Chinese aid and local corruption

Ann-Sofie Isaksson and Andreas Kotsadam

Abstract:

Abstract: 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, which lingers after the project implementation period, is seemingly not driven by an increase in economic activity, but rather seems to signify that the Chinese presence impacts norms. Moreover, China stands out from the World Bank and other bilateral donors in this respect. 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 fuelling local corruption.

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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 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 effects of aid has recently been labelled 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). Largest among the 'new' donors is China, and with the explosion of Chinese funds, concerns over its donor practices has 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; Penhelt, 2007; 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 (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 differ from the projects of other major donors 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 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. Moreover, China does indeed stand out from other donors in this respect. Replicating our analysis for World Bank projects and for projects of other bilateral donors, we do not observe an equivalent pattern. Comparing China and the World Bank, for which there is also geo-referenced data available for a large multi-country African sample, suggests that this donor heterogeneity in results is not driven by differences in the sector allocation of aid, nor 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 economic activity, while they indicate the reverse – i.e. that projects stimulate local economic activity but do not 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 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. As

such, focus is on the effects on citizen experiences with petty corruption around aid project sites rather than estimates of national aid inflows and corruption in government.

While studies of aid effectiveness – including those on the relationship between aid and corruption – have traditionally focused on cross-national data, with this paper, we thus contribute to an emerging literature using subnational geocoded aid data to examine the determinants and impacts of the allocation of foreign aid within countries. A number of recent studies investigate the local allocation of aid within a single recipient country (see Francken et al. 2012 on relief aid allocation in Madagascar, Nunnenkamp et al. 2012 on the distribution of World Bank aid in India, Dionne et al. 2013 on aid allocation in Malawi, and Briggs, 2014 and Jablonski, 2014, both on political capture of aid in Kenya). Kelly et al. (2016) investigate the cross-sectional relationship between aid and perceptions of corruption in 44 villages in Tanzania, finding that Chinese aid projects are correlated with higher perceptions of corruption. As they lack a temporal analysis of aid and as the location of the aid projects is highly correlated with natural resources, which have been shown to have an independent effect on corruption (e.g. Knutsen et al., 2016), they are rightly cautious of interpreting the correlations causally. Others consider subnational aid allocation in a selection of countries (see e.g. Findley et al. 2011 on aid and conflict, Powell and Findley 2012 on donor coordination, Öhler and Nunnenkamp 2014 on factors determining the allocation of World Bank and African Development Bank aid, Briggs 2015 on the allocation of aid to richer subnational regions, and Dreher and Lohmann 2015 on aid and growth at the regional level). Focusing on the subnational allocation of Chinese aid for a large number of recipient countries over a 13 year period, our paper is closest to that of Dreher et al. (2016), who 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. As such, 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. Related Literature and Theoretical Mechanisms

In this section we discuss related literature on the relationship between aid and corruption, most of which focuses on country level measures of aid inflows and high level corruption. Next, we give an account of

some commonly suggested features of Chinese aid that could have implications for corruption. Finally, we discuss theoretical arguments as to why aid could impact local corruption and how these relate to the aforementioned features of Chinese aid.

2.1 Related Literature on Aid and Corruption

Most literature on the relationship between corruption, which we think of as the misuse of public office for private gain (Rose-Ackerman, 1975), and aid focuses on the relationship between country level aid inflows and corruption in the recipient country government. On the one hand, it is suggested that through the infusion of resources and technical assistance aid can potentially boost government effectiveness, for instance in terms of controlling corruption (see the reasoning in Bräutigam and Knack, 2004). It can release governments from binding revenue constraints, thereby enabling them to strengthen domestic institutions and pay higher salaries to civil servants, and it can provide training and technical assistance to build important government functions and institutions such as legal systems and accounting offices. Furthermore, aid can potentially be used to persuade states to embark on reform, for instance in terms of combating corruption (see e.g. Djankov et al., 2008).

Another argument, however, is that aid promotes rent-seeking behavior such as corruption. As described in Tavares (2003), where there are rents to be appropriated and where resources are transferred with substantial discretion and little accountability to the decision maker, there is a high risk for corruption. Foreign aid involves allocating goods or finance at below market prices, and hence provides opportunities for appropriating rents. Furthermore, recipient governments are often allowed considerable discretion in the distribution of funds. One can draw clear parallels to the 'resource curse' literature in this respect, linking natural resource rents to (among other things) greater corruption and weaker government accountability (see the discussion in Djankov et al., 2008; and in Morrison, 2012). Just as natural resource rents, foreign aid provides a windfall of resources to recipient countries, and may result in the same rent seeking behavior. Both sources of funds share the common feature that they can be appropriated by corrupt politicians without them having to resort to unpopular measures like taxation. And when revenues do not depend on the taxes raised from citizens and business, there is less incentive for accountability. Hence, large amounts of aid can potentially reduce the incentives for democratic accountability and thus the democratic pressures to combat corruption.[1]

[1] For an alternative view, highlighting the differences between aid and natural resource rents, mainly originating in the modalities of aid transfer, see Collier (2006).

The empirical evidence on the relationship between country level aid and corruption is mixed. While a number of studies suggest a positive relationship (see e.g. Svensson, 2000; Knack, 2001; Alesina and Weder, 2002; Bräutigam and Knack, 2004; Djankov et al., 2008),[2] Tavares (2003), on the other hand, finds that receiving aid is associated with reduced corruption levels. And similarly, the results of Okada and Samreth (2012) suggest that foreign aid generally involves reduced corruption, but that this reduction varies by different donors. In particular, while multilateral aid is associated with reduced corruption levels, bilateral aid from the world's leading donor countries, including France, the UK, and the US, has no significant effect.

A reason for the mixed results could be the tendency to study the relationship between aid and corruption at the country level. Comparing corruption across countries it is of course difficult (if at all possible) to separate the impact of aid from the effects of problems – consider e.g. colonialism, economic crises, unsustainable debt, civil wars and political instability – that are common in aid receiving countries (see the discussion in Bräutigam and Knack, 2004). To assess the effects of aid on corruption we need to consider changes in aid and corruption over time. However, while available country level corruption measures tend to capture large cross-country differences relatively well, it is questionable whether they are sufficiently refined to pinpoint accurately the short-term changes in corruption within a country over time (Alesina and Weder, 2002). With this in mind, and considering the multitude of factors that could affect country level corruption over time, it is arguably more appropriate to investigate sub-national variation in aid disbursements and corruption over different periods. While aid may have effects in targeted areas, these effects may not be sufficiently large (or may be obscured by omitted variable bias) to be measurable at the country level (see the reasoning on aid and regional growth in Dreher and Lohmann, 2015). The present paper differs from the above studies in that it studies the local corruption effects of aid projects in a large multi-country sample. As such, focus is on the effects on citizen experiences with petty corruption around aid project sites rather than national aid inflows and estimates of grand corruption in government.

2.2 Chinese Aid and Corruption

Two main features could make Chinese aid stand out in terms of corruption effects: China's well-known policy of non-interference in the domestic affairs of recipient countries (see e.g. Tull, 2006; Bräutigam, 2009; Tan-Mullins et al., 2010; Dreher et al., 2016), and their tendency 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).

[2] See also the seminal papers by Reinikka and Svensson (2004) and Olken (2006) which, while not focused on aid projects per se, demonstrate substantial problems with corruption in large public expenditure programmes in a developing country context.

The former principle is clearly spelled out in official Chinese documents; in their 2014 White Paper on Foreign Aid, the Chinese government specifies that "When providing foreign assistance, China adheres to the principles of not imposing any political conditions, not interfering in the internal affairs of the recipient countries and fully respecting their right to independently choosing their own paths and models of development" (State Council, 2014). Some Western observers consider this approach a convenient rationale for economic involvement in undemocratic and corrupt countries, and suggest that it makes Chinese aid particularly easy to exploit for politicians and 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).

Investigating sub-national variation in Chinese aid allocation, Dreher et al. (2016) find that Chinese aid, unlike World Bank aid, is disproportionately allocated to the birth regions of African leaders, supporting the idea that Chinese aid may be particularly easy to exploit for politicians who are engaged in patronage politics. However, channeling funds to their home regions should not necessarily be viewed as corruption, per se. As noted by the authors, China's aid to Africa is often described as demand-driven, with the initiative for aid projects often coming from the recipient side. A request-based system for initiating aid projects should provide opportunities for political leaders to overtly promote a subnational distribution of funding that best serves their interests, without having to resort to outright embezzlement of funds (see also the discussion in Briggs, 2014).

Based on the empirical evidence, it is not clear that China favors corrupt regimes in their allocation of aid. Dreher and Fuchs (2015) find that China's aid is, for the most part, independent of the recipients' institutional characteristics, including control of corruption. Hence, while in line with the non-interference principle, their findings do not indicate that China's aid is biased towards autocratic or corrupt regimes, as is often claimed by its critics. Furthermore, their results suggest that in this respect, China is no different from many other influential donors. Similarly, the results of Dreher et al. (2015) provide no indication that more concessional (or 'ODA-like', see the definition in Section 3) Chinese flows to Africa are tied to domestic political institutions or corruption in recipient countries. On the other hand, though, their results suggest that less concessional Chinese flows are more likely to go to countries with higher levels of corruption. The latter could be due to China being better positioned than Western countries to transact with poorly governed countries because they rely on financial modalities, such as commodity-backed loans, that reduce the risks of financial misappropriation, or to that since state-owned Chinese companies are heavily backed by the government, they can afford to be less risk averse than Western companies and thus invest in risky but strategically important countries (Tull, 2006; Penhelt, 2007; Dreher et al., 2015).

The second feature of Chinese aid, with possible implications for corruption, is China's tendency to maintain control over the projects it funds from the project initiation phase to the project completion phase, often using Chinese contractors for work performed in the recipient countries (see e.g. Bräutigam, 2009). While one could argue that this makes it easier to retain oversight, meaning that Chinese aid could actually be less susceptible to waste and abuse than aid from Western donors (Tan-Mullins, 2010), it has been suggested that Chinese firms operating abroad have laxer attitudes about corruption and use 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), where more than 3,000 business executives worldwide were asked about their views on the extent to which companies from 28 of the world's leading economies engage in bribery when doing business abroad, only Russia scored worse than China.[3] While it is noted that China in 2011 passed a law that makes it a criminal offence for Chinese companies and nationals to bribe foreign government officials, they point to considerable challenges in terms of implementation, enforcement, and ensuring that the authorities treat the issue as a priority. Furthermore, a large share of Chinese development finance to Africa is given for government infrastructure investments, a sector that is notorious for corruption and where the Chinese companies involved do not enjoy a good reputation (Bräutigam, 2009). Considering that Chinese development projects tend to be tied to the use of Chinese companies, they might thus stand out in terms of the use of corrupt practices during the implementation phase.

In the next section we will discuss theoretical mechanisms linking aid and local corruption and how accusations of China having lax attitudes towards corruption in recipient countries and using corrupt practices when implementing development projects relate to these.

2.3 Aid and Local Corruption: Theoretical Mechanisms

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, 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.[4]

[3] The score for each country is based on the views of the business executives who had come into contact with companies from that country. For each of the 28 countries with which they have had a business relationship (for example as supplier, client, partner or competitor) the business executives were asked 'how often do firms headquartered in that country engage in bribery in this country?' (Transparency International, 2011).

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

With regard to the former, economic theories of corruption usually assume that the public official weighs the benefits of corrupt behavior against its costs and chooses to establish a corrupt relationship when the former outweighs the latter (see the reasoning in Glaeser & Saks, 2006). While the benefits of corruption have to do with the public official's ability to extract resources for personal gain, its costs originate in the probability of, and the penalties from, being caught (see e.g. Shleifer & Vishny, 1993).

There are several reasons why aid projects may impact the costs and benefits of local corruption. On the one hand, donor involvement in an area arguably increases local economic activity and thus the flow of resources that are up for grabs, i.e. the benefits of engaging in corrupt activity. This would not only be due to the actual aid inflow, but also to the up- and downstream activities involved in the aid delivery process, including e.g. the supply of inputs to projects, establishing an infrastructure to deliver aid financed goods or services to the poor, or simply catering to the needs of donor personnel. The additional resource flows 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, its very presence in an area could potentially increase the perceived costs of engaging in corruption. 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 toward corrupt practices in recipient countries (De Simone and Taxell, 2014). Against this background, it seems reasonable to assume that the donor could call attention to a problem of corruption and thereby raise the perceived probability of being caught if engaging in corrupt activity.[5]

Which of these effects dominates is an empirical question. However, if the donor in question does not devote resources to monitoring or controlling corruption in recipient countries, the former effect, suggesting that donor involvement could fuel local corruption, should arguably do so. As noted, China's official policy is to not interfere in the domestic affairs of recipient countries, and given this 'no-strings-attached' approach to aid it is difficult to argue that they are committed to fighting corruption.

A different argument is that aid projects may impact local corruption through norm transmission (see e.g. Hauk and Saez-Marti, 2002). Above, we discussed the possibility that the very presence of a donor in an area could raise the perceived probability of being caught if engaging in corrupt activity and thus the costs of corruption. An alternative, and slightly more optimistic, argument is that by raising awareness of problems with corruption donors can influence social norms and thereby instigate actual institutional

[5] Furthermore, donors could raise the cost of corruption by providing funds enabling recipient governments to pay higher wages to civil servants, thereby increasing the returns to staying on the job (see the discussion in e.g. Olken and Pande, 2012). However, Foltz and Opoku-Agyemang (2015) find that increased police salaries in Ghana increased corruption. Lacking data on civil servant wages in the specific project localities we are unable to explore this mechanism further.

change. Donors may be able to establish standards of conduct that delegitimize and stigmatize corrupt practices, i.e. 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, described above, has indeed brought substantial attention to the fight to curb corruption, with likely implications at the local level where aid projects are being implemented.

Unfortunately though, norm transmission might as well work in the other direction, legitimizing and fueling corruption. Here it is useful to distinguish between prescriptive and descriptive norms. Whereas the former tells an actor how it ought to behave, the latter merely describes some observable pattern of behavior among actors (Greenhill, 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. By stigmatizing corrupt practices a donor might be able to influence prescriptive norms. Importantly, however, the donor's own behavior vis-à-vis local actors during the implementation phase could potentially also affect descriptive norms. Hence, the presence of a donor itself engaging in corrupt practices could potentially change descriptive norms on corruption. In addition, there may be an interaction between economic incentives and descriptive norms. Considering that corrupt behavior tends to entail economic gains, competitive pressures may lead non-corrupt individuals to lose out. Hence, corrupt practices may lead to a race to the bottom, whereby agents continually increase their corrupt activity in order to stay competitive. Descriptive norms that 'everyone is corrupt' should fuel this tendency. For instance, in a report on Chinese investments and labor relations in Namibia, an interview respondent commenting on the alleged tendency of Chinese construction companies to be awarded government tenders despite not adhering to the tender rules notes that "once the laws and the state are corrupted, those who are still honest will be in trouble. Corruption becomes a self-reinforcing process of self-destruction" (Jauch and Sakaria, 2009: p.16).

Moreover, there is evidence to suggest that norms are easier to change for the worse than for the better. Fisman and Miguel (2007) study the effects of cultural norms on corruption by analyzing the parking behavior of United Nations officials in Manhattan. Their findings suggest strong effects of corruption norms – diplomats from high-corruption countries were found to accumulate significantly more unpaid parking violations – but also that violations increased with tenure in New York and that these increases were particularly large for diplomats from low-corruption countries. The latter could be taken to suggest that negative social norms may be stickier than positive social norms, or put differently, that people are more likely to assimilate to more selfish norms than to more cooperative norms. This is in line with the reasoning and findings of Zhou et al. (2015), who expose lab participants to a sequence of different subject pools when playing trust games and find that the impact of exposure to a more selfish environment lasted longer and influenced behaviors to a greater extent than exposure to a more cooperative environment. In light of

these findings, there is seemingly a risk that China, having been accused of engaging in corruption in recipient countries, fuel local corruption by affecting descriptive corruption norms for the worse.

Summing up, 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. Given China's alleged lax attitudes towards corruption and suggested use of corrupt practices when implementing development projects, both economic incentive- and normative arguments speak in favor of Chinese aid projects fueling local corruption. In particular, if donor presence in an area increases the benefits of corrupt activity, and China's hands-off approach to aid delivery implies that this increase is not accompanied by intensified monitoring raising the costs of corruption, the net economic incentive effect on local corruption is likely to be positive. Similarly, 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.

Against this background it is interesting to investigate the local corruption effects of Chinese development projects. In particular, do Chinese development projects fuel corrupt activity around the project sites? Do Chinese development projects differ from the projects of other major donors in this respect? And if so, can this variation be explained simply by the composition of Chinese aid or is it more likely to originate in the theoretical mechanisms discussed above, i.e. in donor differences in the effects of aid on economic activity and on norm transmission? In the next section we discuss how to approach these questions 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.[6]

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

[6] 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.

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. 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 and colleagues (2016) geocoded the data, assigning latitude and longitude co-ordinates, providing standardized names of the geographic units of interest 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. Furthermore, many official finance projects listed in the dataset are intangible in the sense that they pertain to bilateral agreements and/or transactions between China and the recipient country that do not have a physical project site (Muchapondwa et al., 2014). 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 coordinates 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 are relatively restrictive in terms of which projects we include, focusing 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).[7] As noted in Dreher and Lohmann (2015), the geographical coding precision tends to reflect the sectoral composition of aid. While projects in sectors such as "Finance" or "Public Administration, Law, and Justice" are often geo-coded at the national scale, projects in sectors like "Transportation" are typically assigned to more precise locations.

For comparability with other donors, we focus on Chinese aid projects that can be classified as overseas development assistance (ODA) according to the OECD-DAC definition. In order to qualify as ODA, 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 percent, 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 less than 25 per cent, are labelled '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'

[7] 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.

when there is insufficient information to classify the project as either OOF- or ODA-like, see Strange et al., 2015). Restricting our sample to include only ODA-like projects with precise geocodes and start-dates we cover 227 Chinese project sites.[8] As can be seen in Table A1, the resulting sample of projects 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'. For reasons discussed above, however, restricting ourselves to projects with precise geocodes, the 'Unallocated/unspecified' share, which constitute 12 percent 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 et al. (2016).[9] As described in greater detail in their paper, the geographic locations of the survey respondents are specified based on various pieces of geographical information in the Afrobarometer. When provided (in South Africa and for a number of regions in Sierra Leone), the official enumeration area boundaries were used to place respondents within their respective enumeration areas (EA). However, for the majority of observations, each respondent was placed on the centroid coordinate of their reported town, village or neighborhood of residence using Google Maps. This turns out to be a surprisingly effective strategy for precisely locating individuals; evaluating the precision of the Google maps based coordinates by measuring the distance between estimated locations and true locations based on EA information from the 2001 South African census, the average distance from the EA, i.e. the geo-location error, is 13 km using the Google maps-based coordinates (Knutsen et al., 2016).

The aid data is linked to repeated cross sectional survey data based on spatial proximity. Specifically, 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 Figure 1 shows the location of all our 8685 Afrobarometer clusters and our 227 geocoded Chinese aid projects. 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

[8] 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.
[9] 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.

survey cluster even if we have both types of information in the same country. Figure 2 shows a map including the aid projects along with 50 km buffer zones around each Afrobarometer cluster. 185 of the aid project locations are within 50 kilometers of at least one Afrobarometer cluster.

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'.[10] 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, 12 percent of the baseline sample, which after sample restrictions is 63,596 observations,[11] have paid a bribe to the police last year and 14 percent 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 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 of the survey. Table 1 shows that 27 percent of the sample lives within 50 kilometers of an active Chinese aid project and 12 percent lives within 50 kilometers of an inactive project, without having any active projects in the same area. We discuss these variables further in the estimation strategy below.

3.1 Estimation Strategy

Our spatial-temporal estimation strategy resembles that used in Knutsen et al. (2016).[12] In particular, we distinguish between sites where an aid project is actually under implementation and sites where the project had yet to be implemented at the time of the survey. The four Afrobarometer survey waves

[10] 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 fixed effects control for country variation in the average level of corruption and focus is on within-country variation in the same.

[11] 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 2 of Table 2) as the baseline sample.

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

covered provide a unique opportunity to study the corruption experiences of African citizens over the recent decade. While the fact that the data does not have a panel structure hinders us from following specific localities over time, with this estimation strategy we can still compare areas before a project has been implemented with areas where a project is currently under implementation, thus making use of the time variation in the data. 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 Chinese project site, 2) within 50 km of a Chinese project site that is yet to open, but not close to any active projects, and 3) more than 50 km from any Chinese project site. Our baseline regression is:

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

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[13] – on a dummy variable *active* capturing whether the individual lives within 50 kilometers 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 ($\mathbf{X}_i$) of individual-level controls from the Afrobarometer. Our baseline set of individual controls are age, age squared, gender, urban/rural residence.[14] To account for correlated errors, the standard errors are clustered at the geographical clusters (i.e., at the enumeration areas which correspond to either a village, a town or a neighborhood).[15] 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. This is a very strong assumption seeing that corruption levels (and other factors correlated with corruption, such as population density, economic activity and infrastructure access) may influence Chinese project location decisions. For instance, the Chinese may well be less inclined to implement projects in highly corrupt areas. An alternative position is that the Chinese may be more likely to win tenders in particularly corrupt locations. In short, simply assuming that there is no correlation between Chinese project localization and the pre-existing institutional characteristics of project sites appears unreasonable.

[13] Instead calculating marginal effects after probit regressions does not change the interpretation of any results (results are available upon request).
[14] 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 3 and 4 of Appendix Table A2.
[15] The results are robust to clustering the standard errors at the region (350 clusters), at both the region and year level in a multi-way clustering (Cameron et al. 2012), as well as at the country level (29 clusters) as seen in columns 1-3 of Appendix Table A2.

However, including *inactive* allows us to compare active project sites to other areas selected as locations for Chinese projects, but where the project were 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, we therefore provide test results for the difference between *active* and *inactive* (i.e. $\beta_1 - \beta_2$), giving us a difference-in-difference type of measure[16] that controls for unobservable time-invariant characteristics that may influence selection into being a Chinese project site. 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 travelling 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 alternative cut-offs (25 and 75 km).

Although the Afrobarometer survey does not have a panel structure, in some cases it happens to revisit the same localities in different survey waves. In Section 4.2 we utilize this and run project fixed effects estimations for the 40 project locations for which we have data on corruption from both before and after the Chinese aid project started.

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 352 sub-national region dummies.

[16] 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).

Looking at the coefficients on *active*, we can note that living within 50 kilometers 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 approximately 5 percentage points more likely to have paid a bribe when dealing with the police and 4 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 use a difference-in-difference approach, comparing experiences with corruption in areas close to sites where a Chinese project is currently 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. 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.

The difference-in-difference estimates ($\beta_1 - \beta_2$) and associated test results presented in the bottom rows of Table 2 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 6 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 4 percentage points. In both cases, the parameter differences are clearly statistically as well as economically significant, implying a 50 percent increase in bribes to the police and a 29 percent increase in bribes for permits (Table 1 gives the average shares reporting to have paid the concerned bribes).

4.2 Sensitivity Analysis

As it turns out, the finding that Chinese aid projects fuel local corruption is remarkably stable across specifications and sub-samples. The results of a number 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, using a 25 km cut-off in Column 1 and a 75 km cut-off in

Column 2. In both cases, 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. As might be expected, the estimated differences between the two are larger when using a smaller cut-off, i.e. when considering the more immediate surrounding of the project site rather than a wider area, seemingly suggesting that the observed corruption effects wear off with distance. The same tendency is revealed in Figure 3, 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 decrease with distance and that the lines eventually cross. This pattern holds for both types of bribes.

In the benchmark setup we exclude respondents who live within the cut-off distance of a site where a Chinese project has been terminated prior to the interview date (approximately 28 percent of respondents). The argument is that this may otherwise bias the effect of having an active project nearby, e.g. by inflating corruption levels among supposedly untreated individuals or by interfering with the effect of treated individuals living close to active or inactive sites. In Column 3 of Table 3, however, we instead keep these individuals in the regression, but include a dummy variable to control for having a terminated project within the cut-off distance. The statistically significant difference in corruption experiences between individuals living close to active and yet inactive Chinese project sites remain. Moreover, we can note that the dummy for having a terminated project nearby has a positive and statistically significant coefficient, both for police and permit bribes. For police bribes, this coefficient is significantly higher than that of *inactive* but significantly lower than that of *active* (the test results are available upon request), seemingly suggesting that the corruption effect of a Chinese project lingers after it has been terminated, but wears off somewhat after the implementation phase. For permit bribes, the coefficients on *active*, *inactive* and *terminated* display the same pattern, but here *terminated* is only statistically different (and only at the 10% level) from *active*.

In Column 4 we use the equivalent ordinal dependent variables described in Section 3. Compared to the dummies used as dependent 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. In any case, the results remain unchanged. In particular, the statistically significant difference between *active* and *inactive* is 0.103 for police bribes and 0.062 for permit bribes, which, put in relation to a sample mean of approximately 0.22 (as shown in Table 1) for both the ordinal dependent variables, is sizeable.

It is often suggested that Chinese aid is likely to be tied to natural resource extraction (see e.g. Kelly et al., 2016). 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. For this purpose, we use geocoded and time-varying 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 further show 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).

Table 4 shows the results of estimations using 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, for water or sanitation services, or to cross a border. 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 for three out of the four variables, corruption is significantly ($p<0.1$) higher in areas close to active as compared to inactive project sites. For the fourth variable, border crossings, we only have 8,822 observations and in addition this type of corruption affects relatively few people (93 percent of the estimation sample respond 'never').[17]

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, we can note that our results are robust to also including the OOF-like and 'vague official finance' projects (see Table A5).

Furthermore, since we have data from many different countries we are able to explore some possible heterogeneities in our results. Using data from the Quality of Government (QoG) database (Teorell et.al., 2015) we test if the effects are similar in rich vs. poor countries, more or less corrupt countries, and more or less democratic countries. The results are presented in Appendix Table A6. For police bribes there is an effect of Chinese aid in all sub-samples. For permit bribes the difference between active and inactive is statistically significant in the richer and less democratic sub-samples. However, while not always statistically significant at conventional levels, the difference goes in the expected direction in all sub-samples.

[17] Retaining the ordinal coding of the variables does not change the interpretation of the result (available upon request).

Next, we consider possible effects of project timing. While the year dummies included in all regressions will control for general time trends in corruption, there may be timing effects relating specifically to the evolution of Chinese aid. Here, a few clarifications are in order. Our identification strategy utilizes the fact that we know at what point in time and in what localities Chinese 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 Chinese project was ongoing at the time of the survey and compare them with respondents living in areas where we know that a Chinese projects will start, but where implementation had yet to begin at the time of the survey. Importantly, all Afrobarometer survey waves covered contain observations connected to both active and inactive Chinese project sites, meaning that we have variation in project status for both projects implemented earlier and projects implemented later. Hence, there is no direct correspondence between time of project implementation and inactive/active project status; a project implemented comparatively early may well be coded as yet inactive, all depending on at what point in time the Afrobarometer surveyed that particular area.[18]

That said, however, there are more respondents connected to active project sites in the later 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 percent of the individuals connected to active projects were surveyed in wave 5, 25 percent in wave 4, 12 percent in wave 3 and 9 percent in wave 2.

With this in mind, two scenarios could potentially inflate the observed corruption effects. The first would be if the sites selected for projects implemented later had higher levels of pre-existing corruption than the sites selected for projects implemented earlier. The other would be if Chinese aid has become more corrupt over the considered time period. With regard to the former, the fact that the coefficient on *inactive* never is statistically significant suggests that areas selected for Chinese project sites do not stand out in terms of pre-existing levels of corruption. Regarding the latter, considering the attention and criticism directed towards Chinese practices in Africa during the last decade, and the fact that China in 2011 passed a law making it a criminal offence for Chinese companies and nationals to bribe foreign government officials (see the discussion in Section 2.2), the opposite trend arguably appears more plausible.

[18] To illustrate, suppose China implements two projects in Kenya, 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, it will be coded as inactive. And if the project implemented later is in a locality surveyed by the Afrobarometer after 2009, it will be coded as active.

Nevertheless, 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, Table A7 presents 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, indicating a corruption difference between areas close to active and inactive Chinese project sites of 6-7 percentage points for police bribes and of 4-5 percentage points for permit bribes.[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.

Finally, we run project fixed effects estimations, meaning that we restrict the sample to areas that have observations from both before and after a Chinese aid project started. An advantage of this restriction is, of course, that it allows us to evaluate variation in corruption occurring around a project site before and after the project was initiated. An important drawback, however, is that we lose a large share of our sample. As noted, the Afrobarometer is not a panel and only in some cases happen to revisit the same localities in different survey waves. Hence, 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 (Table A8), while less precisely estimated, still suggest higher levels of corruption around active Chinese aid project sites, thus adding further support to our findings.[20]

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.

4.3 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.

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

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 kilometer 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 test whether aid affects the median level of light in an area and find that there is no relationship on average. 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).[21] 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 Table A9) 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 (see Section 3.2). To check if the police bribe results are

[21] 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.

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 (Table A10). 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 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 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. While norms are generally seen as relatively persistent, as discussed in Section 2.3 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 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, which seemingly lingers after the project implementation period, is driven simply by an increase in economic activity. Rather, suggestive evidence arguably points in favour of that the Chinese presence impacts local norms.

Is Chinese aid different in this respect, or is all aid similar? In section 2.3 we pointed to a number of features that could potentially make Chinese aid stand out in terms of its implications for local corruption. 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.

4.4 Chinese and World Bank Aid Compared

As it turns out, we do not find an equivalent pattern around World Bank project sites.[22] Table 7 presents the results of regressions for police bribes (Column 1) and permit bribes (Column 2). For permit bribes, the coefficient on *active* is positive and statistically significant. Importantly, however, so is the coefficient on *inactive*. In neither of the estimations do we find a statistically significant difference in corruption experiences between people living near active and yet inactive World Bank project sites. Hence, we find no evidence of World Bank projects fueling local corruption. If anything, the indication of more permit bribes around project sites is seemingly driven by a selection effect – a tendency to locate World Bank projects in areas with more corruption to begin with – rather than being an effect of the World Bank presence.

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, whereas the results indicate that Chinese aid projects fuel local corruption but have no observable impact on local economic activity, they suggest the opposite, and much more favourable pattern for World Bank aid projects, namely that they stimulate local economic activity without fuelling local corruption. 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 (Tables A11 and A12). 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

[22] 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 6,663 project locations.

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, and contrary to the effects of Chinese aid projects, aid from the World Bank seemingly makes people more likely to think that media should investigate and report on corruption (column 2 of Table 6), thus providing suggestive evidence that the World Bank are successful in affecting social norms in a direction de-legitimizing corruption

To what extent are the Chinese and World Bank projects comparable? Comparing all Chinese and World Bank African aid projects geocoded with the same reported level of precision[23] we have, to some extent, already narrowed down our selection of projects. Since the geographical coding precision tends to reflect the sectoral composition of aid (Dreher and Lohmann, 2015), the mere fact that we focus on projects with equally precise geocodes should arguably make the selection of Chinese and World Bank projects more comparable. However, important differences are likely to remain. For instance, a large share of Chinese development finance to Africa focuses on infrastructure investments, a sector that is notorious for corruption (Bräutigam, 2009). To investigate whether the sectoral composition of aid is what drives the corruption differences between the two donors, in a next step we compare Chinese and World Bank projects going to the same sectors, focusing on 'Transport and storage' and 'Health'.

These are the sectors receiving the largest shares of the Chinese aid projects in our sample (19 and 22 percent respectively). Nevertheless, looking at projects to one sector alone still means that we have a limited number of active and inactive Chinese project sites to consider (42 for transport and 49 for health, to be precise) spread across a limited number of countries. The World Bank has a greater number of projects spread across Africa, but in order to get a comparable sample, we focus on recipient countries where respondents can be linked to both active and inactive Chinese and World Bank projects to the respective sectors.[24] Due to the limited sample we rely on a specification with country fixed effects instead of sub-national region fixed effects for these regressions.

Considering the transport sector (Table 9, Columns 1 and 2), the pattern observed for overall aid holds, i.e. the estimations suggest a statistically significant difference, in the expected direction, between the corruption experiences of individuals living near active and yet inactive Chinese transport project sites (Panel A). For World Bank transport projects (Panel B, Columns 1-2), we observe no such difference. Hence, the results of these estimations, which admittedly focus on a limited number or countries, suggest

[23] I.e. 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).
[24] Namely, Mali, Mozambique and Nigeria for transport projects, and Ghana, Kenya, Madagascar and Uganda for health projects.

that the difference in corruption experiences around Chinese and World Bank project sites is not simply the result of a disproportionate share of Chinese aid going to a sector particularly prone to corruption.

Nevertheless, the extent to which Chinese aid projects fuel local corruption seems to vary across sectors. In the health sector (Columns 3-4), there is no evidence that neither Chinese nor World Bank projects fuel local corruption. Interestingly, for World Bank health projects, when there is a difference between *active* and *inactive* (see Column 4), it in fact goes in the opposite direction, indicating more widespread corruption in areas around yet inactive project sites than in areas where projects are being implemented. If anything, the results thus suggest that World Bank health projects tend to be located in areas with higher pre-existing corruption, but help reduce corruption once they are being implemented.

4.5 Chinese and Other Bilateral Aid Compared

Do the different corruption experiences observed around Chinese and World Bank project sites simply reflect differences in the impact of bilateral and multilateral aid? Indeed, a common argument is that bilateral aid is often tied to the political agenda of the donor country and that it is less focused on promoting good governance in the recipient country. In comparison, multilateral donors tend to have explicit anti-corruption policies as part of their agenda and are often seen as relatively more impartial. The World Bank, in particular, has been at the forefront of what has been labeled the 'anti-corruption movement', initiated among major international organizations in the mid-1990s (see the discussion in Charron, 2011). Our results so far – suggesting that bilateral Chinese aid fuel local corruption but finding no such evidence for the multilateral World Bank aid – could be said to be in line with this idea. Furthermore, when considering the health sector alone, they provide some suggestive evidence in support of Okada and Samreth's (2012) finding that multilateral aid is associated with reduced corruption levels at the country level. These authors, however, found no equivalent country level relationship between bilateral aid and corruption.

To investigate if the bilateral-multilateral distinction is what drives the observed differences among donors, in a next step we compare the suggested local corruption effects of Chinese aid projects to those of other bilateral donors, for which there is geocoded aid project data available for a small selection of African countries. In particular, for Nigeria, Uganda and Senegal there is geocoded aid data for both China and other bilateral donors, thus allowing for comparison (Table A13 lists the other bilateral donors and their

number of geocoded projects sites in the concerned recipient countries).[25] Table 10 presents the results of estimations focusing on Chinese and other bilateral aid to these countries.

In this sub-sample of recipient countries we observe no differences in police bribes between individuals living close to active as compared to yet inactive project sites – neither for Chinese projects nor for the projects of other bilateral donors. Turning to experiences with corruption when applying for documents and permits, the difference between individuals living close to active as compared to yet inactive project sites is sizeable and statistically significant for Chinese aid projects. For other bilateral donors the estimation provides suggestive evidence of a similar pattern, although the difference is smaller and not quite statistically significant at conventional levels. However, considering the diverse range of donors included in this group, the results may be driven by heterogeneous effects and may thus not be representative for any particular bilateral donor. Hence, in a next step, we narrow down the comparison to selected bilateral donors.

Looking at Table A13, we can note that out of the 537 project sites considered, 346 are American and 113 are Japanese. Seeing that the US is another major bilateral donor active in the area, comparing Chinese and US aid is of course interesting. Furthermore, it has been suggested that comparing Chinese and Japanese aid is particularly instructive since both countries have previously been aid recipients, have both recently undergone industrialization and drastically reduced poverty, and since the projects of both donors tend to focus on infrastructure and growth without being tied with packages of political or economic reforms (Tan-Mullins, et al., 2010). Indeed, Brautigam (2009) argues that China seeks to replicate the successes of Japanese aid to Africa. It is also interesting to note that in Okada and Samreth's (2012) study of the macro level relationship between aid flows and corruption, the US, the UK and Japan were the only bilateral donors whose aid flows were found to be significantly related with changes in recipient country corruption levels. In particular, they found that whereas aid flows from the US and the UK were associated with increased corruption, Japan was the only bilateral donor whose aid was associated with reduced corruption levels.

Since the great majority of the geocoded American and Japanese project sites are in Uganda, we compare the corruption experiences around US, Japanese and Chinese project sites in Uganda (see Table 11). The difference between individuals living close to active as compared to yet inactive project sites does not come out statistically significant for Chinese aid projects sites in this sub-sample. This is not surprising seeing that there are only 32 Chinese project sites in Uganda and considering that all but one of the 201 individuals living within the cut-off distance of an inactive site are in fact connected to the same

[25] Note that since we only include projects that have precise geocodes and start dates in the Aid data database, this list is not necessarily representative of the donor presence in Uganda, Nigeria and Senegal.

Chinese project. The results for US and Japanese aid are interesting, however. In line with Okada and Samreth's (2012) macro level findings, and unlike what we have found for Chinese aid, Japanese aid projects indeed appear to reduce local corruption. In particular, individuals living close to active as compared to yet inactive Japanese project sites are less likely to have been asked to pay a bribe when applying for documents and permits. Given the results of Okada and Samreth, one may expect the opposite to hold for US aid projects. If anything, however, the results suggest lower corruption – a lower probability to have been asked for bribes by the police – around active US project sites. Hence, even when comparing with other bilateral donors, who just as China might not have an equally explicit anti-corruption agenda as the World Bank, Chinese aid projects seemingly stand out in terms of their estimated effects on 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 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.

First of all, the results consistently indicate that Chinese aid projects fuel local corruption. Moreover, the effect seemingly lingers after the project implementation period, and does not appear to be driven simply by an increase in economic activity, but rather seems to imply that the Chinese presence impacts local norms.

Second, Chinese aid projects do indeed stand out from the projects of other major donors in this respect. Running equivalent estimations for World Bank aid projects, for which there is also geo-referenced data available for a large multi-country African sample, we do not observe 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 fuelling local corruption. Indeed, if anything, they suggest the opposite; there is some indication that World Bank health projects help reduce corruption. In line with this, suggestive evidence indicates that World Bank aid projects are successful in raising awareness of corruption. This is interesting considering that the World Bank has been at the forefront of the 'anti-corruption movement' among major international organizations, with explicit anti-corruption policies as part of their agenda. Comparing with other bilateral donors, who just as China might not have an equally explicit anti-corruption agenda as the World Bank, Chinese aid projects still stand out in terms of their estimated effects on local corruption. Indeed, in Uganda, Japanese and American aid projects, if anything, appear to bring reduced local corruption. Hence, the comparison of the local corruption effects of Chinese and other aid does not speak in China's favor.

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 how corruption mediates the effects of aid on economic development.

Figures and Tables

Figure 1

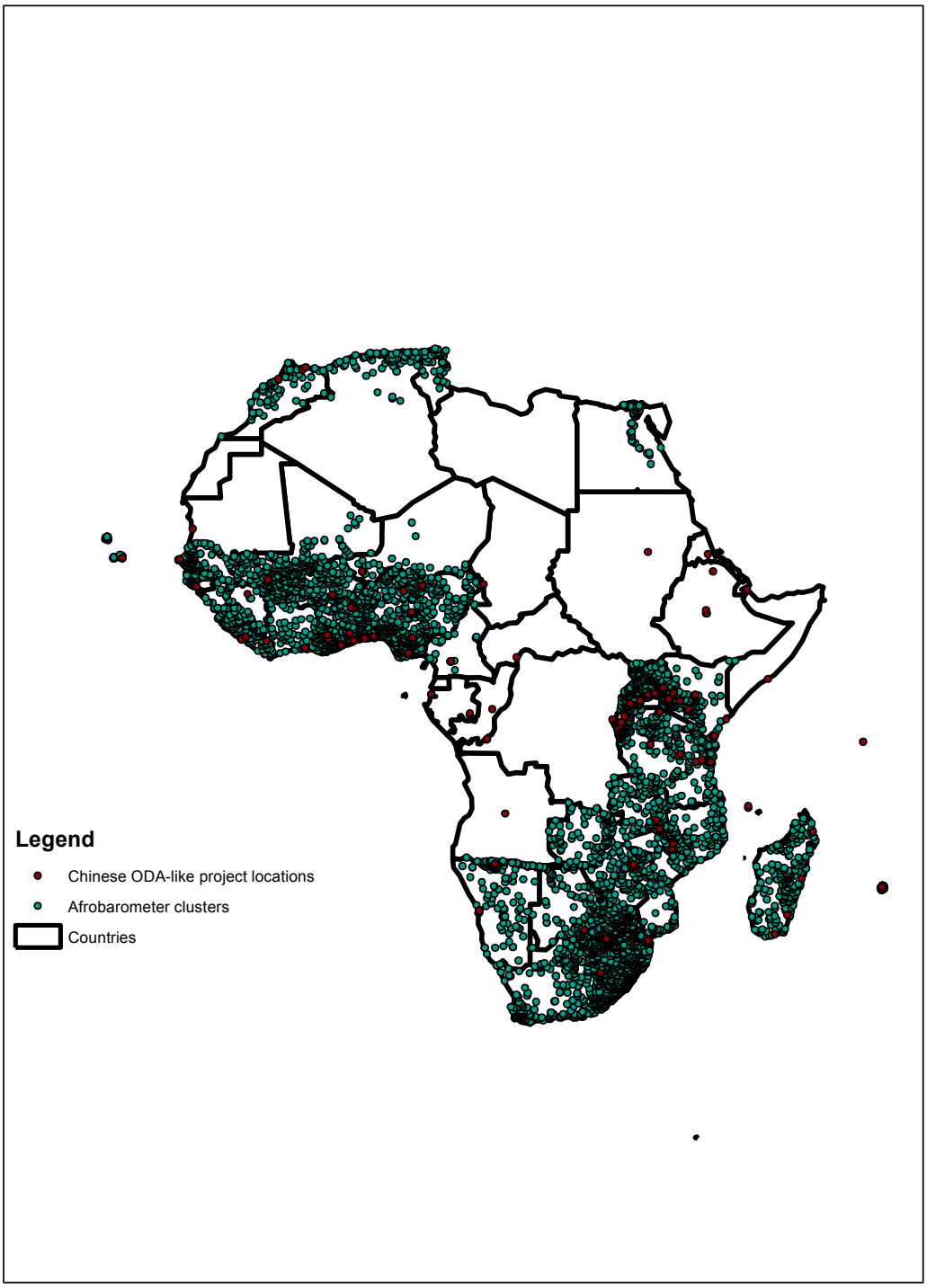


Figure 2

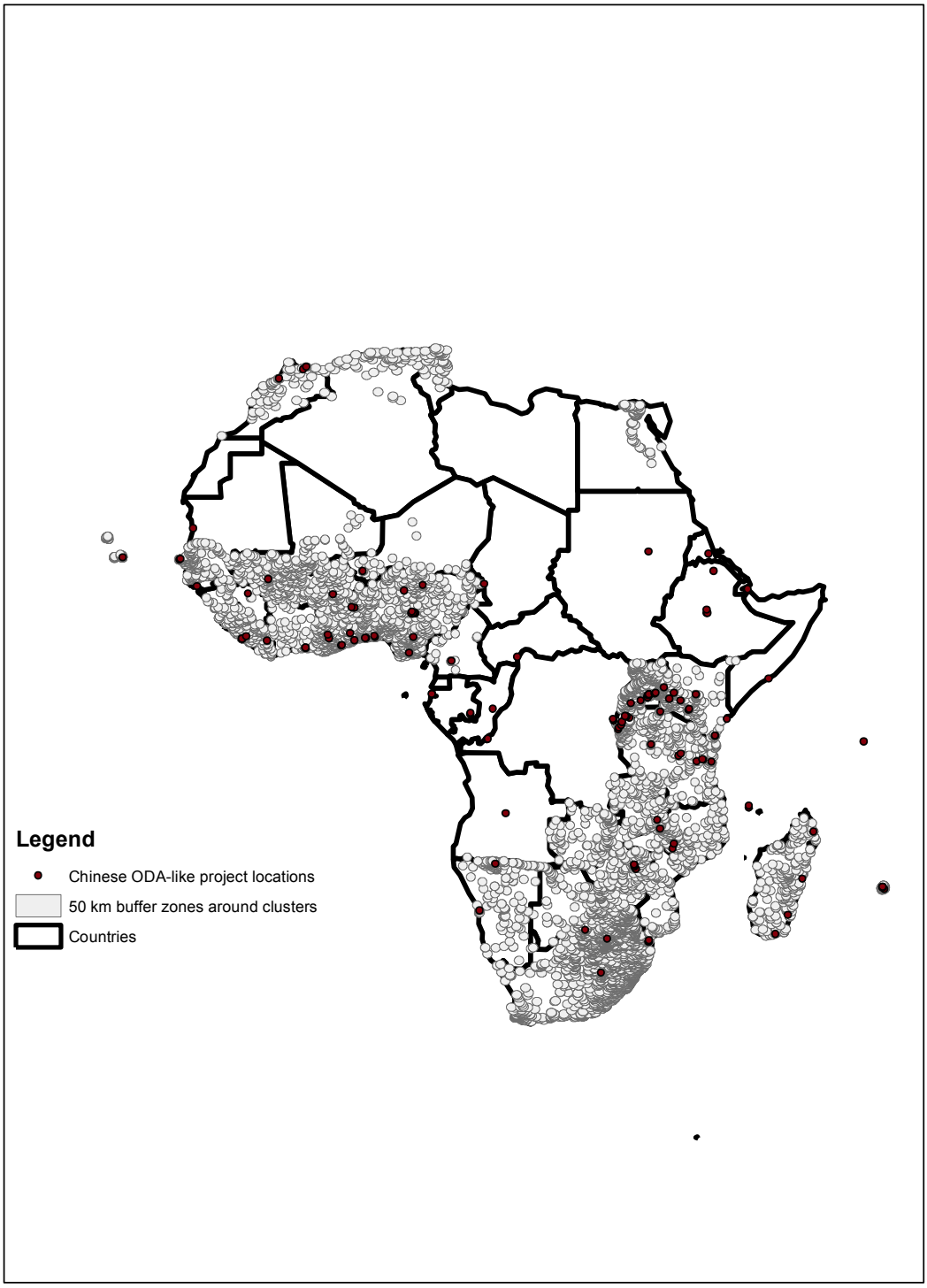
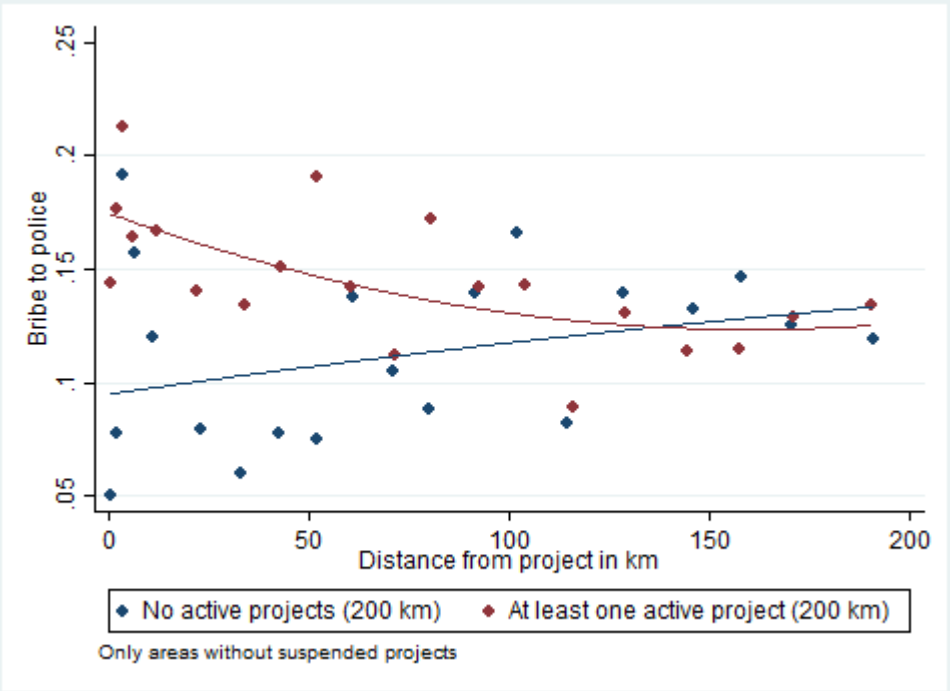

Legend
Chinese ODA-like project locations
50 km buffer zones around clusters
Countries

Figure 3

Panel A



Panel B

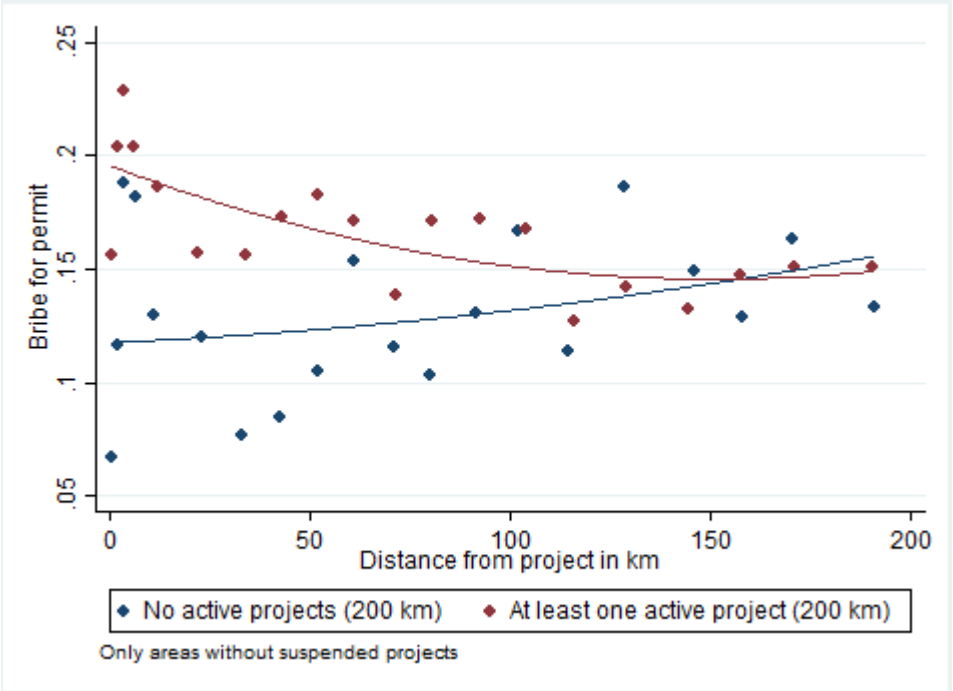


Table 1. Descriptive statistics based on the baseline sample for Chinese aid projects

	N	Mean	SD
Main outcome variables			
Police bribe dummy	63596	0.12	0.33
Police bribe ordinal	63596	0.22	0.65
Permit bribe dummy	63373	0.14	0.34
Permit bribe ordinal	63373	0.22	0.62
Aid variables			
Distance to closest project (km)	63596	154.52	165.47
active75	63596	0.29	0.45
inactive75	63596	0.16	0.37
active50	63596	0.27	0.44
inactive50	63596	0.12	0.32
active25	63596	0.19	0.39
inactive25	63596	0.07	0.26
Control variables			
age	63596	36.33	14.49
age2 (divided by 100)	63596	15.30	12.88
female	63596	0.50	0.50
urban	63596	0.45	0.50

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 A2.

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

	(1)	(2)
VARIABLES	Bribe police dummy	Bribe permit dummy
active50	0.052***	0.042***
	(0.009)	(0.009)
inactive50	-0.010	0.002
	(0.008)	(0.008)
Baseline controls	YES	YES
Year FE	YES	YES
Region FE	YES	YES
Difference in difference active50-inactive50	0.0616	0.0401
F test: active50-inactive50=0	40.79	16.83
p value of F test	1.83e-10	4.14e-05
Observations	63,596	63,684
R-squared	0.094	0.077

Robust standard errors (clustered by the survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 3. Robustness checks

Panel A: Police bribes				
VARIABLES	(1) Bribe police dummy	(2) Bribe police dummy	(3) Bribe police dummy	(4) Bribe police ordinal
active50			0.048***	0.082***
			(0.008)	(0.018)
inactive50			-0.009	-0.021
			(0.008)	(0.016)
active25	0.062***			
	(0.011)			
inactive25	-0.005			
	(0.009)			
active75		0.043***		
		(0.008)		
inactive75		-0.006		
		(0.007)		
terminated50			0.026***	
			(0.006)	
Diff-in-diff active-inactive	0.0666	0.0493	0.0573	0.103
F test: active-inactive=0	30.73	35.01	6.363	29.11
P-value of F-test	3.10e-08	3.46e-09	0.0117	7.10e-08
Observations	59,904	67,919	89,969	63,596
R-squared	0.095	0.091	0.096	0.096

Panel B: Permit bribes				
VARIABLES	(1) Bribe permit dummy	(2) Bribe permit dummy	(3) Bribe permit dummy	(4) Bribe permit ordinal
active50			0.033***	0.060***
			(0.008)	(0.016)
inactive50			0.002	-0.002
			(0.008)	(0.014)
active25	0.044***			
	(0.011)			
inactive25	-0.001			
	(0.009)			
active75		0.032***		
		(0.008)		
inactive75		0.007		
		(0.007)		
terminated50			0.014**	
			(0.007)	
Diff-in-diff active-inactive	0.0456	0.0251	0.0309	0.0615
F test: active-inactive=0	13.02	8.792	5.150	12.06
P-value of F-test	0.000311	0.00304	0.0233	0.000518
Observations	60,001	68,002	90,050	63,684
R-squared	0.078	0.075	0.080	0.078

Robust standard errors (clustered by the survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All estimations include baseline controls, region- and year fixed effects.

Table 4. Other bribe outcomes

VARIABLES	(1) Brime medical dummy	(2) Bribe for water dummy	(3) Bribe for school dummy	(4) Bribe for border control dummy
active50	0.053***	0.026***	0.018**	-0.056**
	(0.014)	(0.009)	(0.008)	(0.022)
inactive50	0.020	0.001	-0.004	-0.019
	(0.013)	(0.013)	(0.007)	(0.012)
Baseline controls	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Difference in difference	0.0326	0.0243	0.0222	-0.0366
F test: active50-inactive50=0	4.946	3.168	6.700	2.389
p value	0.0262	0.0752	0.00967	0.122
Observations	34,027	34,859	44,048	8,822
R-squared	0.112	0.087	0.075	0.082

Robust standard errors (clustered by the survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Nighttime light results for Chinese aid

Panel A: Police bribes

VARIABLES	(1) Bribe police dummy	(2) Bribe police dummy	(3) Median Light	(4) Bribe police dummy	(5) Bribe police dummy	(6) Bribe police dummy
active50	0.046***	0.069***	0.654**		0.069***	0.079***
	(0.009)	(0.012)	(0.285)		(0.012)	(0.013)
inactive50	-0.010	-0.012	0.751***		-0.013	
	(0.008)	(0.008)	(0.230)		(0.008)	
median				0.001	0.001	0.001
				(0.001)	(0.001)	(0.001)
Active*median						-0.002
						(0.002)
Diff-in-diff active-inactive	0.0562	0.0818	-0.0966		0.0819	
F test: active-inactive=0	27.20	38.94	0.115		39.04	
p value of F-test	1.90e-07	4.76e-10	0.734		4.51e-10	
Observations	6,569	5,218	5,219	5,219	5,219	5,219
R-squared	0.312	0.304	0.810	0.296	0.304	0.304

Panel B: Permit bribes

VARIABLES	(1) Bribe permit dummy	(2) Bribe permit dummy	(3) Median Light	(4) Bribe permit dummy	(5) Bribe permit dummy	(6) Bribe permit dummy
active50	0.038***	0.051***	0.654**		0.051***	0.061***
	(0.009)	(0.012)	(0.285)		(0.012)	(0.012)
inactive50	0.001	-0.007	0.751***		-0.008	
	(0.009)	(0.009)	(0.230)		(0.009)	
median				0.001	0.001	0.001
				(0.001)	(0.001)	(0.001)
Active*median						-0.002*
						(0.001)
Difference in difference	0.0372	0.0584	-0.0966		0.0585	
F test: active50-inactive50=0	11.81	20.18	0.115		20.27	
p value	0.000592	7.23e-06	0.734		6.90e-06	
Observations	6,570	5,219	5,219	5,220	5,220	5,220
R-squared	0.282	0.261	0.810	0.257	0.261	0.261

Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All estimations include the baseline controls, year fixed effects and region fixed effects.

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

VARIABLES	(1) Media should report corruption Chinese aid	(2) Media should report corruption World Bank aid
active50	0.006	0.058***
	(0.016)	(0.021)
inactive50	0.053***	0.018
	(0.019)	(0.025)
Baseline controls	YES	YES
Year FE	YES	YES
Region FE	YES	YES
Diff-in-diff active-inactive	-0.0475	0.0402
F test: active-inactive=0	5.425	3.334
p value of F-test	0.0199	0.0679
Observations	35,093	46,681
R-squared	0.062	0.057

Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 7. World Bank aid and local corruption

VARIABLES	(1) Bribe police dummy	(2) Bribe permit dummy
Active50	0.022*	0.036***
	(0.011)	(0.009)
Inactive50	0.010	0.027***
	(0.009)	(0.008)
Baseline controls	YES	YES
Year FE	YES	YES
Region FE	YES	YES
Diff-in-diff active-inactive	0.0116	0.00923
F test: active-inactive=0	1.331	0.985
p value	0.249	0.321
Observations	76,986	77,034
R-squared	0.090	0.074

Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 8. Nighttime light results for World Bank aid

Panel A: Police bribes						
VARIABLES	(1) Bribe police dummy	(2) Bribe police dummy	(3) Median Light	(4) Bribe police dummy	(5) Bribe police dummy	(6) Bribe police dummy
active50	0.015	0.010	-0.578***		0.010	0.005
	(0.010)	(0.012)	(0.095)		(0.012)	(0.012)
inactive50	0.014	0.014	-0.883***		0.015	
	(0.010)	(0.010)	(0.178)		(0.010)	
median				0.000	0.000	0.000
				(0.001)	(0.001)	(0.001)
Active*median						0.002
						(0.005)
Diff-in-diff active-inactive	0.00156	-0.00417	0.304		-0.00428	
F test: active-inactive=0	0.0168	0.0834	4.422		0.0878	
p value of F-test	0.897	0.773	0.0355		0.767	
Observations	7,854	5,692	5,693	5,692	5,692	5,692
R-squared	0.311	0.302	0.790	0.302	0.302	0.302
Panel B: Permit bribes						
VARIABLES	(1) Bribe police dummy	(2) Bribe permit dummy	(3) Median Light	(4) Bribe permit dummy	(5) Bribe permit dummy	(6) Bribe permit dummy
active50	0.026***	0.029***	-0.578***		0.029***	0.019*
	(0.009)	(0.011)	(0.095)		(0.011)	(0.011)
inactive50	0.020**	0.019**	-0.883***		0.020**	
	(0.009)	(0.009)	(0.178)		(0.009)	
median				0.000	0.001	0.000
				(0.001)	(0.001)	(0.001)
Active*median						0.012**
						(0.005)
Difference in difference	0.00594	0.00971	0.304		0.00955	
F test: active50-inactive50=0	0.307	0.554	4.422		0.536	
p value	0.580	0.457	0.0355		0.464	
Observations	7,855	5,693	5,693	5,693	5,693	5,693
R-squared	0.283	0.257	0.790	0.256	0.257	0.258

Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All estimations include the baseline controls, year fixed effects and region fixed effects.

Table 9. Chinese and World Bank transport aid and local corruption

	Transport projects		Health projects	
VARIABLES	(1) Bribe police dummy	(2) Bribe permit dummy	(3) Bribe police dummy	(4) Bribe permit dummy
Panel A: Chinese projects				
active50	0.065***	0.130***	0.004	-0.005
	(0.023)	(0.027)	(0.013)	(0.014)
inactive50	-0.051***	-0.007	0.014	0.016
	(0.019)	(0.020)	(0.014)	(0.014)
Diff-in-diff active-inactive	0.116	0.136	-0.0106	-0.0212
F test: active-inactive=0	16.31	15.68	0.336	1.217
p-value of F test	5.77e-05	7.98e-05	0.563	0.270
Observations	12,648	12,708	14,466	14,451
R-squared	0.051	0.035	0.079	0.071
Panel B: World Bank projects				
active50	-0.004	0.023*	-0.010	0.004
	(0.011)	(0.012)	(0.011)	(0.011)
inactive50	0.026	0.040	0.005	0.027***
	(0.024)	(0.030)	(0.011)	(0.010)
Diff-in-diff active-inactive	-0.0298	-0.0169	-0.0149	-0.0232
F test: active-inactive=0	1.601	0.362	1.072	2.944
p-value of F test	0.206	0.547	0.301	0.0864
Observations	12,055	12,109	20,036	20,017
R-squared	0.043	0.027	0.067	0.056

Robust standard errors (clustered by survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All estimations include the baseline individual controls and year fixed effects. Due to the limited variation in our data in these estimations (there are only 42 Chinese transport project sites and 49 Chinese health project sites) we use country rather than region fixed effects. Columns 1 and 2 restrict the sample to include only recipient countries where respondents can be linked to both active and inactive Chinese and World Bank transport aid projects (Mali, Mozambique and Nigeria). Columns 3 and 4 restrict the sample to include only recipient countries where respondents can be linked to both active and inactive Chinese and World Bank health aid projects (Ghana, Kenya, Madagascar and Uganda).

Table 10. Chinese and other bilateral aid project sites in Nigeria, Uganda and Senegal

	Other bilateral aid		Chinese aid	
VARIABLES	Bribe police dummy	Bribe permit dummy	Bribe police dummy	Bribe permit dummy
active50	0.014	0.043***	-0.010	0.037*
	(0.017)	(0.016)	(0.018)	(0.020)
inactive50	-0.003	0.018	-0.012	-0.027
	(0.019)	(0.018)	(0.021)	(0.022)
Diff-in-diff active-inactive	0.0172	0.0254	0.00118	0.0640
F test: active-inactive=0	1.376	3.633	0.00322	8.529
p-value of F test	0.241	0.0569	0.955	0.00357
Observations	16,216	16,213	11,742	11,746
R-squared	0.073	0.042	0.086	0.048

Robust standard errors (clustered by survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All estimations include the baseline individual controls, year fixed effects and region fixed effects.

Table 11. Comparing US, Japanese and Chinese aid to Uganda

	US aid to Uganda		Japanese aid to Uganda		Chinese aid to Uganda	
VARIABLES	Bribe police dummy	Bribe permit dummy	Bribe police dummy	Bribe permit dummy	Bribe police dummy	Bribe permit dummy
active50	-0.037	-0.003	-0.040	-0.088***	-0.029	-0.015
	(0.029)	(0.031)	(0.037)	(0.031)	(0.023)	(0.031)
inactive50	0.038	-0.031	-0.001	-0.007	0.019	-0.006
	(0.024)	(0.024)	(0.023)	(0.017)	(0.036)	(0.037)
Diff-in-diff active-inactive	-0.0745	0.0274	-0.0385	-0.0810	-0.0482	-0.00952
F test: active-inactive=0	3.540	0.409	1.658	7.994	1.733	0.0723
p-value of F test	0.0603	0.523	0.198	0.00484	0.189	0.788
Observations	8,743	8,723	8,594	8,574	4,621	4,614
R-squared	0.035	0.039	0.035	0.041	0.048	0.042

Robust standard errors (clustered by survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All estimations include the baseline individual controls, year fixed effects and region fixed effects.

Appendix

Table A1. Sectoral composition of our sample of Chinese aid projects

Sector	Freq.	Percent	Cum.
Agriculture, Forestry and Fishing	9	3.96	3.96
Communications	11	4.85	8.81
Developmental Food Aid/Food Security ..	2	0.88	9.69
Education	20	8.81	18.50
Emergency Response	6	2.64	21.15
Energy Generation and Supply	5	2.20	23.35
Government and Civil Society	38	16.74	40.09
Health	49	21.59	61.67
Industry, Mining, Construction	1	0.44	62.11
Other Multisector	4	1.76	63.88
Other Social infrastructure and servi..	20	8.81	72.69
Population Policies / Programmes and ..	7	3.08	75.77
Trade and Tourism	1	0.44	76.21
Transport and Storage	42	18.50	94.71
Water Supply and Sanitation	10	4.41	99.12
Women in Development	2	0.88	100.00
Total	227	100.00	

Table A2. Further robustness tests

VARIABLES	(1) Bribe police dummy cluster region	(2) Bribe police dummy multiway clustering: region and year	(3) Bribe police dummy cluster country	(4) Bribe police dummy Less controls	(5) Bribe police dummy Extra controls
active50	0.052**	0.052***	0.052***	0.056***	0.047***
	(0.023)	(0.020)	(0.013)	(0.009)	(0.009)
inactive50	-0.010	-0.010	-0.010	-0.010	-0.011
	(0.011)	(0.010)	(0.012)	(0.008)	(0.007)
Observations	63,596	63,596	63,596	66,307	62,969
R-squared	0.094	0.094	0.094	0.081	0.098
Baseline controls	YES	YES	YES	No	Extra
Year FE	YES	YES	YES	YES	YES
Country FE	NO	NO	NO	NO	NO
Region FE	YES	YES	YES	YES	YES
Difference in difference	0.0616	0.0616	0.0616	0.0659	0.0580
F test: active50-inactive50=0	5.997	9.275	12.43	45.30	38.97
p value	0.0148	0.00232	0.00147	0	4.62e-10

Robust standard errors in parentheses. The standard errors are clustered at the region level in column 1, at the country level in column 2 and at the Afrobarometer cluster level in columns 3 and 4. Column 3 omits the baseline controls and column four contains controls for working, being without cash, and education (see the variable description in Table A1)

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

Table A3. Variable descriptions

Dependent variables, experiences with corruption

Police bribe dummy: Dummy variable equal to one if, during the past year, the respondent has 'had to pay a bribe, give a gift, or do a favour to government officials in order to 'Avoid a problem with the police (like passing a checkpoint or avoiding a fine or arrest)'; zero otherwise.

Police bribe ordinal: ranging between 0 and 3, capturing the response categories 'Never', 'Once or twice', 'A few times', and 'Often', respectively, given in response to the question if the respondent has 'had to pay a bribe, give a gift, or do a favour to government officials in order to 'Avoid a problem with the police (like passing a checkpoint or avoiding a fine or arrest)' during the past year.

Permit bribe dummy: Dummy variable equal to one if, during the past year, the respondent has 'had to pay a bribe, give a gift, or do a favour to government officials in order to get a document or permit'; zero otherwise.

Permit bribe ordinal: ranging between 0 and 3, capturing the response categories 'Never', 'Once or twice', 'A few times', and 'Often', respectively, given in response to the question if the respondent has 'had to pay a bribe, give a gift, or do a favour to government officials in order to get a document or permit' during the past year.

Proximity to Chinese project sites

Active50: Dummy variable equal to one if the respondent lives within 50 km of a site where a Chinese aid project is being implemented at the time of the interview, zero otherwise.

Active25: Same as Active50 but using a 25 km cut-off.

Active75: Same as Active50 but using a 75 km cut-off.

Inactive50: Dummy variable equal to one if the respondent lives within 50 km of a Chinese projects site where the implementation of the project had not yet started at the time of the interview and do not have any active or terminated project within this same distance, zero otherwise.

Inactive25: Same as Inactive50 but using a 25 km cut-off.

Inactive75: Same as Inactive50 but using a 75 km cut-off.

Terminated50: Dummy variable equal to one if the respondent lives within 50 km of a terminated Chinese project and do not have any active project within this same distance, zero otherwise.

Terminated25: Same as Terminated50 but using a 25 km cut-off.

Terminated75: Same as Terminated50 but using a 75 km cut-off.

Individual control variables

Female: Dummy variable equal to one if the respondent is female; zero otherwise.

Urban: Dummy variable equal to one if the respondent lives in an urban area; zero otherwise.

Age variables: Age in years and age squared.

Year dummies: Dummies for interview year, 2002-2013

Sub-national region dummies: Dummies for the 352 sub-national regions (first-order administrative division, indicated region or province in the Afrobarometer) in the sample

Table A4. Controlling for mining

VARIABLES	(1) Active mines 50km	(2) Bribe police dummy	(3) Bribe permit dummy
Active aid 50	-0.045**	0.052***	0.042***
	(0.019)	(0.009)	(0.009)
Inactive aid 50	-0.052***	-0.010	0.002
	(0.018)	(0.008)	(0.008)
Active mine 50		0.007	0.002
		(0.007)	(0.007)
Inactive mine 50		-0.026**	-0.033**
		(0.012)	(0.014)
Observations	64,153	63,596	63,684
R-squared	0.692	0.094	0.078
Baseline controls	YES	YES	YES
Year FE	YES	YES	YES
Country FE	NO	NO	NO
Region FE	YES	YES	YES
Difference in difference aid	0.00687	0.0616	0.0401
F test: active-inactive=0	0.348	40.62	16.81
p value aid	0.555	1.99e-10	4.19e-05
Difference in difference mine		0.0332	0.0351
F test: active-inactive=0 (mine)		6.070	5.990
p value mine		0.0138	0.0144

Robust standard errors (clustered by the survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table A5. Chinese development projects including both 'ODA-like', 'OOF-like' and 'vague' official finance

VARIABLES	(1) Bribe police dummy	(2) Bribe permit dummy
active50	0.064***	0.051***
	(0.009)	(0.009)
inactive50	0.002	0.012
	(0.008)	(0.008)
Baseline controls	YES	YES
Year FE	YES	YES
Region FE	YES	YES
Diff-in diff	0.0619	0.0392
F test: active-inactive=0	46.31	18.78
p value of F-test	0	1.49e-05
Observations	66,002	66,085
R-squared	0.096	0.079

Robust standard errors (clustered by the survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table A6. Sample splits

VARIABLES	(1) Richer	(2) Poorer	(3) More corrupt	(4) Less corrupt	(5) More democratic	(6) Less democratic
Panel A: Bribe police dummy						
active50	0.033***	0.065***	0.031***	0.029	0.028***	0.005
	(0.012)	(0.013)	(0.009)	(0.018)	(0.010)	(0.018)
inactive50	0.005	-0.013	-0.005	-0.034**	-0.002	-0.043**
	(0.009)	(0.009)	(0.007)	(0.015)	(0.007)	(0.018)
Diff-in-diff active-inactive	0.0276	0.0776	0.0368	0.0625	0.0294	0.0481
F test: active-inactive=0	4.777	34.84	12.55	10.40	8.900	5.047
p value of F-test	0.0289	4.23e-09	0.000402	0.00128	0.00287	0.0248
Observations	33,058	28,097	32,753	28,402	39,021	22,134
R-squared	0.100	0.087	0.052	0.088	0.088	0.073
Panel B: Bribe permit dummy						
active50	0.057***	0.025*	0.013	0.030*	0.032**	0.003
	(0.013)	(0.013)	(0.012)	(0.017)	(0.013)	(0.018)
inactive50	0.011	0.003	0.002	-0.012	0.013*	-0.052***
	(0.009)	(0.010)	(0.007)	(0.016)	(0.007)	(0.018)
Difference in difference	0.0461	0.0225	0.0111	0.0425	0.0187	0.0545
F test: active50-inactive50=0	11.44	2.370	0.712	3.675	1.941	5.869
p value	0.000726	0.124	0.399	0.0554	0.164	0.0155
Observations	33,089	28,152	32,780	28,461	39,096	22,145
R-squared	0.084	0.072	0.052	0.073	0.083	0.060

Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All estimations include baseline controls, year fixed effects and region fixed effects.

Table A7. Chinese aid and local corruption: Effects in different time periods.

	Police bribes			Permit bribes		
VARIABLES	Waves 2-3	Waves 3- 4	Waves 4-5	Waves 2-3	Waves 3-4	Waves 4-5
active50	0.059***	0.066***	0.031***	0.040***	0.040***	0.030***
	(0.017)	(0.011)	(0.010)	(0.013)	(0.011)	(0.009)
inactive50	-0.012	-0.003	-0.025*	-0.010	-0.001	-0.005
	(0.009)	(0.010)	(0.013)	(0.008)	(0.010)	(0.012)
Baseline controls	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES
Diff-in-diff	0.0715	0.0685	0.0559	0.0501	0.0416	0.0352
F test: active-inactive=0	15.07	24.05	18.93	10.75	9.232	6.749
p value of F test	0.000106	9.75e-07	1.39e-05	0.00105	0.00239	0.00941
Observations	32,389	44,939	50,250	32,401	44,996	50,331
R-squared	0.108	0.093	0.117	0.082	0.065	0.106

Robust standard errors (clustered by the survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table A8. Chinese aid and local corruption. Project location fixed effects

VARIABLES	(1) Bribe police dummy	(2) Bribe police dummy	(3) Bribe police dummy	(4) Bribe permit dummy	(5) Bribe permit dummy	(6) Bribe permit dummy
active50	0.029*			0.015		
	(0.016)			(0.020)		
active25		0.037*			0.014	
		(0.021)			(0.029)	
active75			0.025*			0.008
			(0.013)			(0.017)
Observations	14,477	9,018	19,392	14,474	9,014	19,393
R-squared	0.108	0.107	0.089	0.107	0.103	0.100
Baseline controls	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES
Project FE	YES	YES	YES	YES	YES	YES

Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1; All estimations include the baseline controls, year fixed effects, project fixed effects, and region fixed effects.

Table A9. Chinese aid and having no experience with the police or with applying for documents and permits

VARIABLES	(1) No experience with police	(2) No experience with permit
active50	0.045**	0.040**
	(0.019)	(0.019)
inactive50	-0.012	-0.028*
	(0.016)	(0.015)
Observations	53,560	53,657
R-squared	0.128	0.098
Baseline controls	YES	YES
Year FE	YES	YES
Country FE	NO	NO
Region FE	YES	YES
Difference in difference	0.0577	0.0682
F test: active50-inactive50=0	6.235	9.517
p value	0.0126	0.00205

Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table A10. Chinese aid and presence of police

VARIABLES	(1) Police station	(2) Police officer
Active50	-0.075***	-0.017
	(0.025)	(0.023)
Inactive50	-0.012	-0.026
	(0.026)	(0.021)
Baseline controls	YES	YES
Year FE	YES	YES
Country FE	NO	NO
Region FE	YES	YES
Difference in difference	-0.0636	0.00828
F perception1: active50-inactive50=0	3.004	0.126
p value	0.0831	0.722
Observations	63,242	63,964
R-squared	0.247	0.247

Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table A11. World Bank aid and presence of police

VARIABLES	(1) Police station	(2) Police officer
Active50	-0.022	-0.005
	(0.029)	(0.030)
Inactive50	-0.071**	-0.009
	(0.034)	(0.036)
Baseline controls	YES	YES
Year FE	YES	YES
Country FE	NO	NO
Region FE	YES	YES
Difference in difference	0.0485	0.00408
F perception1: active50-inactive50=0	2.320	0.0132
p value	0.128	0.909
Observations	76,478	77,375
R-squared	0.254	0.239

Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table A12. World Bank aid and having no experience with the police or with applying for documents and permits

VARIABLES	(1) No experience with police	(2) No experience with permit
active50	0.016	0.030*
	(0.017)	(0.017)
inactive50	0.014	0.001
	(0.020)	(0.021)
Observations	66,279	66,345
R-squared	0.110	0.092
Baseline controls	YES	YES
Year FE	YES	YES
Country FE	NO	NO
Region FE	YES	YES
Difference in difference	0.00123	0.0282
F test: active50-inactive50=0	0.00407	1.976
p value	0.949	0.160

Robust standard errors (clustered by the geographical survey clusters) in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table A13. Other bilateral donors involved in Uganda, Nigeria and Senegal

Donors	Number of project sites
Austria	6
Belgium	2
Belgium / France	3
Canada	11
France	25
Germany	5
Saudi fund	4
Ireland	8
Italy	1
Japan	113
Netherlands	1
Norway	2
Spain	2
Switzerland / Sweden	6
United Kingdom	2
United States of America	346
Total	537

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