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Chinese aid and local corruption☆

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A B S T R A C T

Considering the mounting criticisms concerning Chinese aid practices, the present paper investigates whether Chinese aid projects fuel local-level corruption in Africa. To this end, we geographically match a new geo-referenced dataset on the subnational allocation of Chinese development finance projects to Africa over the 2000–2012 period with 98,449 respondents from four Afrobarometer survey waves across 29 African countries. By comparing the corruption experiences of individuals who live near a site where a Chinese project is being implemented at the time of the interview to those of individuals living close to a site where a Chinese project will be initiated but where implementation had not yet started at the time of the interview, we control for unobservable time-invariant characteristics that may influence the selection of project sites. The empirical results consistently indicate more widespread local corruption around active Chinese project sites. The effect is seemingly not driven by an increase in economic activity, but rather seems to signify that the Chinese presence impacts norms. Moreover, Chinese aid stands out from World Bank aid in this respect. In particular, whereas the results indicate that Chinese aid projects fuel local corruption but have no observable impact on short term local economic activity, they suggest that World Bank aid projects stimulate local economic activity without any consistent evidence of it fuelling local corruption.

1. Introduction

Foreign aid has the potential of reducing global income inequality by transferring resources from rich to poor countries. Proponents of aid argue that it may save lives and even eliminate poverty (e.g. Sachs, 2006). Critics, on the other hand, argue that foreign aid is unlikely to have a positive transformative impact and that it may even act as to worsen institutions and thereby be harmful for development (e.g. Easterly and Easterly, 2006; Deaton, 2013). While trillions of dollars have been transferred in foreign aid since the 1950s, the empirical evidence of the effects of aid is highly disputed (see e.g. Roodman, 2007) and the debate has recently been labeled one of the most controversial in development economics (Qian, 2015). In the midst of this controversy, new actors are appearing, changing the very nature of aid. We analyze the effects of aid on a crucial mediating factor in the aid-growth nexus, namely corruption, for China, the largest and most influential of the new aid actors.

Recent years have seen a changing aid landscape with a sharp increase in development finance from non-Western donors, both in absolute terms and as a share of global foreign assistance (see e.g. Strange et al., 2015; Dreher et al., 2011; Dreher et al., 2015). With the explosion of Chinese funds, concerns over its donor practices have followed. Critics claim that Beijing uses their development finance to create alliances with the leaders of developing countries, to secure commercial advantages for their domestic firms, and to prop up corrupt and undemocratic regimes in order to gain access to their natural resource endowments (see the discussion in e.g. Tull, 2006; Kaplinsky et al., 2007; Naím, 2007; Pehnelt, 2007; Tan-Mullins et al., 2010; Marantidou and Glosserman, 2015). Others praise China for its responsiveness to recipient needs and its ability to get things done in a timely manner without placing an extensive administrative burden on strained public bureaucracies in the developing world (see the discussion in e.g. Bräutigam, 2009; Dreher et al., 2016).

Considering that China's influence on international aid policy is likely to increase even further by the creation of the Asian Infrastructure Investment Bank and the BRICS' New Development Bank

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(Dreher et al., 2016), evaluating the effects of their aid practices is central. Until very recently, however, there has been a lack of systematic empirical evidence on the effects of, and principles guiding, Chinese development assistance. Unlike the OECD-DAC donors, the Chinese government does not release detailed, project-level financial information about its foreign aid activities (Strange et al., 2013). This lack of transparency has made evaluation of Chinese aid notoriously difficult, and as a result, China's aid to Africa is the subject of much speculation. However, a new comprehensive data material (Strange et al., 2015) now allows for systematic quantitative analysis of Chinese aid flows.

We investigate whether Chinese development finance has an effect on local-level corruption in Africa. More specifically, we ask 1) whether the implementation of Chinese development projects gives an increase in corrupt activity around the project sites, 2) whether Chinese development projects are different in this respect, and if so, 3) what drives this difference.

To this end, we geographically match a new georeferenced dataset on the subnational allocation of Chinese development finance projects to Africa over the 2000–2012 period with 98,449 respondents from four Afrobarometer survey waves across 29 African countries. By comparing the corruption experiences of individuals who live near a site where a Chinese project is being implemented at the time of the interview to those of individuals living near a site where a Chinese project will appear in the future, but where implementation had yet to be initiated once the Afrobarometer covered that particular area, we get a difference-in-difference type of estimate that controls for unobservable time-invariant characteristics that may influence the selection of project sites.

The empirical results consistently indicate more widespread local corruption around active, as compared to not yet opened Chinese project sites. Replicating our analysis for World Bank projects, for which there is also geo-referenced data available for a large multi-country African sample, we do not observe an equivalent pattern. The observed donor heterogeneity in results is seemingly not driven by Chinese aid fueling economic activity to a greater extent than World Bank aid. Indeed, using satellite data on night time light to proxy for local economic activity, the results suggest that Chinese aid projects fuel local corruption but not short term economic activity, while they indicate the reverse – i.e. that projects stimulate local economic activity but do not seem to contribute to local corruption – for World Bank aid. Considering criticisms concerning China's aid practices and the World Bank's explicit anti-corruption policies, this is interesting.

Our paper relates to the literature on foreign aid and the quality of government, which provides mixed empirical evidence on the relationship between aid and corruption (see e.g. Svensson, 2000; Alesina and Weder, 2002; Tavares, 2003; Bräutigam and Knack, 2004; Djankov et al., 2008; Okada and Samreth, 2012; Asongu, 2012). A reason for the inconclusive results could be the tendency to study the relationship between aid and corruption – commonly defined as the misuse of public office for private gain (Rose-Ackerman, 1975) – at the country level. Considering the multitude of factors that could affect country level corruption, being interested in identifying possible corruption effects of receiving foreign assistance, a sensible approach is arguably to investigate sub-national variation in aid disbursements and corruption over time. Aid is not distributed evenly within countries, and while it may have clear effects on corruption in targeted local areas, this effect may be obscured by omitted variable bias or may not be sufficiently large to be measurable at the country level.

The present paper differs from the above studies in that it studies the local corruption effects of a multitude of aid projects in a large multi-country sample, focusing on the effects on citizen experiences with petty corruption around aid project sites rather than estimates of national aid inflows and corruption in government. As such, we contribute to an emerging literature using subnational geocoded aid data to examine the determinants and impacts of the allocation of foreign aid within countries (e.g. Findley et al., 2011; Francken et al., 2012; Dionne et al., 2013; Briggs, 2014; Jablonski, 2014; Öhler and Nunnenkamp, 2014; Dreher and Lohmann, 2015; and Briggs, 2017). In line with the results in this paper, but focusing on cross-sectional variation in one country, Brazys et al. (2017) find that the location of Chinese aid projects is associated with people more often reporting experiences with corruption in Tanzania. Focusing on the subnational allocation of Chinese aid for a large number of recipient countries over a 13 year period, however, our paper is closest to that of Dreher et al. (2016), who study elite capture of aid and find that Chinese aid is disproportionately allocated to the birth regions of African leaders.

To our knowledge, this is the first paper using geocoded project level data to systematically investigate the local corruption effects of Chinese development finance in a wide selection of African recipient countries. Hence, the paper also contributes to an emerging quantitative literature on the determinants and effects of China's aid allocation, most notably consisting of the pioneering work of Dreher, Fuchs, and various co-authors (Dreher and Fuchs, 2015; Dreher et al., 2015; Dreher et al., 2016). Considering China's increased presence in Africa and the mounting criticism concerning Chinese aid practices, empirical evidence on the possible corruption effects of Chinese development finance is central.

2. Chinese aid and local corruption

We suggest two principal channels through which aid projects may impact local corruption in recipient countries. First, the potential effect could work via economic incentives (e.g. Shleifer and Vishny, 1993; Glaeser and Saks, 2006), i.e. through the presence of donors affecting the costs and benefits of engaging in corrupt activity. Second, aid projects may impact local corruption by means of norm transmission.[1]

With respect to the former, donor involvement in an area could on the one hand increase local economic activity and thus the flow of resources that are up for grabs. The additional resource flows could stimulate corrupt activity among local actors and risk making the area a 'honey pot' attracting corrupt actors (see Karl, 2007). On the other hand, if a donor is committed to fighting corruption, and devote resources to monitoring and controlling corruption, its presence in an area could potentially increase the perceived costs of engaging in corruption.

Aid projects may also impact local corruption through norm transmission (see e.g. Hauk and Saez-Marti, 2002). By raising awareness of problems with corruption, and establishing standards of conduct that delegitimize and stigmatize corrupt practices, donors could potentially influence social norms and thereby instigate institutional change. That is, they not only fight corruption by raising its cost but also by managing to establish that it is wrong (see the discussion in Sandholtz and Gray, 2003). The anti-corruption movement among international organizations has indeed brought substantial attention to the fight to curb corruption, with likely implications at the local level where aid projects are being implemented.[2]

However, norm transmission might as well work in the other direction, legitimizing and fueling corruption. Indeed, there is evidence to suggest that norms are easier to change for the worse than for the better (Fisman and Miguel, 2007; Zhou et al., 2015). By stigmatizing corrupt practices a donor might be able to influence prescriptive norms, i.e. norms on how people ought to behave. Importantly, though, the donor's own behavior vis-à-vis local actors during the implementation phase could potentially also affect descriptive norms, i.e. norms merely describing some observable pattern of behavior among actors (Greenhill,

[1] See the parallel reasoning of Sandholtz and Gray (2003), on the impact of international integration on corruption.

[2] As described in Charron (2011), the mid 1990s saw the beginning of an 'anti-corruption movement' among major international donors, and today, many donors indeed use a 'zero tolerance for corruption' to signal a tough stance towards corrupt practices in recipient countries (De Simone and Taxell, 2014).

2010; Zhou et al., 2015). As described in Hauk and Saez-Marti (2002), statements such as 'I was corrupt but so was everybody else' reveal that a corrupt environment can serve as a justification for one's own corrupt behavior. Hence, the presence of a donor itself engaging in corrupt practices could potentially change descriptive norms on corruption.

In light of these arguments, two main features of Chinese aid could have implications for local corruption. First, China's well-known policy of non-interference in the domestic affairs of recipient countries suggests that they are unlikely to take an active role in fighting corruption in the same. The principle, which is clearly spelled out in official Chinese documents (State Council, 2014), is described as a way to promote local ownership of development policy. However, many observers consider the approach a convenient rationale for economic involvement in undemocratic and corrupt countries, and argue that it runs against attempts by the global aid-community to promote better governance in Africa (see e.g. Tull, 2006; Kaplinsky et al., 2007; Naím, 2007; Penhelt, 2007; Marantidou and Glosserman, 2015). Given the hands-off approach implied by the non-interference principle, it seems unlikely that the Chinese presence should involve increased monitoring and de-legitimization of corrupt practices.

Second, China has been accused of engaging in corrupt practices when implementing development projects, potentially suggesting that their presence could affect descriptive corruption norms for the worse. China tends to maintain control over development projects throughout the entire implementation phase, often using Chinese contractors for work performed in the recipient countries (see e.g. Bräutigam, 2009). This is relevant since Chinese firms operating abroad have been accused of using corrupt practices to win contracts away from more honest companies in recipient countries (Bräutigam, 2009). Indeed, in Transparency International's most recent Bribe Payer's Index (Transparency International, 2011), focusing on the extent to which companies from the world's leading economies engage in bribery when doing business abroad, only Russia scored worse than China.

Hence, both economic incentive- and normative arguments arguably speak in favor of Chinese aid projects fueling local corruption. In the next section we discuss how to approach this empirically.

3. Data and empirical strategy

To analyze the effects of Chinese aid on local corruption, we geographically match new spatial data on China's official financial flows to Africa over the period 2000–2012 to 98,449 respondents from 4 Afrobarometer survey waves in 29 African countries over the period 2002–2013.[3]

The data on Chinese aid projects is obtained from georeferenced project-level data of version 1.1 of AidData's Chinese Official Finance to Africa dataset, introduced by Strange et al. (2015) and geocoded by Dreher et al. (2016). Given that the Chinese government does not release official, project-level financial information about its foreign aid activities, this data is based on AidData's Tracking Underreported Financial Flows (TUFF) methodology. As described in great detail in Strange et al. (2013 and 2015), this is an open-source media based data collection technique, synthesizing and standardizing a large amount of information on Chinese development finance to African countries. While information extracted from public media outlets is of course an imperfect substitute for complete statistical data from official sources, the authors provide a careful description of how they dealt with challenges in the data collection process.[4] Furthermore, the only information we use is whether, where and when a project was implemented. That is, we do not make use of potentially sensitive and less reliable information on e.g. specific aid volumes or details from the implementation process. We are relatively confident in the reliability of this basic information. Despite the short time since the release of the dataset, the country-level data has already been used in a number of (forthcoming) publications (see e.g. Dreher et al., 2015; Dreher and Fuchs, 2015; Strange et al., 2015).

Dreher et al. (2016) geocoded the data, assigning latitude and longitude co-ordinates and information about the precision of the location identified (for details about the methodology used, see Strandow et al., 2011). While some development projects are implemented in a limited geographical area, such as a village or city, others are realized at more aggregate levels, such as a district or greater administrative region. Locations are recorded for each Chinese development project, but are coded into different categories depending on the degree of precision of the specified location (ranging from category 1 for co-ordinates to an exact location to 8 when the location is estimated to be a seat of an administrative division or the national capital, see Strandow et al., 2011). Since this paper focuses on local corruption effects of Chinese development projects we focus on projects with recorded locations coded as corresponding to an exact location or as 'near', in the 'area' of, or up to 25 km away from an exact location (precision categories 1 and 2 in Strandow et al., 2011).[5]

For comparability with other donors, our benchmark estimations focus on Chinese aid projects that have been classified as official development assistance (ODA). In order to qualify as ODA according to the OECD-DAC definition, an aid flow must be provided by official agencies to developing countries on the DAC list of ODA recipients. Moreover, it should be concessional in character, with a grant element of at least 25%, and its main objective should be the promotion of economic development of developing countries. Transactions which do not qualify as ODA, either because they are not primarily aimed at development or because they have a grant element of < 25%, are labeled 'other official flows', or OOF (OECD-DAC glossary, 2016). Due to the lack of official reporting on Chinese foreign aid activities, the classification used here is based on coders' defining a project as 'ODA-like' (as opposed to 'OOF-like', or 'vague official finance' when there is insufficient information to classify the project as either OOF- or ODA-like, see Strange et al., 2015). We also run estimations for total flows with no change in results (see Section 4.2).

Restricting our sample to include only ODA-like projects with precise geocodes and start-dates we cover 227 Chinese project sites.[6] As can be seen in Table A1, the projects included cover a wide range of sectors, the main ones being 'Health' (22%) and 'Transport and storage' (19%). Indeed, throughout the above sample restrictions, the largest shares of Chinese aid consistently go to 'Health', 'Transport and storage', 'Government and civil society' and 'Education'. Nevertheless, the sample of projects should not be seen as representative of all Chinese aid. 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 'Unallocated/unspecified' share, which constitutes 12% of overall ODA-like projects, is not part of our estimation sample, and neither are projects classified as 'Banking and financial services', 'Business and other services', 'Action relating to debt' and 'General budget support'.

We use the point coordinates in the aid data to link aid projects to local survey respondents in the Afrobarometer. For geo-locating the Afrobarometer survey respondents, we draw on the efforts of Knutsen

[3] Namely Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Cote D'Ivoire, Ghana, Guinea, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritius, Morocco, Mozambique, Namibia, Niger, Nigeria, Senegal, Sierra Leone, South Africa, Tanzania, Togo, Uganda, Zambia and Zimbabwe.

[4] See also Muchapondwa et al., 2014, for a validation using a 'ground-truthing' methodology.

[5] Doing so we consider a smaller selection of projects than e.g. Dreher et al. (2016), who focus their analysis on the first and second order regional division, i.e. also include Chinese projects coded with precision 3 or 4.

[6] In particular, 813 out of the 2046 ODA-like project sites in the database have geocodes in precision categories 1 and 2, and 227 out of these have information about the start-date of the project.

et al. (2016).[7] The coordinates of the surveyed Afrobarometer clusters (consisting of one or several geographically close villages or a neighborhood in an urban area) are used to match individuals to aid project sites for which we have precise point coordinates. We measure the distance from the cluster center points to the aid project sites and identify the clusters located within a cut-off distance of at least one project site.

The map in Fig. 1 shows the location of the 227 geocoded Chinese aid project sites and of the 8685 Afrobarometer clusters, each encircled by a 50 km buffer zone. While we have a good spread of both projects and survey data, some countries are not covered by the Afrobarometer. Furthermore, in some cases, aid projects are too far away from any survey cluster even if we have both types of information in the same country. 185 of the aid project locations are within 50 km of at least one Afrobarometer cluster.

The four Afrobarometer survey waves covered provide a unique opportunity to study the corruption experiences of African citizens over the recent decade. Specifically, our main dependent variables focus on individual experiences with corruption in dealing with public officials. That is, the focus is on individuals' direct experiences with petty corruption as opposed to their perceptions of corruption among public officials, which may suffer from bias due to incomplete information (Olken, 2009) or as highly corrupt environments normalize corruption which could lead to the amount of perceived corruption being lower (Knutsen et al., 2016). We employ two Afrobarometer questions on experiences with bribes. Respondents are asked if they, during the past year, have 'had to pay a bribe, give a gift, or do a favor to government officials in order to' a) 'Avoid a problem with the police (like passing a checkpoint or avoiding a fine or arrest)', b) 'Get a document or a permit'.[8] Based on these questions we construct two dummy variables indicating if the respondent has experienced the respective situations at least once during the past year. As seen in Table 1, 13% of the baseline sample, which after sample restrictions is 89,969 observations,[9] have paid a bribe to the police last year and 14% have paid a bribe for a permit last year. We also construct two corresponding ordinal variables ranging between 0 and 3, capturing the response categories 'Never', 'Once or twice', 'A few times', and 'Often'.

Our main explanatory variables, which will be described in greater detail below, focus on living near a Chinese project site – either a site where a project is being implemented at the time of the survey or a site where a project will be opened but where implementation had not yet been initiated at the time the Afrobarometer covered that particular area. Table 1 shows that 19% of the sample lives within 50 km of an active Chinese aid project and 9% lives within 50 km of a yet inactive project, without having any active projects in the same area.

3.1. Estimation strategy

Studying the causal effects of aid on local corruption comes with challenges. The distribution of aid within countries is not random, implying that some individuals and sub-national areas, with certain characteristics, will presumably be more likely to be targeted by aid than others. Of particular relevance for this study, pre-existing local corruption levels, and other factors correlated with corruption (such as population density, economic activity and infrastructure access) are likely to influence project location decisions. For instance, a donor not prepared to pay bribes may be less inclined to implement a project in a highly corrupt area. Or alternatively, a donor willing to engage in corrupt activity themselves may be more likely to set up aid projects (and possible connected business ties) in particularly corrupt locations. While the exact nature of the relationship is difficult to ascertain, it does not seem plausible to assume ex-ante that there is *no* relationship between project localization and the pre-existing institutional characteristics of project sites. The implication is that it is problematic to draw conclusions about the causal effect of aid on local corruption from a direct comparison of the corruption experiences of people living close to and far away from project sites.

In order to deal with these identification problems, we use a spatial–temporal estimation strategy resembling that in Knutsen et al. (2016).[10] In particular, we compare the corruption experiences of survey respondents living near sites where a development project is currently under implementation and those of respondents instead living near sites where a project will be opened but where implementation had not yet been initiated at the time the Afrobarometer covered that particular area.

While the fact that the Afrobarometer typically visits different areas in different years hinders us from following specific localities over time, before and after a project was initiated, with this estimation strategy we can still make use of the time variation in the data. Specifically, we utilize the fact that we know at what point in time and in what localities aid projects have been implemented, and that we have survey data covering different localities at different points in time. This allows us to identify respondents living in areas where a project was ongoing at the time of the survey and compare them with respondents living in areas where we know that a project will start, but where implementation had yet to begin at the time of the survey.

Assuming that corruption is affected within a cut-off distance, our main identification strategy includes three groups of individuals, namely those 1) within 50 km of at least one active project site (*active*), 2) within 50 km of a site where a project will start, but where implementation was yet to begin at the survey date and not close to any active projects (*inactive*), and 3) > 50 km from any project site (our omitted reference category in the regressions).[11] Our baseline regression is:

$$Y_{ivt} = \beta_1 \cdot active_{it} + \beta_2 \cdot inactive_{it} + \alpha_s + \delta_t + \gamma \cdot \mathbf{X}_{it} + \varepsilon_{ivt} \quad (1)$$

where the corruption outcome Y for an individual i in cluster v at year t is regressed – in the benchmark setup using easy-to-interpret OLS and linear probability models[12] – on a dummy variable *active* capturing whether the individual lives within 50 km of an active Chinese development project, and a dummy *inactive* for living close to a site where a Chinese project is planned but not yet implemented at the time of the survey. To control for variation in average corruption levels across time and space, the regressions include spatial fixed effects (α_s) – 352 sub-national region dummies – and year fixed effects (δ_t). To control for individual variation in experiences with corruption, we include a vector (X_i) of individual-level controls from the Afrobarometer. Our baseline set of individual controls are age, age squared, gender, urban/rural residence.[13] To account for correlated errors, the standard errors are clustered at the geographical clusters (i.e., at

[7] For a detailed description of the methodology used, see the Knutsen et al. (2016) paper. See also Nunn and Wantchekon (2011) who used geo-referenced data from Wave 3 of the Afrobarometer when studying effects of the slave trade on trust levels in Africa, and Deconinck and Verpoorten (2013), who replicated the analysis of Nunn and Wantchekon using Wave 4 of the Afrobarometer survey.

[8] As discussed in Isaksson (2015), the perception of what constitutes a bribe is likely to vary across cultures. In some developing countries, it has for instance been suggested that gift-exchange is customary in business transactions (Bardhan, 1997). However, the survey question asks about situations where the individual was required to offer the public official something in order to get the service, that is, before it was provided rather than as a courtesy afterwards. Moreover, country (or sub-national) fixed effects control for country variation in the average level of corruption and focus is on within-country variation in the same.

[9] The effective sample varies across estimations. However, as a point of reference, we refer to the sample of individuals retained in the regression of police bribes on the main variables, including region fixed effects (column 1 of Table 2) as the baseline sample.

[10] See also Kotsadam and Tolonen (2016).

[11] We exclude respondents who live within the cut-off distance of a site where the implementation of a project has been completed prior to the interview date.

[12] Instead calculating marginal effects after probit regressions does not change the interpretation of any results (results are available upon request).

[13] The results are robust to altering this set of controls, e.g. leaving out the control variables entirely or adding potentially endogenous controls for education, employment and economic standing as seen in columns 5 and 6 of Appendix Table A2.

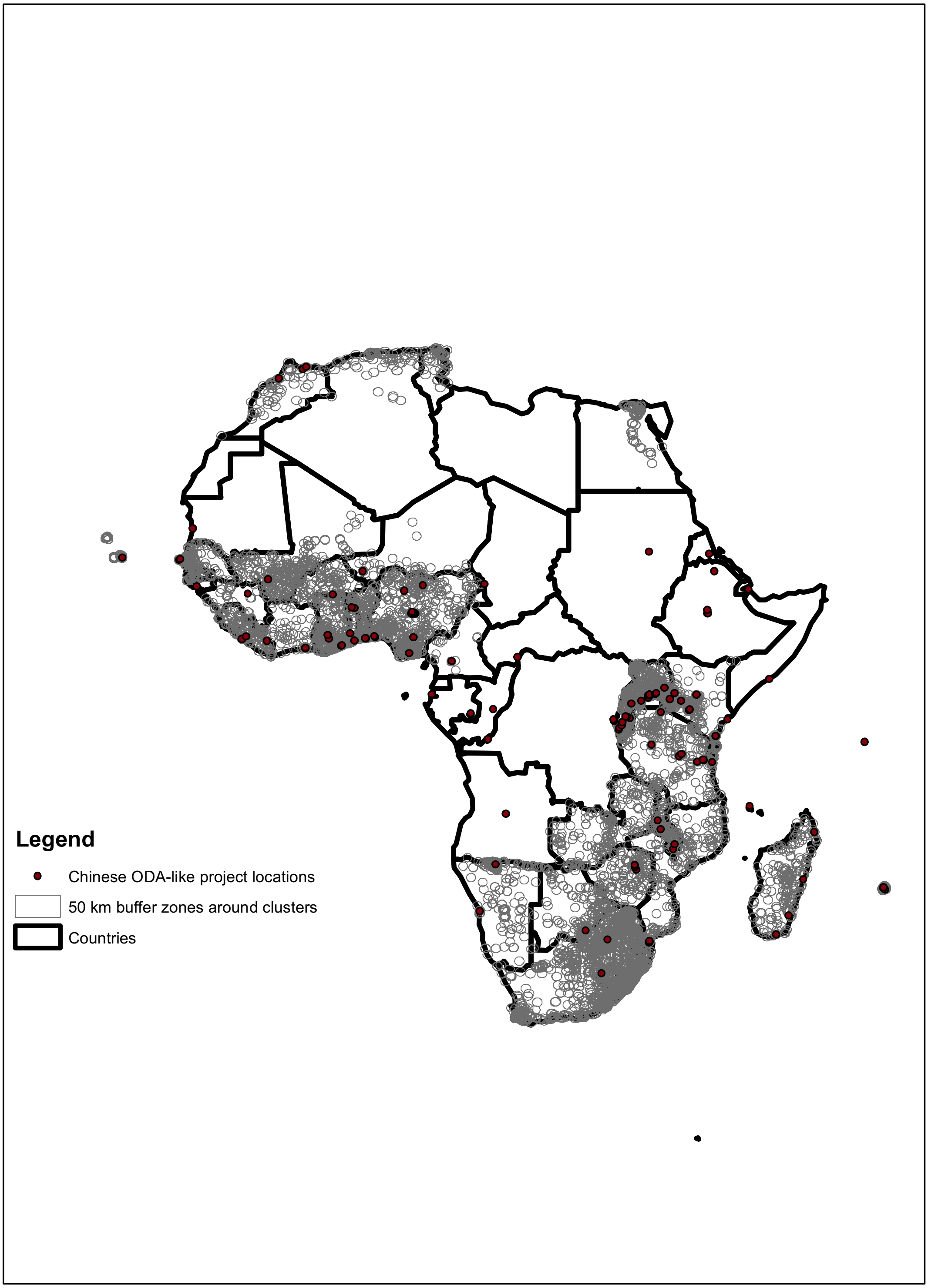

Fig. 1. Location of Chinese aid project sites and Afrobarometer survey clusters.

the enumeration areas which correspond to either a village, a town or a neighborhood).[14] For variable descriptions, see Table A3.

Interpreting the coefficient on *active* (β_1) in isolation as capturing an effect of Chinese development projects on local corruption would necessitate that the location of Chinese development projects is not correlated with pre-existing local corruption levels. As can be understood from the above discussion, we do not deem this assumption reasonable. Including *inactive* allows us to compare active project sites to other areas selected as locations for Chinese projects, but where the project was yet to be initiated at the time of the survey. That is, we can compare areas before a project has been implemented with areas where a project is currently under implementation, and not only areas close to and far away from project sites. For all regressions, our focus is thus on the parameter difference between *active* and *inactive* (i.e. $\beta_1 - \beta_2$, with associated test results), giving us a difference-in-difference type of

[14] The results are robust to clustering the standard errors at the region (439 clusters), at both the region and year level in a multi-way clustering (Cameron et al., 2011), at the cluster and year level, as well as at the country level (29 clusters) as seen in columns 1–4 of Appendix Table A2.

Table 1
Descriptive statistics based on the baseline sample for Chinese aid projects.

	N	Mean	SD
Main outcome variables			
Police bribe dummy	89,969	0,13	0,33
Police bribe ordinal	89,969	0,22	0,65
Permit bribe dummy	89,670	0,14	0,35
Permit bribe ordinal	89,670	0,22	0,62
Aid variables			
Distance to closest project (km)	89,969	173,78	182,43
active50	89,969	0,19	0,39
inactive50	89,969	0,09	0,29
active25	89,969	0,14	0,34
inactive25	89,969	0,06	0,23
Control variables			
age	89,969	36,66	14,63
age2 (divided by 100)	89,969	15,58	13,05
female	89,969	0,50	0,50
urban	89,969	0,42	0,49

The baseline sample is the sample of individuals retained in the regression of police bribes on the main variables, including region fixed effects (column 1 of Table 2). Variable descriptions are provided in Table A1.

Table 2
Chinese aid and local corruption (police and permit bribes).

Variables	(1)	(2)
	Bribe police dummy	Bribe permit dummy
active50	0.035***	0.027***
	(0.008)	(0.007)
inactive50	−0.006	0.006
	(0.008)	(0.009)
Observations	89,969	90,050
R-squared	0.096	0.080
Baseline controls	YES	YES
Year FE	YES	YES
Country FE	NO	NO
Region FE	YES	YES
Difference in difference	0.041	0.021
F test: active50-inactive50 = 0	20.352	4.907
p value	0.000	0.027

The baseline controls are age, age-squared, urban, and female. The "Difference in difference" result gives the difference between active and inactive areas and we present the associated *F*-test and the p-value of the *F*-test. Robust standard errors (clustered by the survey clusters) in parentheses; *** $p < .01$, ** $p < .05$, * $p < .1$.

measure[15] that controls for unobservable time-invariant characteristics that may influence selection into being a Chinese project site.

A potential concern would be if active/inactive project status picks up project timing and projects starting later differ systematically from projects starting earlier. Here it is important to note that there is no direct correspondence between when a project was implemented and whether it is coded as active or yet inactive; active/inactive status depends on project status at the time the Afrobarometer survey covered the particular area in question. To illustrate, suppose China implements two projects in a country, one starting in, say, 2005 and one starting in, say, 2009. This does not necessarily imply that the early project is coded as active and that the later project is coded as yet inactive. Rather, if the project implemented earlier is in a locality surveyed before that, in a pre-2005 Afrobarometer survey wave, this means that the project had not begun at the time of the interview and thus that it will be coded as yet inactive. By the same reasoning, if the project implemented in 2009 is in a locality surveyed by the Afrobarometer after that, this implies that the project had begun at the time of the interview and thus that it will be coded as active. Furthermore, this type of time variation – with some areas being surveyed earlier and others later – exist within as well as across survey waves, meaning that each of the individual Afrobarometer survey waves covered contains observations connected to both active and inactive Chinese project sites.

Considering the concern that projects implemented later may differ systematically from projects implemented earlier, this is reassuring. That said, however, there is an over-representation of respondents connected to active project sites in the later survey waves, why the possible effects of project timing will be evaluated in Section 4.3, including project fixed effects estimations for a smaller sub-sample of projects locations for which we have data on corruption from both before and after the Chinese aid project started.

Being interested in whether Chinese development projects leave a footprint on local corruption, we need to make an assumption about the geographical reach of this mark. If Chinese development projects affect local corruption, individuals traveling to nearby market places and dealing with nearby local authorities are likely to experience the results. Individuals living sufficiently far from a project site, however, should not. As discussed in Knutsen et al. (2016), the appropriate cut-off distance from a project – within which an individual will be considered treated – is an empirical question, and a trade-off between noise and size of the treatment group. With a too small cut-off distance, we get a small sample of individuals linked to active and (in particular) inactive project sites. On the other hand, a too large cut-off distance would include too many untreated individuals into the treatment group, leading to attenuation bias. The choice of a 50 km cut-off follows the main specification in Knutsen et al. (2016), but we also present results using an alternative cut-off of 25 km.

4. Results

4.1. Main results: Chinese aid and local corruption

The results indicate that Chinese aid projects fuel local corruption. Table 2 presents the results of our baseline regressions, focusing on experiences with corruption when dealing with the police (Column 1) and when applying for documents and permits (Column 2) during the past year, including the baseline individual controls, year fixed effects and 439 sub-national region dummies.

Looking at the coefficients on *active*, we can note that living within 50 km of sites where Chinese projects are currently being implemented is, indeed, associated with a greater probability of having experienced corruption. In particular, compared to individuals who do not live close to any Chinese project site, respondents with an active project site in their vicinity are 3.5 percentage points more likely to have paid a bribe when dealing with the police and 2.7 percentage points more likely to have done so in order to get a document or permit.

As noted, however, interpreting the coefficient on *active* in isolation as capturing an effect of Chinese development projects on local corruption requires that the location of Chinese development projects is not correlated with pre-existing local corruption levels, an assumption which we do not deem plausible. In order to account for the likely endogenous placement of projects we instead compare the experiences with corruption in areas close to sites where a Chinese project was being implemented at the time of the survey (*active*) with those in areas close to sites where a Chinese project will take place but where implementation was yet to be initiated at the time of the interview (*inactive*).

Looking at the coefficients on *inactive*, we can note that unlike in areas with active Chinese projects, we here see no clear divergent pattern in corruption experiences. Hence, there is no evidence that the Chinese choose to establish their projects in areas that stand out in

[15] Comparing the difference between post-treatment individuals (with an active Chinese project within 50 km) and control individuals (with no Chinese project – active or inactive – within 50 km) with the difference between pre-treatment individuals (with a yet inactive Chinese project within 50 km) and control individuals within the same country/region and year (due to country/region and year fixed effects).

terms of pre-existing corruption levels. Nevertheless, we should account for the strong possibility that sites selected for Chinese development projects differ from other areas in respects relevant for local corruption.

As it turns out, the difference-in-difference estimates ($\beta_1 - \beta_2$) and associated test results presented in the bottom rows of Table 2 clearly indicate more widespread local corruption close to active compared to yet inactive Chinese project sites. In comparison with people in the same region/province living close to yet inactive Chinese project sites, individuals living near sites where Chinese projects are currently being implemented are 4.1 percentage points more likely to have paid a bribe when dealing with the police. For bribes when applying for documents and permits, the equivalent difference is 2.1 percentage points. In both cases, the parameter differences are clearly statistically as well as economically significant, implying a 32% increase in bribes to the police and a 15% increase in bribes for permits (Table 1 gives the average shares reporting to have paid the concerned bribes).

4.2. Sensitivity analysis

The finding that Chinese aid projects fuel local corruption is stable across a wide range of specifications and sub-samples. The results of a first set of robustness tests are presented in Table 3, for police bribes (Panel A) and permit bribes (Panel B) respectively. First, we test whether altering the cut-off distance from project sites changes our results (Column 1). Using a 25 km cut-off the results still indicate more widespread corruption near active as compared to inactive Chinese project sites, the differences being highly statistically significant both for police and permit bribes.

The same tendency is revealed in Fig. 2, where we plot the levels of corruption as a function of the distance to the closest aid project in kilometers. Each dot represents a local average so that there are equally many observations in each of the 20 dots of each color (red for areas close to active projects and blue for areas close to inactive projects). We can note that the closer we get to the project site, the greater is the corruption difference between active and inactive areas. We also see that the difference between active and inactive areas decreases with distance and that the lines eventually cross. This pattern holds for both types of bribes.

In Column 2 we use the equivalent ordinal dependent variables described in Section 3. Compared to the dummies used as dependent variables in the benchmark setup, these variables have the advantage that they contain more information on the prevalence of corruption, but arguably do not come with an equally straightforward interpretation. The results remain qualitatively similar. In particular, the statistically significant difference between *active* and *inactive* is 0.073 for police bribes, which, put in relation to a sample mean of approximately 0.22 (as shown in Table 1), is sizeable. For permit bribes the difference is 0.029 and statistically significant only at the 10% level.[16]

In Column 3 we restrict the sample to respondents that have a Chinese project – either active or yet inactive – nearby, thus comparing the corruption experiences of these people directly rather than in relation to respondents with no Chinese project nearby. As the sample is restricted, the coefficient on *active* gives us the difference between *active* and *inactive* directly. We see that the result for police bribes are similar, despite the large reduction in sample size. For permit bribes, *active* is not statistically significant.[17]

Table 3
Initial robustness checks.

Variables	(1)	(2)	(3)	(4)
	Dummy	Ordinal	Dummy	Dummy
Panel A: Police bribes				
active25	0.031***			
	(0.009)			
inactive25	−0.009			
	(0.010)			
active50		0.061***	0.048***	0.041***
		(0.015)	(0.015)	(0.007)
inactive50		−0.012		−0.003
		(0.016)		(0.007)
Observations	89,969	89,969	25,261	89,969
R-squared	0.095	0.099	0.108	0.096
Baseline controls	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Country FE	NO	NO	NO	NO
Region FE	YES	YES	YES	YES
Difference in difference	0.041	0.073		0.044
F test: active-inactive = 0	14.688	16.524		0.000
p value	0.000	0.000		33.508
Panel B: Permit bribes				
active25	0.018**			
	(0.009)			
inactive25	−0.004			
	(0.009)			
active50		0.035***	0.012	0.023
		(0.013)	(0.014)	(0.019)
inactive50		0.006		
		(0.016)		
Observations	90,050	90,050	25,250	14,981
R-squared	0.080	0.080	0.097	0.106
Baseline controls	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Country FE	NO	NO	NO	NO
Region FE	YES	YES	YES	YES
Difference in difference	0.022	0.029		
F test: active25-inactive25 = 0	4.028	2.831		
p value	0.045	0.093		

Robust standard errors (clustered by the survey clusters) in parentheses; *** $p < .01$, ** $p < .05$, * $p < .1$; All estimations include baseline controls (are age, age-squared, urban, and female), region- and year fixed effects. The "Diff-in-diff" result gives the difference between active and inactive areas and we present the associated *F*-test and the p-value of the *F*-test. All columns present results estimated on the baseline sample, except column 3 which uses a smaller sample of respondents that are linked to either active or inactive projects. Column 4 presents results of a wider set of Chinese projects including both 'ODA-like', 'OOF-like' and 'vague' official finance.

For comparability with other donors, we restrict the benchmark analysis to ODA-like projects. A failure to make the distinction between more and less concessional Chinese flows has been criticized, most forcefully by Dreher et al. (2015), who argue that it has resulted in analysts making 'apples-to-dragon fruits' comparisons between Chinese and Western 'aid'. Nonetheless, in Column 4 we can note that our results are robust to also including the OOF-like and 'vague official finance' projects.[18]

It is often suggested that Chinese aid is likely to be tied to natural resource extraction (see e.g., Brazys et al. (2017). While recent studies actually find no support for this claim at the national (Dreher and Fuchs, 2015) and regional levels (Dreher et al., 2016), we nonetheless want to investigate whether resource extraction is an important time varying confounder in our setting. Using geocoded and time-varying

[16] The results (available upon request) are qualitatively similar and the conclusions remain the same if we use ordered logit or ordered probit models to take into account that the distance between the categories are not cardinal.

[17] We have also tested the robustness of these results to different lagged specifications as the bribe payments refer to bribes being paid 'during the past year'. The results (available upon request) are very similar if we either classify the active areas that are surveyed within a year after the project start as missing or if we classify these observations as inactive. We further test and confirm that the results are robust to restricting the control inactive areas to include only those surveyed the year before or within five years before project start (results are available upon request).

[18] Table A5 shows the results of estimations using three alternative bribe outcomes. In particular, the respondents are asked if they have had to pay a bribe: for school placement, to get medicine or medical attention, and for water or sanitation services. While these variables are interesting we do not include them in the baseline specification as they are not part of all survey rounds. Nevertheless, we can note that the results, while less precisely estimated, point in the same direction.

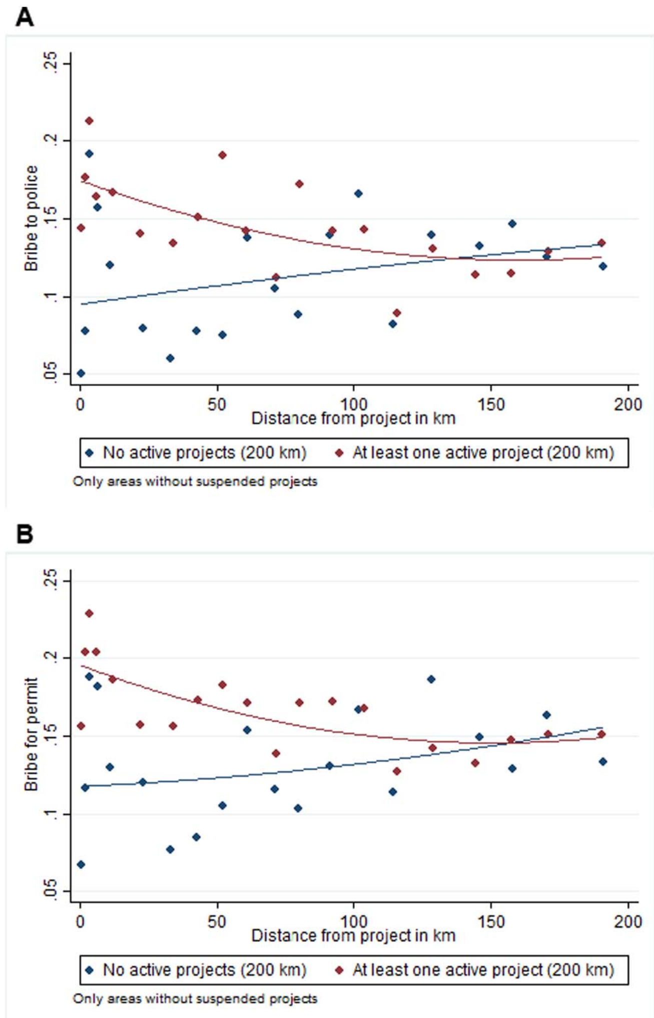

Fig. 2. Corruption and distance to the closest active or inactive aid project site (in km).

data on all industrial scale mines in Africa from the Raw-Minerals-Database (see Knutsen et al., 2016, for a detailed description of the data), in Appendix Table A4 we show that Chinese aid is allocated to areas with less rather than more mining and that it has no effect on the probability of a mine opening in the area (column 1). We also see that controlling for active and inactive mines does not alter our results and that both factors seem to have independent effects on local level corruption (columns 2 and 3).

4.3. Project timing

Next, we consider possible effects of project timing. While the year dummies included in all regressions will control for general time trends in corruption, a potential concern would be if active/inactive project status picks up project timing and there are timing effects relating specifically to the evolution of Chinese aid. In particular, projects implemented later may differ systematically from projects implemented earlier, either because the nature of Chinese aid has changed over the period or because the project sites selected earlier differ from those selected later.

As discussed in Section 3.1, whether a project is coded as active or inactive depends on when the Afrobarometer surveyed the particular area in question, meaning that there is no direct correspondence between when a project was implemented and active/inactive status. That said, however, there are more respondents connected to active project sites in the later Afrobarometer survey waves. In the last wave, conducted 2011–2013, implementation of most covered aid projects had begun, meaning that by construction the great majority of respondents living within the cutoff distance of a Chinese project site are connected to an active project site. In particular, 53% of the individuals connected to active projects were surveyed in wave 5, 25% in wave 4, 12% in wave 3 and 9% in wave 2.

To investigate whether our results are affected by a different character of Chinese aid projects implemented, or project sites selected, early and late in the covered period, columns 1–6 of Table 4 present the results of our baseline regressions focusing on sub-samples containing respondents from consecutive survey waves only (i.e. waves 2–3, 3–4 and 4–5, respectively). The results are remarkably stable.[19] Furthermore, in none of the estimations the coefficient on *inactive* comes out statistically significant, suggesting that the pattern observed in the full sample – i.e. that areas selected for Chinese project sites do not stand out in terms of pre-existing levels of corruption – does not change over the period. Hence, while we cannot rule out that Chinese aid has evolved over time, our results do not appear to be driven by a distinct shift in Chinese aid practices or in the character of sites selected for Chinese aid projects.[20]

We further investigate project timing in columns 7 and 8 of Table 4, presenting results of estimations investigating the possible influence of project duration for those living close to active project sites, and years until project start for those living close to yet inactive project sites.[21] The coefficient on project duration is positive and statistically significant, suggesting that (within the limited time frame considered here) respondents experience more corruption the longer the active project in their vicinity has been going on. This is reasonable considering that the corruption effects of projects are likely to operate with a lag. Years until project start – applying for those with yet inactive projects within the cut-off distance – is, however, not significantly related to corruption, thus again providing no evidence of a time trend in selection of project sites.

Finally, we run project fixed effects estimations, restricting the sample to areas that have observations from both before and after a Chinese aid project started. As noted, the Afrobarometer is not a panel, and only in some cases happens to revisit the same localities in different survey waves. Hence, while an advantage of using project fixed effects is that it allows us to evaluate variation in corruption occurring around a project site before and after the project was initiated, an important drawback is that we lose a large share of our sample; there are only 40 project locations for which we have data on corruption from both before and after project start. Nonetheless, the results when using project fixed effects in columns 9 and 10 of Table 4, while less precisely estimated, still suggest higher levels of corruption around active Chinese aid project sites, thus adding further support to our findings.[22]

To summarize our findings so far, they consistently indicate that Chinese aid projects fuel local corruption around project sites. In the next section we will explore the theoretical mechanisms potentially underlying this result.

[19] Running equivalent regressions focusing on individual survey wave sub-samples (available upon request), this difference in local corruption comes out economically and statistically significant in rounds 3 and 4, which makes sense since this is where we have most variation in our main explanatory variables *active* and *inactive*.

[20] The results are also robust to including regional specific linear time trends and region-year fixed effects (see columns 7–8 of Appendix Table A2).

[21] We consider the start year of earliest starting project within 50 km. Project duration is defined as interview year - start year, when this difference is positive (in order to keep them in the full sample estimations, we give zeros to those with only yet inactive projects or no project within the distance). Correspondingly, years until project start is defined as the absolute value of interview year - start year, when this difference is negative (and equivalently, in order to keep them in the full sample estimation, we give zeros to those with an already started project or no project within the distance).

[22] Note that since we now focus on variation over time in specific project sites, we can directly interpret the coefficient on active.

Table 4
Chinese aid and local corruption: timing effects.

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Police	Police	Police	Permit	Permit	Permit				
	Waves 2 and 3	Waves 3 and 4	Waves 4 and 5	Waves 2 and 3	Waves 3 and 4	Waves 4 and 5	Bribe police	Bribe permit	Bribe police	Bribe permit
active50	0.058***	0.066***	0.033***	0.041***	0.043***	0.029***	0.017	0.007	0.033**	0.023
	(0.016)	(0.011)	(0.010)	(0.012)	(0.011)	(0.009)	(0.011)	(0.010)	(0.016)	(0.019)
inactive50	−0.013	−0.001	−0.001	−0.009	0.005	0.018	−0.003	0.023		
	(0.008)	(0.009)	(0.016)	(0.008)	(0.009)	(0.018)	(0.014)	(0.016)		
Project duration							0.005***	0.006***		
							(0.002)	(0.002)		
Time until start							−0.002	−0.006		
							(0.004)	(0.004)		
Observations	33,142	49,705	56,810	33,149	49,752	56,890	89,969	90,050	14,984	14,981
R-squared	0.106	0.097	0.110	0.081	0.066	0.098	0.096	0.080	0.112	0.106
Baseline controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country FE	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO
Region FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Project FE	NO	NO	NO	NO	NO	NO	NO	NO	YES	YES
Difference in difference	0.070	0.068	0.034	0.049	0.039	0.011	0.0196	−0.0155		
F test: active-inactive = 0	16.735	25.506	4.515	12.246	8.926	0.419	1.514	0.802		
p value	0.000	0.000	0.034	0.000	0.003	0.517	0.219	0.370		

Robust standard errors (clustered by the survey clusters) in parentheses; *** p < .01, ** p < .05, * p < .1; All estimations include baseline controls (are age, age-squared, urban, and female) region and year fixed effects. The "Diff-in-diff" result gives the difference between active and inactive areas and we present the associated *F*-test and the p-value of the *F*-test. In columns 9 and 10 project fixed effects are also added and the sample is one for which there are respondents both before and after project start.

4.4. Exploring theoretical mechanisms

Considering China's alleged lax attitudes towards corruption and suggested use of corrupt practices when implementing development projects, we argued that both economic incentive- and normative arguments speak in favor of Chinese aid projects fueling local corruption. While the data does not allow us to clearly distinguish between these two channels we can explore suggestive evidence speaking for or against the respective mechanisms.

If the increase in corruption around aid project sites is primarily due to a surge in economic activity and thus in the flow of resources that are up for grabs, we would expect to observe an effect of Chinese aid projects on economic activity, and of economic activity on corruption. To proxy for local economic activity we use satellite data on nighttime light. Following Knutsen et al. (2016) we use data on median and average light within a 50 km buffer around each Afrobarometer cluster. This measure has been shown to correlate with economic activity at both the country and sub-national level (e.g. Henderson et al., 2012), and is available for every square kilometer and year between 1992 and 2010. Dreher and Lohmann (2015) and Dreher et al. (2016) have previously used this data to measure the effects of aid on regional economic development.

As the measure of nighttime light is at the cluster level we collapse the data accordingly. Column 1 of Table 5 shows that the baseline results are robust to this. Since the concerned data on nighttime light does not reach beyond 2010 the sample is further reduced. Column 2 shows that this has little impact on our results. In column 3 we see that areas with active and inactive Chinese project sites do not differ in terms of their median levels of night time light. While speaking against the idea that the increase in local corruption around Chinese project sites is driven simply by a surge in economic activity, considering the limited timespan under study (and the general lack of in-depth investigation) it should not be taken to indicate that Chinese aid has no effect on economic activity. We further show that there is no relationship between paying a bribe and the median level of light in an area (Column 4). Furthermore, controlling for the median level of light does not reduce the strength of our relationship between aid projects and bribes (Column 5) and there does not seem to be any differential relationship between economic activity and corruption in active aid non-active aid areas (Column 6).[23] Hence, we find no evidence to suggest that the relationship between Chinese aid and corruption is driven merely by increased economic activity.

Similarly, we find no indication that the results are driven by an increased tendency to apply for documents and permits or to be involved with the police near active Chinese project sites. Running estimations using dummy variables capturing having no experience with applying for a documents or permit or to have been in contact with the police as dependent variables (see columns 1 and 2 of Table A6) the results in fact suggest that individuals living close to active as compared to inactive Chinese project sites tend to have less experience of the concerned activities. That is, the results indicate that people living near active Chinese project sites are less involved with the police and with applying for documents and permits, but still experience more corruption in connection to these activities. An interpretation of this finding could be that the increase in corruption discourages people from applying for documents and permits and makes them avoid the police.

Neither do we find any evidence that the results are driven by increased resource flows making the project areas into 'honey pots' attracting corrupt actors. To check if the police bribe results are driven by more police officers or police stations in the area, we investigate whether the survey enumerator has seen any police station or police in the survey cluster (columns 3 and 4 of Table A6). As it turns out, there are, if anything, fewer police stations in the active aid areas than in the inactive aid areas.

Our second suggested mechanism focused on norm transmission. We proposed that Chinese aid projects might fuel local corruption since China's non-interference policy implies that they are unlikely to affect

[23] We reach similar conclusions and the results are very similar if we instead use average luminosity instead of the median luminosity or if we use the continuous measure of corruption instead of a dummy.

Table 5
Nighttime light results for Chinese aid.

Panel A: Police bribes

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Bribe police dummy	Bribe police dummy	Median Light	Bribe police dummy	Bribe police dummy	Bribe police dummy
active50	0.035***	0.061***	0.484**		0.061***	0.068***
	(0.008)	(0.010)	(0.215)		(0.010)	(0.011)
inactive50	−0.005	−0.008	0.569***		−0.008	
	(0.008)	(0.008)	(0.197)		(0.008)	
median				0.000	0.000	0.000
				(0.001)	(0.001)	(0.001)
Active * median						−0.002
						(0.001)
Observations	9467	6861	6862	6861	6861	6861
R-squared	0.317	0.299	0.786	0.294	0.299	0.300
Difference in difference	0.040	0.068	−0.085		0.068	
F test: active-inactive = 0	15.801	31.240	0.120		31.258	
p value	0.000	0.000	0.729		0.000	

Panel B: Permit bribes

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Bribe permit dummy	Bribe permit dummy	Median Light	Bribe permit dummy	Bribe permit dummy	Bribe permit dummy
active50	0.028***	0.046***	0.484**		0.046***	0.051***
	(0.008)	(0.010)	(0.215)		(0.010)	(0.010)
inactive50	0.004	−0.003	0.569***		−0.003	
	(0.008)	(0.008)	(0.197)		(0.008)	
median				0.000	−0.000	0.000
				(0.001)	(0.001)	(0.001)
Active * median						−0.002*
						(0.001)
Observations	9468	6862	6862	6862	6862	6862
R-squared	0.290	0.247	0.786	0.244	0.247	0.247
Difference in difference	0.024	0.048	−0.085		0.048	
F test: active-inactive = 0	6.272	17.779	0.120		17.783	
p value	0.012	0.000	0.729		0.000	

Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** p < .01, ** p < .05, * p < .1; All estimations include the baseline controls (age, age-squared, urban, and female), year fixed effects and region fixed effects. The "Difference in difference" result gives the difference between active and inactive areas and we present the associated *F*-test and the p-value of the *F*-test.

prescriptive norms in a direction delegitimizing corruption, and their alleged use of corrupt practices in recipient countries risk affecting descriptive norms in a way that legitimizes corruption. Ideally, we would want a measure capturing corruption norms, in order to investigate whether people in areas close to active Chinese project sites have become more accepting of corruption. The closest we get to this is a question focusing on whether the media should investigate and report on corruption, available in rounds 4 and 5 of the Afrobarometer. While not perfect, it could for instance proxy for perceived level of corruption, it could help shed light on to what extent respondents take the issue seriously. Column 1 of Table 6 presents results of estimations using a dummy variable for believing that the media should investigate and report on corruption – as dependent variable (using the benchmark set of explanatory variables). We see a statistically significant difference between individuals living close to active and yet inactive project sites. According to this estimation, individuals living near active project sites are indeed less likely to report that media need to do so, possibly revealing more accepting attitudes towards corruption. The fact that our results consistently indicate that Chinese projects fuel corrupt activity around project sites arguably speaks against the alternative interpretation that the media question simply picks up perceived level of corruption. While norms are generally seen as relatively persistent, there is some evidence to suggest that they are easier to change for the worse than for the better (Fisman and Miguel, 2007; Zhou et al., 2015). Hence, unlike the empirical results on economic activity, which

Table 6
Aid and corruption norms for Chinese and World Bank aid.

Variables	(1)	(2)
	Media should report corruption Chinese aid	Media should report corruption World Bank aid
active50	−0.012	0.024**
	(0.014)	(0.011)
inactive50	0.050***	0.018
	(0.015)	(0.015)
Observations	54,843	54,892
R-squared	0.060	0.062
Baseline controls	YES	YES
Year FE	YES	YES
Country FE	NO	NO
Region FE	YES	YES
Difference in difference	−0.062	0.006
F test: active50-inactive50 = 0	11.188	0.194
p value	0.001	0.660

The baseline controls are age, age-squared, urban, and female. The "Difference in difference" result gives the difference between active and inactive areas and we present the associated *F*-test and the p-value of the *F*-test. Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** p < .01, ** p < .05, * p < .1.

suggested no effect of Chinese aid projects, these estimations could be said to provide some, admittedly suggestive, evidence that Chinese aid projects affect norms in a way legitimizing corruption.

Summing up our results so far, they consistently indicate that Chinese aid projects fuel local corruption. Moreover, we find no evidence that the effect is driven simply by an increase in economic activity. Rather, suggestive evidence arguably points in favor of that the Chinese presence impacts local norms.

Is Chinese aid different in this respect, or is all aid similar? In the next section we compare China to a major Western donor, running equivalent estimations for World Bank aid projects for which there is also geo-referenced data available for a large multi-country African sample.

4.5. Chinese and World Bank aid compared

As it turns out, we do not find an equivalent pattern around World Bank project sites.[24] Table 7 presents the results of regressions for police bribes (Panel A) and permit bribes (Panel B). While the *active* coefficient is often positive and statistically significantly different from zero, it is not different from the *inactive* coefficient (except at the 10% level in one specification). Furthermore, the effect is statistically insignificant in the project fixed effect specification for police bribes and actually negative and statistically significant at the 10% level for permit bribes. Hence, there is no consistent evidence of World Bank projects fueling local corruption.

On the other hand, Table 8 shows that in contrast to Chinese aid, World Bank projects seem to increase the level of economic activity in the areas as measured by nighttime light (column 3). Hence, although the nighttime light results should not be given too much emphasis given the limited timespan under study, it is interesting to note that whereas they indicate that Chinese aid projects fuel local corruption but have no observable impact on local economic activity, they suggest a more favorable pattern for World Bank aid projects, namely that they stimulate local economic activity while there is no consistent evidence of a corresponding corruption effect.[25] Furthermore, for World Bank aid projects we find no statistically significant difference in the active aid areas as compared to in the inactive aid areas with respect to either police presence or experience with police or permit situations for the World Bank aid (Table A7). Our results on light emissions differ from previous analyses that have investigated the relationship at the regional level. Dreher and Lohmann (2015) find no causal effects of World Bank aid on light at the administrative region level and Dreher et al. (2016) find an effect of Chinese aid on regional light emissions. There are several possible reasons for our results being different. First of all, we measure the effects at a lower level of aggregation, at the cluster- rather than at the regional level. Secondly, the light results in Dreher et al. (2016) are local average treatment effects where they measure the effect of increased aid due to increased Chinese steel production. It is possible that such aid has different effects than Chinese aid in general. Similarly, the compliers in Dreher and Lohmann (2015) are areas that receive changes in aid due to crossing a threshold value for receiving International Development Association's concessional aid. As there are few such crossings in Africa, the results speak little to the effects of World Bank aid in Africa. Thirdly, we also have different samples as we focus on areas where we also have corruption data, i.e. buffer zones around our Afrobarometer clusters, and on Chinese projects with precise geocodes and start dates. However, we should note that the question of whether aid impacts local economic activity is not the main focus of the present paper and clearly warrants careful investigation in a study of its own. Moreover aid from the World Bank does not make people less likely to think that media should investigate and report on corruption (column 2 of Table 6), if anything the results point in the opposite direction.

Table 7
World Bank aid and local corruption.

Variables	(1)	(2)	(3)	(4)	(5)
	Dummy	Dummy	Ordinal	Dummy	Dummy
Panel A: Police bribes					
active50	0.017***		0.032***	0.011	0.007
	(0.006)		(0.011)	(0.007)	(0.018)
inactive50	0.006		0.012		
	(0.006)		(0.012)		
active25		0.012**			
		(0.005)			
inactive25		0.010			
		(0.008)			
Observations	90,022	90,022	90,022	65,381	10,457
R-squared	0.089	0.089	0.087	0.092	0.124
Baseline controls	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Country FE	NO	NO	NO	NO	NO
Region FE	YES	YES	YES	YES	YES
Project FE	NO	NO	NO	NO	YES
Difference in difference	0.011	0.002	0.020		
F test: active-inactive = 0	2.849	0.819	2.675		
p value	0.091	0.819	0.102		
Panel B: Permit bribes					
active50	0.023***		0.041***	0.003	−0.031*
	(0.006)		(0.010)	(0.008)	(0.019)
inactive50	0.017***		0.026**		
	(0.006)		(0.011)		
active25		0.014***			
		(0.005)			
inactive25		0.015**			
		(0.007)			
Observations	90,099	90,099	90,099	65,424	10,458
R-squared	0.072	0.072	0.069	0.071	0.109
Baseline controls	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Country FE	NO	NO	NO	NO	NO
Region FE	YES	YES	YES	YES	YES
Project FE	NO	NO	NO	NO	YES
Difference in difference	0.006	−0.001	0.014		
F test: active-inactive = 0	0.788	0.018	1.551		
p value	0.375	0.894	0.213		

Robust standard errors (clustered by the survey clusters) in parentheses; *** $p < .01$, ** $p < .05$, * $p < .1$; All estimations include baseline controls (age, age-squared, urban, and female), region- and year fixed effects. The "Diff-in-diff" result gives the difference between active and inactive areas and we present the associated *F*-test and the p-value of the *F*-test. Columns 1–3 present results estimated on the baseline sample, while column 4 uses a smaller sample of respondents linked to either active or inactive projects and column 5 only includes individuals in areas that are surveyed before and after aid projects.

In the Online Appendix we develop the donor comparisons further. In Section B we break the analysis down by sector in order to explore whether the different corruption effects observed for Chinese and World Bank aid are driven by different sectoral compositions of their aid portfolios. Considering the transport sector, the pattern observed for overall aid holds, and focusing on health aid, the results if anything suggest that World Bank health projects help reduce corruption. To further ensure comparability of (ODA-like) Chinese and World Bank flows, we also run estimations for a restricted sample consisting only of countries falling below the income threshold making them eligible for the World Bank's concessional IDA assistance (World Bank, 2017a, 2017b). For both donors, the observed pattern remains intact.

[24] Using data from AidData (World Bank IBRD-IDA, Level 1, Version 1.4.1), covering all World Bank projects approved between 1995 and 2014. We again limit the sample to projects with precise geocodes and information about start year, giving us 688 World Bank projects spread across 6663 project locations.

[25] It is also interesting to note that whereas the World Bank seemingly locates their projects in areas that are poorer to begin with, as measured by their average night time light, the opposite holds for China, where inactive project areas display higher night time light.

Table 8
Nighttime light results for World Bank aid.

Panel A: police bribes

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Bribe police dummy	Bribe police dummy	Median Light	Bribe police dummy	Bribe police dummy	Bribe police dummy
active50	0.016***	0.018**	−0.333***		0.018**	0.017**
	(0.006)	(0.007)	(0.060)		(0.007)	(0.007)
inactive50	0.004	0.005	−0.538***		0.005	
	(0.007)	(0.007)	(0.103)		(0.007)	
median				0.000	0.000	0.000
				(0.001)	(0.001)	(0.001)
Active * median						−0.003
						(0.004)
Observations	9472	6861	6862	6861	6861	6861
R-squared	0.301	0.294	0.786	0.294	0.294	0.294
Difference in difference	0.013	0.013	0.205		0.013	
F test: active-inactive = 0	3.220	2.611	8.383		2.595	
p value	0.073	0.106	0.004		0.107	

Panel B: Permit bribes

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Bribe police dummy	Bribe permit dummy	Median Light	Bribe permit dummy	Bribe permit dummy	Bribe permit dummy
active50	0.022***	0.022***	−0.333***		0.022***	0.014**
	(0.006)	(0.007)	(0.060)		(0.007)	(0.007)
inactive50	0.010	0.012*	−0.538***		0.012*	
	(0.007)	(0.007)	(0.103)		(0.007)	
median				0.000	0.000	−0.000
				(0.001)	(0.001)	(0.001)
Active * median						0.009**
						(0.004)
Observations	9473	6862	6862	6862	6862	6862
R-squared	0.269	0.245	0.786	0.244	0.245	0.245
Difference in difference	0.011	0.010	0.205		0.010	
F test: active-inactive = 0	2.627	1.824	8.383		1.812	
p value	0.105	0.177	0.004		0.178	

Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** $p < .01$, ** $p < .05$, * $p < .1$; All estimations include the baseline controls (age, age-squared, urban, and female), year fixed effects and region fixed effects. The "Difference in difference" result gives the difference between active and inactive areas and we present the associated *F*-test and the p-value of the *F*-test.

Moreover, in order to investigate whether the different corruption experiences observed around Chinese and World Bank project sites simply reflect differences in the impact of bilateral and multilateral aid, in the Online Appendix Section C we run equivalent estimations for US and Japanese aid projects in Uganda. The results, if anything, suggest that the projects of these donors bring reduced local corruption.

5. Conclusions

Considering China's increased presence in Africa and the mounting criticism concerning Chinese aid practices, the present paper investigates whether Chinese development finance fuels local-level corruption in Africa. The paper differs from most studies in the literature on foreign aid and corruption by investigating the local corruption effects of a multitude of aid projects in a large multi-country sample, focusing on the effects on people's everyday experiences with corruption around aid project sites rather than perceptions or estimates of national aid inflows and corruption in government. Aid is not distributed evenly within countries, and while it may have clear effects on corruption in targeted local areas, this effect may be obscured by omitted variable bias or may not be sufficiently large to be measurable at the country level.

We suggest two principal channels through which aid projects may impact local corruption in recipient countries – through the presence of donors affecting the costs and benefits of engaging in corrupt activity and by means of norm transmission. Considering China's alleged lax attitudes towards corruption and suggested use of corrupt practices when implementing development projects, we argue that both economic incentive- and normative arguments speak in favor of Chinese aid projects fueling local corruption.

To investigate the empirical validity of this claim, we geographically match a new georeferenced dataset on the subnational allocation of Chinese development finance projects to Africa over the 2000–2012 period with 98,449 respondents from four Afrobarometer survey waves across 29 African countries. By comparing the corruption experiences of individuals who live near a site where a Chinese project is being implemented at the time of the interview to those of individuals living near a site where a Chinese project will take place but is yet to be implemented at the time of the interview, we control for unobservable time-invariant characteristics that may influence the selection of project sites.

The results consistently indicate that Chinese aid projects fuel local corruption. Investigating possible underlying theoretical mechanisms, the effect does not appear to be driven simply by an increase in economic activity. Rather, suggestive evidence seems to suggest that the Chinese presence impacts local norms.

Running equivalent estimations for World Bank aid projects, for which there is also geo-referenced data available for a large multi-country African sample, the estimations provide no consistent evidence of a corresponding increase in local corruption around project sites. In particular, whereas the results indicate that Chinese aid projects fuel local corruption but have no observable impact on local economic activity, they suggest that World Bank aid projects stimulate local economic activity without any consistent evidence of it fuelling local corruption.

The results are in line with both economic incentive- and normative arguments on the impact of aid on local corruption. However, considering the lack of evidence for a relationship between Chinese aid and local economic activity as proxied by night time light as well as the lack of a relationship between economic activity and bribe payments, and the suggestive evidence on the existence of norm transmission, there is arguably more support for the normative channel. While China's non-interference policy implies that they are unlikely to affect prescriptive norms in a direction delegitimizing corruption, their alleged use of corrupt practices in recipient countries risk affecting descriptive norms in a way that legitimizes corruption. Further research is needed on longer run effects of aid on economic activity and on how corruption mediates the effects of aid on economic development.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2018.01.002.

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