CESifo WORKING PAPERS

8352
2020

June 2020

# Chinese Aid and Health at the Country and Local Level

*John Cruzatti C., Axel Dreher, Johannes Matzat*

CESifo

**Impressum:**

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editor: Clemens Fuest
https://www.cesifo.org/en/wp
An electronic version of the paper may be downloaded
· from the SSRN website: www.SSRN.com
· from the RePEc website: www.RePEc.org
· from the CESifo website: https://www.cesifo.org/en/wp

CESifo *Working Paper No. 8352*

# Chinese Aid and Health at the Country and Local Level

## Abstract

We investigate whether and to what extent Chinese development finance affects infant mortality, combining 92 demographic and health surveys (DHS) for a maximum of 53 countries and almost 55,000 sub-national locations over the 2002-2014 period. We address causality by instrumenting aid with a set of interacted variables. Variation over time results from indicators that measure the availability of funding in a given year. Cross-sectional variation results from a sub-national region's "probability to receive aid." Controlled for this probability in tandem with fixed effects for country-years and provinces, the interactions of these variables form powerful and excludable instruments. Our results show that Chinese aid increases infant mortality at sub-national scales, but decreases mortality at the country-level. In several tests, we show that this stark contrast likely results from aid being fungible within recipient countries.

JEL-Codes: I150, F350.

Keywords: health aid, fungibility, infant mortality.

*John Cruzatti C.*
*Alfred-Weber-Institute for Economics*
*Heidelberg University / Germany*
*john.cruzatti@awi.uni-heidelberg.de*

*Axel Dreher*
*Alfred-Weber-Institute for Economics*
*Heidelberg University / Germany*
*mail@axel-dreher.de*

*Johannes Matzat*
*Faculty of Business and Economics*
*Georg-August University Göttingen / Germany*
*johannes.matzat@uni-goettingen.de*

Mai 27, 2020
We thank Roland Hodler and Paul Raschky for generously sharing their birth region data for this project, Lennart Kaplan and Lukas Wellner for helpful comments, Tobias Hellmundt and Riyoko Shibe for excellent research assistance, and Harrison Bardwell for proof-reading.

# 1 Introduction

Much has been written about foreign aid. According to its critics, donors allocate aid to ensure privileged access to the recipients' natural resources, create export markets for their goods, and reward strategic allies (Alesina and Dollar 2000, Dreher et al. 2009). Whether and to what extent aid increases recipient countries' economic growth is highly debated (Galiani et al. 2016, Doucouliagos 2019). Economic growth depends however on a large number of factors that are in turn affected by aid in different directions. The lack of robust evidence is then perhaps unsurprising. What is more, in many countries the aid is geographically highly concentrated. Even if aid affects outcomes in the regions it is given to, the effects might be insufficiently substantial to be measurable at the country-level. This observation has led recent studies to focus on sub-national regions, investigating the effects of aid on development at the province or district level, or even finer sub-national scales (Dreher and Lohmann 2015, Bitzer and Goeren 2018, Isaksson and Kotsadam 2018, Gehring et al. 2019, Maseland and Minasyan 2019). However, subnational regressions can be misleading if aid displaces other funds across space which is likely within but not across countries.

In this study, we focus on health-related outcomes, which are more tangible than overall measures of development, such as economic growth. We combine the advantages of sub-national data with those of country-level analysis, comparing results at different levels of aggregation.[1] Such comparison allows us to to uncover indirect benefits from aid to areas not directly targeted by it. Specifically, we explore the effects of Chinese development finance on the probability that a newborn baby will die before it reaches the age of one—the so-called infant mortality rate.[2] China is the only bilateral provider of development finance for which geocoded data are available for a large number of recipient countries. Consequently, other bilateral donors or lenders cannot be included in this study. We however compare the effect of Chinese aid to those of the World Bank, for

[1] We are the first to investigate the causal effect of aid on health at the sub-national level for a large number of countries spreading across different continents. Sub-national studies we are aware of focus on infant mortality in Nigeria (Kotsadam et al. 2018) and Côte d'Ivoire (Wayoro and Ndikumana 2019), health outcomes and perceived healthcare quality in Malawi (De and Becker 2015, Marty et al. 2017), and the disease burden and severity in Uganda (Odokonyero et al. 2018). Most closely related to this paper is Martorano et al. (2020) who investigate the effect of Chinese aid on household welfare, focusing on 13 countries in Sub-Sahara Africa, and report positive correlations between Chinese aid and lower infant mortality in a difference-in-differences framework. Greßer and Stadelmann (2019) provide conditional correlations between the sub-national presence of World Bank projects and health-related outcomes in a sample of almost 40 countries. They find a positive correlation between projects and health quality. A large number of papers investigates the correlation between aid and health at the country-level, with mixed results (e.g., Williamson 2008, Chauvet et al. 2009, Sonntag 2010, Dietrich 2011, Nunnenkamp and Öhler 2011, Wilson 2011, Doucouliagos et al. 2019, and Kaplan et al. 2019).

[2] We use the terms "development finance" and "aid" for Official Development Assistance (ODA) and Other Official Finance (OOF) alike. During the time of our study, ODA was defined as financial flows that mainly aim at promoting the welfare and development of the recipients and have a grant element of at least 25 percent. OOF are official transactions that do not meet at least one of these criteria.

which geocoded data are equally available. This allows comparing two donors that the literature considers to affect development very differently—one perceived as being rather selfish and thus potentially harmful for recipient-country development, the other being a multilateral organization with much-appraised standards for allocating its aid (e.g., Gehring et al. 2019). We expect China's non-interference and "on-demand" policy to make its aid particularly fungible: To the extent that China responds to recipients' requests for aid without any policies in place that would ensure the aid is additional, recipient governments can easily use it to finance projects it would have funded anyway. The World Bank, to the contrary, has policies in place that aim to ensure the additionality of its aid, carefully monitoring the use of its funds (Dreher et al. 2020).

According to Dijkstra and White (2003, p. 468), fungibility is "the idea that aid pays not for the items which it is accounted for but for the marginal expenditure it makes possible."[3] Chinese aid might replace government expenditures or other donors' aid in two ways.[4] First, Chinese aid might finance the exact same project that the recipient would have built absent the aid. The recipient could use the funds that are now available in its budget either to finance something entirely different, such as military expenditures, or—to the extent that the aid creates leeway in budgets related to health expenditures only—rather a similar project, in the same geographic region or elsewhere. Second, China might fund a project similar to but different from what the recipient government or other donors would have financed without China's support. The recipient government might then prefer to not implement a related project at all, or implement it in a different geographic area. This would prevent the financing of two similar projects, in one location, at the same time. The project with a focus different from those of the government is likely less effective in achieving the recipient's goals and would thus make the aid appear less effective than the identical project at regional scales. This is because outcomes would be more likely to improve in regions that do not receive Chinese aid compared to those that do. At the aggregate, country-level, effects of the government-financed project would however register. Aid can therefore appear less effective at the subnational than at the country level.

As we explain in Section 2, our sub-national analyses test the effect of aid on infant mortality reported by surveyed households in the area within a radius of 0.5°, which amounts to roughly 55 km at the equator. To this end, we merge health indicators from 92 demographic and health (DHS) surveys on 53 developing countries over the 2002-2014 period and combine them with a geocoded dataset on Chinese development finance. Our first regressions follow the method of Isaksson and Kotsadam (2018), comparing the effect of projects that have disbursed aid ("active" projects for short) to those that have not

[3] As cited in Leiderer (2012, p. 4).

[4] Hernandez (2017) shows that the World Bank competes with China by reducing the number of conditions in its aid programs when China is also present in a country. Zeitz (2020) also shows that the World Bank is responsive to Chinese competition.

("inactive" projects). Comparing observations that have been selected as project-sites at a certain point in time to others that have been selected as well (but are as yet inactive) reduces the omitted-variables bias compared to a simple cross-sectional specification. We thus compare the effect of active projects on outcomes relative to no-aid projects, to the effect of inactive projects on outcomes relative to no-aid projects in a region.

We refine our basic regressions to address causality further using an instrumental variable (IV) approach, at the local and country levels. Our instruments for aid follow Dreher et al. (2019b, 2020) and Bluhm et al. (2020), who suggest yearly production volumes of physical inputs into (tied) Chinese aid projects and the availability of foreign currency reserves as proxies for the availability of aid at any point in time. As they explain in some detail, China produces substantially more steel, cement and other production materials than it can use domestically. It then uses them as inputs in its foreign aid projects. When production is high, the supply of aid becomes cheaper and should thus increase. Along similar lines, China uses its aid projects to earn interest on its foreign currency reserves. At times when the reserves are higher, the supply of aid should also be higher. We interact these indicators of supply with the probability that a country or region receives smaller or larger shares of the year-to-year fluctuations in aid. China could be expected to expand its aid program beyond its traditional clients in years with abundant supply. Alternatively, regions that have received China's aid in the past might receive more of it when supply is high. The latter effect has been shown to prevail in Dreher et al. (2019b, 2020), as seems to be the case for bilateral donors more broadly (Nunn and Qian 2014, Dreher and Langlotz 2020).

The intuition of our IV regressions follows those of a difference-in-differences approach. We exploit the differential effect of China's production volumes of physical inputs into aid projects and the availability of foreign reserves on Chinese health projects in regions with a high probability of receiving aid compared to those with a low probability of receiving aid. The identifying assumption is that health outcomes in sub-national regions with differing probabilities of receiving aid will not be affected differently by changes in the supply of the inputs into aid projects and reserves, other than via the impact of aid, controlled for the probability to receive aid, country-year-fixed effects, province fixed effects (or country- and year-fixed effects in the country-year setting), and the other variables in the model. In other words, as in any difference-in-differences setting, we rely on a treatment that is (conditionally) exogenous and the absence of different pre-trends across groups. Controlling for a set of fixed effects that capture the direct effects of these variables, currency reserves and physical inputs cannot be correlated with the error term and are indeed (conditionally) exogenous to aid. For different pre-trends to bias our coefficients, patterns across regions with a high compared to a low probability of receiving aid would have to vary in tandem with year-to-year changes in these supply factors. We test this possibility below and find no evidence of such a threat to our identification strategy.

We report our main results in Section 3. They show that Chinese health aid *increases* infant mortality at the local level, but *decreases* mortality when focusing on countries instead. We argue that these differences can best be explained by fungibility, and test this channel in a number of ways. First, we show that the availability of clinics in the vicinity of Chinese aid projects is not affected by aid, potentially indicating that recipient governments channel the aid to build facilities they would have financed themselves or with aid from other donors absent of Chinese support. Second, we find that the number of deliveries in health clinics is reduced by aid. This can explain how fungibility renders the effect of aid at the sub-national level negative rather than just null. Aid-financed facilities specialize in areas that are less effective in reducing infant mortality than the facilities replaced by aid, or alternatively, poach skilled staff from an existing health clinic. Service provision thus decreases both in terms of quantity and quality (also see Deserranno et al. 2020). Third, and in line with this interpretation, we show that the number of births attended by skilled health staff is reduced by aid, while more births are attended by traditional health staff in turn. Fourth, the turnover of health staff at existing clinics seems to increase, and average (educational) quality to decline as a consequence of aid. It thus seems that aid-financed projects poach staff from existing clinics. Fifth, the effect of aid on mortality is larger when governments are already allocating relatively high levels of domestic public expenditures to the health sector prior to the receipt of aid and when more aid has been received from Western donors. This is in line with the interpretation that fungibility should play a larger role when alternative sources of funding are available. More directly, at the local level, we also find that World Bank aid for health is reduced as a consequence of Chinese support to the same localities. Sixth, we test whether results in a sector of particular interest to China—but not necessarily to the recipients of its aid—improve as a consequence of aid, which would be the case if total funds focus more strongly on these sectors as a consequence of China's interventions. In line with this expectation, we find that Chinese health aid increases the probability that women took anti-malaria pills during pregnancy (with the fight against malaria being a major goal of Chinese operations).[5]

We compare our results for China to those of World Bank aid (in Section 4) and test a potential alternative explanation for our results as well as their robustness (in Section 5). Given that the World Bank carefully chooses which projects to fund and monitors its projects throughout the implementation period, fungibility should be lower when

[5] The three-year plan resulting from the Forum on China-Africa Cooperation in 2009, for instance, states in one out of the three points addressing public health: "The two sides noted with pleasure the deepening health cooperation between the two sides. In particular, the hospitals and anti-malaria centers that China has undertaken to build will play a positive role in improving the health care level and protecting people's health in African countries." The ongoing Chinese anti-malaria campaigns have also not gone unnoticed in the media. Reuters for example headlined in 2009 "China adopts "Malaria diplomacy" as part of Africa push" (see https://www.focac.org/eng/zywx˙1/zywj/t626387.htm, https://www.reuters.com/article/idUSSP503140, accessed April 17, 2020).

compared to China.[6] In line with this expectation, we do not find evidence in support of the hypothesis that the World Bank's health-related aid is fungible. Just like Greßer and Stadelmann (2019) we even find that World Bank projects reduce infant mortality at local scales. Regarding an alternative interpretation of our results for China, a skeptical reader might think that the composition of women giving birth is affected by aid. To the extent that aid differentially affects the number of mothers from vulnerable populations who give birth, increases in mortality could be the consequence of compositional differences. Our results indeed show that aid affects fertility and mortality differentially for different age groups and ethnicity of the mothers. However, when we hold these differences constant, our key results remain unchanged.

On balance, our results thus seem to indicate that fungibility makes Chinese aid appear to be negative at sub-national scales, while the overall effect of aid is positive. This observation bears important implications for policy-makers and research alike (which we discuss in the concluding Section 6). To the extent that donors of aid are interested in outcomes in a particular locality, the incentives of recipient governments to re-direct their own funds or aid from other donors to alternative sectors or areas would also need to be considered. While fungibility is a problem many donors seem duly aware of, it is usually understood in a way less subtle than the mechanisms uncovered here. For example, Langlotz and Potrafke (2019) show that aid increases recipients' military expenditures, implying that part of the aid is used for entirely different purposes. In this paper, we show that fungibility can be important, even if aid is spent in the same sector but in different geographic areas, or in the same area in a related sub-sector. Our study has implications for the recent trend in aid effectiveness-studies that focuses on small sub-national areas rather than broader regions or countries. While the sub-national analysis of data bears important advantages—in terms of a greater number of observations and more rigorous identification—the effects of aid reflected at this level might turn out inconsequential or negative even if the aggregate effect is positive.

## 2 Data and Methods

The literature on the effectiveness of foreign aid below the country-level is scarce, mainly due to a lack of geo-coded data on aid, relevant outcomes, and important control variables. In this study, we rely on data for more than 2,000 Chinese aid project locations in the years 2000-2014 that have been geo-coded in Bluhm et al. (2020).[7] For comparison, we also use data on geo-coded World Bank projects that have been approved in the

[6] Also see Van de Walle and Mu (2007).

[7] The number of projects made available by Dreher et al. (2020) is substantially larger (3,485 projects). This is because Bluhm et al. (2020) geo-code only those projects that were completed or in implementation during the period their sample covers, and due to the lack of sufficiently precise geographic information for some of the projects.

health sector over the same period.[8] Though not free from geopolitical interference of its donors (Dreher et al. 2009, Dreher, Lang, and Richert 2019), the World Bank has rigorous standards for the ex ante evaluation of potential projects. In contrast, China gives substantial leeway to recipient countries on where to spend its aid (Dreher et al. 2018, 2019a). These two donors are thus very different in terms of how they exercise control over the recipients' use of aid, allowing us to compare the effects of aid for a donor that is particularly lenient to one that is particularly stringent among the set of all official donors of aid (Gehring et al. 2019).[9]

We combine the data on aid with health indicators from 92 demographic and health surveys in a maximum of 53 countries over the 2002-2014 period. There are two main types of DHS—Standard and Interim. Standard surveys cover between 5,000-38,000 households per country and are typically conducted every five years. Interim surveys include fewer indicators and smaller populations. Both DHS types are however nationally and regionally representative.[10] Data are available for census enumeration areas, which can be villages in rural regions or blocks of a city in urban spaces. On average, these enumeration areas cover 0.38 (0.34) km2 for urban (rural) clusters and survey 25-30 households. The center of each enumeration area ("DHS cluster") is geo-referenced but slightly displaced to protect the anonymity of the observed units (see ICF 2013 for details).

One key advantage of the DHS is the inclusion of information about children born prior to a survey. This allows us to extract information about the health status of children also in years without a DHS so that we can exploit the full range of years for which we have information on aid flows. The resulting dataset includes yearly information on infant mortality for 103,008 children per year, on average, over the 2002-2014 period, defined as the number of children that died before they were 12 months old in 1,000 children born alive. The total number of children included in our sample is around 1.3 million, and the average infant mortality rate is 55. We make use of 54,946 different DHS clusters, with an average of 3.7 children covered per cluster and year. 390,869 (344,934) births were registered in areas with active (inactive) Chinese projects. There are 2,161 Chinese aid project locations in our sample; 1,507 of these locations are within a radius of 55 km of at least one DHS cluster, and 217 of these are health-related projects.

[8] AidData (2017) provides these data in collaboration with the World Bank. We only include projects where the project geo-coordinates (i) correspond to an exact location, or (ii) are within 25 km of an exact location (AidData precision codes 1-2). While data are available as of 1995, we restrict the sample to the 2000-2014 period to ensure comparability.

[9] It is important to note, however, that China only grants a small share of its aid as budget aid and does not simply finance any type of project proposed by recipient countries. Obviously, recipient governments could otherwise simply ask China to support its military aid budget rather than asking it to finance a health-related project, and use the additional budgetary leeway in support of the recipient's preferred spending.

[10] Our sample includes 90 Standard surveys, and two Interim surveys (Rwanda in 2008 and Egypt 2003), out of the available 296 Standard and 6 Interim surveys. We do not use the full sample either for the lack of sufficiently precise geo-referenced information on aid projects or the lack of geo-referenced information in DHS.

For further tests, we also extract information on whether or not a birth was attended by skilled health personnel for 46,913 children per year, on average. Skilled health personnel is defined as a doctor, nurse, or midwife. We contrast the use of modern health services by examining reliance on informal and local health services. To this end, we use data on the number of births attended by traditional birth attendants. To examine whether changes in the frequency that skilled staff assists births are driven by developments in the private or public sector, we look at the place of delivery. In particular, we examine the number of deliveries that took place at private and public health facilities respectively, per 1,000 deliveries. In the next step, we examine whether health aid reduces the number of women that did not deliver at a hospital because the distance is too far. To broaden our analyses of the health sector and capture potential interdependencies between different areas within the health sector, we examine whether the number of pregnant women (out of 1,000) who took antimalarial pills is affected by aid (the fight against Malaria is a major part of Chinese health-related aid interventions). We also evaluate if the supply of hospitals is altered relying on a geocoded dataset covering almost 100,000 health facilities in Sub-Sahara Africa provided by Maina et al. (2019).[11] To test whether aid affects the composition of staff, we leverage surveys of staff in some of these facilities (from the Service Provision Assessments which are part of the DHS program). We make use of nine surveys covering seven countries, 8,514 health facilities and 260,616 workers.[12] Specifically, we assess whether more or fewer people (out of those interviewed) were hired in a given year and whether they are more or less educated than other interviewed workers that were already employed in the previous year.

Absent any fungibility of aid, one would expect the effect of aid to be most visible when we focus on small areas in the vicinity of aid disbursements. Aid is often highly concentrated on certain regions inside a country (Dreher and Lohmann 2015). Even if the aid is effective there, improved development outcomes might not visibly materialize at the country level. We expect the opposite to be true if aid is fully fungible. The scenario under which fungibility would make the local effect of aid appear most harmful is one in which the aid is fungible between geographic areas, or over time within the same sector. For instance, assume that a recipient government intends to use its own funds to reduce infant mortality if no external donor is available.[13] Assuming that the government would instead finance that project at a later point in time, or elsewhere, under a scenario where China finances an anti-malaria project, the effect of Chinese aid on infant mortality would appear negative, given that any region is more likely to reduce infant mortality at a time

[11]The dataset is based on national lists which are compiled by the respective Ministry of Health and are supposed to be a complete and authoritative listing of health facilities. They are most comprehensive for facilities managed by the government, non-governmental organizations, or faith-based organizations. To our knowledge it is the most comprehensive dataset covering geocoded health facilities in Sub-Sahara Africa available.

[12]The seven countries are Bangladesh, Haiti, Kenya, Malawi, Namibia, Senegal and Tanzania.

[13]Figure A.1 in the Appendix illustrates this graphically.

they do not receive aid from China compared to when they receive it.

We test the local effect of aid by focusing on small geographic areas within a radius of 0.5° around the center of each DHS cluster.

We estimate our basic regressions as follows:

$$Health_{i,t} = \beta_1 active_{i,t} + \beta_2 inactive_{i,t} + \beta_3 X_{l,t} + \beta_4 \eta_{j,t} + \beta_5 \gamma_k + \epsilon_{i,t}, \tag{1}$$

where $Health_{i,t}$ is the infant mortality rate in recipient cluster $i$ in year $t$. Our sample covers all 54,946 sub-national clusters within a radius of 55 km for which we have geocoded data on Chinese projects in the 2002-2014 period.[14] Variation in the availability of aid within DHS clusters over time is insufficient to allow the inclusion of cluster-fixed effects. We however include fixed effects for country-years $\eta_{j,t}$ and ADM2 regions (such as districts and municipalities) $\gamma_k$. We cluster standard errors at the ADM2-year level ($\epsilon_{i,t}$ in equation (1) represents the error term). All regressions use cluster-weights as recommended by DHS.

Rather than comparing clusters that received aid to those that did not, we follow the approach in Isaksson and Kotsadam (2018) and compare health outcomes in clusters with projects that have started disbursing aid ($active_{i,t}$) before the birth year of the child to outcomes in clusters that also received projects during the sample period, but that have not started to disburse before the child was born ($inactive_{i,t}$).[15] In our sample, 25 percent of the observations refer to enumeration areas that are located within 55 km of an active Chinese project, while 21 percent refers to projects where the implementation period has not yet started. Rather than comparing cluster regions that have been selected as project-sites to others that have not, we thus exclusively compare health outcomes in regions that have been selected as project sites, but at different points in time. We expect this to reduce the importance of omitted variables bias to a considerable extent.

In order to estimate the effect of aid, we test whether $\beta_1$ is significantly different from $\beta_2$ in equation (1) above, in addition to testing whether any of the two is significantly different from zero. In order to code a cluster as active or inactive, we rely on the first aid project in each cluster in our sample. This ignores the potential effect of earlier projects (on which we have no data). Regions with active aid projects earlier in the sample period might be more likely to have received projects in the recent past compared to regions that

[14]Focusing on the sub-national level addresses a further important problem of the aid effectiveness literature. According to Ioannidis et al. (2017), only about one percent of the 1,779 estimates in the aid and growth literature surveyed have adequate statistical power (see also Doucouliagos 2019). Given the small number of observations available at the country-year level, there is nothing that researchers can do to increase power. Focusing on the sub-national level—and the substantially larger number of observations available there—thus is an important step forward in this literature.

[15]Isaksson and Kotsadam (2018) make use of this approach to show that Chinese aid in Africa fuels corruption. Kotsadam and Tolonen (2016) use the same approach to investigate the effect of natural resources on gender inequality in Africa. We also tried to make use of data for children born to the same mothers, but had insufficient data on projects nearby to allow us controlling for mother fixed effects.

receive a project at a later point in time. The effect of active projects that we measure is thus not necessarily the effect of the specific projects in our sample, but might to some extent reflect the effects of earlier projects as well.[16]

To further reduce the importance of omitted variables bias, we control for a number of variables at the cluster level, included in the vector $X_{l,t}$. We include variables that we expect to be correlated with health outcomes and the probability to receive an aid project in a particular year, mostly taken from the DHS geospatial covariates dataset (Mayala et al. 2018): an index variable ranging from zero to one (with one referring to extremely urban, and zero to extremely rural), indicating the proportion of built-up infrastructure nearby; a categorical variable of the predominant mode of settlement in the region ("SMOD population"), which distinguishes between rural, urban, or urban center; an indicator of a region's average slope (measured in degrees, where one degree roughly equals 111 km); rainfall (in meters per location and year); travel time to the next larger settlement of 50,000 people or more (measured in days); a vegetation index measuring the density of green leaves (between zero and one with larger values indicating denser vegetation); and the logarithm of the number of people living within the 2 km (urban) or 10 km (rural) buffer surrounding the DHS survey cluster location.[17] We control for distance to (i) the nearest water body, (ii) the nearest area protected by the United Nations if any, and (iii) the border of the nearest neighboring country (all measured in kilometers).[18] All time-varying variables are lagged by one year.[19]

Our second set of sub-national regressions focuses on the number of projects a

[16]Just like Isaksson and Kotsadam (2018) we test robustness along several dimensions. First, we estimate project-fixed effects regressions for the reduced sample that we have information for health outcomes before and after a project started to be implemented. Second, we control for the duration of active projects and the time until an inactive project becomes active (expecting the former to have a positive effect on outcomes, and the latter to be insignificant). We also test whether the results remain robust when we code projects as inactive once they are completed, and when we focus on projects that have been active in the five years prior to an interview.

[17]The data were generated by the Center for International Earth Science Information Network of Columbia University in 2016. They have a 1 km by 1 km spatial resolution for the years 2000, 2005, 2010, and 2015; we linearly interpolate the years in between.

[18]A number of these variables—the urban areas indicator, travel time, and all distance data—do not vary over time. Others are not available for every year. Data for population count, mean temperature and the vegetation index therefore refer to the year 2000; data for slope are from 1996. Precipitation, aridity, and mean temperature refers to the year of the respective DHS. Note that we do not include data derived from the household level, such as education or wealth, as these are likely to be affected by aid as well (Martorano et al. 2020). Appendix B reports the exact definitions of all variables and their sources. Appendix C shows descriptive statistics.

[19]In tests for robustness, we also control for luminosity as an indicator of development and a binary indicator that is one when the head of government was born in grid-cell $i$. Controlling for development is crucial and problematic at the same time. According to Bruederle and Hodler (2018), nightlight and health indicators are correlated at the local level. Omitting nightlight, our coefficient for aid in the OLS regressions might just capture an effect of aid on development more broadly. On the other hand, luminosity might be a transmission channel by which aid affects health. Hodler and Raschky (2014) show that leaders favor their birth regions, leading to better development. Dreher et al. (2019a) show that one channel for such favoritism can be Chinese foreign aid. We therefore do not include these variables in the main regressions. When we include their lagged values our results are however robust.

cluster region receives rather than the binary indicator.[20] The second stage of our 2SLS regressions is:

$$Health_{i,t} = \beta_1 Aid_{i,t-2} + \beta_2 p_i + \beta_3 Pop_{l,t-1} + \beta_4 \eta_{j,t} + \beta_5 \gamma_k + \epsilon_{i,t}. \quad (2)$$

The aid effectiveness literature typically uses disbursements of aid rather than commitments, given that aid can only register impacts if it has been disbursed. Such data are however unavailable for China, so we rely on projects committed.[21] In our main specifications, we lag aid by two years following previous work (e.g., Dreher et al. 2020). One might expect aid to take longer to potentially affect outcomes, given that commitments need some time to disburse. However, while this might hold true for Western donors, Chinese aid typically disburses quickly, so that we do not add further lags in our main specifications (e.g., Dreher et al. 2019a,b). Given that the appropriateness of these lags is largely an empirical question, we however test different timings in further specifications.[22] In addition to focusing on health aid, we also investigate the totality of aid projects. Such aid might be less likely to affect health outcomes. On the other hand, to the extent that aid is fungible initial labels might not matter much. What is more, aid given to areas other than health might easily affect health outcomes as well.[23]

Of course, aid is likely to be endogenous to health outcomes. One likely source of endogeneity is reverse causation in which recipient economic features influence the allocation of aid. On the one hand, China might provide more aid to poorer regions. On the other, donors might prefer to channel more aid to wealthy regions if these recipients provide more attractive commercial opportunities (Frey and Schneider 1986, Dreher et al. 2019a) or might want to avoid difficult geographic regions.[24] It should also be noted that a large number of variables that are excluded from our models are potentially correlated with aid as well as with our health indicators, introducing omitted variables bias. To address the potential endogeneity of aid, we employ an instrumental-variables strategy.

Our proxy for the availability of Chinese aid in a year follows Dreher et al. (2019b),

[20] Note however that in most cases sub-national regions receive just one health-related project at any point in time. Using a binary variable that indicates whether or not a new project has been committed in a year instead of project numbers does not change our results.

[21] One might think of using commitment amounts rather than project numbers. However, 43 percent of the health projects in our sample have no data on the amount of aid. Therefore, we prefer to focus on project numbers but report results using amounts in tests for robustness below.

[22] In line with Dreher and Lohmann (2015) and Galiani et al. (2016), we below also average aid projects over three years to test robustness.

[23] This also comes with the advantage of a much-increased number of aid locations that we can exploit in the analyses.

[24] Empirical research on Chinese aid allocation demonstrates a strong, negative correlation between Chinese ODA and the per-capita income of recipient countries (Dreher and Fuchs 2015, Dreher et al. 2019a). However, Chinese OOF (in Africa) tends to favor creditworthy countries (with higher loan repayment capacity) and countries that import more from China (Dreher et al. 2019a). Cervellati et al. (2020) show that Chinese (infrastructure) projects are less likely to be implemented in areas with higher risks of malaria.

Bluhm et al. (2020) and Dreher et al. (2020). Dreher et al. (2019b) introduce (logged) annual Chinese steel production as part of an instrument for Chinese aid. They argue that Chinese officials are rewarded for producing as much steel as possible, resulting in production that is in excess of domestic needs. Parts of the excess supply is then shipped abroad as exports, foreign direct investment, or aid. The larger the volume of steel available in a year, the cheaper are additional Chinese aid projects using this material, consequently increasing the supply of aid. Countries with established aid relationships are then expected to receive a larger chunk of this aid, all else equal. Since Dreher et al. (2019b) have first introduced this instrument in 2016, a number of follow-up studies have used it, including Brazys and Vadlamannati (2018), Gehring et al. (2019), Humphrey and Michaelowa (2019), and Zeitz (2020). The instrument was further refined in Bluhm et al. (2020). Rather than just using China's production of steel, they suggest focusing on several additional factors that are important inputs for Chinese aid projects: timber, glass, aluminum, and cement. Given that the production of these materials trends over time, Bluhm et al. (2020) de-trend them rather than only relying on country-year fixed effects to absorb the trend. They then extract the first principal factor of the five de-trended (logged) input factors and interact it with the number of years each region has received positive amounts of Chinese aid over the sample period. The resulting product is our first instrument for Chinese aid.

Our second instrument is introduced in Dreher et al. (2020). As they point out, a key reason for China to grant non-concessional, dollar-denominated loans is the possibility to earn interest rates above what would be possible domestically. At the same time, concessional projects should become more easily available as well. This is because the income effect of cheaper aid provision should make the availability of both types of aid more likely (though a substitution effect would work in favor of non-concessional projects).[25] In years in which China's reserves of foreign currency are high, Dreher et al. (2020) therefore expect (and show) the supply of aid to increase. Their instrument is then the interaction of changes in China's net currency reserves and the location-specific probability to receive Chinese aid. In this study, the instruments are the interactions of the two indicators for Chinese aid which vary over time but not between sub-national regions or countries ($Input_{t-1}$, $Reserves_{t-1}$), and the probability of receiving aid from China, $p_i$, which varies across recipient locations but not over time. We calculate the probability of receiving aid as the share of years in the 2000-2014 period a region has received positive amounts of aid.[26] More precisely, we define the probability of receiving aid from China as $p_i = \frac{1}{15}\sum_{(y=1)}^{15} p_{(i,t)}$, where $p_{i,t}$ is a binary variable that equals one when recipient location $i$ received a (health) project from China in year $t$. The first-stage

[25] To the extent that part of the materials used in a project are purchased in international markets, the availability of reserves makes such inputs appear cheaper as well.

[26] This directly follows the analyses in Nunn and Qian (2014), Bluhm et al. (2020), Dreher and Langlotz (2020), and Dreher et al. (2019b, 2020). Also see Werker et al. (2009).

regression is:

$$Aid_{i,t-2} = \beta_1 Input_{t-3} * p_i + \beta_2 Reserves_{t-3} * p_i + \beta_3 p_i + \beta_4 Pop_{l,t-1} + \beta_5 \eta_{jt} + \beta_6 \gamma_k + \mu_{i,t}. \quad (3)$$

In our IV regressions we control for (log) population, $Pop_{l,t-1}$, in a cluster $l$ but omit the other control variables. While their inclusion would increase the efficiency of the estimator, they are also potentially correlated with aid and health and therefore introduce endogeneity to the estimates. Given that our identification strategy does not require their inclusion, we estimate our main regressions without them (but test robustness to their inclusion below). Also note that we control for the cluster-specific probability $p_i$ to receive health projects in the first and second stages of the regressions.[27] As before, we cluster standard errors at the ADM2-year level and include country-year- and ADM2-fixed effects. Our regressions at the country-level are in analogy, with the probability to receive aid and the other variables measured at the country-level, and fixed effects for years and countries instead. Given that the probability of receiving aid does not vary for a country over time, it is captured by the fixed effects. We cluster standard errors at the country-level there.

One might think that the Local Average Treatment Effect (LATE) we estimate with physical inputs into aid projects would not well predict projects in the health sector. However, almost all of the larger health projects in our sample involve the construction of hospitals and other health facilities. These projects rely heavily on Chinese raw materials (and workers).[28] What is more, the abundance of physical aid inputs arguably also increases the supply of aid projects that are unrelated to such inputs. This is the case if the income effect of cheaper aid provision exceeds the substitution effect away from the (relatively more expensive) projects that do not require physical inputs in years in which such inputs are more easily available (Dreher et al. 2020). Given that China's aid responds to recipient requests for aid projects, and recipients have no reason to alter the composition of their requests in response to Chinese production of raw materials, we consider it likely that all types of aid projects are more easily available in years the provision of aid becomes cheaper. We also have no reason to assume that projects financed due to the easy availability of reserves differ fundamentally from projects financed independently of reserve availability. While we use both instruments jointly in our main regressions, we test robustness to including them individually.[29]

One might also be concerned that our instruments violate the exclusion restriction

[27] This variable is however omitted from the OLS regressions that we also show below.

[28] For details, see Dreher et al. (2020).

[29] We also tested an alternative instrument that includes China's aid budget as part of the interacted IV (Temple and Van de Sijpe 2017, Dreher et al. 2020). However, first-stage statistics are very low in the country-level analyses, so we do not report these results.

because the probability of receiving aid may directly affect population health. However, our second-stage regressions control for the probability of receiving aid itself. China's aid inputs are captured through the inclusion of country-year-fixed effects (or country- and year-fixed effects in the country-level setting). Controlling for country-year-fixed effects, these variables cannot be correlated with the error term and are thus (conditionally) exogenous to aid. Given that we control for the probability of receiving aid, its interaction with an exogenous variable results in an exogenous instrument under the assumption of parallel trends before the intervention (Bun and Harrison 2018, Nizalova and Murtazashvili 2016).[30] The intuition of this approach is that of a difference-in-differences regression, where we investigate a differential effect of the ease of access to aid projects in regions with a high compared to a low probability of receiving aid. The identifying assumption is that health outcomes in regions with differing probabilities of receiving aid will not be affected differently by changes in China's production of aid inputs and the availability of reserves, other than via the impact of aid, controlling for the probability to receive aid and the country-year-fixed effects. In other words, as in any difference-in-differences setting, we rely on an (conditionally) exogenous treatment and the absence of different pre-trends across groups. In order for different pre-trends to bias our estimates, trends across regions with a high probability compared to a low probability of receiving aid would have to vary in tandem with the year-to-year changes in China's production of raw materials and reserve availability.

Following Christian and Barrett (2017), we plot the variation in Chinese aid inputs and currency reserves in concert with the variation in the average number of new aid projects world-wide and the infant mortality rate for two different groups that are defined according to the median of the probability to receive aid (given that any aid was received). Figure 1 plots this graph. The results give little reason to believe that the parallel trends assumption is violated in our case. More precisely, the probability-specific trends in infant mortality seem rather parallel across the regular recipients (those with a probability of receiving aid that is above the median) and the irregular recipients (those with a probability of receiving aid that is below the median). There is also no obvious non-linear trend in regular recipients compared to irregular recipients that are similar in aid and mortality.

The excludability of our interacted instrument would be violated if changes in China's physical inputs into aid projects or reserves availability would affect sub-national health outcomes differentially in regions with a high probability of receiving aid compared to regions with a low probability of receiving aid for reasons unrelated to aid. Reserves and

[30] Also see Goldsmith-Pinkham et al. (2020). As Bluhm et al. (2018) point out, identification in our setting can be purely based on exogenous variation in the time-series shocks, as shown by Borusyak et al. (2018), to the extent that the covariance between the time-varying components of the instruments and a weighted average of the within location variation over time in unobserved factors that also affect the dependent variables approaches zero in large samples.

input production are arguably correlated with a large number of other variables, and some of them might differentially affect health in regions that are frequent or infrequent recipients of aid. Most plausibly, the availability of reserves could be correlated with the worldwide business cycle. Potentially, frequent recipients of Chinese aid are also more severely affected by recessions. This could imply that any differential effects of aid on health that we observe result from the world's business cycle rather than aid. Chinese aid inputs could also be correlated with China's exports and foreign direct investment, and the probability of receiving aid might be correlated with the probability to trade or receive investments. To address these concerns, we control for yearly worldwide GDP growth as well as Chinese FDI or exports to a country—all interacted with the probability to receive aid—in robustness tests below.

# 3 Results

Table 1 shows the main results of how Chinese (health) aid affects infant mortality. According to the difference-in-differences results of column 1, infant mortality is lower in regions with active Chinese health projects, on average, though with an insignificant coefficient. However, infant mortality is also lower in regions with inactive projects, pointing to selection bias with respect to regions that are not selected to receive projects at all. Regions with an active health aid project experience lower infant mortality compared to regions without projects—the same, however, holds for regions with inactive projects. As can be seen at the bottom of the table, while the coefficient is larger for inactive projects compared to active ones, the difference between the two is not statistically significant at conventional levels.

In column 2, we turn to the (lagged) number of Chinese health projects committed rather than the binary indicators. The OLS results in Panel A show that the (conditional) correlation between health aid and infant mortality is positive and significant. Panel B turns to our instrumental variables results. As can be seen, the coefficient of aid stays positive and significant. The coefficient implies that an additional health project increases the infant mortality rate in regions receiving it by more than nine children in 1,000 children born alive, at the five-percent level of significance. The first-stage F-statistic (shown at the bottom of the table) indicates the power of our instruments.[31]

Columns 3 and 4 turn to the country-level for comparison, including all projects that can be attributed to a sub-national location to facilitate comparability. Focusing on all countries with available data, the results of column 3 show that Chinese health aid reduces infant mortality at the country-level, at the one-percent level, and with a coefficient that

[31] These results stand to some extent in contrast to those of Martorano et al. (2020), who find that Chinese aid improves household health, wealth and education levels. They focus on only 13 countries in Sub-Sahara Africa and do not employ an instrumental variable though, limiting comparability.

exceeds that obtained at local scales by more than 50 percent in magnitude. To rule out that these contrasting results arise from differences in the countries and projects included in the local and country-level samples, we replicate the analysis focusing on those countries and projects that are included in the sub-national analysis of column 2. Results remain similar (see column 4). Specifically, we find that an additional aid project from China reduces infant mortality by about 10 children for every 1,000 live births.[32]

In columns 5 to 8, we replicate the analyses focusing on all aid from China rather than just health aid. To the extent that better infrastructure makes hospitals more accessible or better education improves the quality of health staff, aid broadly is the better measure to capture the effects of aid on health. When aid is fungible not just within but also between sectors, focusing on all aid is preferable to investigating the effects of specific types of aid, as the labels given to aid would then be irrelevant. If fungibility is limited, to the contrary, focusing on all aid rather than health aid could be expected to decrease the magnitude and precision of our estimates. The results are overall similar, the exception being an insignificant (but positive) coefficient in the sub-national 2SLS regression of column 6. All three coefficients are smaller in magnitude, in line with an environment of limited fungibility across sectors.

The stark differences in results we obtain at the local- compared to the country-level can potentially be explained with aid being fungible within the health sector. The previous literature is mixed in this regard. For example, Van de Sijpe (2013) finds that health aid is fungible to a limited degree, while Dykstra et al. (2019) find fungibility to be substantial. In the context of our paper, fungibility has a number of observable implications. Some refer to health outcomes, others to inputs. In the following tables, we test several such implications, focusing on health aid to reduce clutter. However, we would like to stress from the outset that the results we obtain are overall similar for all aid from China.[33]

Table 2 tests potential transmission channels, focusing on our local-level 2SLS estimates. We investigate the effect of aid on the number of births that are attended by (i) skilled or (ii) traditional health staff. To the extent that aid-financed projects focus on areas less directly related to infant mortality and recipient governments or other donors provide fewer services in return to Chinese largesse, fewer births should be attended by skilled health staff. We would then expect births to be attended by traditional birth attendants instead (which are less likely to be poached or replaced by Chinese aid projects). We further investigate how deliveries develop due to aid at public hospitals compared to private hospitals. Again we would expect fungibility to reduce the number of deliveries in public hospitals, but not necessarily in private ones. This is

[32]Note that while the first-stage F-statistic is substantially lower compared to the sub-national analysis, the test has sufficient power in the smaller, more comparable, sample.

[33]Detailed results are available on request.

because private hospitals, on average, pay higher wages compared to public ones (McCoy et al. 2008). What is more, they are arguably less likely to withdraw their services from a certain area as a consequence of foreign aid, given that the aid is granted on the governments' request and thus in those areas the governments would likely have prioritized their own spending too. We further explore whether fewer births might have been delivered in hospitals because respondents to the DHS surveys in our sample consider them too far.[34] In concert, these tests of availability and uptake thus provide evidence on whether the availability of hospitals is affected by aid, or rather that aid affects the uptake of services for a given supply of hospitals, which would suggest that donor-financed hospitals substitute government-financed ones, potentially specializing on the provision of different services. We also test this suggestion more closely by investigating whether aid improves outcomes related to malaria, which is a focus area of Chinese health-related aid.[35] Finally, we focus on the number of new hires at existing hospitals, as well as the staff's level of education. To the extent that poaching is relevant for our results, staff turnover should increase, while the quality of staff at existing hospitals should decline, with the most qualified staff being poached first.

Column 1 of Table 2 shows that an additional Chinese health project reduces the number of births attended by skilled health staff by almost 70 in 1,000 live births delivered, at the one-percent level of significance. In column 2 we focus on the number of births attended by traditional health workers. As can be seen, the coefficient is positive and substantial (indicating that an additional project increases the number of births attended by almost 133 in 1,000 live births delivered).

Columns 3 and 4 test how China's health aid affects the availability of public and private health clinics, using a sub-set of the data that allows testing whether private and public health facilities settle close to Chinese aid projects. Columns 5 and 6 focus on the number of deliveries at public and private clinics; column 7 tests whether having a Chinese project nearby affects survey respondents' perception of how distant they are to a health facility (i.e., whether they respond they did not deliver at a hospital because they consider it too far). According to the results, the availability of clinics is not affected by aid. On the contrary, Chinese aid reduces the number of deliveries at public health clinics, at the one-percent level of significance. While it also reduces the number of deliveries at private health clinics, the coefficient is less than one-third of those for public clinics (and the difference is statistically significant, with a p-value of 0.006). Taking these results at face value, they are in line with the suggestion that the services provided in aid-financed clinics differ from government-financed ones: Total availability of hospitals does neither increase nor decrease as a consequence of aid, while uptake for delivery declines and

[34] The exact question we rely on is "Q: Why didn't you deliver in a health facility? R: TOO FAR."

[35] Appendix D shows that malaria-related interventions are the bulk of Chinese health projects among projects with a specified purpose. Also see Anshan (2011).

hospitals are perceived to be more distant. We test the potential shift in focus within the health sector as a consequence of aid more specifically in column 8, where we find that aid increases the probability that women took anti-malaria pills during pregnancy, at the one-percent level of significance. Though the increased use of anti-malaria pills should reduce rather than increase infant-mortality, this positive effect seems to be dominated by shifts in other services provided by hospitals.[36]

We conclude this table investigating staff turnover and educational quality. Columns 9 and 10 focus on staff in health facilities close to Chinese health-aid projects. According to the results, health facilities hired significantly more workers after health aid projects were committed nearby. This could either indicate that the aid was used to expand existing facilities that, in turn, require more workers or that existing workers are replaced, potentially as a consequence of poaching. According to column 10, the average years of education of interviewed staff already employed in the facility declines by 1.1 years due to staff with fewer years of education being hired as a consequence of aid committed two years earlier. In concert, these results thus suggest that new staff is hired as a result of aid, replacing staff poached by aid-financed projects.[37]

In summary—while not providing bullet-proof evidence—the results of Table 2 are in line with the fungibility-hypothesis. In Table 3 we investigate channels that we expect to facilitate the fungibility of aid. First, we expect fungibility to be more substantial if government expenditures on health are larger when aid is given. Larger expenditures give governments more leeway to reallocate resources, either from the health budget to other areas of government spending or to alternative health projects in different areas of the country. In column 1 we therefore interact the number of Chinese health projects with the recipient government's health spending (relative to all government expenditures) a year before the aid project was received. While the level of health expenditures itself is captured by country-year fixed effects, we can still test whether and to what extent larger health budgets make foreign aid less effective in increasing the number of skilled health staff.[38] As can be seen, the coefficient of the interaction is negative and significant at the one-percent level. In tandem with the positive coefficient of aid projects, its magnitude implies that the number of deliveries attended by skilled health staff increases with aid

[36] As can be seen in Appendix D, the fight against Malaria stands first among China's health projects targeted at specific diseases, with the second-largest share—the fight against HIV/AIDS, Ebola and Tuberculosis combined—receiving less than three percent of the projects. Unsurprisingly, we consequently do not find a significant effect of China's projects on the number of people ever tested for HIV/AIDS (specific results available on request).

[37] While this result is in line with fungibility, the alternative explanation that the aid is effective in increasing the supply of hospital services so that more staff with fewer years of education are hired cannot be ruled out based on these regressions alone. The previous regressions show however no evidence for this.

[38] We instrument aid and the interaction of aid and health expenditures with our previous instrument, as well as the interaction of this instrument with health expenditures. We follow Nunn and Qian (2014) and include the interaction of health expenditures interacted with the probability of receiving aid as an additional instrument. Results in the regression reported here are robust to excluding the interaction.

to a larger extent when health expenditure shares are smaller.

Along similar lines, we would expect Chinese aid to be more fungible when recipient countries receive more aid from all of its donors combined. Absent of Chinese funding, the very same project might have received funding from another donor, with potentially differential effectiveness. We would ideally like to test whether Chinese aid projects crowd out aid from other donors at the local level. Given that we do not have geocoded data for most other donors, we instead interact the number of Chinese health projects committed to a specific area with the total health-related aid received from other donors in the previous year (relative to GNI). Given that we have geocoded data for the World Bank we also directly test whether Chinese aid crowds out World Bank aid at the local level.[39]

The results show that the (negative) effect of Chinese health projects on the number of births assisted by skilled staff gets stronger at times a recipient country received more aid from other donors (column 2). This is in line with the idea that Chinese aid is more likely to have replaced a project from another donor if such aid was given in abundance, and the project that has been replaced by China was targeted at assisted births to a larger extent.[40]

Column 3 shows that there is no significant effect of Chinese health aid on World Bank health aid two years after commitment. However, the coefficient is negative and significant at the five-percent level one year after aid from China is committed (see column 4). It thus seems that the World Bank commits less aid to areas that received Chinese aid. To the extent that World Bank projects are more effective in reducing infant mortality than Chinese aid (a question to which we turn to below), such fungibility can explain our key results in Table 1 above.

# 4 Comparison to the World Bank

This section replicates the main analyses focusing on the World Bank. Our data consist of 2,065 geo-coded World Bank health-related projects spreading over 51 recipient countries that have been approved over the 2002-2014 period.

Our instruments are similar to those for China. We proxy available liquidity with two separate indicators for the World Bank's International Development Association (IDA) and the International Bank for Reconstruction and Development (IBRD). We proxy the availability of IDA aid in a specific location with the interaction of an indicator for the

[39] The analysis thus also contributes to the literature investigating the extent to which donors engage in competition to aid from China or others (see Mascarenhas and Sandler 2006, Humphrey and Michaelowa 2019, Asmus et al. 2020, Fuchs et al. 2020, and Zeitz 2020).

[40] We use the interaction of Chinese aid projects and aid from other donors as a second instrument. We again follow Nunn and Qian (2014) and include the interaction of aid from other donors interacted with the probability of receiving aid as an additional instrument. Results in the regression reported here are not robust to excluding the interaction as an instrument however and should thus be taken with caution.

resources that the IDA has available for its lending and the regional probability to receive IDA aid. We obtain the IDA's "funding position" from Dreher et al. (2020), defined by the World Bank as "the extent to which IDA can commit to new financing of loans, grants and guarantees given its financial position at any point in time and whether there are sufficient resources to meet undisbursed commitments of loans and grants" (IDA 2015: 24).[41] This indicator is disclosed in the World Bank's annual financial statements since 2008; we rely on Dreher et al.'s (2020) calculations for earlier years in our sample. In order to measure the availability of IBRD resources, Dreher et al. (2020) use its equity-to-loans ratio, which is reported in the IBRD's annual financial statements. The equity-to-loans ratio is a measure of the IBRD's "ability to issue loans without calling its callable capital" (Bulow 2002, p. 245). Our second instrument for World Bank projects is the interaction of IBRD liquidity with a region's probability to receive aid.

Note that it is not a priori obvious whether increases in liquidity increase World Bank lending at the intensive or extensive margin. While previous work has shown that an increase in resources leads to larger aid volumes for existing recipients of bilateral aid (Dreher and Langlotz 2020), Lang (2020) finds that the International Monetary Fund uses the availability of additional resources to extend its lending beyond its traditional recipients. This also holds true for the World Bank in Dreher et al. (2020).[42]

We replicate our main regressions for the World Bank in Table 4, focusing on the number of committed health projects in a particular cluster or country and year. As can be seen, results stand in some contrast to those obtained for China above. Column 1 shows that World Bank aid reduces rather than increases infant mortality at the local level, with a first-stage F-statistic above 17 indicating the power of the instruments.[43] When we turn to the country-level in column 2, the power of the instruments is considerably lower. While the coefficient of aid is positive and marginally significant in the full sample of countries with data available, the first-stage F-statistic is below two. When we restrict the sample to those countries included in the sub-national analysis, the coefficient of aid turns insignificant (though with a positive coefficient, see column 3). In column 4 we make use of the additional years that geocoded data are available for the World Bank—extending the sample to the 1997-2014 period—which leads to a considerably higher first-stage F-statistic. The coefficient switches its sign and is close in magnitude to those at sub-national scales, but imprecisely estimated. Either way, the negative coefficient at

[41]The approach of Dreher et al. (2020) follows Lang (2020), who suggests the IMF's liquidity ratio interacted with the probability of a country to be under an IMF program as an instrument for IMF loans.

[42]For approaches that mirror ours in instrumenting World Bank aid, see Gehring et al. (2019) and Jensen et al. (2019). See Figure A.2 in the Appendix for a graphical depiction of (potentially spurious) trends.

[43]Note however, that this result is less robust than the comparable result for China above. For example, when we lag aid by three rather than two years, the coefficient turns insignificant. The same holds when we focus on the extended sample starting in 1995, for aid lagged by two as well as by three years.

the local level in concert with a positive or insignificant coefficient at the country-level does not provide evidence in line with the fungibility-hypothesis.

The further columns of Table 4 replicate the regressions of Table 2 for World Bank health projects. The results show that there is no significant effect of aid on the number of births attended by skilled staff. In contrast, the number of births attended by traditional health staff decreases rather than increases as a consequence of aid. The availability of health facilities is not significantly affected by aid; however, uptake of public (but not private) hospitals increases with aid. There is no significant effect on women responding that they did not deliver children in a health clinic because it is too far, and World Bank health aid reduces rather than increases the probability that women took anti-malaria pills during pregnancy. There is no significant effect of aid on the number of hires at health clinics, though—in line with the result for China—average years of education of staff that has already been employed before the aid was committed declines. On balance, few of these results are as one would expect in support of the fungibility hypothesis.

# 5 Alternative Explanation and Tests for Robustness

In Table 5 we return to Chinese health projects and investigate an alternative explanation for our results. As one possibility, Chinese health projects might affect the composition of mothers in our sample. To the extent that aid increases the probability that families with an ex ante higher risk of infant mortality have more children, infant mortality would increase, on average. Columns 1-7 of Table 5 therefore test whether health projects affect the fertility of mothers differentially according to their education, age, and (minority or majority) ethnicity. We find that Chinese health aid does not significantly affect the fertility of either educated or uneducated mothers (defined as their highest educational level being above/below the location-specific median). On the contrary, we find that the age-composition of mothers is affected by aid: While fertility of young and older women (those below 20 years and above 29 years, respectively) remain unchanged, fertility by women aged between 20–29 years significantly increases. We also find that aid increases the fertility of women belonging to an ethnic minority while it decreases those of women belonging to their location's major ethnicity.[44] While aid thus seems to change the composition of mothers, potentially affecting infant mortality, on average, such changes in composition do not affect our results. As columns 8 and 9 show, results are hardly changed when we control for the number of children (multiplied by 1,000) born to mothers belonging to the ethnic minority or to mothers aged 20–29, respectively. We can thus rule out that changes in the composition of mothers in terms of education, age, or ethnicity are key drivers of our results.

[44] A location's major ethnicity is defined as the modal ethnicity of a DHS-cluster. On the political economy of health aid allocation on sub-national scales see Widmer and Zurlinden (2019).

We next test the timing after which aid commitments affect outcomes in some detail. We focus on the key regressions reported in columns 2–4 of Table 1, testing how aid affects infant mortality sub-nationally, at the country-level in the full sample, and at the country-level in the sample restricted to those countries that are also included in the sub-national analysis ("DHS countries"). To reduce clutter, we only report the coefficient and standard errors of the variable of interest, in concert with the number of countries and observations as well as first-stage F-statistics.

While the previous analysis considered the effect of aid two years after commitment, shorter or longer delays with which aid affects health-outcomes are well possible. Unfortunately, while we are confident that the exclusion restriction for our instrument holds with respect to variables unrelated to aid, the same does not hold for aid given from the same donor in different years. To some extent, aid is concentrated on the same regions for prolonged periods of time. Given that our instruments are also correlated over time (for example, the correlation of our instruments with their values one year earlier exceeds 0.8 for both of them), the effect of aid in any year that we detect with our instrumental variables strategy can partly also reflect the effect of aid in earlier or future years. To rule this out, we ran regressions that included up to four lags and—as placebo variables—two leads of the aid variable, instrumented with the respective lags of our instruments. Unfortunately, the power of the instruments is very low in these regressions, so we do not report them here.

Table 6 instead shows results of individual regressions, where we include one lag of aid at a time and instrument aid with our instrumental variables (lagged by one additional year). As can be seen, our previous results hold in the year of commitment already. The magnitude of the coefficients declines after the second year—corresponding to the lag structure in the main analyses of this paper. At sub-national scales, the effect of Chinese health aid turns insignificant commencing with the third lag. At the country-level, aid remains significant until the fourth lag in the smaller sample, and is significant for all lags in the larger sample. Given that aid projects and the instruments are correlated over time, and the lack of power in the first-stage does not allow us to include more than one aid variable at the same time, it is however not possible to differentiate the effects of different lags in a bullet-proof way. For what it is worth, the final rows of Table 6 report results for the (instrumented) second lag, where we include different leads and lags of aid without instrumenting for them. Our results are again highly similar.

We conclude this section by testing the robustness of our key results along various dimensions. First, we measure Chinese health aid as either (logged) commitment amounts or a binary project indicator rather than project numbers.[45] Second, we report results using our two instruments separately rather than jointly. Third, we average our data

[45]Note that we have added a value of one before taking logs in order to avoid losing values with zero aid.

over three-year periods. Fourth, we focus on sub-national clusters within a 111 km-radius rather than 55 km. Fifth, we control for all variables that we did also include in the basic regressions in column 1 of Table 1 above, but omitted from the instrumental variables regressions. Sixth, we examine—in three individual regressions for each test of robustness—whether our results hold when we control for one of three variables, each interacted with the probability to receive aid: world GDP growth, Chinese exports to a specific recipient country, and Chinese Foreign Direct Investment (FDI) to each recipient country. To the extent that our instruments are correlated with the world business cycle, Chinese exports or Chinese FDI, and at the same time regions that receive more aid are more likely to be affected by shocks related to the business cycle, trade, or FDI, our aid variable might reflect in part the effect of these variables rather than aid itself. Controlling for these variables tests this possibility. We also aggregate all data at the ADM2-level, testing whether aid given to ADM2 regions—which are geographic units of sizes in between the local and country areas we have so far focused on—affects infant mortality there. And finally, we test the effect of ODA and OOF (health-related) projects separately rather than including them in one variable.

Table 7 shows the results, again focusing on our three key specifications. As can be seen, our results are overall robust to these tests. Though some coefficients turn insignificant in some of the regressions (as one would expect when running a large number of them), all combinations of regressions are in line with fungibility. That is, in cases the local effect of aid is significant in increasing infant mortality, effects at the country-level are either negative and significant or insignificant. At the same time, when we do not find a consistent effect of aid at local scales, aid is effective in reducing mortality at the country-level. Most importantly, we find that the effect of aid on mortality persists when we control for other variables potentially correlated with parts of our instruments—the world business cycle, or Chinese exports and FDI. While this does not rule out that other omitted variables correlated with the input factors into aid projects or changes in China's foreign currency reserves differentially affect health outcomes in regions with different probabilities to receive aid, we do not expect such variables to be consequential for our results given that controlling for the most obvious candidates has little impact.

Interestingly, the effect of aid at the ADM2 level is insignificant, thus being in between the results we obtain at local and country levels. Finally, it seems that our results are driven by ODA—i.e., aid in the strict sense—rather than OOF. Note however that the bulk of projects in the health sector are coded as ODA so that the estimates for OOF rely on a small number of observations.[46]

[46] When we replicate these results for all OOF projects rather than just health-related ones the country-level results are significant at least at the ten-percent level, with negative coefficients.

# 6 Conclusions

Recent work in the aid effectiveness literature shifts the unit of analysis from the country to sub-national areas such as provinces or districts or even small grids within these areas. Although this has the advantage of greater statistical power and could facilitate the detection of local effects that are insufficiently strong to be recorded when countries are the unit of analysis, it can also make the aid seem less effective than it is. To the extent that aid is fungible within sectors, and donors finance projects that are less effective compared to government-funded projects they replace, aid can appear harmful at the local level.

In this paper, we have compared the effects of Chinese health aid at the local level with those at the country level. According to our results, China's projects increase infant mortality locally but reduce mortality at the level of countries. We explain this stark contrast with aid being fungible. In particular, we find that aid reduces the number of births attended by skilled staff, while it increases the number of births attended by traditional staff. Though the number of hospitals available is not affected by aid, aid reduces the number of deliveries in public hospitals by more than it does reduce them in private ones. At the same time, mothers are more likely to perceive hospitals as too distant to deliver there as a consequence of aid. We find staff turnover at hospitals to increase and average years of education of pre-existing staff to decline with aid. These findings are consistent with a setting where scarce domestic human capital is poached from existing providers of these services, which are more effective than those replacing them (Deserranno et al. 2020). We also find evidence in line with shifting priorities resulting from aid, documented by increases in the probability that women took anti-malaria pills during pregnancy as a consequence of aid (with the fight against malaria being a major goal of Chinese health operations).

These results bear important implications. First, we find that Chinese health aid, and its aid more broadly, reduce infant mortality overall. In line with several earlier papers (e.g., Dreher et al. 2020) this adds further evidence against the claim that Chinese aid is unequivocally harmful for its recipients ("Rogue Aid," for a discussion see, e.g., Isaksson and Kotsadam 2018). Second, our results show that China's aid does not bring its positive effects at those localities it was delivered to. Quite the contrary, Chinese aid seems to increase infant mortality in the vicinity of aid projects. To the extent that China aims to add to local development in the areas it nominally supports it needs to rethink its non-interference policy, changing its aid allocation process in a way that is more similar to those of the World Bank (for which we did not find evidence in line with the fungibility hypothesis here). And finally, our results should remind researchers that the benefits of sub-national analysis have to be weighed carefully against their drawbacks. Ideally, questions of aid effectiveness should thus be investigated at different levels of analysis, allowing for the possibility that fungibility at sub-national scales makes the aid appear more negative than it actually is.

# References

AidData (2017). *World Bank Geocoded Research Release Level1 v1.4.2 Geocoded Dataset.* Williamsburg, VA and Washington, DC: AidData.

Alesina, A. and D. Dollar (2000). Who Gives Foreign Aid to Whom and Why? *Journal of Economic Growth 5*(1), 33–63.

Anshan, L. (2011). Chinese Medical Cooperation in Africa: With Special Emphasis on the Medical Teams and Anti-Malaria Campaign. Discussion Paper 52, Nordiska Afrikainstitutet, Uppsala.

Asmus, G., V. Z. Eichenauer, A. Fuchs, and B. Parks (2020). China-India Aid Competition? An Analysis of Chinese and Indian Development Projects. Mimeo.

Bitzer, J. and E. Goeren (2018). Foreign Aid and Subnational Development: A Grid Cell Analysis. Working Papers V-407-18, University of Oldenburg, Department of Economics.

Bluhm, R., A. Dreher, A. Fuchs, B. C. Parks, A. Strange, and M. J. Tierney (2020). Connective Financing—Chinese Infrastructure Projects and the Diffusion of Economic Activity in Developing Countries. CEPR Discussion Paper 14818, Centre for Economic Policy Research, London.

Bluhm, R., M. Gassebner, S. Langlotz, and P. Schaudt (2018). Fueling Conflict? (De)Escalation and Bilateral Aid. HiCN Working Paper 265, Institute of Development Studies, University of Sussex.

Borusyak, K., P. Hull, and X. Jaravel (2018). Quasi-experimental Shift-Share Research Designs. Working Paper 24997, National Bureau of Economic Research.

Brazys, S. R. and K. C. Vadlamannati (2018). Aid Curse with Chinese Characteristics? Chinese Development Flows and Economic Reforms. GLOBUS Research Paper 5/2018, ARENA Centre for European Studies, University of Oslo.

Bruederle, A. and R. Hodler (2018). Nighttime Lights as a Proxy for Human Development at the Local Level. *PLOS ONE 13*(9), e0202231.

Bulow, J. (2002). First World Governments and Third World Debt. *Brookings Papers on Economic Activity 2002*(1), 229–255.

Bun, M. J. G. and T. D. Harrison (2018). OLS and IV Estimation of Regression Models Including Endogenous Interaction Terms. *Econometric Reviews 38*(7), 814–827.

Center for International Earth Science Information Network at Columbia University (2016). Gridded Population of the World, Version 4 (GPWv4): Population Count Adjusted to Match 2015 Revision of UN WPP Country Totals. Accessed August 21, 2017. http://dx.doi.org/10.7927/H4SF2T42.

Cervellati, M., E. Esposito, U. Sunde, and S. Yuan (2020). Malaria and Chinese Economic Activities in Africa. Mimeo.

Chauvet, L., F. Gubert, and S. Mesplé-Somps (2009). Are Remittances More Effective Than Aid to Improve Child Health? An Empirical Assessment Using Inter and Intra-Country Data. Document de Travail DT/2009-11, IRD DIAL, Paris.

Christian, P. J. and C. B. Barrett (2017). Revisiting the Effect of Food Aid on Conflict: A Methodological Caution. Policy Research Working Paper Series 8171, The World Bank.

Climate Hazards Group (2017). Climate Hazards Group InfraRed Precipitation with Station data 2.0. Accessed August 21, 2017. http://chg.geog.ucsb.edu/data/chirps/index.html.

De, R. and C. Becker (2015). The Foreign Aid Effectiveness Debate: Evidence from Malawi. AidData Working Paper 6, AidData at William & Mary, Williamsburg, VA.

Department of State's Office of the Geographer (2014). Department of State Large-Scale International Boundary (LSIB). Via ESRI. Accessed August 21, 2017. http://www.arcgis.com/home/item.html?id=3e650cfe52b84bffacb86d028f1f0514.

Deserranno, E., A. Nansamba, and N. Qian (2020). Aid Crowd-Out: The Effect of NGOs on

Government-Provided Public Services. NBER Working Paper 26928, National Bureau of Economic Research.

Didan, K. (2016). NASA MEaSUREs Vegetation Index and Phenology (VIP) Phenology EVI2 Yearly Global 0.05Deg CMG. Accessed August 21, 2017. https://doi.org/10.5067/measures/vip/vipphen_evi2.004.

Dietrich, S. (2011). The Politics of Public Health Aid: Why Corrupt Governments Have Incentives to Implement Aid Effectively. *World Development 39*(1), 55–63.

Dijkstra, G. and H. White (2003). *Programme Aid and Development: Beyond Conditionality.* London: Routledge.

Doucouliagos, H. (2019). The Politics of International Aid. In R. D. Congleton, B. Grofman, and S. Voigt (Eds.), *The Oxford Handbook of Public Choice, Volume 2*, pp. 696–724. Oxford University Press.

Doucouliagos, H., J. Hennessy, and D. Mallick (2019). Health Aid, Governance and Infant Mortality. IZA Discussion Papers 12166, Institute of Labor Economics (IZA).

Dreher, A. and A. Fuchs (2015). Rogue Aid? An Empirical Analysis of China's Aid Allocation. *Canadian Journal of Economics 48*(3), 988–1023.

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

Dreher, A., A. Fuchs, R. Hodler, B. C. Parks, P. A. Raschky, and M. J. Tierney (2019b). Is Favoritism a Threat to Chinese Aid Effectiveness? A Subnational Analysis of Chinese Development Projects. AidData Working Paper 83, AidData at William & Mary, Williamsburg, VA.

Dreher, A., A. Fuchs, B. C. Parks, A. Strange, and M. J. Tierney (2020). Aid, China, and Growth: Evidence from a New Global Development Finance Dataset.

Dreher, A., V. Lang, B. P. Rosendorff, and J. R. Vreeland (2018). Buying Votes and International Organizations: The Dirty Work-Hypothesis. CEPR Discussion Paper 13290, Centre for Economic Policy Research, London.

Dreher, A., V. F. Lang, and K. Richert (2019). The Political Economy of International Finance Corporation Lending. *Journal of Development Economics 140*, 242–254.

Dreher, A. and S. Langlotz (2020). Aid and Growth. New Evidence Using an Excludable Instrument. *Canadian Journal of Economics*, forthcoming.

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

Dreher, A., J.-E. Sturm, and J. R. Vreeland (2009). Development Aid and International Politics: Does Membership on the UN Security Council Influence World Bank Decisions? *Journal of Development Economics 88*(1), 1–18.

Dykstra, S., A. Glassman, C. Kenny, and J. Sandefur (2019). Regression Discontinuity Analysis of GAVI's Impact on Vaccination Rates. *Journal of Development Economics 140*, 12–25.

Earth Resources Observation and Science Center (1996). Global 30 arc-second elevation (gtopo30). Accessed August 21, 2017. https://lta.cr.usgs.gov/GTOPO30.

Frey, B. S. and F. Schneider (1986). Competing Models of International Lending Activity. *Journal of Development Economics 20*(2), 225–245.

Fuchs, A., S. Y. Kim, and A. M. Strange (2020). Competing for Aid Recipients? Donor Responses to Chinese Development Finance. Mimeo.

Galiani, S., S. Knack, L. C. Xu, and B. Zou (2016). The Effect of Aid on Growth: Evidence from a Quasi-Experiment. *Journal of Economic Growth 22*(1), 1–33.

Gehring, K., L. C. Kaplan, and M. H. Wong (2019). China and the World Bank: How Contrasting Development Approaches Affect the Stability of African States. AidData Working Paper 87, AidData at William & Mary, Williamsburg, VA.

Goldsmith-Pinkham, P., I. Sorkin, and H. Swift (2020). Bartik Instruments: What, When, Why, and How. *American Economic Review* (forthcoming).

Greßer, C. and D. Stadelmann (2019). Evaluating Water- and Health Related Development Projects: A Cross-Project and Micro-Based Approach. CREMA Working Paper Series 2019-06, Center for Research in Economics, Management and the Arts (CREMA).

Hernandez, D. (2017). Are "New" Donors Challenging World Bank Conditionality? *World Development 96*, 529–549.

Hodler, R. and P. A. Raschky (2014). Regional Favoritism. *The Quarterly Journal of Economics 129*(2), 995–1033.

Humphrey, C. and K. Michaelowa (2019). China in Africa: Competition for Traditional Development Finance Institutions? *World Development 120*, 15–28.

ICF (2004-2017). Demographic and Health Surveys (various) [Datasets]. Funded by USAID. Rockville, Maryland: ICF [Distributor].

ICF (2013). *Incorporating Geographic Information into Demographic and Health Surveys: A Field Guide to GPS Data Collection*. Calverton, MD: ICF International.

Ioannidis, J. P. A., T. D. Stanley, and H. Doucouliagos (2017). The Power of Bias in Economics Research. *The Economic Journal 127*(605), F236–F265.

Isaksson, A.-S. and A. Kotsadam (2018). Chinese Aid and Local Corruption. *Journal of Public Economics 159*, 146–159.

Jensen, N. M., N. P. Johnston, C. Lee, and H. Sahin (2019). Crisis and Contract Breach: The Domestic and International Determinants of Expropriation. *Review of International Organizations*, forthcoming.

Kaplan, L. C., J. Kuhnt, K. Richert, and S. Vollmer (2019). What Explains the Uptake of Development Interventions? Considering the Theory of Planned Behaviour. Discussion Paper 261, Georg-August-Universität Göttingen, Courant Research Centre—Poverty, Equity and Growth (CRC-PEG).

Kotsadam, A., G. Østby, S. A. Rustad, A. F. Tollefsen, and H. Urdal (2018). Development Aid and Infant Mortality. Micro-Level Evidence from Nigeria. *World Development 105*, 59–69.

Kotsadam, A. and A. Tolonen (2016). African Mining, Gender, and Local Employment. *World Development 83*, 325–339.

Lang, V. F. (2020). The Economics of the Democratic Deficit: The Effect of IMF Programs on Inequality. Mimeo, University of Zurich.

Langlotz, S. and N. Potrafke (2019). Does Development Aid Increase Military Expenditure? *Journal of Comparative Economics 47*(3), 735–757.

Leiderer, S. (2012). Fungibility and the Choice of Aid Modalities: The Red Herring Revisited. Working Paper 2012/68, UNU-WIDER.

Maina, J., P. O. Ouma, P. M. Macharia, V. A. Alegana, B. Mitto, I. S. Fall, A. M. Noor, R. W. Snow, and E. A. Okiro (2019). A Spatial Database of Health Facilities Managed by the Public Health Sector in Sub Saharan Africa. *Scientific Data 6*(1).

Martorano, B., L. Metzger, and M. Sanfilippo (2020). Chinese Development Assistance and Household Welfare in Sub-Saharan Africa. *World Development*, forthcoming.

Marty, R., C. B. Dolan, M. Leu, and D. Runfola (2017). Taking the Health Aid Debate to the Subnational Level: The Impact and Allocation of Foreign Health Aid in Malawi. *BMJ Global Health 2*(1), e000129.

Mascarenhas, R. and T. Sandler (2006). Do Donors Cooperatively Fund Foreign Aid? *Review of International Organizations 1*(4), 337–357.

Maseland, R. and A. Minasyan (2019). Does Chinese Aid Undermine Good Governance? University of Groningen, mimeo.

Mayala, B., T. D. Fish, D. Eitelberg, and T. Dontamsetti (2018). *The DHS Program Geospatial Covariate Datasets Manual (Second Edition)*. Rockville, MD: ICF International.

McCoy, D., S. Bennett, S. Witter, B. Pond, B. Baker, J. Gow, S. Chand, T. Ensor, and B. McPake (2008). Salaries and Incomes of Health Workers in Sub-Saharan Africa. *The Lancet 371*(9613), 675–681.

National Bureau of Statistics of China (NBSC) (2019). Accessed October 12, 2019. http://www.stats.gov.cn/english/statisticaldata/yearlydata/YB1999e/m12e.htm.

National Centers for Environmental Information (2015). 2015 VIIRS Nighttime Lights Annual Composite. Accessed August 21, 2017. https://ngdc.noaa.gov/eog/viirs/download_dnb_composites.html.

Nelson, A. (2008). Estimated Travel Time to the Nearest City of 50,000 or More People in year 2000. Accessed August 21, 2017. http://forobs.jrc.ec.europa.eu/products/gam/.

Nizalova, O. Y. and I. Murtazashvili (2016). Exogenous Treatment and Endogenous Factors: Vanishing of Omitted Variable Bias on the Interaction Term. *Journal of Econometric Methods 5*(1).

Nunn, N. and N. Qian (2014). US Food Aid and Civil Conflict. *American Economic Review 104*(6), 1630–1666.

Nunnenkamp, P. and H. Öhler (2011). Throwing Foreign Aid at HIV/AIDS in Developing Countries: Missing the Target? *World Development 39*(10), 1704–1723.

Odokonyero, T., R. Marty, T. Muhumuza, A. T. Ijjo, and G. O. Moses (2018). The Impact of Aid on Health Outcomes in Uganda. *Health Economics 27*(4), 733–745.

OECD (2017). Creditor Reporting System. Accessed March 15, 2017. https://stats.oecd.org/DownloadFiles.aspx?DatasetCode=CRS1.

Pesaresi, M., D. Ehrlich, A. Florczyk, S. Freire, A. Julea, T. Kemper, P. Soille, and V. Syrris (2015). GHS built-up grid, derived from Landsat, multitemporal (1975, 1990, 2000, 2014). Accessed August 21, 2017. http://data.europa.eu/89h/jrc-ghsl-ghs_built_ldsmt_globe_r2015b.

Pesaresi, M. and S. Freire (2016). GHS Settlement grid following the REGIO model 2014 in application to GHSL Landsat and CIESIN GPW v4-multitemporal (1975-1990- 2000-2015). Accessed August 21, 2017. http://data.europa.eu/89h/jrc-ghslghs_smod_pop_globe_r2016a.

Sonntag, D. (2010). *AIDS and Aid: A Public Good Approach*. Springer Science Business Media.

Temple, J. and N. Van de Sijpe (2017). Foreign Aid and Domestic Absorption. *Journal of International Economics 108*, 431–443.

UNEP-WCMC and IUCN (2017). Protected Planet: The World Database on Protected Areas (WDPA). Accessed August 21, 2017. www.protectedplanet.net.

United Nations Conference on Trade and Development (UNCTAD) (2017). World Investment Report 2017: Investment and the Digital Economy. United Nations, Geneva.

United States Geological Survey (USGS) (2019). Aluminum statistics and information. Accessed October 12, 2019. https://www.usgs.gov/centers/nmic/aluminum-statistics-and-information.

Van de Sijpe, N. (2013). The Fungibility of Health Aid Reconsidered. *Journal of Development Studies 49*(12), 1746–1754.

Van de Walle, D. and R. Mu (2007). Fungibility and the Flypaper Effect of Project Aid: Micro-Evidence for Vietnam. *Journal of Development Economics 84*(2), 667–685.

Wayoro, D. and L. Ndikumana (2019). Impact of Development Aid on Infant Mortality: Micro-Level Evidence from Cote d'Ivoire. UMASS Amherst Economics Working Papers 2019-07, University of Massachusetts Amherst, Department of Economics.

Werker, E., F. Z. Ahmed, and C. Cohen (2009). How Is Foreign Aid Spent? Evidence from a Natural Experiment. *American Economic Journal: Macroeconomics 1*(2), 225–244.

Wessel, P. and W. Smith (2017). A Global Self-consistent, Hierarchical, High-resolution Geography Database Version 2.3.7. Accessed August 21, 2017. http://www.soest.hawaii.edu/pwessel/gshhg/.

Wessel, P. and W. H. F. Smith (1996). A global, self-consistent, hierarchical, high-resolution shoreline database. *Journal of Geophysical Research 101*, 8741–8743.

Widmer, P. and N. Zurlinden (2019). Born in the Right Place? Health Ministers, Foreign Aid and Infant Mortality. Economics Working Paper Series 1911, University of St. Gallen.

Williamson, C. R. (2008). Foreign Aid and Human Development: The Impact of Foreign Aid to the Health Sector. *Southern Economic Journal 75*(1), 188–207.

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

World Bank (2020). World development indicators. Accessed February 15, 2020. https://datacatalog.worldbank.org/dataset/world-development-indicators.

Zeitz, A. (2020). Emulation or Differentiation? China's Development Finance and Traditional Donor Aid in Developing Countries. *Review of International Organizations*, forthcoming.

**Figure 1** – Testing Spurious Trends: China


*Notes:* The first panel shows the first principal factor of China's (logged and detrended) production of aluminum (in 10,000 tons), cement (in 10,000 tons), glass (in 10,000 weight cases), iron (in 10,000 tons), steel (in 10,000 tons), and timber (in 10,000 cubic meters) over time. It also shows the (detrended) change in net foreign exchange reserves (in trillions of constant 2010 US dollars). The second panel shows the average number of Chinese health projects within the group that is below the median of the probability of receiving projects and the group that is above the median (conditional on receiving any project). The lower panel shows the infant mortality rate within these two groups.

**Table 1** – Chinese Aid and Infant Mortality, 2002-2014

| | *Health Aid* | | | | *All Aid* | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | *Panel A: OLS* | | | | | | | |
| Chinese projects (t-2) | | 2.205** | -0.331 | -0.016 | | 0.078 | -0.328** | -0.222 |
| | | (0.883) | (0.218) | (0.194) | | (0.312) | (0.130) | (0.135) |
| China active | -4.869 | | | | 0.303 | | | |
| | (3.241) | | | | (1.547) | | | |
| China inactive | -6.043** | | | | -0.288 | | | |
| | (2.850) | | | | (1.443) | | | |
| | *Panel B: Second Stage* | | | | | | | |
| Chinese projects (t-2) | | 9.040** | -15.219*** | -10.284* | | 2.139 | -5.722*** | -7.197** |
| | | (4.039) | (5.051) | (5.559) | | (1.319) | (1.453) | (2.732) |
| | *Panel C: First Stage* | | | | | | | |
| Input (t-3) x Prob. | | -0.032 | 0.395 | 0.602 | | 0.973*** | 0.082 | -0.143 |
| | | (0.178) | (0.332) | (0.517) | | (0.153) | (0.288) | (0.487) |
| Reserves (t-3) x Prob. | | 7.957*** | 0.311 | -0.982 | | 2.859*** | 4.076* | 5.692 |
| | | (1.245) | (2.479) | (4.219) | | (1.082) | (2.132) | (4.017) |
| Number of countries | 53 | 52 | 151 | 52 | 53 | 52 | 151 | 52 |
| Number of observations | 457,419 | 363,932 | 1,713 | 525 | 457,419 | 363,932 | 1,713 | 525 |
| Sample | local | local | all countries | DHS countries | local | local | all countries | DHS countries |
| China: Difference active-inactive | 1.174 | | | | 0.591 | | | |
| China: active = inactive (p-value) | 0.672 | | | | 0.578 | | | |
| Adjusted R-squared | 0.035 | 0.05 | 0.95 | 0.94 | 0.035 | 0.05 | 0.95 | 0.85 |
| Kleibergen-Paap F | | 91.79 | 3.49 | 11.89 | | 186.85 | 17.31 | 5.81 |

*Notes:* All regressions in Panels B and C control for (log) population. Columns 1, 2 and 5, 6 include country-year fixed effects, the probability to receive aid, and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. Columns 3, 4 and 7, 8 include country and year fixed effects. Standard errors are clustered at the country level. DHS countries are those that are also included in the local analyses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

**Table 2** – Transmission Channels at the Local Level

| | (1)<br>Assisted births<br>skilled | (2)<br>Assisted births<br>traditional | (3)<br>Public facility<br>availability | (4)<br>Private facility<br>availability | (5)<br>Public hospital<br>uptake | (6)<br>Private hospital<br>uptake | (7)<br>Hospital<br>too far | (8)<br>Antimalarial<br>medication | (9)<br>Staff<br>hires | (10)<br>Staff<br>education |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | *Panel A: OLS* | | | | | |
| Chinese projects (t-2) | 4.362 | 0.442 | 18.433** | 1.374* | 2.286 | -2.159 | -0.008 | 2.920*** | 0.034 | -0.133*** |
| | (3.351) | (2.654) | (8.794) | (0.759) | (3.291) | (2.373) | (0.012) | (0.950) | (0.024) | (0.037) |
| | | | | | *Panel B: Second Stage* | | | | | |
| Chinese projects (t-2) | -69.416*** | 132.852*** | 58.448 | 2.193 | -72.770*** | -22.855*** | 0.064* | 10.557*** | 0.372*** | -1.115*** |
| | (11.236) | (16.699) | (56.973) | (1.975) | (13.769) | (8.778) | (0.038) | (3.191) | (0.093) | (0.210) |
| | | | | | *Panel C: First Stage* | | | | | |
| Factor (t-3) x Prob. | -0.031 | 0.943*** | 4.251*** | 4.251*** | -0.054 | -0.054 | 2.196*** | -0.099 | -0.059 | 0.045 |
| | (0.294) | (0.155) | (1.000) | (1.000) | (0.282) | (0.282) | (0.425) | (0.183) | (0.118) | (0.142) |
| Reserves (t-3) x Prob. | 9.450*** | 1.059** | 7.488 | 7.489 | 9.473*** | 9.473*** | -18.643*** | 8.003*** | 3.808*** | 3.027*** |
| | (1.908) | (0.440) | (6.325) | (6.325) | (1.839) | (1.839) | (3.065) | (1.304) | (0.859) | (1.032) |
| Number of countries | 51 | 51 | 27 | 27 | 51 | 51 | 25 | 33 | 7 | 7 |
| Number of observations | 190,223 | 199,594 | 9,798 | 9,798 | 206,346 | 206,346 | 34,634 | 232,653 | 100,770 | 70,369 |
| Adjusted R-squared | 0.56 | 0.46 | 0.96 | 0.85 | 0.43 | 0.36 | 0.38 | 0.38 | 0.22 | 0.99 |
| Kleibergen-Paap F | 74.46 | 56.54 | 9.53 | 9.53 | 76.14 | 76.14 | 19.39 | 80.37 | 83.64 | 49.65 |

*Notes:* All columns include (log) population, the probability to receive aid, country-year fixed effects, and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

**Table 3** – Chinese Health Aid and Other Resources

| | (1)<br>Assisted births skilled | (2)<br>Assisted births skilled | (3)<br>World Bank projects | (4)<br>World Bank projects |
|---|---|---|---|---|
| Chinese projects (t-2) | -23.436 | -40.446*** | -0.170 | |
| | (19.585) | (13.824) | (0.127) | |
| Health exp./Gov. exp. x Chinese projects | -5.878*** | | | |
| | (2.151) | | | |
| Other aid/GNI x Chinese projects | | -928.447* | | |
| | | (484.971) | | |
| Chinese projects (t-1) | | | | -0.419** |
| | | | | (0.171) |
| Number of countries | 49 | 51 | 52 | 52 |
| Number of observations | 172,053 | 190,223 | 363,930 | 406,678 |
| Adjusted R-squared | 0.57 | 0.55 | 0.62 | 0.55 |
| Kleibergen-Paap F | 21.89 | 24.68 | 69.23 | 70.23 |

*Notes:* All columns include (log) population, the probability to receive aid, country-year fixed effects, and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

**Table 4** – World Bank Aid and Health Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Infant mortality | Infant mortality | Infant mortality | Infant mortality | Assisted births skilled | Assisted births traditional | Public facility availability |
| | | | | *Panel A: Second Stage* | | | |
| WB projects (t-2) | -8.450** | 30.640* | 9.101 | -10.963 | 7.077 | -103.597*** | 36.987 |
| | (4.180) | (18.431) | (10.319) | (10.235) | (18.529) | (38.537) | (26.228) |
| | | | | *Panel B: First Stage* | | | |
| IDA (t-3) x Prob. | 0.175*** | 0.028* | 0.104*** | -0.029*** | 0.155*** | 0.138*** | 0.557* |
| | (0.050) | (0.016) | (0.032) | (0.010) | (0.050) | (0.045) | (0.290) |
| IBRD (t-3) x Prob. | -0.625*** | 0.066 | 0.179 | 0.109** | 0.070 | 0.055 | . |
| | (0.131) | (0.068) | (0.119) | (0.043) | (0.089) | (0.096) | . |
| Number of countries | 52 | 151 | 52 | 53 | 51 | 51 | 27 |
| Number of observations | 363,932 | 1,713 | 525 | 846 | 190,223 | 199,594 | 9,798 |
| Sample | local | all countries | DHS countries | DHS countries | local | local | local |
| Period | 2002-2014 | 2002-2014 | 2002-2014 | 1997-2014 | 2002-2014 | 2002-2014 | 2002-2014 |
| Adjusted R-squared | 0.05 | 0.92 | 0.97 | 0.91 | 0.56 | 0.43 | 0.95 |
| Kleibergen-Paap F | 17.47 | 1.82 | 5.23 | 8.01 | 5.18 | 4.98 | 3.69 |

continued on next page

**Table 4 (Continued)**

| | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
|---|---|---|---|---|---|---|---|
| | Private facility availability | Public hospital uptake | Private hospital uptake | Hospital too far | Antimalarial medication | Staff hires | Staff education |
| | | | | *Panel A: Second Stage* | | | |
| WB projects (t-2) | -0.406 | 95.302** | 2.244 | -0.027 | -15.721*** | 0.180 | -2.359*** |
| | (1.643) | (39.691) | (20.500) | (0.079) | (5.080) | (0.282) | (0.774) |
| | | | | *Panel B: First Stage* | | | |
| IDA (t-3) x Prob. | 0.557* | 0.138*** | 0.138*** | 0.108*** | 0.211*** | -0.026*** | -0.033*** |
| | (0.290) | (0.045) | (0.045) | (0.033) | (0.057) | (0.007) | (0.008) |
| IBRD (t-3) x Prob. | . | 0.045 | 0.045 | 0.753 | 0.250** | . | . |
| | . | (0.089) | (0.089) | (0.662) | (0.108) | . | . |
| Number of countries | 27 | 51 | 51 | 25 | 33 | 7 | 7 |
| Number of observations | 9,798 | 206,346 | 206,346 | 34,634 | 232,653 | 100,770 | 70,369 |
| Sample | local | local | local | local | local | local | local |
| Period | 2002-2014 | 2002-2014 | 2002-2014 | 2002-2014 | 2002-2014 | 2002-2014 | 2002-2014 |
| Adjusted R-squared | 0.87 | 0.43 | 0.38 | 0.43 | 0.38 | 0.19 | 0.94 |
| Kleibergen-Paap F | 3.69 | 4.95 | 4.95 | 5.99 | 9.65 | 13.97 | 17.74 |

*Notes:* All regressions control for (log) population; the local-level regressions control for the probability to receive aid. Columns 1 and 5-12 include country-year fixed effects and and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. Columns 2, 3 and 4 include country and year fixed effects, with standard errors clustered at the country level. Columns 13 and 14 include facility and country-year fixed effects. DHS countries are those that are also included in the local analyses. Columns 7, 8, 13, and 14 include no IBRD projects; the equity-to-loans ratio-IV is consequently omitted as instrument. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

**Table 5** – Composition of Mothers

| | (1) Fertility uneducated | (2) Fertility educated | (3) Fertility young | (4) Fertility middle | (5) Fertility older | (6) Fertility ethnic minority | (7) Fertility ethnic majority | (8) Infant mortality | (9) Infant mortality |
|---|---|---|---|---|---|---|---|---|---|
| Chinese projects (t-2) | -0.002 | 0.006 | -22.758 | 89.584* | -25.991 | 282.296*** | -286.823*** | 8.619** | 9.678* |
| | (0.020) | (0.064) | (21.568) | (46.246) | (33.239) | (54.653) | (91.934) | (4.115) | (5.157) |
| Children middle-aged mothers | | | | | | | | 0.005*** | |
| | | | | | | | | (0.000) | |
| Children ethnic minority mothers | | | | | | | | | 0.001*** |
| | | | | | | | | | (0.000) |
| Number of countries | 52 | 52 | 52 | 52 | 52 | 29 | 29 | 52 | 29 |
| Number of observations | 361,750 | 361,750 | 361,750 | 361,750 | 361,750 | 206,424 | 206,424 | 361,750 | 206,424 |
| Adjusted R-squared | 0.17 | 0.44 | 0.22 | 0.28 | 0.27 | 0.33 | 0.44 | 0.05 | 0.04 |
| Kleibergen-Paap F | 86.93 | 86.93 | 86.93 | 86.93 | 86.93 | 46.91 | 46.91 | 86.94 | 46.93 |

*Notes:* All regressions control for (log) population and include country-year fixed effects, the probability to receive aid, and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. DHS countries are those that are also included in the local analyses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

**Table 6** – Chinese Aid and Infant Mortality, Different Lags

| | (1) Local | (2) All countries | (3) DHS countries |
|---|---|---|---|
| Chinese projects (t) | 10.719** | -15.287*** | -12.566* |
| | (4.949) | (5.227) | (6.444) |
| | 72.95/53/454,785 | 13.70/154/2,026 | 10.15/53/632 |
| Chinese projects (t-1) | 9.954** | -14.392*** | -11.261* |
| | (4.245) | (4.539) | (5.863) |
| | 81.69/52/406,680 | 4.19/152/1,866 | 14.58/53/579 |
| Chinese projects (t-2) | 9.040** | -15.219*** | -10.284* |
| | (4.039) | (5.051) | (5.559) |
| | 91.79/52/363,932 | 3.48/151/1,713 | 11.89/52/525 |
| Chinese projects (t-3) | 3.248 | -14.387*** | -9.197* |
| | (4.631) | (5.202) | (5.353) |
| | 75.42/52/319,260 | 3.03/151/1,563 | 10.84/51/472 |
| Chinese projects (t-4) | 3.352 | -12.593*** | -7.989* |
| | (5.375) | (4.539) | (4.557) |
| | 47.20/51/275,735 | 2.92/148/1,409 | 8.04/50/420 |
| Chinese projects (t-5) | 1.862 | -9.521*** | -5.094 |
| | (6.932) | (3.546) | (3.898) |
| | 30.86/50/234,617 | 3.58/144/1,257 | 9.89/50/370 |
| Chinese projects (t-6) | 12.656 | -8.240** | -2.941 |
| | (10.872) | (3.304) | (2.646) |
| | 26.43/50/195,544 | 3.10/143/1,112 | 8.03/48/318 |
| | *Controlling for projects in t+1, t, t-1, t-3* | | |
| Chinese projects (t-2) | 10.701*** | -9.539*** | -5.477 |
| | (4.104) | (3.619) | (3.366) |
| | 67.86/51/225,111 | 2.57/151/1,565 | 8.97/51/472 |
| | *Controlling for projects in t, t-1* | | |
| Chinese projects (t-2) | 10.391*** | -12.207*** | -7.934* |
| | (3.744) | (4.153) | (4.399) |
| | 83.31/52/332,695 | 3.04/151/1,713 | 11.66/52/525 |

*Notes:* All regressions control for (log) population. All specifications in column 1 include country-year fixed effects, the probability to receive aid, and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. All specifications in columns 2 and 3 include country and year fixed effects, with standard errors clustered at the country level. The last row of each specification reports the following: Kleibergen-Paap F-statistic/number of countries/number of observations. DHS countries are those that are also included in the local analyses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

**Table 7** – Tests for Robustness

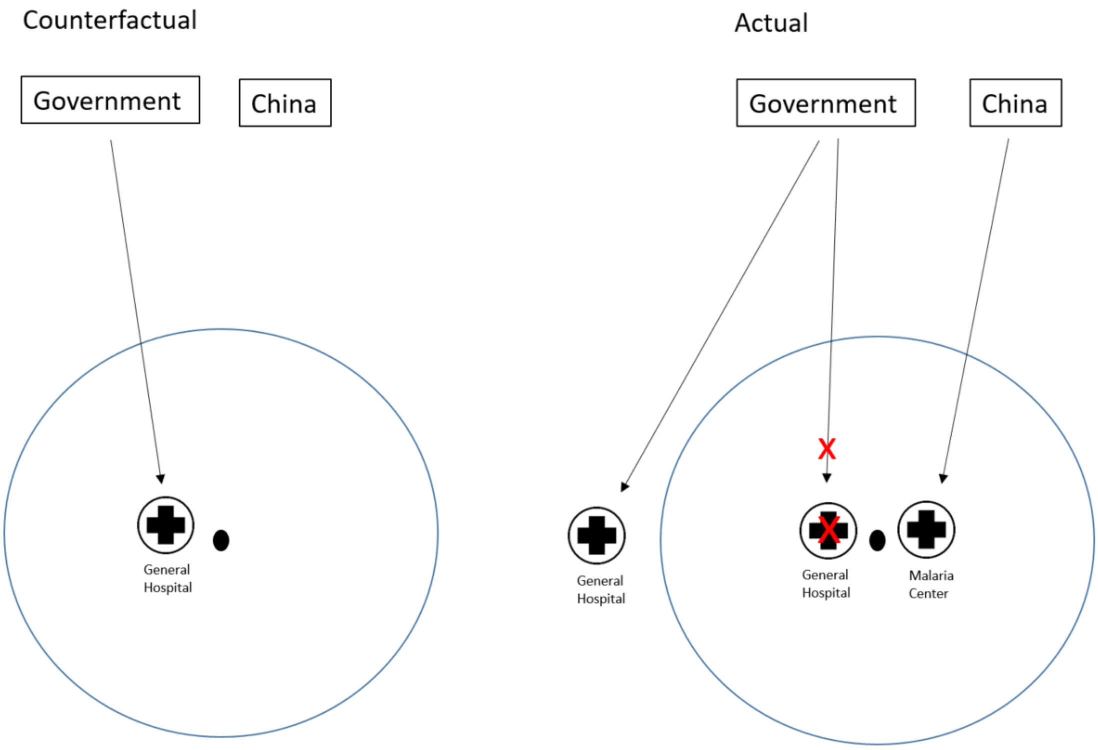
| | (1)<br>Local | (2)<br>All countries | (3)<br>DHS countries |
|---|---|---|---|
| (log) Commitment amounts | 1.486* | -0.612*** | -0.4089 |
| | (0.795) | (0.232) | (0.315) |
| | 76.44/52/363,932 | 6.42/151/1,713 | 9.02/52/525 |
| Health Aid (binary) | 15.218** | -17.302*** | -12.389* |
| | (7.269) | (4.989) | (6.463) |
| | 129.37/52/363,932 | 5.65/151/1,713 | 8.65/52/525 |
| Input interaction IV | 6.159 | -14.949*** | -10.674* |
| | (4.693) | (4.864) | (6.076) |
| | 156.34/52/363,932 | 6.48/151/1,713 | 19.98/52/525 |
| Reserves interaction IV | 8.968** | -17.646** | -12.238 |
| | (4.044) | (7.365) | (8.851) |
| | 183.37/52/363,932 | 4.54/151/1,713 | 6.86/52/525 |
| Averages, three years | 8.761 | -12.279*** | -8.416* |
| | (6.837) | (3.974) | (4.941) |
| | 108.25/50/189,557 | 4.43/148/1,408 | 11.09/50/420 |
| 111 km | 8.668** | | |
| | (3.380) | | |
| | 174.51/52/363,932 | | |
| Full set of controls | 8.989** | | |
| | (4.126) | | |
| | 87.79/52/357,054 | | |
| World GDP growth (t-3) x Prob. | 10.990*** | -15.772*** | -10.852* |
| | (3.167) | (5.414) | (6.026) |
| | 92.99/52/363,932 | 3.40/151/1,713 | 11.27/52/525 |
| Chinese exports (t-3) x Prob. | 23.874* | -6.302*** | -7.852** |
| | (13.851) | (2.220) | (3.697) |
| | 7.67/52/363,932 | 3.16/151/1,713 | 7.59/52/525 |
| Chinese FDI (t-3) x Prob. | 13.181** | -9.127*** | -8.049** |
| | (6.309) | (2.842) | (3.890) |
| | 28.94/52/363,932 | 3.86/151/1,713 | 4.58/52/525 |
| ADM2 | 19.528 | | |
| | (15.144) | | |
| | 14.96/52/61,592 | | |
| ODA projects | 9.537** | -15.749*** | -9.935* |
| | (4.564) | (5.749) | (5.821) |
| | 73.13/52/363,932 | 2.86/151/1,713 | 7.36/52/525 |
| OOF projects | -28.436 | 3.750 | 2.594 |
| | (25.901) | (2.484) | (1.835) |
| | 8.73/52/363,932 | 6.67/151/1,713 | 5.81/52/525 |

*Notes:* All regressions control for (log) population. All specifications in column 1 include country-year fixed effects, the probability to receive aid, and ADM2 fixed effects. Standard errors are clustered at the ADM2-year level and reported in parentheses. All specifications in columns 2 and 3 include country and year fixed effects, with standard errors clustered at the country level. The last row of each specification reports the following: Kleibergen-Paap F-statistic/number of countries/number of observations. DHS countries are those that are also included in the local analyses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

# Appendix

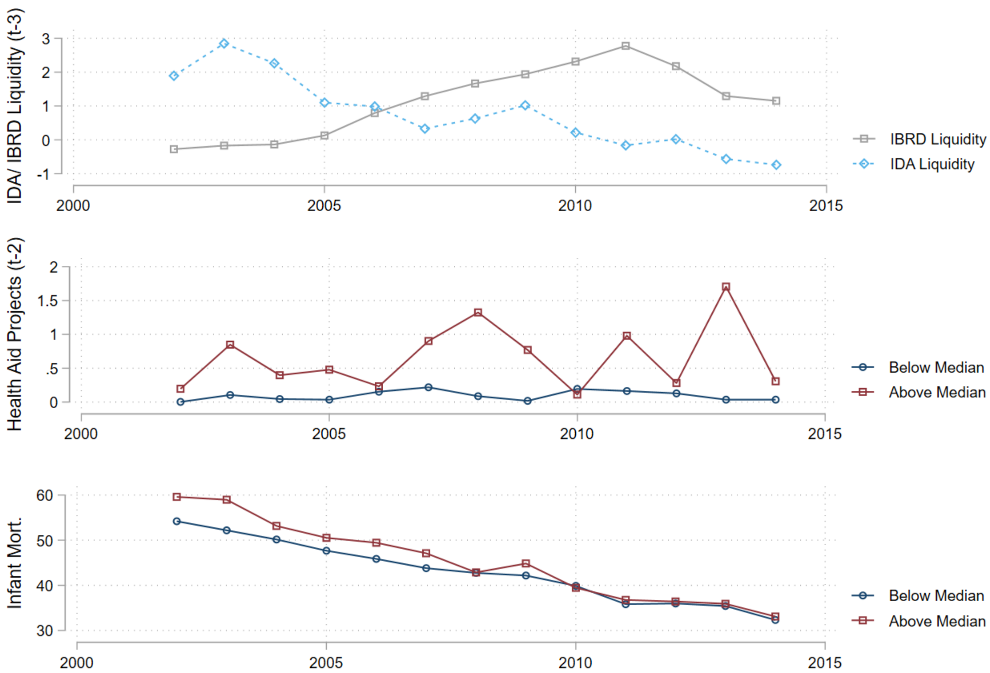
## A Additional Figures

**Figure A.1** – The Local Effect of Aid on Infant Mortality with Fungibility




*Notes:* The figure illustrates fungibility. The figure on the left shows what would have happened without the Chinese health project. The figure on the right shows what happens with China implementing a health project. As can be seen in the figure, the government shifts its resources to another location in order to not spend resources on similar projects in the same location in response to the Chinese health project. Thus, the exemplary location receives a Chinese-funded project instead of a government-financed one. However, since China focuses more on Malaria treatment, the focus of the health project shifted from general to Malaria which can render the effect on infant mortality negative compared to the counterfactual.

**Figure A.2** – Testing Spurious Trends: World Bank




*Notes:* The first panel shows the IBRD's equity-to-loans ratio and the IDA's funding position over time. The second panel shows the average number of World Bank health projects within the group that is below the median of the probability of receiving projects and the group that is above the median (conditional on receiving any project). The lower panel shows the infant mortality rate within these two groups.

# B Sources and Definitions

## Local Level

| Variable | Definition | Data Source |
|---|---|---|
| Infant Mortality | Number of children that did not survive the first year out of 1,000 live births in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Chinese health projects | Total number of Chinese health-related projects committed within 55km of a DHS cluster. | Bluhm et al. (2020) |
| Chinese projects, all | Total number of Chinese aid projects committed within 55km of a DHS cluster. | Bluhm et al. (2020) |
| Chinese health commitments | Total amount of Chinese health-related projects committed within 55km of a DHS cluster. | Bluhm et al. (2020) |
| Chinese health projects, dummy | Dummy = 1 if at least one Chinese health-related project is committed within 55km of a DHS cluster. | Bluhm et al. (2020) |
| Chinese health projects (ODA) | Total number of Chinese health-related Official Development Assistance projects committed within 55km of a DHS cluster. | Bluhm et al. (2020) |
| Chinese health projects (OOF) | Total number of Chinese health-related Other Official Finance projects committed within 55km of a DHS cluster. | Bluhm et al. (2020) |
| World Bank health projects | Total number of World Bank health-related projects committed within 55km of a DHS cluster. | AidData (2017) |
| Reserves | China's yearly net currency reserves. | World Bank (2020) |
| Inputs | China's detrended first factor of yearly production of steel, timber, glass, aluminum, and cement. | National Bureau of Statistics of China (NBSC 2019), United States Geological Survey (USGS 2019) |
| Probability | Share of years (2000-2014) that at least one Chinese health related project is committed within 55km of a DHS cluster. | Own construction based on Bluhm et al. (2020) |
| Probability (ODA) | Share of years (2000-2014) that at least one Chinese health related ODA-project is committed within 55km of a DHS cluster. | Own construction based on Bluhm et al. (2020) |
| Probability (OOF) | Share of years (2000-2014) that at least one Chinese health related OOF-project is committed within 55km of a DHS cluster. | Own construction based on Bluhm et al. (2020) |
| Probability (amounts) | Share of years (2000-2014) that at least one Chinese health related project with known amounts is committed within 55km of a DHS cluster. | Own construction based on Bluhm et al. (2020) |
| Population (log) | (log) number of people within the 2 km (urban) or 10 km (rural) buffer surrounding a DHS survey cluster location. | Center for International Earth Science Information Network at Columbia University (2016) |
| Distance to National Borders | Straight-line distance to the nearest international border. | Department of State's Office of the Geographer (2014) |
| Distance to Protected Areas | Straight-line distance to the nearest protected area. | UNEP-WCMC and IUCN (2017) |
| Distance to Water | Straight-line distance to the nearest major water body. | Wessel and Smith (1996, 2017) |
| Travel Time | The average travel time of the cells whose centroid falls within a radius of 10 km (for rural points) or 2 km (for urban points) to reach a settlement of 50,000 or more people in the year 2000. | Nelson (2008) |
| Slope | The average slope of the cells whose centroid falls within a radius of 10 km (for rural points) or 2 km (for urban points). | Earth Resources Observation and Science Center (1996) |

continued on next page

## Local Level (continued)

| Variable | Definition | Data Source |
|---|---|---|
| Nightlights | The yearly average nighttime luminosity of the area within the 2 km (urban) or 10 km (rural) buffer surrounding the DHS survey cluster location. | National Centers for Environmental Information (2015) |
| Rainfall | The yearly average rainfall of the cells whose centroid falls within a radius of 10 km (for rural points) or 2 km (for urban points). | Climate Hazards Group (2017) |
| SMOD Population | Divides classes in 1: rural cells, 2: urban clusters, and 3: urban centres, and take the mode of the Settlement Model (SMOD) of the cells whose centroid falls within a radius of 10 km (for rural points) or 2 km (for urban points). | Pesaresi and Freire (2016) |
| Vegetation Index | Yearly vegetation index value between 0 (least vegetation) and 10,000 (Most vegetation). Spatial resolution at 0.05 degrees at the equator. | Didan (2016) |
| Built Population | Built-up index between 0.00 (extremely rural) and 1.00 (extremely urban). Constructed as the average built-up index of the cells whose centroid falls within a radius of 10 km (for rural points) or 2 km (for urban points). | Pesaresi et al. (2015) |
| Leader Birth Region | Dummy = 1 if the birth region of the country's head of government is within 55km of a DHS cluster. | Hodler and Raschky (2014) |
| World GDP Growth | Global yearly GDP growth. | World Bank (2020) |
| Chinese FDI | Outflow of Chinese foreign direct investments to a recipient country. | UNCTAD (2017) |
| Chinese Exports | Chinese exports to a recipient country. | World Bank (2020) |
| Assisted births skilled | The number of deliveries with a skilled health worker present out of 1,000 live births in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Assisted births traditional | The number of deliveries with a traditional health worker present out of 1,000 live births in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Staff hires | The number of new staff employed at a health facility in a year. | Service Provision Assessments, DHS (ICF, 2004-2017) |
| Staff education | The average years of education of interviewed staff already employed in a facility in a year. | Service Provision Assessments, DHS (ICF, 2004-2017) |
| Public hospital uptake | The number of deliveries at public clinics out of 1,000 live births in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Private hospital uptake | The number of deliveries at private clinics out of 1,000 live births in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Antimalaria medication | The probability of women taking anti-malaria pills during pregnancy in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Hospital too far | Share of mothers in a DHS cluster and year who did not deliver at a hospital because it was too far. | DHS (ICF, 2004-2017) |
| Fertility ethnic minority | Total number of births of mothers of an ethnic minority in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Fertility ethnic majority | Total number of births of mothers of an ethnic majority in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Fertility young | Total number of births of below-20-years-old mothers in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Fertility middle | Total number of births of mothers aged between 20 and 29 years old in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Fertility old | Total number of births of above-29-years-old mothers in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Fertility uneducated | Total number of births of mothers with below median education in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Fertility educated | Total number of births of mothers with above median education in a DHS cluster and year. | DHS (ICF, 2004-2017) |
| Private hospital availability | Number of private hospitals near a Chinese aid project. | Maina et al. (2019) |
| Public hospital availability | Number of public hospitals near a Chinese aid project. | Maina et al. (2019) |
| Health exp./Gov. Exp. | Amount of government health expenditure as a share of total government expenditure. | World Bank (2020) |
| Other aid/GNI | Total amount of other donors' health aid as a share of recipient Gross National Income. | OECD (CRSI 2017) and World Bank (2020) |

continued on next page

## Country Level

| Variable | Definition | Data Source |
|---|---|---|
| Infant Mortality | Number of children that did not survive the first year out of 1,000 live births per country and year. | DHS (ICF, 2004-2017) |
| Chinese health projects | Total number of Chinese health-related projects committed within a country/year. | Bluhm et al. (2020) |
| Chinese projects, all | Total number of Chinese projects committed within a country/year. | Bluhm et al. (2020) |
| Chinese health commitments | Total amount of Chinese health-related project commitments within a country/year. | Bluhm et al. (2020) |
| Chinese health projects, dummy | Dummy = 1 if at least one Chinese health-related project is committed within country/year. | Bluhm et al. (2020) |
| Chinese health projects (ODA) | Total number of Chinese health-related Official Development Assistance projects committed within a country/year. | Bluhm et al. (2020) |
| Chinese health projects (OOF) | Total number of Chinese health-related Other Official Finance projects committed within a country/year. | Bluhm et al. (2020) |
| World Bank health projects | Total number of World Bank health-related projects committed within a country/year. | AidData (2017) |
| Reserves | China's yearly net currency reserves. | World Bank (2020) |
| Inputs | China's detrended first factor of yearly production of steel, timber, glass, aluminum, and cement. | National Bureau of Statistics of China (NBSC 2019),<br>United States Geological Survey (USGS 2019) |
| Probability | Share of years (2000-2014) that at least one Chinese health related-project is committed in a country. | Bluhm et al. (2020) |
| Probability (all aid) | Share of years (2000-2014) that at least one Chinese project is committed in a country. | Bluhm et al. (2020) |
| Probability (ODA) | Share of years (2000-2014) that at least one Chinese health-related ODA-project is committed in a country. | Bluhm et al. (2020) |
| Probability (OOF) | Share of years (2000-2014) that at least one Chinese health-related OOF-project is committed in a country. | Bluhm et al. (2020) |
| Probability (amounts) | Share of years (2000-2014) that at least one Chinese health-related project with known amounts is committed in a country. | Bluhm et al. (2020) |
| Population (log) | The logarithm of population size. | World Bank (2020) |
| World GDP Growth | Global yearly GDP growth. | World Bank (2020) |
| Chinese FDI | Outflow of Chinese foreign direct investments in a recipient country. | World Investment Report (2017) |
| Chinese Exports | Chinese exports to a recipient country. | World Bank (2020) |

# C Descriptive Statistics

| | N | Mean | SD | Min | Max |
|---|---|---|---|---|---|
| | | | *Panel A: Local* | | |
| Infant Mortality | 363,932 | 47.566 | 134.604 | 0.000 | 1,000.000 |
| Chinese health projects | 363,932 | 0.031 | 0.267 | 0.000 | 8.000 |
| Chinese projects, all | 363,932 | 0.211 | 0.932 | 0.000 | 18.000 |
| Chinese health commitments | 363,932 | 130,028.736 | 3,019,772.306 | 0.000 | 1.353e+08 |
| Chinese health projects, dummy | 363,932 | 0.020 | 0.140 | 0.000 | 1.000 |
| Chinese health projects (ODA) | 363,932 | 0.026 | 0.224 | 0.000 | 6.000 |
| Chinese health projects (OOF) | 363,932 | 0.000 | 0.022 | 0.000 | 1.000 |
| World Bank health projects | 363,932 | 0.150 | 1.133 | 0.000 | 54.000 |
| Reserves | 363,932 | 0.071 | 0.142 | -0.086 | 0.326 |
| Inputs | 363,932 | -0.090 | 0.975 | -1.206 | 1.583 |
| Probability | 363,932 | 0.024 | 0.069 | 0.000 | 0.600 |
| Probability (ODA) | 363,932 | 0.010 | 0.041 | 0.000 | 0.533 |
| Probability (OOF) | 363,932 | 0.000 | 0.004 | 0.000 | 0.133 |
| Probability (amounts) | 363,932 | 0.010 | 0.040 | 0.000 | 0.533 |
| Population (log) | 363,932 | 9.786 | 1.977 | -1.533 | 15.866 |
| Distance to National Borders | 363,932 | 71.862 | 78.878 | 0.000 | 594.383 |
| Distance to Protected Areas | 363,932 | 63.026 | 57.761 | 0.000 | 496.120 |
| Distance to Water | 363,932 | 131.150 | 140.431 | 0.000 | 1,054.545 |
| Travel Time | 363,921 | 8.095 | 10.216 | 0.037 | 205.341 |
| Slope | 363,457 | 1.654 | 2.191 | 0.000 | 22.804 |
| Nightlights | 361,937 | 1.894 | 4.242 | 0.000 | 36.243 |
| Rainfall | 360,528 | 1.230 | 0.853 | 0.000 | 8.736 |
| SMOD Population | 363,932 | 0.944 | 1.176 | 0.000 | 3.000 |
| Vegetation Index | 360,197 | 0.305 | 0.107 | -0.006 | 0.661 |
| Built Population | 363,932 | 0.118 | 0.233 | 0.000 | 1.000 |
| Leader Birth Region | 363,932 | 0.172 | 0.377 | 0.000 | 1.000 |
| World GDP Growth | 363,932 | 3.167 | 1.370 | -1.679 | 4.403 |
| Chinese FDI | 363,932 | 14,724.045 | 20,214.108 | 915.777 | 74,654.039 |
| Chinese Exports | 363,932 | 6.554e+11 | 4.677e+11 | 2.044e+11 | 2.006e+12 |
| Assisted births skilled | 190,223 | 677.891 | 380.383 | 0.000 | 1,000.000 |
| Assisted births traditional | 199,594 | 265.842 | 359.545 | 0.000 | 1,000.000 |
| Staff hires | 100,770 | 0.246 | 0.611 | 0.000 | 15.000 |
| Staff education | 70,369 | 10.331 | 6.883 | 0.000 | 34.000 |
| Public hospital uptake | 206,346 | 504.298 | 414.600 | 0.000 | 1,000.000 |
| Private hospital uptake | 206,346 | 121.436 | 270.623 | 0.000 | 1,000.000 |
| Antimalaria medication | 232,653 | 55.575 | 100.359 | 0.000 | 1,000.000 |
| Hospital too far | 34,634 | 0.292 | 0.397 | 0.000 | 1.000 |
| Fertility ethnic minority | 206,424 | 0.854 | 1.413 | 0.000 | 22.000 |
| Fertility ethnic majority | 206,424 | 3.373 | 2.836 | 0.000 | 40.000 |
| Fertility young | 361,750 | 0.599 | 0.922 | 0.000 | 14.000 |
| Fertility middle | 361,750 | 1.936 | 1.722 | 0.000 | 28.000 |
| Fertility old | 361,750 | 1.191 | 1.376 | 0.000 | 20.000 |
| Fertility uneducated | 361,750 | 0.403 | 0.808 | 0.000 | 13.000 |
| Fertility educated | 361,750 | 3.247 | 2.707 | 0.000 | 42.000 |
| Private hospital availability | 9,798 | 0.905 | 6.440 | 0.000 | 116.000 |
| Public hospital availability | 9,798 | 109.026 | 153.505 | 0.000 | 1,206.000 |
| Health exp./Gov. Exp. | 172,053 | 7,867 | 4,089 | 1,221 | 23,245 |
| Other aid/GNI | 190,223 | 0.009 | 0.012 | 0 | 0.053 |

**(Continued)**

| | N | Mean | SD | Min | Max |
|---|---|---|---|---|---|
| | *Panel B: All Countries* | | | | |
| Infant Mortality | 1,713 | 37.947 | 26.741 | 3.500 | 134.600 |
| Chinese health projects | 1,713 | 0.107 | 0.407 | 0.000 | 7.000 |
| Chinese projects, all | 1,713 | 0.653 | 1.286 | 0.000 | 18.000 |
| Chinese health commitments | 1,713 | 830,948.490 | 10,202,393.439 | 0.000 | 3.450e+08 |
| Chinese health projects, dummy | 1,713 | 0.078 | 0.268 | 0.000 | 1.000 |
| Chinese health projects (ODA) | 1,713 | 0.095 | 0.352 | 0.000 | 4.000 |
| Chinese health projects (OOF) | 1,713 | 0.004 | 0.084 | 0.000 | 3.000 |
| World Bank health projects | 1,713 | 0.047 | 0.223 | 0.000 | 3.000 |
| Reserves | 1,713 | 0.124 | 0.152 | -0.120 | 0.318 |
| Inputs | 1,713 | 0.245 | 0.975 | -1.206 | 1.583 |
| Probability | 1,713 | 0.074 | 0.118 | 0.000 | 0.600 |
| Probability (all aid) | 1,713 | 0.303 | 0.271 | 0.000 | 1.000 |
| Probability (ODA) | 1,713 | 0.068 | 0.114 | 0.000 | 0.600 |
| Probability (OOF) | 1,713 | 0.002 | 0.014 | 0.000 | 0.133 |
| Probability (amounts) | 1,713 | 0.049 | 0.092 | 0.000 | 0.533 |
| Population (log) | 1,713 | 15.602 | 2.181 | 9.187 | 21.034 |
| World GDP Growth | 1,713 | 3.017 | 1.686 | -1.679 | 4.403 |
| Chinese FDI | 1,713 | 26,508.090 | 26,712.235 | 915.777 | 74,654.039 |
| Chinese Exports | 1,713 | 9.244e+11 | 5.517e+11 | 2.572e+11 | 2.006e+12 |
| | *Panel C: DHS Countries* | | | | |
| Infant Mortality | 525 | 56.491 | 25.983 | 7.600 | 134.600 |
| Chinese health projects | 525 | 0.223 | 0.600 | 0.000 | 7.000 |
| Chinese projects, all | 525 | 1.185 | 1.725 | 0.000 | 18.000 |
| Chinese health commitments | 525 | 1,941,847.545 | 17,349,036.217 | 0.000 | 3.450e+08 |
| Chinese health projects, dummy | 525 | 0.152 | 0.360 | 0.000 | 1.000 |
| Chinese health projects (ODA) | 525 | 0.192 | 0.490 | 0.000 | 4.000 |
| Chinese health projects (OOF) | 525 | 0.011 | 0.151 | 0.000 | 3.000 |
| World Bank health projects | 525 | 0.067 | 0.257 | 0.000 | 2.000 |
| Reserves | 525 | 0.123 | 0.141 | -0.086 | 0.326 |
| Inputs | 525 | 0.176 | 1.002 | -1.206 | 1.583 |
| Probability | 525 | 0.142 | 0.147 | 0.000 | 0.600 |
| Probability (all aid) | 525 | 0.455 | 0.283 | 0.000 | 1.000 |
| Probability (ODA) | 525 | 0.132 | 0.143 | 0.000 | 0.600 |
| Probability (OOF) | 525 | 0.006 | 0.024 | 0.000 | 0.133 |
| Probability (amounts) | 525 | 0.084 | 0.120 | 0.000 | 0.533 |
| Population (log) | 525 | 16.377 | 1.293 | 13.276 | 18.962 |
| World GDP Growth | 525 | 3.040 | 1.663 | -1.679 | 4.403 |
| Chinese FDI | 525 | 22,453.612 | 24,605.778 | 915.777 | 74,654.039 |
| Chinese Exports | 525 | 8.434e+11 | 5.146e+11 | 2.572e+11 | 2.006e+12 |

*Notes:* "DHS countries" refers to those countries that are also included in our regressions at the local level.

# D Share of Aid within Health Aid

| Sector | Percent |
|---|---|
| *Purpose specified* | |
| Malaria | 14.29 |
| HIV, Ebola, Tuberculosis | 2.72 |
| Reproductive | 2.72 |
| Eyes | 1.36 |
| Traditional Medicine | 1.36 |
| Cancer | 0.68 |
| *Purpose not specified* | |
| Construction & Rehabilitation | 26.53 |
| Equipment | 16.33 |
| Medical Teams | 25.85 |
| Other | 8.16 |

*Notes:* Shows the percentage of the number of Chinese health projects committed for a purpose or sub-sector in the total number of health projects.