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Development aid and infant mortality. Micro-level evidence from Nigeria

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ABSTRACT

While there is a vast literature studying the effects of official development aid (ODA) on economic growth, there are far fewer comparative studies addressing how aid affects health outcomes. Furthermore, while much attention has been paid to country-level effects of aid, there is a clear knowledge gap in the literature when it comes to systematic studies of aid effectiveness below the country-level. Addressing this gap, we undertake what we believe is the first systematic attempt to study how ODA affects infant mortality at the subnational level. We match new geographic aid data from the AidData on the precise location, type, and time frame of bilateral and multilateral aid projects in Nigeria with available georeferenced survey data from five Nigerian Demographic and Health Surveys. Using quasi-experimental approaches, with mother fixed-effects, we are able to control for a vast number of unobserved factors that may otherwise be spuriously correlated with both infant mortality and ODA. The results indicate very clearly that geographical proximity to active aid projects reduces infant mortality. Moreover, aid contributes to reduce systematic inter-group, or horizontal, inequalities in a setting where such differences loom large. In particular, we find that aid more effectively reduces infant mortality in less privileged groups like children of Muslim women, and children living in rural, and in Muslim-dominated areas. Finally, there is evidence that aid projects are established in areas that on average have lower infant mortality than non-aid locations, suggesting that there are biases resulting in aid not necessarily reaching those populations in greatest need.

1. Introduction

Foreign aid has been the subject of increasing critique since the 1980s and there has been extensive research on aid effectiveness,[1] particularly focusing on the impact of aid on aggregate economic growth (Arndt, Jones, & Tarp, 2014; Bigsten & Tengstam, 2015). Despite massive efforts, the scholarly literature remains inconclusive when it comes to the question to what extent development aid actually works (Qian, 2015). This is true both for the general studies of aid effectiveness for overall economic growth, but also for studies on the impact of aid on non-growth outcomes, such as education and health.

One reason for the inconclusive results of the aid effectiveness studies can be that the large majority of the empirical investigations have relied on cross-country analyses. First, such analyses may fail to control for differences across countries, leading to spurious effects between aid and various outcomes (Odokonyero, Alex, Robert, Tony, & Godfrey, 2015). Second, the lack of robust results regarding the effects of aid on development could arguably be a result of the effects of aid being too small and localized to affect aggregate outcomes (Briggs, 2017; Dreher & Lohmann, 2015). Starting from the premise that the country-level may be a too highly aggregated unit of analysis to clearly identify effects of development aid, this study addresses within-country effects across a very extensive empirical material, and focusing on an outcome that has received much policy interest, but less attention in studies of aid effectiveness, namely infant mortality.

In general, the lack of systematic studies of aid effectiveness on health indicators below the country-level represents a clear gap in the literature. Existing databases on foreign aid – the OECD's Creditor Reporting System and now AidData (Tierney et al., 2011) – do in fact contain information at the project level. Yet, the large majority of empirical analyses of aid effectiveness using these data aggregate to the country-year level, thereby losing project specific

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[1] By aid effectiveness we understand the ability of aid in achieving stated development goals (e.g. reduced poverty, increased income, social improvements) in the recipient countries relative to the resources spent.

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0305-750X/

information (Findley, Powell, Strandow, & Tanner, 2011). A few exceptions exist. Using the geographically disaggregated AidData containing information on the exact location of aid projects, scholars have found a positive effect of aid on development (Dreher & Lohmann, 2015), as well as a conflict-reducing effect of aid (van Weezel, 2015), while Briggs (2017) finds that aid is not distributed to the poorest regions, suggesting that aid is not as effective as it could be in reducing poverty. For the health sector in Malawi specifically, De and Becker (2015) find that aid is associated with reduced prevalence and severity of diarrhea, while Marty, Dolan, Leu, and Runfola (2017) find that aid contributed to reducing the prevalence of malaria as well as improved quality of self-reported health care. Odokonyero et al. (2015) find that aid has reduced the overall disease severity and burden in Uganda.

Our study makes several contributions to the small but rapidly growing body of literature focusing on the local effects of aid.[2] First, to the best of our knowledge, we are the first to investigate the effects of aid on infant mortality, a key development outcome, using both a sound identification strategy and a local level design. By spatially linking new data from the AidData on the precise location, type, and time frame of bilateral and multilateral aid projects in Nigeria to micro-level information on infant mortality from household surveys, we provide a systematic attempt at studying how Official Development Aid (ODA) affects infant mortality at the subnational level. Investigating infant mortality has the advantage over other outcomes that we are able to investigate a long period at a local level, controlling for a wide array of possible confounders. While a primary rationale for selecting Nigeria as a case study was data availability, due to coverage both by AidData and through several successive and extensive Demographic Health Surveys, Nigeria is a major aid recipient with great local variation in economic, social and demographic conditions, including the most extensive group inequalities documented on the continent (Østby & Urdal, 2014). Second, we find that geographical proximity to aid projects indeed reduces the risk of infant mortality, as well as child and neonatal mortality. Third, we explore heterogeneous effects and find that the mortality-reducing potential of aid seems to be particularly strong for children of Muslim women, in rural areas, and in Muslim areas. Aid thereby seems to reduce horizontal inequalities in a setting where such inequalities loom large. Fourth, we also demonstrate that aid is allocated to areas with less infant mortality to start with. At the very least, this implies that the possibility of aid to reduce vertical inequalities has not reached its full potential, adding to an emerging literature indicating that aid not necessarily reaches those who need it the most. Finally, we assess other effects of aid, and find effects on wealth, female employment, and female education for Muslim mothers, but not for Christian mothers. These factors are likely to explain the heterogeneity in effects that we observe.

The remainder of the article proceeds as follows: The next section provides a brief literature review of the aid effectiveness literature, including the impact of aid for health outcomes. In the third section, we outline a framework for how official development aid is expected to impact infant mortality. The fourth section presents the data, the fifth section outlines our empirical strategies, the sixth section presents our results, and in the seventh section, we investigate some possible mechanisms. The final section concludes.

[2] For other prominent examples, see Francken, Minten, and Swinnen (2012) on relief aid allocation in Madagascar; Powell and Findley (2012) on donor coordination; Briggs (2014) and Jablonski (2014), both on political capture of aid in Kenya; Öhler and Nunnenkamp (2014) on factors determining the allocation of World Bank and African Development Bank aid; Dreher et al. (2016), on allocation of Chinese aid to the birth regions of African leaders; Isaksson and Kotsadam (2016), on the effects of Chinese aid on corruption; and Kelly, Samuel, and Elkink (2016), on the relationship between Chinese aid and perceptions of corruption in Tanzania.

2. Aid effectiveness: a brief review of the literature

Over the years, the empirical literature on aid effectiveness has yielded unclear and ambiguous results, and to date, there appears to be no consensus as to whether aid plays a positive role for growth and development in recipient countries.

In a set of meta-analyses surveying the aid effectiveness literature, Doucouliagos and Paldam (2009) concluded that aid has not been effective. The main critique centers around the failure to significantly improve growth and reduce poverty. Furthermore, some have argued that official development aid may be effective only under certain conditions, such as e.g. only in democracies (Boone, 1996; Burnside & Dollar, 2000), or when aid is outsourced to non-state actors in countries with bad governance (Dietrich, 2016). But even in the presence of these conditions, aid may still be ineffective (e.g. Hansen & Tarp, 2000), or be hindered by weak institutions in recipient countries (Kosack, 2003). Bourguignon and Platteau (2017) argue that donors should consider the tradeoff between need and governance capacity when allocating aid.

As a contrast to the above studies there is also an increasing amount of macro-level evidence for a positive impact of aid on economic growth, possibly shifting the weight of evidence to a positive (albeit moderate) contribution of aid (e.g. Arndt et al., 2014; Clemens, Radelet, Bhavnani, & Bazzi, 2012, Juselius, Møller, & Tarp, 2014; Mekasha & Tarp, 2013).

Another strand of the aid effectiveness literature focuses on the impact of aid on non-growth outcomes. Proponents of this approach have argued that focusing exclusively on the effect of aid on growth may overlook important benefits from aid on other outcomes, such as health (Mishra & Newhouse, 2009). Systematic evidence on how aid affects health outcomes in particular is surprisingly scarce. This limited literature can be divided into three categories: Macro-level studies that look at the impact of aggregate aid on aggregate health outcomes (such as e.g. country level infant mortality); meso-level studies that focus on the effects of *health* aid specifically on aggregate *health* outcomes, and micro-level studies that look at the effectiveness of health aid to a specific health program (e.g. maternal health) or disease (such as e.g. HIV/AIDS) and on outcomes in that particular health program or disease (Gyimah-Brempong, 2015).

When it comes to the two first categories of studies, there is little consensus in the literature on how aggregate aid (both in general and to the health sector) affect health outcomes. For example, Kosack (2003) study the impact of aid on human development indicators and find that aid is only effective in democracies; Ndikumana and Pickbourn (2017) investigate the effect of aid on access to water and sanitation using panel data and find that increased aid to the sectors increase service provision, albeit non-linearly (see also Salami, Stampini, Kamara, Sullivan, and Namara (2014)).

Murdie and Hicks (2013) find that when health services are provided by international nongovernmental organizations, they also increase the governmental spending on health services, and Savun and Tirone (2012) suggest that foreign aid can help mitigate conflict risk in low-income countries during periods of economic depression. The relationship between institutions and health aid may also differ from the relationship between institutions and aid in general. Dietrich (2011) argues that health aid need not be ineffective in corrupt countries as compliance in this sector is cheap and the countries may therefore strategically comply. Han and Koenig-Archibugi (2015) argue that aid fragmentation up to a certain extent is beneficial for health aid as there is more possibilities to select the programs that work. Mishra and Newhouse (2009) find no effect of aggregate aid on infant mortality rates, but they find that health-specific aid indeed reduces infant mortality.

However, also for the second category of studies that focus on the effectiveness of health aid on health outcomes, there is no consensus on the aid effectiveness. While some studies find a positive effect, others find no effect. Pickbourn and Ndikumana (2016) find that increased aid to the health sector reduces maternal mortality, Yogo and Mallaye (2015) find that health aid has decreased the HIV-prevalence and child mortality in Africa, and Gyimah-Brempong (2015), who is among the first few studies to document the impact of health aid in African countries in particular, find that health aid indeed has a positive effect on various health outcomes.[3] In contrast to these studies, Wilson (2011), for example, finds no effect of health aid on various types of mortality.

When it comes to the third strand in the literature, that focuses on the effectiveness of health aid allocated to a particular disease or program on outcomes in that sector, there seems to be far less inconsistency, and a general agreement that targeted health aid improves health outcomes in the targeted areas (see e.g. Bendavid and Bhattacharya (2009), Rasschaert et al. (2011) and Shiffman, Berlan, and Hafner (2009), who find that aid to HIV/AIDS programs have significantly decreased HIV-related deaths, and Taylor, Hayman, Crawford, Jeffrey, and Smith (2013), who find that aid targeted to support maternal health programs significantly reduces maternal mortality rates).

Another way of categorizing the aid-health literature is to look at various levels of analysis. On the one hand, a set of cross-country studies fail to find that aid spurs improvements in various health indicators, including IMR, both considering the overall effect of aid (e.g. Boone, 1996), and when using sector-specific aid data (Gebhard, Katherine, Ashley, Daniel, & Sven, 2008; Williamson, 2008; Wilson, 2011). Lee and Lim (2014) find that health aid at the country level increases when the health deteriorated but according to Wilson (2011): 2032 aid has been '*following* success, rather than *causing* it.' By this, he means that aid has largely gone to countries that have experienced health gains rather than aid promoting those gains.

Due to the lack of empirical support for the effect of health aid, some scholars have placed greater emphasis on domestic efforts in improving health outcomes (e.g. Williamson, 2008). In contrast to this negative interpretation of the effectiveness of health aid, a handful of country-level studies have also found that aid has a *positive* effect on health outcomes. (e.g. Bendavid, 2014; Mishra & Newhouse, 2009), although the effect is modest. However, as noted by the authors themselves, although the effect of aid is identified using within-country changes in aid and IMR over time, the estimated effect is nonetheless just an average across a very heterogeneous set of countries. Hence, they encourage future research to conduct detailed case studies of the effects of health aid in individual countries. Moreover, as we highlighted in the introduction, cross-country analyses may fail to control for differences within countries, leading to spurious effects between aid and various outcomes (Odokonyero et al., 2015), and the effects of aid (be it aggregate or sector-specific) could be too small and localized to affect various health outcomes (Briggs, 2017; Dreher & Lohmann, 2015). These challenges motivate our current study, where we focus on the effect of aid on infant mortality at the *local* level in Nigeria.

3. Official development aid and infant mortality

According to the World Bank, the infant mortality rate (IMR) for Sub-Saharan Africa as a whole was 56 deaths below the age of one per 1000 live born in 2015, compared to an average of 6 in the OECD countries.[4] For Nigeria, the IMR score was estimated to be higher than the continent's average, standing at 69 in 2015, however there are large geographical variations within the country. To what extent can we expect that official development aid can contribute to reducing the level of infant mortality? In order to address this question, it is useful to take a step back and look at what the literature says about the determinants of infant mortality in general.

3.1. The impact of aid on infant mortality

The chance that an infant makes it to her or his first birthday depends on a variety of direct and indirect determinants (e.g. Sartorius & Sartorius, 2014; Schell, Reilly, Rosling, Peterson, & Ekström, 2007), as depicted in Fig. 1. Among the proximate determinants are the health of the mother, infections, accidents, and use of health services, such as immunizations. Examples of intermediate determinants are access to food, safe water, sanitation, and electricity. More distal, but yet important, determinants include broader socioeconomic conditions like household poverty, infrastructure, sanitation, clean water, and the education of the parents, in particular the mother.

Since so many factors may be determinants of health in developing countries, there may be benefits in considering the impact of the provision of aid more broadly rather than focusing narrowly on aid within the health sector. Arguably, projects aimed at increasing literacy, female empowerment, electricity, safe water, infrastructure or agricultural productivity may all positively impact child survival. Indeed, White (2007), who investigated specific health interventions in Bangladesh, concluded that health outcomes were not related to health aid specifically, but to a larger degree to aid given to other sectors.

The relevance of different types of aid could further differ depending on context, such as rural vs. urban residence. In a study of 60 low-income countries between 1990 and 1999, Wang (2003) found that mortality in urban areas was highly correlated with access to electricity, household wealth and female secondary education, while mortality in rural areas was associated with access to piped water, access to electricity, female education, household wealth and vaccination coverage.

The AidData disbursement data provide an opportunity to identify the time of implementation of aid projects, while DHS data allows for a comparison between those born just before and just after the aid project started. However, the effect of aid on health outcomes may be fast or slow. Some directed efforts like postnatal checkups and care for deadly, but treatable diseases like diarrhea and fever, may have an immediate impact on child survival, others, like immunization, will have a positive effect on survival in the medium run up to a few months, while other forms of aid meant to improve female education or agricultural productivity may improve infant and child survival over a much longer time horizon. We start by testing for a total effect of aid and propose the following main hypothesis:

The geographical proximity of any aid project is associated with a greater chance of survival for children born after the establishment of the project, than for children born prior to the establishment of the project.

While the discussion on aid effectiveness has primarily centered around economic development and the ability of aid to deliver aggregate economic growth, less, albeit increasing, attention has been paid to whether or not aid contributes to reduce various forms of inequalities (see Dollar & Kraay, 2002; Castells-Quintana & Larrú, 2015; Chong, Gradstein, & Calderon, 2009; Herzer &

[3] See Ndikumana and Pickbourn (2016) for a comprehensive review of the literature on aid effectiveness in Africa and a discussion of the orientation of future research in this area.

[4] http://data.worldbank.org/indicator/SP.DYN.IMRT.IN.

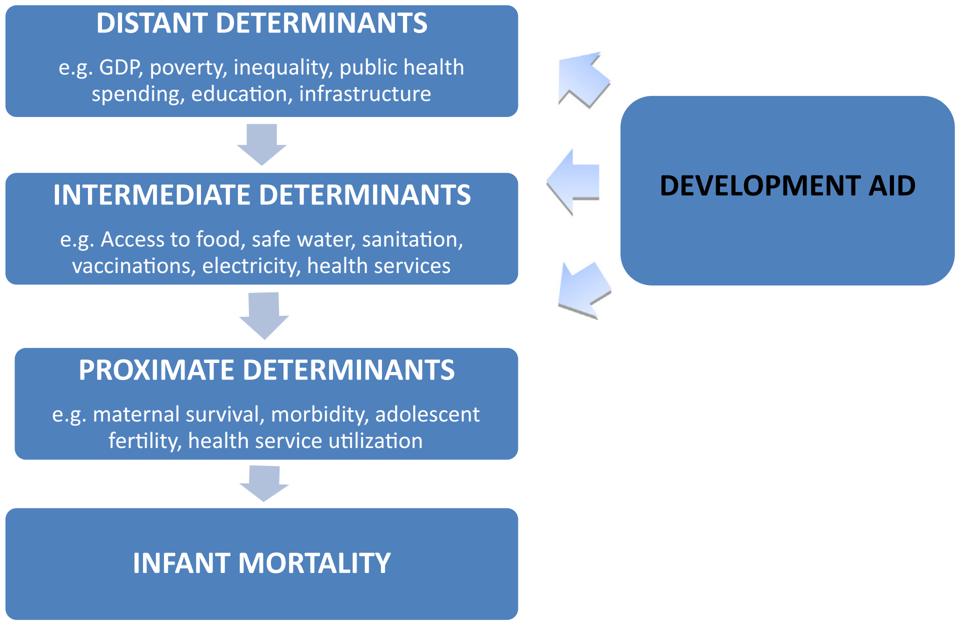

Fig. 1. Conceptual framework of the hierarchy determining infant mortality. Amended from Schell et al. (2007: 290) and Sartorius and Sartorius (2014: 2).

Nunnenkamp, 2012). So far, the research on the aid-inequality nexus is scarce and inconclusive, particularly when it comes to the question of whether aid reduces (or increases) systematic inequalities between identity groups, known as 'horizontal inequalities' (see Brown, Stewart and Langer, 2010; Stewart, 2008). Hence, we also address whether aid has contributed to reduce inequalities in health in Nigeria, a country that both has extensive systematic horizontal inequalities between Christians and Muslims along a number of dimensions like health, income, and education, and a history of significant inter-group conflict. For example, Christian women have about three times as many years of schooling than Muslim women (Østby & Urdal, 2014). Furthermore, data from the DHS surveys reveal that the discrepancy between Christian and Muslim women has indeed been increasing since the 1960s, and that it is particularly Muslim women living in predominantly Muslim areas that are lagging behind (see Section E in the Appendix for figures and a longer description).

4. Data

4.1. Aid data

To measure localized effects of aid, we use data from the USAID-sponsored AidData project. This is an open access database covering geo-coded bilateral as well as multilateral aid projects. The AidData project has produced both global datasets for certain donors, as well as specific and very detailed country dataset for select countries, among them Nigeria, covering a high number of donors. The data comes from various sources including OECD's Creditor Reporting System, annual reports and project documents published by donors, web-accessible databases and project documents, and spreadsheets and data exports obtained directly from donor agencies.[5]

The aid data from Nigeria was released in August 2015 (Version "Release Level 1 v 1.0"). It contains a total of 621 aid projects, covering a total of 1843 locations. The locations vary from highly precise GPS points to regional/state and central government levels, and the dataset includes precision coding to indicate how detailed the location coding is. The projects range from agricultural support, health and education to government/civil society, banking, and infrastructure. Many projects also span several different sectors.

To be able to assess the effect of aid on infant mortality we need to know the date when the project was established. However, since the precise *actual start date* is unreported for a high number of projects we use the *planned start date*.[6] The correlation between the actual start and planned start dates was above 0.9 for the projects for which we have information on both. Furthermore, they both have the mean starting year in 2011 and the median as well as modal starting year in 2013.

Further, in order to test the localized aspects of aid effectiveness, we need to know the specific location of the projects. We only use projects that correspond to AidData precision coding 3 and below, which defines a project as specific to a local government area. These two restrictions reduce the number of projects to 97.[7] However, many of these projects have several locations, so a total of 726 project locations meet our coding criteria. This includes aid projects across all sectors. 18 of the projects are directly linked to health, representing a total of 64 locations. Restricting the analysis to projects with precise geocodes implies that we focus on projects with physical project sites as opposed to projects or bilateral agreements of a more intangible nature. Hence, the projects we focus on need not be representative for all types of aid to Nigeria. Table 1 disaggregates the type of projects that are included in the analysis. The earliest project included in the analysis was established in 1990 and the latest in 2014.[8] Among the project locations we use in this analysis there is a large share from the World Bank as well as the Bill & Melinda Gates Foundation, while there are surprisingly few projects from the UK (see Table A20 on donor breakdown in the Appendix). One likely reason for this is that much of the aid given through for

[5] http://aiddata.org/user-guide.

[6] This increases the number of projects in the analysis from 37 to 97.

[7] In order to include more projects, we also ran an analysis on state level, testing whether a state had an active project at the time of birth. This analysis yields the same conclusions as the ones based on the results included in the main tables (see Appendix Table A5), but we deem them too crude and prefer to use the more disaggregated data.

[8] A breakdown of project and location by planned start year can be found in Table A19 in the Appendix.

Table 1
Overview of project types.

Type of project	Number of projects	Number of locations
Health	18	64
Agriculture	31	144
Government and civil society	14	32
Energy generation and supply	6	21
Banking and financing	2	14
Commodity aid and general programme assistance	4	7
Water and sanitation	2	3
Trade policy and regulations	2	3
Education	2	2
Communication	1	1
Unspecified	29	470

example DFID is given at state level, thus it is not included in our analysis.

The number of projects in the table is higher than the total number of projects included in the analysis, because one project can include several elements. From Table 1 we see that a number of projects are *unspecified*. This does not mean that we do not know the content of these projects, only that they did not fit squarely into the pre-specified categories from AidData. These projects include, among others, infrastructure, emergency aid, gender-related projects and some unspecified agricultural projects. In addition to the main models, we have also run separate analyses of the 18 health projects.[9]

4.2. Demographic data from DHS

The source of the demographic data used in this analysis is Demographic Health Surveys conducted over several years in Nigeria. In a DHS, a sample of households is selected throughout the entire country. Women between the ages of 15 and 49 are interviewed about sexual and reproductive health, nutrition, family and other demographic factors. The survey instrument also includes a number of additional items, such as ethnicity, education, and household assets. Table 2 shows descriptive statistics for the total sample as well as for Muslims and Non-Muslims separately. The measure of household wealth is based on the wealth index provided in the DHS. This index is a standardized measure of assets and services for households in a given survey, such as type of flooring, water supply, electricity, and the ownership of durable goods such as a radio or a refrigerator. The wealth index is standardized within the country and survey year, thus providing information on the relative wealth for households in a survey. We divide the wealth index by 10,000 for presentational purposes.

DHS surveys typically cover several thousand respondents nationally, representing urban and rural areas and provinces/states. DHS surveys are conducted every four to five years in most countries, with the same questions asked in each survey to facilitate comparisons across time and space. Several of the DHS surveys include detailed information about the exact location of each sample cluster, providing geographical coordinates for each surveyed location (village/town/city).

We use data from the five DHS survey rounds that have been conducted in Nigeria in 1990, 2003, 2008, 2010 and 2013, totaling 2686 clusters, in which 67,396 mothers who had given birth to 294,835 live children were interviewed. In order to test the effect of aid on each of the children, the unit of analysis in this article is not the women interviewed, but each live birth reported by the women. Thus, a mother with five children would have five entries in the dataset.

We match the DHS data with the georeferenced aid data and the location of the households of the live-born children under the age of one year, within 25 km and 50 km distances from each aid project. The map in Fig. 2 shows the distribution of aid projects and DHS clusters. It also illustrates how the data is structured with aid project 'buffer zones', indicating which DHS clusters (black dots) are within the relevant distances of the aid projects (red crosses) and which are not. The light gray circles around the projects indicate a 50 km buffer zone while the darker is the 25 km buffer zone. A visual inspection suggests that the North-Eastern region has very few aid projects. This is also one of the most marginalized regions in Nigeria.

Fig. 3 shows in more detail the data structure for the North-Western region of Nigeria. We can clearly see that there is a good distribution of DHS clusters both among those that are located near an aid project (within in the grey areas), and those that are not.

5. Infant mortality

We use infant mortality to study aid effectiveness. In the Demographic Health Surveys, mothers are asked to provide information about each child they have ever given birth to. These children are the units of analysis. The information given about each child includes the time of their birth, and if they died, the time of death. The variable is coded 1 if the child died before it was 12 months old, and 0 if it survived its first 12 months. Among the 294,835 children included in the dataset 26,927 died before turning one year, representing 9.2 percent of all children, or an Infant Mortality Rate (IMR) of 92 per 1000 live-born (see also Table 1).[10] Fig. 4 illustrates how infant mortality has generally declined in Nigeria since 1960.

Fig. 5 indicates the rate of children who died before 12 months within each grid cell shown on the map. The data in each grid cell is based on the DHS clusters that fall within each cell. We see that the level of infant mortality is generally higher in the northern areas, and in particular in the North-West. Comparing this to Fig. 6 indicating where the aid projects are, we see that there is an overlap between areas where there is high infant mortality and *no* aid projects. However, this does not take selection into account, so it is difficult to assess based on this whether this negative correlation is due to effective aid, or whether aid projects are not established in the marginalized areas.

5.1. Testing heterogeneous impacts of aid

In order to test whether the effect of aid on reducing infant mortality is greater among children born to Muslim women, and for children living in Muslim areas or living in rural areas, we must identify these groups. The data defining the mother's religion and whether she lives in an urban or rural area come from the DHS. To define Muslim areas (among the buffer zones around the aid projects) we split the sample on the median of the share of religion for each area, so that areas with more than the median share of Muslims are defined as Muslim dominated areas. In Table 2 we see that infant mortality is higher among Muslims and they also have less schooling and wealth.

[9] We also conduct an analysis for a sample of projects excluding projects relating to 'trade policy and regulations' and 'government and civil society'. The results are identical.

[10] It is further likely that this number is somewhat underreported as it is more likely that mothers will fail to report dead children than living children.

Table 2
Descriptive statistics.

	Observations	Total mean	Muslim	Non-muslim
Dependent variable				
Death (infant mortality)	289,53	0,092	0,100	0,082
Main independent variables				
Active 50	289,53	0,150	0,113	0,192
Inactive 50	289,53	0,746	0,731	0,767
Other variables				
Rural	270,464	0,698	0,738	0,645
Wealth*	242,341	−2,531	−5,112	0,876
Work	269,82	0,762	0,679	0,870
Work for cash	248,577	0,543	0,556	0,526
Years of schooling	270,294	3,669	1,672	6,229

* Wealth is a relative wealth index (see main text). It is divided by 10,000 for presentational purposes.

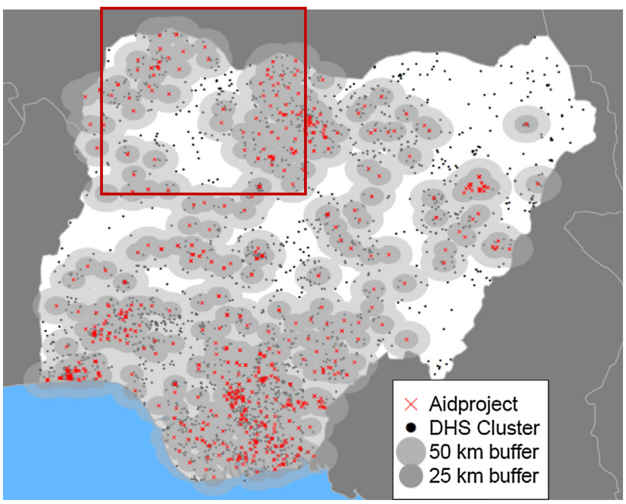

Fig. 2. Aid projects and DHS distribution including 50 km and 25 km buffer zones.

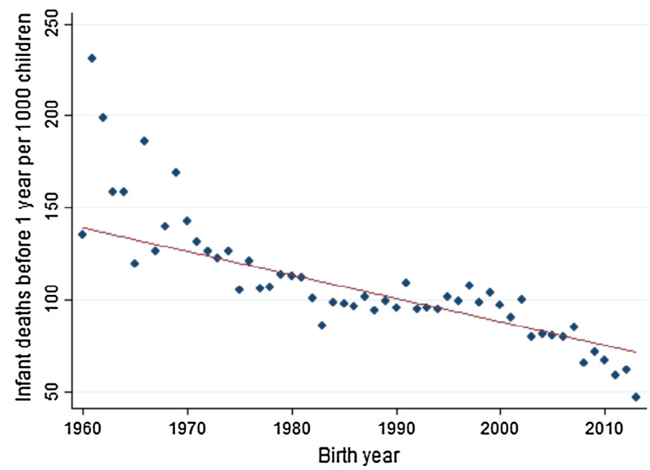

Fig. 4. Infant mortality over time in Nigeria since 1960.

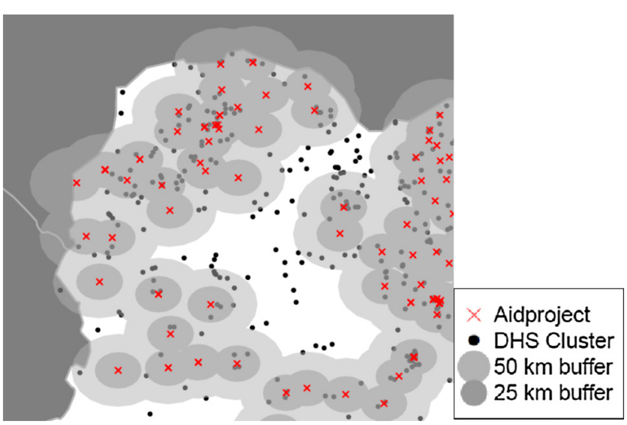

Fig. 3. Snapshot of the north-west region based on Fig. 2.

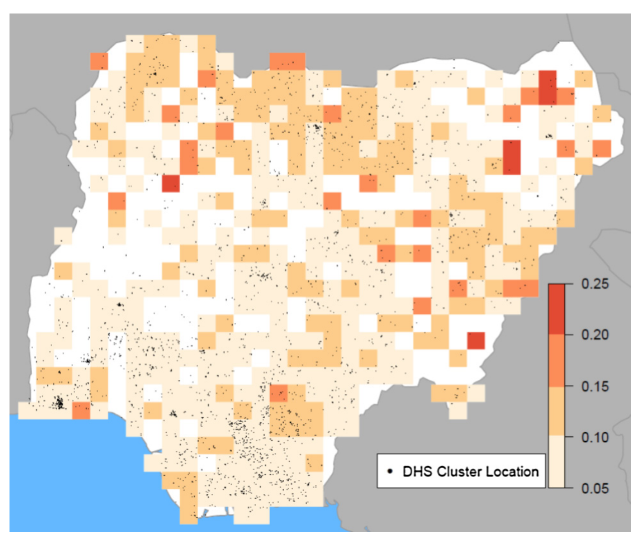

Fig. 5. Infant mortality rate based on the five DHS surveys.

6. Empirical strategy

6.1. Difference-in-differences

The structure of the data that we are using in this article allows us to make comparisons over both time and geographical location. Since we know when and where an aid project is to be established, we can compare the level of infant mortality in areas close to projects before and after the projects have started to infant mortality in areas further away from projects. To do so we build on the spatial-temporal strategy presented in Kotsadam and Tolonen (2016) and Knutsen, Kotsadam, Olsen and Wig (2017) and use a *difference-in-differences* method.[11] The model compares the likelihood of dying before the age of one, both before and after the intro-

[11] Isaksson and Kotsadam (2016, 2017) also use the method to investigate the effects of aid on corruption and trade union involvement respectively.

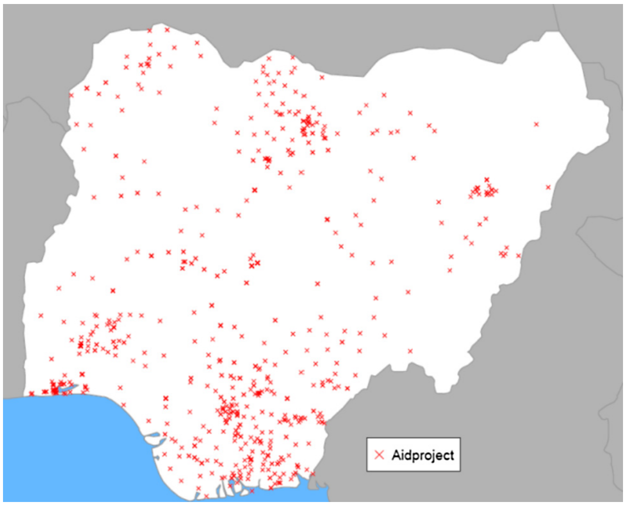

Fig. 6. Aid project locations.

duction of an aid project nearby. More specifically, we use each child born as the unit of analysis and estimate the following baseline linear probability model:

$$Y_{ivt} = \beta_1 \cdot active + \beta_2 \cdot inactive + \lambda_t + \theta_{it} + \varepsilon_{ivt}, \quad (1)$$

where the outcome Y of a child i, cluster v and for year of birth t is regressed on active and inactive. We first define the DHS clusters that could be expected to be positively affected by aid, which we set to 50 km distance from the project location point in our baseline estimation following previous spatial analyzes with similar data (Knutsen et al., 2017; Kotsadam & Tolonen, 2016). We also present results using a 25 km buffer zone.

We include projects established both before and after the year of birth of the kids. For each unit of observation, the children included in our dataset, we create two dummy variables for each distance: One called active50 (active25) and one called inactive50 (inactive25). The active variable equals one if at least one aid project was established when the child was born and zero otherwise, while the inactive variable equals one if we know that there will be an aid project in this area in the future, but that it was not yet established when the child was born. That is, as we also have historical data on births, we can go back in time and if the birth took place before the project started we call the observation inactive. For example, if a project starts in 2001 and one child is born in year 2000 in the same area he or she will be coded as inactive while another child in the area born in 2003 will be coded as active. The active50 variable includes 13,545 children and the smaller area of active25 includes 5,443 children. Children that are not related to any aid project become the reference category in the analysis (275,985 and 282,393 children respectively).

The difference-in-differences strategy implies the comparison of two differences. First, it allows us to compare the death rates of children living in active and inactive aid areas to the rest of the country (the first difference). Only comparing death rates between active areas and the rest of the country would be equivalent to assuming that areas receiving aid and areas not receiving aid are expected to be equal (i.e. that aid is randomly allocated). The comparison between inactive areas and the rest of the country will show us whether there are indeed signs of selection into becoming an aid area. Secondly, we can compare the difference between the two differences (the second difference). That is, we compare the difference between active areas and the rest of the country to the same difference for inactive areas. The strategy thereby purges away the selection effect captured by the inactive measure and, as such, this strategy controls for the potential selection effects. For example, areas receiving aid could be generally poorer than the rest of the country, hence addressing the effect of aid on infant mortality by comparing the proximate areas of the aid projects with the rest of the country might yield biased results. The regression further includes linear trends in year of birth λ_t, (as we see in Fig. 4 that infant mortality declines generally in the sample over the period) and we control for the time-varying variables in all regressions by adding the vector θ_{it}. These variables are birth order and a dummy for being part of a multiple birth (e.g. twins). We believe these to be the most important time varying factors (that are not themselves potential effects of aid) are birth order (which differs by definition and which is correlated with infant mortality), dummy variables for being part of a multiple birth (which increases with birth order and is correlated with infant mortality). By including such controls we are removing the independent influence of such factors. The standard errors are clustered at the level of the primary sampling unit so that we take into account that the observations are not independent within each cluster.

6.2. Mother fixed effects

As we have retrospective fertility data and many mothers in our sample we are able to exploit the data even further by comparing the death rates of siblings that were born before and after aid projects had started. Hence, in our second estimation strategy we control for selection by including mother fixed effects and the estimated effects of aid are thus estimated using only *within-sibling variation*.[12] The advantage of such a design, over for example cross-country or even within-country regression analyses, is that we are able to control for a vast amount of variables that may otherwise be spuriously correlated with both infant mortality and aid. In fact, our approach implies controlling for all the observed and unobserved factors that are likely fixed over time for each mother, such as education level, religion, and rural/urban residency. Importantly, selection into areas depending on pre-existing level differences in mortality is completely controlled for as well. It also ensures that the estimated effect is not driven by endogenous population changes that may occur as an effect of aid. The specification is shown in Eq. (2)

$$Y_{ivmt} = \beta_1 \cdot active + \alpha_m + \lambda_t + \theta_{it} + \varepsilon_{ivt}, \quad (2)$$

where Y is now the outcome for child i born by mother m in cluster v in year t. The mother fixed effects, α_m, ensure that we are comparing the effects of sibling births with as similar conditions as possible but for the aid projects. As we now compare the same mother before and after aid we only need to include the active coefficient. Note that the vector of time varying control variables, θ_{it} (birth order and multiple birth), vary across siblings.

7. Empirical findings

In columns 1–4 of Table 3 we present the basic difference-in-differences models of Eq. (1), assessing the risk of dying among children born in 'active' areas, that is areas with an ongoing aid project, and among children born in 'inactive' areas, that is areas that we know will get an aid project in the future. If aid project locations had been selected at random, there should be no statistically significance difference in child survival between children born in the areas that will never receive aid projects (the reference

[12] When running fixed effects models, individuals with no variation over time are not dropped but they are not used to estimate the coefficient of interest. Dropping these individuals is not recommended as they help improve efficiency and contribute to correct estimations of r-squares.

Table 3
Effects of aid projects on infant mortality.

Variables	(1) Death	(2) Death	(3) Death	(4) Death	(5) Death	(6) Death
Active 50 km	−0.013***	−0.028***			−0.010***	
	(0.002)	(0.005)			(0.002)	
Inactive 50 km		−0.018***				
		(0.004)				
Active 25 km			−0.018***	−0.026***		−0.006*
			(0.002)	(0.003)		(0.003)
Inactive 25 km				−0.014***		
				(0.003)		
Observations	289,530	289,530	287,836	287,836	289,530	287,836
R-squared	0.019	0.020	0.019	0.020	0.291	0.291
Mother FE	No	No	No	No	Yes	Yes
Mean in sample	0.0918	0.0918	0.0920	0.0920	0.0920	0.0920
Difference in difference		−0.0106		−0.0118		
F test: active-inactive = 0		21.76		23.79		
p value		.000		.000		

Robust standard errors clustered at the DHS cluster level in parentheses. All regressions control for a multiple birth dummy, birth order fixed effects, and a linear trend in birth year.
** $p < .05$.
*** $p < .01$.
* $p < .1$.

category) and those born in 'inactive' areas (since the treatment has not yet been implemented). The models in both columns (2) and (4) show, however, that children born in areas that will receive an aid project in the future have lower mortality than children born in areas that will not receive an aid project. This relationship captures a selection effect, suggesting that aid projects are established in areas that on average have lower mortality than the average non-aid location. This supports earlier findings (e.g. Briggs, 2017) that aid is not primarily reaching those that need it the most. There could be many possible explanations for such bias, including that aid projects may be established predominantly in urban areas with high population densities or more generally in areas with better infrastructure. Comparing areas with ongoing (active) projects with those with future projects, the positive effects is greater and the difference is statistically significant, indicating that there is a positive effect of aid on child survival also when selection is taken into account.

In these models we also include a linear variable of the year of birth of the child, controlling for the general improvement in infant health over time. As aid is increasing over time as well, the failure of including such time variable could easily overestimate the effect of aid. We find similar results whether we use a 25 km or a 50 km buffer zone.

In Columns 5 and 6 we introduce mother fixed effects as in Eq. (2). The mother fixed effects models essentially only use variation from mothers that have given birth to children both before and after an aid project has started nearby, allowing us to study the impact of aid once all potential confounding factors associated with the mothers are controlled for. Because of this restriction we have fewer observations. While in the baseline regression there were 289,530 children born by 66,604 mothers, the result in column 5 is in fact based on 71,537 children born by the 14,071 mothers who gave birth both before and after an aid project started within a 50 km buffer zone. The results are very similar to the difference in difference estimates and they also show a substantial reduction in infant mortality as an effect of aid. The point estimate in column 5 (which is our preferred specification) suggests that aid decreases the infant mortality rate by 1 percentage point, or more directly by 10 children per 1000 born. As compared to the average share of infant mortality in the sample, which is 92 children dying per 1000 born the effect of aid corresponds to a reduction of 11%.

Separating between different sub-groups, still using mother fixed effects, we find in Table 4 that aid is particularly effective

Table 4
Effects of aid projects on infant mortality for different groups.

Variables	(1) Death	(2) Death	(3) Death
Active 50	−0.004	−0.002	−0.002
	(0.004)	(0.004)	(0.004)
Active 50*Rural	−0.010*		
	(0.005)		
Active 50*Muslim		−0.017***	
		(0.005)	
Active 50*Muslim area			−0.013***
			(0.005)
Observations	270,464	269,264	289,530
R-squared	0.291	0.291	0.291
Mother FE	Yes	Yes	Yes
Mean in sample	0.0921	0.0921	0.0918

Robust standard errors clustered at the DHS cluster level in parentheses. All regressions control for a multiple birth dummy, birth order fixed effects, and a linear trend in birth year.
** $p < .05$.
*** $p < .01$.
* $p < .1$.

in reducing mortality among children born in rural areas, among Muslim children, and among children born in Muslim areas. These findings would garner evidence for our expectation that aid contributes to reduce group inequalities in health access. That is, the effect of aid seems to be strongest for the most disadvantaged groups. However, we also know that the allocation of aid is to areas with less infant mortality, so the total effect on inequality is uncertain.

We conduct a series of robustness tests to our preferred specification (the one including mother fixed effects to control for selection). In particular we investigate whether the effects are ronust to distance and migration. In the Appendix we show that the results are robust to only including individuals within a distance of 200 km in the control group (Table A1). In Table A2 we restrict the sample to kids born to mothers that we know have always lived in the same area. This reduces the sample a lot partly because the question is not asked in all survey rounds (we have the information only for the surveys conducted in 1990, 2003, and 2008). Nevertheless, the coefficients are similar albeit the statistical significance is lower. In Table A3, we restrict the sample to health projects only. Having a health project in the vicinity is positively associated, though statistically insignificant, to infant mortality. In fact, we only have 18 health projects and the results are therefore so imprecisely estimated that we cannot reject that the effects are the same as in the baseline regression even though the signs are different. The relatively fewer dedicated health projects and the broader possible influence on child survival of improved education, sanitation, electricity and governance speak to the soundness of assessing the impact of aid more broadly.

In Table A4 we show that the effects seem to be driven by having at least one project in the area and the marginal effect of getting an additional project is negative but very small and statistically insignificant.

We further find that there is an effect on child mortality (children aged 0–5), on children aged 1–5 and on neonatal mortality (first month). For these outcomes we see that the heterogeneity point in the same direction as for infant mortality. These results are presented in the Appendix, Tables A6–A11.

8. Mechanisms

So far, we have found that aid seems to reduce infant mortality, and more so in rural and Muslim areas and for Muslim mothers. To investigate the mechanisms behind these findings we further analyze the effects of aid on wealth, employment, and education. As these factors are likely to be important for child survival, the analysis will give us an indication of possible intermediate factors. These variables are, however, not available for each birth year, but are asked to the mother in the year of interview. The analysis in this section is therefore slightly different as we use observations on the mother level at the time of interview As we only have one observation per mother for these variables, we will rely on the difference in difference strategy, i.e. the strategy comparing active to inactive areas. As seen in the analysis above (Table 2), the estimates when using this strategy in the main analysis are very similar to the estimates using mother fixed effects so we are confident that they capture most of the endogeneity concerns.

Table 5 shows that wealth seems to increase, but the difference is not statistically significant. Female employment seems to increase, however, but not necessarily cash employment. In column 4 we see that years of schooling are higher in active than in inactive areas. We can here be more precise and drop all women from the data that lived in an area that became active after they were 20 years old. This is not necessary but it is comforting to see that the effect is much stronger and almost doubled if we do so as seen by comparing the difference in difference estimates of column 4 and column 5.

In Table 6 we run separate regressions for Muslims and Christians as we have identified stronger effects for Muslim mothers. It is interesting to see that Muslim women increase their wealth, their employment, and their years of schooling whereas there are no corresponding effects for Christian women. If anything, their employment seems to be reduced, but this is only statistically significant at the 7% level. In the Appendix (Tables A10–A11) we present the same analyses for 25 km and find similar patterns.

In the Appendix we further present results showing that birthweight (in grams) is affected (Tables A14–A15). These results are

Table 5
Effects of aid projects on other variables of interest.

Variables	(1) Wealth	(2) Working	(3) Cash Paid	(4) School years	(5) School years (exp)
Active 50 km	8.425***	0.044***	0.042***	4.123***	4.715***
	(0.548)	(0.011)	(0.013)	(0.231)	(0.223)
Inactive 50 km	7.659***	0.017*	0.058***	3.466***	3.450***
	(0.419)	(0.009)	(0.011)	(0.182)	(0.182)
Observations	78,275	76,891	73,630	83,423	74,872
R-squared	0.150	0.002	0.035	0.132	0.132
Difference in difference	0.767	0.0275	−0.0165	0.657	1.265
F test: active-inactive = 0	1.613	7.154	1.773	6.876	27.07
p value	.204	.00754	.183	.00879	2.14e−07

Robust standard errors clustered at the DHS cluster level in parentheses. All regressions control for a multiple birth dummy, birth order fixed effects, and a linear trend in birth year.
** $p < .05$.
*** $p < .01$.
* $p < .1$.

Table 6
Effects of aid projects on other variables of interest for different groups.

Variables	(1) Musl. Wealth	(2) Musl. Working	(3) Musl. School years (exp)	(4) Chr. Wealth	(5) Chr. Working	(6) Chr. School years (exp)
Active 50 km	6.810***	0.052***	3.383***	6.851***	−0.015	2.341***
	(0.790)	(0.019)	(0.407)	(0.641)	(0.013)	(0.199)
Inactive 50 km	4.390***	0.014	1.378***	7.718***	−0.034***	2.350***
	(0.572)	(0.014)	(0.200)	(0.505)	(0.012)	(0.170)
Observations	38,552	38,345	37,907	38,393	37,018	35,497
R-squared	0.088	0.003	0.043	0.121	0.002	0.093
Difference in difference	24,201	0.0380	2.005	−8671	0.0189	−0.00914
F test: active-inactive = 0	7.187	3.568	21.43	1.944	3.424	0.00283
p value	.00743	.0591	3.96e−06	.163	.0645	.958

Robust standard errors clustered at the DHS cluster level in parentheses. All regressions control for a multiple birth dummy, birth order fixed effects, and a linear trend in birth year.
** $p < .05$.
* $p < .1$.
*** $p < .01$.

more difficult to interpret, however, as aid may affect the probability of being weighted in the first place. We actually find indications of this, see (Table A16). Furthermore, when regressing aid on the probability of ever being vaccinated using the fixed effects framework we see that aid increases the probability of having had any vaccination. Again, this increase is especially strong for kids of Muslim mothers and kids born in Muslim areas (See Tables A17–A18).

9. Conclusion

Local-level data on aid and health outcomes can be very useful for policymakers and practitioners, both when it comes to evaluating the effectiveness of health interventions and to inform decisions on how and where to allocate aid. We are the first to investigate the effects of aid on infant mortality using a sound identification strategy and the first to investigate this question using a local level spatial design. Combining georeferenced data on official development aid and infant mortality data from Nigerian household surveys, we find that children born to mothers who live in locations close to one or more aid projects indeed have a lower risk of dying before the age of 12 months. Furthermore, the general relationship between aid projects and infant mortality is stronger for less privileged groups like children of Muslim women, and for children living in rural and in Muslim-dominated areas. Aid thereby seems to reduce horizontal inequalities in a setting where such inequalities loom large. We also show, however, that aid is allocated to areas with less infant mortality to start with. At the very least, this implies that the potential of aid to reduce vertical inequalities has not reached its full potential. We further assess other effects of aid and find effects on wealth, female employment, and female education for Muslim mothers, but not for Christian mothers. These factors are likely to explain the heterogeneity in effects that we observe. Due to the small sample sizes we are restricted in how deep we can go into different mechanisms of the effects of aid and we particularly urge future studies to investigate the effects in other datasets, perhaps including several countries, to dig deeper into the details of which projects work best.

Conflict of interest

None.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.worlddev.2017.12.022.

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