year (*D*T*)	0.290***	0.086***	-0.005**	0.004	0.007
	[0.080]	[0.027]	[0.002]	[0.019]	[0.020]
Most Recent Survey Year (*T*)	0.527***	0.143**	-0.004	0.182***	0.159***
	[0.147]	[0.049]	[0.003]	[0.015]	[0.019]
Constant	9.372***	2.973***	-0.071	1.511***	1.631***
	[1.761]	[0.741]	[0.042]	[0.467]	[0.474]
Observations	2,031	2,031	2,058	2,027	2,027
R-squared	0.706	0.701	0.251	0.121	0.133

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

Note: All the variables are calculated at the area-level. See Section 2.2 for a description of how geographic areas have been constructed. All regressions include controls (Female HH; Age of HH members; HH size; access to electricity; radio; car; type of flooring; rural area; dependency ratio and the number of HHs in the area); area fixed effects and country-year fixed effects. Standard errors are clustered at the country level. Economic projects include projects in the following Sectors (CRS code in parenthesis): Transport and Storage (210); Communications (220); Energy Generation and Supply (230); Banking and Financial Services (240); Business and Other Services (250); Agriculture, Forestry and Fishing (310); Industry, Mining, Constructurion (320); Trade and Tourism (330). Social projects include projects in the following Sectors (CRS code in parenthesis): Education (110); Health (120); Population Policies (130); Water Supply and Sanitation (140); Government and Civil Society (150); Other Social Infrastructure and Services (160); Women in Development (420); Developmental Food Aid (520); Non-Food Commodity Assistance (530).

3.3 Project timing

In the analysis presented in Table 4, we address an important feature of our data: the temporal distance between the year in which the treatment occurs and the post-treatment survey wave which provides us with the post-treatment measurement of our outcome variables. By considering the variance in this temporal distance between the timing of treatment and the post-treatment survey wave, we can assess to what extent treatment effects may take more or less time to materialize, or die down over time. For example, education projects may take a couple of years until they show an effect while health benefits materialize quicker.

To account for this timing effect, we make a simple adjustment to our data. For each area-country pair, we count the number of projects occurring at any year. We then create two different treatments, trying to distinguish among more and less recent aid projects by measuring if the area is receiving at least a project at any time in the years *preceding (<)* or *following (>)* a given threshold year *n*. Thus, setting n=3 means that we count back three years from the relevant post-treatment survey wave. For instance, in the case of Senegal this means that we consider a first treatment covering projects taking place from 2010 to 2012, and the second covering projects taking place from 1997 to 2009. We have set two different thresholds using 3 and 5 year lags from the endline survey.

The results, summarized in Table 4, show that results seem to be largely driven by projects taking place in more recent periods. More specifically, treatment effects for education outcomes dissipate for projects that are more than five years back in time. For child mortality, treatment effects dissipate for projects that occurred more than three to five years back in time.

Table 4. Project Timing

	1	2	3	4	5
	Average Years of Education	Average Educational Attainment	Child Mortality	Weight for Height Z-score	BMI for age Z-score
Treated(<3yrs)*Survey Year (*D*T*)	0.291**	0.085**	0.001	-0.007	-0.003
	[0.107]	[0.039]	[0.002]	[0.015]	[0.018]
Treated(>3yrs)*Survey Year (*D*T*)	0.226**	0.076**	-0.007**	-0.001	-0.010
	[0.094]	[0.035]	[0.003]	[0.012]	[0.013]
Most Recent Survey Year (*T*)	0.528***	0.140**	-0.006**	0.183***	0.160***
	[0.145]	[0.048]	[0.002]	[0.014]	[0.017]
Constant	9.436***	2.996***	-0.069	1.522***	1.637***
	[1.738]	[0.731]	[0.041]	[0.468]	[0.472]
Observations	2,031	2,031	2,058	2,027	2,027
R-squared	0.706	0.702	0.254	0.122	0.133
Treated(<5yrs)*Survey Year (*D*T*)	0.254*	0.077	0.001	-0.017	-0.026*
	[0.123]	[0.046]	[0.002]	[0.009]	[0.013]
Treated(>5yrs)*Survey Year (*D*T*)	0.300***	0.095***	-0.006**	0.009	0.012
	[0.079]	[0.026]	[0.003]	[0.008]	[0.013]
Most Recent Survey Year (*T*)	0.506***	0.133**	-0.005*	0.176***	0.148***
	[0.154]	[0.055]	[0.003]	[0.012]	[0.014]
Constant	9.360***	2.972***	-0.070	1.515***	1.637***
	[1.737]	[0.726]	[0.042]	[0.468]	[0.475]
Observations	2,031	2,031	2,058	2,027	2,027
R-squared	0.707	0.702	0.252	0.122	0.135

Robust standard errors in brackets

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

3.4 Distribution and scale of Chinese project aid

In our main analysis, we focus on whether a project is implemented or not and refrain from using potentially less reliable information on the number of projects or project finance volume. In the analysis presented in this section, we take treatment intensity into account by replacing the binary treatment dummies with (a) the number of projects going to certain areas, and (b) the actual financial volume of the project. Moreover, we test for non-linear effects of aid that have been identified by earlier macro- and project-level studies. In order to do this, we apply the *propensity score-based marginal mean weighting through stratification* (MMWS) method to our data (Hong, 2010; Hong, 2012; Huang et al 2005).

MMWS combines *propensity score stratification* and *inverse probability of treatment weighting* to reduce selection bias, and is applicable to so-called multivalued treatments which can take on more than one value (Linden et al. 2014). Based on the distribution of the number of projects (Table 5) we consider two, hence multivalued, ordinal treatment levels: (1) equal or lower than the median value; and (2) higher than the median value (i.e. 2 projects). Based on the distribution of financial project size, we consider the following two treatment levels: (1) equal or lower than the median value and; (2) higher than the median value (i.e. 149.92 $ million). As before, areas not located within a 25km radius of a Chinese aid project serve as the control group. MMWS reduces selection bias by achieving balance on baseline (and observable) characteristics between all treatment levels (Linden et al. 2014). The empirical strategy relies on four steps. First, we compute the generalized propensity score for each area. To this end, we use an ordered logistic regression and regress our ordinal treatment variable on the same set of covariates used in the previous selection model (see Section 2.2). Second, the generalized propensity scores that we computed for each area is stratified into three strata (zero projects, below and above median values). Third, we compute the MMWS weights for each area corresponding to its stratum and treatment level. Finally, we re-estimate our main specifications using the MMWS as probability weights.

Table 5. Number of projects (#) and total project amount (constant $ million)

	Mean	Median	25th percentile	75th percentile
Number of projects	6.45	2	1	6
Total amount	1428.87	149.92	68.57	540.04

The results presented in Table 6 indicate that the treatment intensity in terms of the number of projects matters: the effect on our educational outcome variables is non-linear. More specifically, Chinese aid projects seem to have a significant impact on educational outcomes in areas receiving only one or two projects. By contrast, the effect is no longer statistically significant in areas with a higher number of projects. The results are not significant on child mortality. Table 7 reports the regressions that include treatment intensity based on financial project volume. The results mirror those reported in Table 6: the impact of Chinese assistance is only statistically significant below median values of financial disbursement.

Table 6: OLS weighted (MMWS) – Multilevel Treatment (Number of Projects)

	1	2	3	4	5
	Average Years of Education	Average Educational Attainment	Child Mortality	Weight for Height Z-score	BMI for age Z-score
Treat_Level 1*Survey Year (*D*T*)	0.196**	0.050**	-0.001	-0.002	-0.012
	[0.069]	[0.023]	[0.004]	[0.022]	[0.018]
Treat_Level 2*Survey Year (*D*T*)	0.131	0.044	-0.003	0.014	0.001
	[0.127]	[0.052]	[0.004]	[0.016]	[0.019]
Most Recent Survey Year (*T*)	0.994***	0.290***	0.005	-0.019	-0.018
	[0.174]	[0.052]	[0.003]	[0.011]	[0.011]
Constant	2.991***	0.935***	0.075	1.523***	1.523***
	[0.876]	[0.295]	[0.048]	[0.191]	[0.170]
Observations	2,423	2,423	2,484	2,415	2,415
R-squared	0.664	0.656	0.282	0.165	0.124

*** p<0.01, ** p<0.05, * p<0.1; [1]HH: Household

Note: All the variables are calculated at the area-level. See Section 2.2 for a description of how geographic areas have been constructed. All regressions include controls (Female HH; Age of HH members; HH size; access to electricity; radio; car; type of flooring; rural area; dependency ratio and the number of HHs in the area); area fixed effects and country-year fixed effects. Standard errors are clustered at the country level.

Table 7: OLS weighted (MMWS) – Multilevel Treatment (Financial Project Volume)

	1	2	3	4	5
	Average Years of Education	Average Educational Attainment	Child Mortality	Weight for Height Z-score	BMI for age Z-score
Treat_Level 1*Survey Year (*D*T*)	0.578**	0.174**	0.003	0.014	-0.003
	[0.209]	[0.072]	[0.005]	[0.022]	[0.025]
Treat_Level 2*Survey Year (*D*T*)	0.027	0.017	-0.004	0.001	-0.015
	[0.094]	[0.046]	[0.005]	[0.016]	[0.016]
Most Recent Survey Year (*T*)	-0.334***	-0.107***	-0.006***	-0.062***	-0.072***
	[0.059]	[0.022]	[0.002]	[0.011]	[0.008]
Constant	2.939**	0.908**	0.077**	1.418***	1.418***
	[1.069]	[0.343]	[0.028]	[0.241]	[0.178]
Observations	2,423	2,423	2,484	2,415	2,415
R-squared	0.657	0.657	0.283	0.190	0.143

*** p<0.01, ** p<0.05, * p<0.1; [1]HH: Household

Note: All the variables are calculated at the area-level. See Section 2.2 for a description of how geographic areas have been constructed. All regressions include controls (Female HH; Age of HH members; HH size; access to electricity; radio; car; type of flooring; rural area; dependency ratio and the number of HHs in the area); area fixed effects and country-year fixed effects. Standard errors are clustered at the country level.

In summary, the results in Tables 6 and 7 are an important addition to our work. On the one hand, they support our core results by pointing to a positive relation between Chinese aid and household social welfare that is robust to a treatment indicator that accounts for the scale of the projects. On the other hand, our results show that the relationship between treatment level and outcome variables is non-linear and that the effectiveness of Chinese aid on social outcomes seem confined to lower scale interventions.

4. Robustness

We check for the robustness of our main results in different ways. First, we exclude potential outliers: areas that received a large number of projects. Second, we test whether the results are sensitive to enlarging the buffer zone that is used to assign areas to treatment and control groups from 25 km to 50 km. Third, we run a placebo test based on project implementation status: we re-estimate our main regressions from Table 2 with projects that are categorized as being in the “pipeline”. Those projects have either not yet been started or completed. If our identification strategy works, we should not find any treatment effects when using the sample of projects that have not yet been started or implemented.

4.1 Excluding outliers

The distribution of projects across areas shows that the 25th percentile benefitted from only 1 project while the 95th percentile benefited from more than 36 projects on average. Therefore, we test to what degree our results may be driven by areas recording a large number of projects. For this purpose, we exclude areas recording more than 36 projects on average. The regressions reported in Table A2 in the Appendix show that the results are robust to the exclusion of outlier areas.

4.2 Altering the buffer zone

We test to what extent choosing a buffer zone of 50 km instead of 25 kilometers in order to assign areas to control and treatment groups affects our main findings. The results presented in Table A3 in the Appendix indicate that Chinese aid projects still have a positive impact on the social welfare of households (areas) that live up to 50km away from the project site. However, the effect such projects on outcomes apparently wears off with increasing distance. This seems reasonable: education-related interventions as well as health-related interventions are likely local in nature (such as the construction of schools, hospitals or provision of other services), reaching a limited amount of people. This, in turn, may limit spillover effects beyond a certain geographical distance.

4.3 Placebo test

We also take advantage of the information project status in the AidData dataset in order to test our identification strategy. As discussed in Section 2.1, a relatively large share of Chinese aid projects (around 32%) were not yet completed or implemented during the period covered by our analysis. Such projects, classified as "in the pipeline" can be used to run a placebo test. Since potential effects of these projects should not yet have materialized, they should not significantly affect our outcomes variables. In order to run this test, we constructed an alternative database following exactly the same procedure as described in Section 2, but only including pipeline projects. We then repeat our DiD analysis based on this project sample. We find no significant effect of pipeline projects on our outcome variables. This indicates that our identification strategy seems valid and that observed effects are not based on a spurious relationship between treatment and outcome variables. The regression results are presented in Table A4.

5. Discussion

China has become an important donor to low-income countries, especially in sub-Saharan Africa. Lack of reliable data on Chinese foreign aid has thus far limited the possibilities to quantitatively assess its impacts on the ground. Recent work provides first evidence of positive effects of Chinese aid on national and sub-national economic growth – e.g. Dreher et al. (2016) and Dreher et al. (2017). Further analysis on the effects of Chinese aid on development outcomes in sub-Saharan Africa is urgently needed given its significance for the continent. In this spirit, this research provides novel evidence on the impact of Chinese aid projects on the welfare of households in 13 sub-Saharan African countries. The results of our analysis provide evidence of an overall positive development impact of Chinese project assistance on non-economic development outcomes, namely education and child mortality. Households in areas that receive Chinese projects tend to stay in school longer, have a higher educational attainment, and experience a reduction in child mortality. In this respect, China is similar to Western donors for which a positive effect on these important development outcomes was demonstrated as well.

Our empirical strategy relies on combining a rich set of georeferenced data on Chinese foreign assistance projects with household-level information from the DHS. Moreover, we use quasi-experimental impact evaluation methods in order to estimate the impact of Chinese aid on households' well-being. The present paper is a first step towards a causal analysis of the impact of Chinese foreign assistance on household-level welfare. Still, due to the some limitations of the data, additional research is needed to provide a more comprehensive assessment on the effectiveness of Chinese aid. This includes, for instance, to look at other dimensions of households' welfare (e.g. consumption). Moreover, further work is still needed to correctly identifying the mechanisms through which Chinese aid affects people's life, and to investigate the extent to which they are similar or not to those characterizing the effectiveness of aid from traditional donors.

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APPENDIX

Table A1: DHS Data Availability

Country	Baseline	After Treatment
Benin	1996	2012
C. d'Ivoire	1999	2012
Ethiopia	2000	2010
Ghana	1998	2014
Guinea	1999	2012
Kenya	2003	2014
Malawi	2004	2015
Namibia	2000	2013
Nigeria	2003	2013
Senegal	1997	2012
Togo	1998	2013
Uganda	2001	2006
Zimbabwe	1999	2015

Table A2: Results, OLS Weighted (IWP) – excluding outliers

	1	2	3	4	5
	Average Years of Education	Average Educational Attainment	Child Mortality	Weight for Height Z-score	BMI for age Z-score
Treated*Survey Year (*D*T*)	0.407***	0.124***	-0.005	-0.008	-0.004
	[0.095]	[0.036]	[0.003]	[0.012]	[0.016]
Most Recent Survey Year (*T*)	0.587***	0.160***	-0.035***	0.181***	0.152***
	[0.140]	[0.047]	[0.003]	[0.014]	[0.016]
Constant	9.509***	3.005***	-0.042	1.533***	1.622***
	[1.790]	[0.697]	[0.045]	[0.475]	[0.487]
Observations	1,988	1,988	2,015	1,985	1,985
R-squared	0.711	0.715	0.256	0.121	0.135

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

Note: Regressions exclude areas at 95^{th} percentiles of the distribution of Chinese aid projects. All the variables are calculated at the area-level. See Section 2.2 for a description of how geographic areas have been constructed. All regressions include controls (Female HH; Age of HH members; HH size; access to electricity; radio; car; type of flooring; rural area; dependency ratio and the number of HHs in the area); area fixed effects and country-year fixed effects. Standard errors are clustered at the country level.

Table A3: Results, OLS weighted (IWP) – Buffer Zone 50 km

	1	2	3	4	5
	Average Years of Education	Average Educational Attainment	Child Mortality	Weight for Height Z-score	BMI for age Z-score
Treated*Survey Year (*D*T*)	0.071	0.02	-0.007	0.004	0.007
	[0.097]	[0.029]	[0.005]	[0.048]	[0.051]
Most Recent Survey Year (*T*)	1.347***	0.388***	-0.019***	0.034	-0.06
	[0.093]	[0.028]	[0.003]	[0.088]	[0.094]
Constant	2.654*	0.761	0.008	-0.025	0.747
	[1.358]	[0.474]	[0.030]	[0.784]	[0.901]
Observations	2,125	2,125	2,152	2,120	2,120
R-squared	0.662	0.661	0.261	0.213	0.202

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

Note: Treatment is calculated using a 50km buffer zone from the centroid of the Chinese aid projects. All the variables are calculated at the area-level. See Section 2.2 for a description of how geographic areas have been constructed. All regressions include controls (Female HH; Age of HH members; HH size; access to electricity; radio; car; type of flooring; rural area; dependency ratio and the number of HHs in the area); area fixed effects and country-year fixed effects. Standard errors are clustered at the country level.

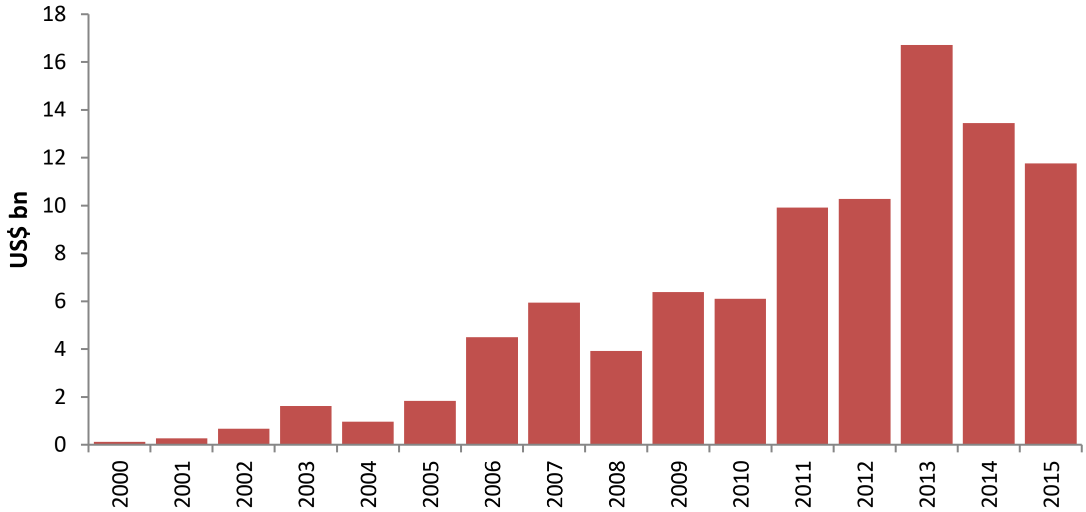
Table A4: Results, OLS weighted (IWP) – Placebo

	(1)	(2)	(3)	(4)	(5)
	Average Years of Education	Average Educational Attainment	Child Mortality	Weight for Height Z-score	BMI for age Z-score
Treated*Survey Year (*D*T*)	0.015	0.013	-0.002	-0.016	-0.021*
	[0.165]	[0.057]	[0.003]	[0.012]	[0.011]
Most Recent Survey Year (*T*)	0.858***	0.236***	-0.037***	0.038***	-0.000
	[0.208]	[0.069]	[0.002]	[0.012]	[0.010]
Constant	7.095**	2.455**	-0.078	1.755**	1.841***
	[3.133]	[1.037]	[0.052]	[0.638]	[0.571]
Observations	1,284	1,284	1,300	1,279	1,279
R-squared	0.709	0.691	0.272	0.144	0.137

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

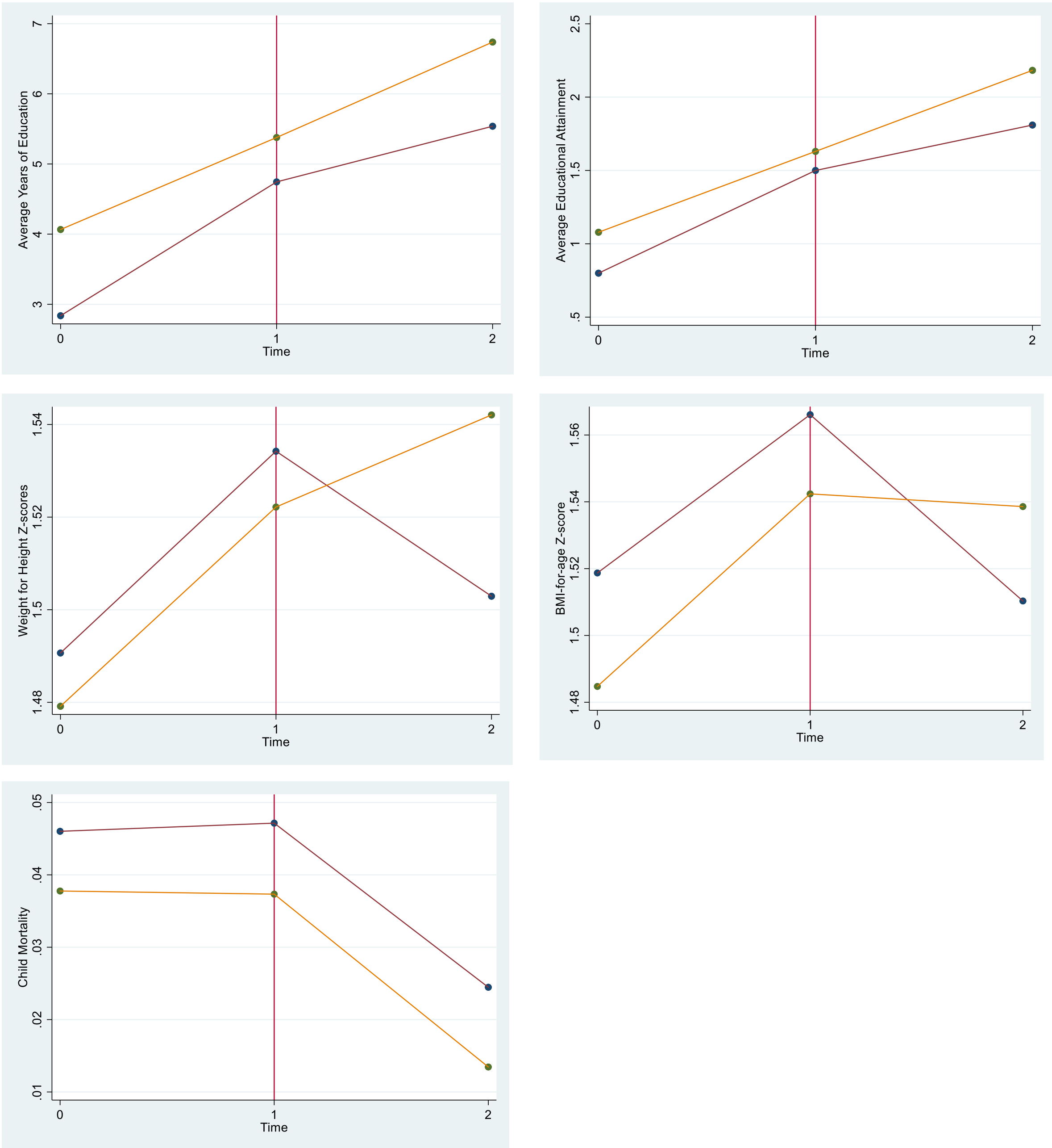
Note: Treatment is constructed on the basis of pledged (but not realized) projects. All the variables are calculated at the area-level. See Section 2.2 for a description of how geographic areas have been constructed. All regressions include controls (Female HH; Age of HH members; HH size; access to electricity; radio; car; type of flooring; rural area; dependency ratio and the number of HHs in the area); area fixed effects and country-year fixed effects. Standard errors are clustered at the country level.

Figure A1. Distribution of Chinese Loans to Africa since 2000



Source: China-Africa Research Initiative, Johns Hopkins University (accessed on Dec 13, 2017, at: http://www.sais-cari.org/data-chinese-loans-and-aid-to-africa)

Figure A2. Parallel trend assumption tests on our main dependent variables



Source: authors' elaboration on DHS data

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