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# Chinese Presence in Africa: For better or worse?

A Study on the Effects of Chinese Aid on Press Freedom in Africa

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# Abstract

Over the last 20 years, China's growth has increased exponentially, and one aspect of this growth is increasing Chinese presence in many international and economic settings. In this study we focus on the international impact of Chinese foreign aid and in more detail how it influences the press freedom in Africa on a cross-national and sub-national level. On an aggregate level, we measure how the number of projects sites in a country affect the press freedom score using Varieties of Democracy's press freedom index. To capture the effect on a micro-level, we measure the difference in perceived level of press freedom when living close to an ongoing Chinse aid project site and a projected Chinese aid project site. The results suggest strongly significant and negative effect of Chinese presence locally while we find no evidence for such an effect at an aggregated level.



**Keywords:** Foreign Aid, China, Africa

**JEL Classification:** F35, O55

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We would like to thank our supervisor Joseph Vecci for valuable inputs throughout the whole process. All faults are our own.

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# 1 Introduction

In 2013 Xi Jinping announced China's latest major investment program: the "One Belt, One Road"-initiative (today called Belt and Road Initiative (BRI)). The BRI has played a crucial role to promote growth in Africa and already in 2009, China became the largest trade partner to the continent (Bohman 2018). While the project has been praised by many as a large investment in infrastructure which has contributed to economic growth for developing countries (Bohman, 2018; Dreher et al., 2021), others have expressed concerns as they believe China to be pursuing a "debt trap diplomacy", utilizing the smaller economies inability to repay loans given by China to increase their influence over strategically important areas (Bohman, 2018:22; Bräutigam, 2011). As an example, in May of 2017 Djibouti inaugurated their newly built, high-tech deep-water port which was financed predominantly through Chinese loans. A couple of months later, China announced the opening of their first military base outside its own borders, placed in Djibouti (Bohman, 2018). China's economic rise has made them one of the largest creditors worldwide and alongside this, also one of the largest aid donors in the world (Dreher et al., 2021; Horn et al., 2021).

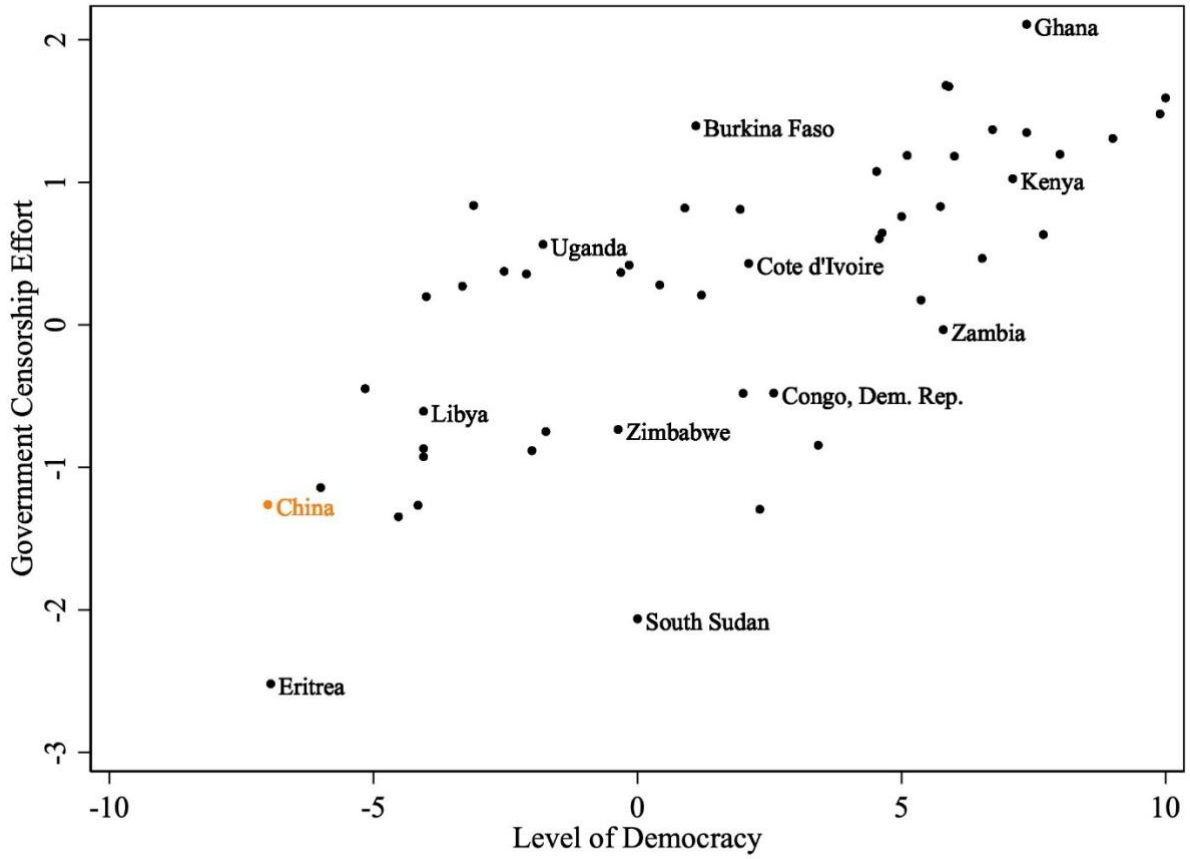
As Chinese aid increases it is important to understand its impacts. One area in which its influence is plausible is on the press freedom in the recipient countries (Dutta and Williamson, 2016). Along all major press freedom indices, China has continued to rank as one of the worst countries in the world (Reporters Without Borders, 2022; Freedom House, 2020; Varieties of Democracy, 2022; The World Press Freedom Index, 2022). Recent research has highlighted negative effects of Chinese aid (Isaksson and Kotsadam, 2018a; 2018b; 2020; Cha, 2020; Iacoella et al., 2021), with some evidence suggesting a possible spread of Chinese censorship practices to its closer allies (Gamso, 2021). In this study we therefore focus on the international impact of Chinese growth and ask whether Chinese presence – in the form of Chinese aid – influences press freedom in Africa. We then further explore potential differences between actual press freedom and perceived press freedom, in the spirit of the recognition that no improvements in press freedom is likely if no perceived need is established among citizens. To investigate this in more detail, we structure this thesis into two distinct parts. The first aims to capture the effect of Chinese aid on aggregate levels of press freedom. The second instead focuses on the local effects of Chinese aid, specifically on the perceptions and beliefs about press freedom among the local populations most exposed to it. More formally, we ask the following two research questions:

**Research question 1:** How does Chinese aid in Africa affect the cross-national levels of press freedom determined by Varieties of Democracies (V-Dem).

**Research question 2:** How does exposure to Chinese aid through aid project sites affect the local perception of press freedom in Africa.

Traditionally, levels of press freedom have been closely linked to levels of democracy (Högström, 2013), and to get an initial sense of how China relates to the African countries along these parameters, Figure 1 presents an illustrative overview.

**FIGURE 1: Average Level of Democracy and Government Censorship, 2000-2018**




Note: Democracy is the Polity IV's variable *Polity2* ranging from -10 (fully authoritarian) to 10 (fully democratic). Government censorship effort is V-Dem's variable *v2mecenefm* ranging from -5 (direct and frequent efforts to censor media) to 5 (government rarely attempts to censor major media, and when attempts are discovered, the responsible officials are usually punished).

To estimate the effect of Chinese aid projects on national levels of press freedom in Africa, we use a fixed effects linear model based on panel data from AidData and Varieties of Democracy. To estimate the effect on a sub-national level, we build upon the econometrical framework of Knutsen et al. (2016) and follow a spatial-temporal strategy to perform a difference-in-difference type of estimation. The chief advantage of this approach is that it reduces the Chinese

selection effect of its aid allocation (where to implement the aid project sites). We are then able to estimate the local effects of proximity to Chinese aid project sites on perceptions of press freedom in Africa.

At an aggregated level the results are statistically insignificant which would indicate that we cannot capture any possible effect of Chinese aid project sites on the aggregated level of press freedom. This is likely due to the fact we cannot control for the endogeneity bias at a cross-national level. However, when we instead measure the effect on a sub-national level the results are statistically significant. Suggesting that the implementation of Chinese aid project sites is associated with negative effects on the local perceived media freedom in Africa. These results are robust over several model specifications and when comparing the effect of Chinese aid project sites with the effect of World Bank aid, the results stand in the opposite direction to each other.

This study contributes to the literature in several ways. While there are studies investigating the effect of foreign aid on press freedom in general (Dutta and Williamson, 2021), there is no previous research that has analyzed the effect of *Chinese aid* on press freedom in Africa specifically. We also extend the analysis of the local effects of Chinese aid (Isaksson and Kotsadam, 2018a; 2018b; 2020; Cha, 2020) to include perceived levels of press freedom. Further, the discrepancy between Chinese aid and World Bank aid points to Chinese aid being systematically different in its effects when compared to World Bank aid. All the ways in which this difference expresses itself is an active field of research, and the body of knowledge is expanding at an impressive rate. Exploring Chinese aids' local effects on media freedom, our findings contribute to this project.

The second section of this thesis presents a brief overview of the relevant previous research, with subsections covering research on aid generally, Chinese aid specifically, and the literature on press freedom. In Section 3 we present the theoretical considerations informing the hypotheses made. Section 4 presents the data and methodology used to investigate our research questions. Section 5 then presents our results and main findings. Section 6 then discuss the findings, relating them to the previous literature as well as exploring potential mechanisms. Lastly, section 7 concludes, highlighting interesting areas for future research.

# 2 Literature Review

## 2.1 Aid

The effect and efficiency of foreign aid has long been debated among scholars. Consulting the empirical evidence, the overall assessment is best described as mixed (Qian, 2015). Some find that foreign aid has a large positive impact on economic development (Dalgaard and Hansen, 2005; Sachs, 2006; 2014) while others remain more skeptical (Rajan and Subramanian, 2005; 2011; Easterly, 2003; 2006; Deaton, 2013; Islam and Osmani, 2017). According to Clemens et al. (2012), this variation of results could be due to measurement error and endogeneity more generally. They argue that when measuring aid, one must carefully consider the timing of the aid. For example, if the aid is directed towards improving the infrastructure in a country, we are likely to see effects in a comparatively short period of time. However, if the aid was directed towards improving health markers, for example using vaccination campaigns to mitigate the spread of diseases, it is plausible that the effect takes comparatively long to materialize. Hence, not accounting for the timing of aid increases the risk of type-II errors (i.e., not discovering an effect when there is one).

Shifting focus to specifically *Chinese* aid, the most extensive analysis of Chinese aid's effect on growth has been conducted by Dreher et al. (2021a), who provides evidence for a positive growth effect of Chinese aid projects. Using data from 38 African countries between 2000 and 2014, the authors establish a 0.41-1.49 percent increase in short-term regional growth as a result of an additional aid project. At the same time, Brazys and Vadlamannati (2018) find evidence of an 'aid curse' from Chinese aid – where reliance on Chinese aid ultimately undermines necessary economic reforms in the recipient countries.

Literature has also noted a tendency of Chinese aid to be taken advantage by recipient political leaders in Africa. This research suggests 'regional favoritism' (Hodler and Raschky, 2014) – where political leaders give more financial flows to their birth regions – to be more pronounced in Chinese aid flows when compared to World Bank aid (Dreher et al., 2017; 2019). However, following up on their previous work, Dreher et al., (2021b) find that while this skewed allocation may still be present, the economic efficiency of the Chinese aid does not seem to be affected. In other words, the allocation does not indicate that the aid goes to waste. Rather, the Chinese aid still improves local economic development, even in the presence of favoritism; ultimately, not threatening Chinese aid effectiveness. The results are thus in line with the literature on the growth effects of Chinese aid more generally (Dreher et al., 2021a).

Much of Chinese aid focuses on infrastructure projects and developing a 'business-like' relationship with its recipient countries, prioritizing outcomes of 'mutual benefit' (Rebol, 2010; Wang and Ozanne, 2010). This foreign aid practice has received significant criticism (Harchaoui et al., 2021; Alden et al., 2008), and concerns have been raised that Chinese aid may be driven by an agenda to gain dominance through long term debt (Bohman, 2018; Bräutigam, 2011). The aid characteristic of non-conditionality has also been questioned, as conditions and demands surrounding aid projects – even before its implementation – could be imposed by Chinese contractors and financiers just as well, making the aid *de facto* conditional (Bräutigam, 2009).

Applying inventive methods of geo-matching, a new branch of literature on sub-national (local) effects of Chinese aid has emerged. This growing body of literature is highly relevant for this study, noting both the intended and unintended local effects of Chinese aid. Some of these local effects of Chinese aid include higher levels of corruption (Isaksson and Kotsadam, 2018a; Cha, 2020), lower levels of labor union engagement (Isaksson and Kotsadam, 2018b), more frequent protests (Iacoella et al., 2021), and increased mobilization of ethnic identification (Isaksson, 2020). While most of these effects are negative, some research instead find evidence for positive effects: Bluhm et al. (2018) investigates how Chinese aid might affect the income distribution in targeted aid regions and find significant results that Chinese aid is associated with increased equality. Aid in the transport sector exhibited the largest effect, with a significant mechanism being positive spillover effects from extended networks of roads and railways, nurturing economic activity in poorer regions.

Another branch of literature focuses on Chinese efforts to pursue 'soft power' abroad (King, 2013; Wasserman, 2013; 2016; Rønning, 2016; Zhang et al., 2016; Arif, 2017; Abodohoui and Su, 2020; Blair et al., 2021; Wellner et al., 2022). Studying the instrumental use of foreign aid for his pursuit, Wellner et al., (2022) find Chinese aid projects to increase public support for the Chinese government in recipient countries. Using survey data from Gallup World Poll, covering 126 countries over the years 2006-2017, they estimate an additional Chinese project site to increase public support for China by 3 percent in the short run, and 0.2 percent in the long run.

## 2.2 Freedom of Press

Freedom of the press is often deemed to be a foundational component of a free society and functional democracy (Stiglitz, 2002; Norris, 2006; Becker et al., 2007; Karikari, 2004;

Högström, 2013). A relevant aspect of press freedom in this study is within a specific Chinese context. Although recent work, for example by Guo (2020), highlight complexities and nuances in the conceptual history of press freedom in China – containing intricacies beyond the scope of this paper – it is still generally accepted that the nation's current media freedom is severely suppressed, placing it below most African countries on country rankings (Reporters Without Borders, 2022; Freedom House, 2020; Varieties of Democracy, 2022). Moreover, concerns have been raised regarding the potential spread of censorship practices as China's influence at the international stage has increased, with reports on the Chinese state pressuring international media companies (Mozur, 2018) and trying to influence academic environments in the West (Rachman, 2019).

However, despite this fear of Chinese censorship spreading beyond its borders, there has been little empirical research done to support these claims. Gamso (2020) investigates whether the rise of China's presence also includes a rise in export of Chinese culture, norms, and a more progressive censorship. Through analyzing the trade dependency among countries which trade with China, the author finds significant evidence that countries which are more dependent on Chinese trade, also are more likely to adapt Chinese censorship of its domestic media. These results are mediated by regime type and Gamso (2020) conclude that struggling democracies are more susceptible (than authoritarian states) to an increased press freedom suppression as a result of higher reliance on Chinese import and export.

Another relevant body of research is the study of press freedom in a specific African context. Investigating the relationship between press freedom and corruption in Africa, Hamada et al. (2019) find higher levels of press freedom to be associated with lower levels of corruption. Onyango-Obbo (2014) argues that increased levels of democracy in an African context does not result in higher levels of press freedom. Studying democratic development in Nigeria specifically, Akinwale (2010) finds that the role of press freedom facilitated the country's democratic development, albeit facing significant challenges along the way.

Most studies on press freedom use one of the major press freedom indices. An important point for this study is to investigate the potential difference between citizen attitudes of press freedom and such major indices at a country level. Cohen (2020) juxtapose public opinion data from 10 countries in Sub-Saharan Africa with press freedom rankings from Reporters Without Borders, investigating possible differences between citizen perceptions of national press freedom and the evaluation done by country rankings (see Reporters Without Borders, Freedom House, and Varieties of Democracy). The author finds distinct variations between countries and conclude an absence of a clear relationship between the two measures.

To explain the differences, Cohen (2020) propose cultural and political landscapes as probable driving factors. This potential schism between citizen attitudes and largescale evaluations of press freedom informs the structure of this study, where the first part will investigate the effects of Chinese aid on aggregate cross-country levels of press freedom using country rankings, and part two will delve into the effects of Chinese aid project sites on the attitudes among the local population about press freedom.

# 3 Theory, Mechanisms and Hypotheses

There is no existing theoretical model through which predictions on the effects of Chinese aid on media freedom can be made. Instead, relevant literature spanning from different fields will be used to inform potential mechanisms, ultimately guiding the hypotheses made.

## 3.1 Part I – Cross-National: Aggregate effects on media freedom

For part one, we investigate how Chinese aid in Africa affect recipient countries aggregate levels of press freedom, as determined by Varieties of Democracies (V-Dem).

There is a set of literature which suggests the relationship between foreign aid and media freedom is positive. Dutta and Williamson (2016) outline three mechanisms for this positive relationship: conditionality, transfer of knowledge, and direct monetary transfers. Applying these mechanisms of positive effects of foreign aid more generally to *Chinese* aid specifically, uncovers important points on how aid from China differs from aid given by other donors (Bräutigam, 2011), and how this may influence the direction of effects. First, positive effects from *conditionality* are based on the idea that "Aid buys reform" (Collier, 1997); aid incentivizes recipient political leaders to reform in the donor's preferred direction, in this context reforms for increased media freedom. Because of China's explicit *non*-conditionality approach to foreign aid (State Council, 2014; 2021), this channel of positive effect is, however, likely non-existent. Second, positive effects from a *transfer of knowledge* on implementation of policies and technologies promoting media freedom are also highly unlikely. This judgement is based on China's extensive domestic efforts to counteract a freer media, reflected in all country rankings of media freedom (Freedom House, 2020; Reporters Without Borders, 2021; V-Dem, 2020). Third, positive effects from *direct monetary transfers* are based on the recipient country's own investment in freer media. An example of this could be media democratization via investments in expanded media infrastructure, reaching a larger share of the population. This positive mechanism could be active for Chinese aid, however, without any conditionality

or transfer of knowledge, the positive effect is solely based on the will of the receiving political leaders to improve the countries media freedom. All in all, the mechanisms proposed by Dutta and Williamson (2016) – where the relationship between foreign aid and press freedom is positive – seem to be cancelled out by the characteristics of Chinese aid. In other words, the positive effects generally associated with foreign aid seem not applicable when considering aid from the Chinese state, weakening any hypothesizing of a positive effect of Chinese aid on media freedom.

There is still evidence for the positive effects of Chinese aid in terms of economic growth (Dalgard and Hansen, 2005; Dreher and Lohmann, 2015; Dreher, 2017; Bluhm et al., 2018; Dreher et al., 2021; Guo and Jiang, 2021). Research also suggests a reciprocal relationship between growth and press freedom (Alam and Shah, 2013; Roll and Talbott, 2003). Combined, these sets of literature thus indicate a positive – albeit indirect – effect of Chinese aid on media freedom, operating through its positive effects on economic development. We believe this (indirect) positive effect is not strong enough on its own to warrant a hypothesis in the positive direction, but it will still be considered.

Another set of literature suggests instead the relationship between Chinese aid and media freedom is negative. These negative channel stems from characteristics of China as a donor, separating itself from other donors. Three strands of argumentation stand out. The first is about how aid from different donors is used by recipient countries. According to Dreher and Gehring (2012), aid could help to expand the power of the elite, who may well decide against using any aid dollars to increase the rights of the people (e.g., press freedom), and instead prioritize self-interests. China's aid strategy of non-conditionality intuitively strengthens this possibility, and while western aid has been shown to be taken advantage of by recipient political leaders (Hodler and Raschky, 2014), evidence suggest Chinese aid is more susceptible than World Bank aid to political take-over, ultimately ending up in the pockets of the elite (Dreher et al., 2017; 2019).

Another argument which supports a negative relationship between Chinese aid and press freedom revolves around corruption. There is a set of literature which suggests a positive relationship between foreign aid and levels of corruption (Svensson, 2000; Knack, 2001; Alesina and Weder, 2002; Pavlik and Young, 2021). There is also a set of literature covering specifically Chinese aid, finding ties to increases in local levels of corruption (Kelly et al., 2016; Brazys et al., 2017; Isaksson and Kotsadam, 2018a; Cha, 2020). Combined, the two sets suggest a positive relationship between Chinese aid and corruption. Considering the negative effects of corruption on press freedom (Solis and Angeli, 2017; Hamada et al., 2019), one could

argue for an *indirect*, negative effect of Chinese aid on press freedom, mediated by levels of corruption. This indirect effect supports a hypothesis of a negative effect of Chinese aid on press freedom.

Moreover, studying freedom of the press in Africa, VonDoepp and Young (2015) argue a main aspect of hindrance for governments to infringe on press freedom rights is the political cost of doing so. Since China holds one of five permanent seats at the United Nations (UN) Security Council they have the possibility of veto resolutions that might condemn the recipient country or affect the process of pressure abusive regimes in Africa. This has also been done in the past, where China has delayed resolutions by threatening with a veto. Brown and Sriram (2009) further argues that regimes may increase or continue its abuse of human rights "with the promise that China will shield them from the international community's response" (Brown and Sriram, 2009:255). Studying effects of Chinese aid on levels of conflict in Africa, Gehring et al. (2019) find non-lethal government reprimands against civilians to be more prevalent in regions of Chinese presence compared to other donors (World Bank). Having a large actor, such as China, backing the recipient country, may thus reduce the political cost of abusing rights such as press freedom (Gamso 2019). Furthermore, economic ties to China, in the form of trade dependency, has also been shown to positively correlate with governmental efforts to censor media (Gamso, 2021).

To sum up, there is evidence that the effect of Chinese aid on press freedom could be positive or negative. However, considering the effects mentioned above – with small positive effects and possibly larger negative ones – we hypothesize that the relationship between Chinese aid and press freedom is likely to be negative. Formally, the hypothesis takes the following form:

***Hypothesis 1:*** African countries housing more Chinese aid projects are associated with lower levels of media freedom.

## 3.2 Part II – Sub-National: Local effects on attitudes

While we may predict at the aggregate that Chinese aid causes a decrease in press freedom, it is not clear whether this translates into actual beliefs about press freedom. Research on this point has, to our knowledge, simply not been done, which is a main motivation and contribution of this thesis.

Beliefs about press freedom may be driven by several factors. We investigate, to the best of our ability, the role of media coverage and person-to-person interactions. Overall, it is in no way obvious how Chinese aid in Africa would affect local attitudes on matters of press freedom, but with the help of previous research we make two different sets of hypotheses: the first set pertains to normative attitudes on the role and importance of free media, and the second set pertains to attitudes on the quality of existing media.

It is well-known that the Chinese government import Chinese workers when conducting aid projects abroad (Bräutigam 2015). It is therefore plausible to observe cultural exchange through day-to-day interaction between Chinese workers and the local population around project sites. Normative attitudes (on the role and importance of free media) among the local population could be affected by this increased cultural exchange. A similar mechanism of norm diffusion through local interaction is outlined in previous research on local effects of Chinese aid (Isaksson and Kotsadam, 2018a; Cha, 2020). The direction of this effect would be an increase in perceived right for the government to intervene in the news and media, and a decrease in the importance placed on free media. More formally stated, the hypotheses are:

***Hypothesis 2a:*** Exposure to Chinese presence, in the form of aid project sites, positively affect attitudes in favor of government to control of media, and its right to intervene in the news feed.

***Hypothesis 2b:*** Exposure to Chinese presence, in the form of aid project sites, decreases the importance put on free media as a characteristic of a democracy.

One set of literature pertains to the extended efforts from China to influence the media in Africa. Mainly, the suggested effect from this mechanism of media influence is judged to be negative. An assumption in this study is that news of local aid projects is being spread among the local population. This flow of information can be spread both through local media coverage and mouth-to-mouth. China has launched an ambitious ‘going out’ project with the explicit aim of expanding its broadcasting footprint globally (Nelson, 2013). Evidence suggests significant investment by China to establish its own media on the African continent (Zhang and Mwangi, 2016; Zhang et al., 2016; Wassermann, 2012; 2016). Efforts to use foreign aid to improve the image of China in Africa has also been noted in research (Wellner et al., 2022). Taking all this into consideration, this indicates that China may use its media to paint a favorable picture of the impact of its aid investments via its own media channels. On the one hand, this favorable coverage of aid projects could be viewed as propaganda by the local population if skepticism

towards China is widespread. On the other hand, the effects of aid projects could be positively viewed if people hold favorable views towards China. As such, the direction of this effect – mediated by China's use of its own media in Africa – is ambiguous. To investigate this mechanism, we will perform subsample analyses based on attitudes towards China. But at this stage, we view it more likely attitudes of skepticism are more prominent. Thus, this mechanism informs our hypotheses, on the local attitudes of media's truthfulness and efficiency, in a negative direction.

Another negative effect of Chinese aid on local perceptions, revolves around the psychological formation of individual beliefs. Modelling by Bordalo et al. (2016) finds a component of *stereotypical thinking* in the formation of beliefs. This finding informs this study in two ways. First, it grounds the fact that beliefs can be false, unfounded, or exaggerated. Second, stereotypical thinking leads to an overreaction to information that give rise to - or correspond with – a stereotype, and an underreaction to information that go against it. In the context of African beliefs of press freedom, this finding suggests that the belief formation of people more exposed to Chinese aid could, via stereotypical thinking, form more rash judgments of the negative influence of Chinese presence on national affairs. One example of this influence could be an increase in censorship. This would suggest a negative local effect of Chinese aid on beliefs about press freedom. More formally stated, the hypotheses are:

***Hypothesis 3a:*** Chinese presence, in the form of aid project sites, negatively affect local perceptions of the quality of the media in terms of truthfulness.

***Hypothesis 3b:*** Chinese presence, in the form of aid project sites, negatively affect local perceptions of the quality of the media in terms of efficiency.

# 4 Data & Methodology

## 4.1 Data description

To address our research questions, we make use of two different versions of the dataset on Chinese financial flows provided by AidData (Bluhm et al. 2018; Custer et al., 2021). To answer the first research question, we utilize country-level information of Chinese aid from AidData, and levels of press freedom from the media freedom index provided by Varieties of Democracy (V-Dem) (Coppedge et al. 2022a). To answer the second research question, we use

the geocoded version of AidData to spatially match with the geocoded version of the Afrobarometer survey data (BenYishay, 2017).

### 4.1.1 AidData – Independent variable (Part I & Part II)

Reliable data on Chinese aid flows have long been unavailable (Horn et al., 2021). In response to this, a research project was launched at the American research institute *William and Mary's Global Research Institute*. In summary, Custer et al. (2021) made use of the *Tracking Underreported Financial Flows (TUFF)* methodology to investigate and map Chinese development assistance flows to both Africa and the world at large. We acknowledge the fact that this data is based on second-source information and hence may not perfectly reflect Chinese financial transfers. However, all data provided by AidData undertake an extensive and transparent 3-steps collection process[1] with the purpose of verifying and ensuring the quality of the data (for a more detailed description of the methodology, see Custer et al. 2021). The data is available through the open-source database AidData and is divided into categories of aid projects in alignment with the definitions stipulated by the Development Assistance Committee (DAC) – the central aid organ within the OECD. This allows us to compare Chinese aid with aid donated by the different DAC-members. The aid is categorized into Official Development Assistance (ODA) and Other Official Flows (OOF). ODA covers government aid that has the specific aim to promote economic development and welfare in the receiving country. While ODA requires the countries to fulfill certain criterions to be eligible for ODA (for example gross national income (GNI) cannot exceed a certain level), OOF consists of transactions that do not meet these DAC requirements (Dreher et al., 2021). Since China is not part of the DAC, they do not follow the aid framework drawn by DAC, nonetheless the researchers have assigned the labels of Chinese aid as if China were a member (i.e., even though Chinese aid is assigned as ODA it does not suggests that the Chinese government has conditioned the same requirements that DAC may have done). Therefore, the AidData dataset includes a third category, namely "Vague Official Financials". As the name implies this category contains transactions whose purpose could not be identified by the researchers (Custer et al. 2021).

To answer research question one – how Chinese aid in Africa affects the cross-national levels of press freedom – we use the aggregated, country-level data on Chinese aid from

[1] TUFF 2.0 data collection process three steps: (1) Project identification, (2) Project verification and enhancement, (3a) Project-level data quality assurance, and (3b) Quality assurance of the dataset as a whole (Custer et al. 2021).

AidData. This recently updated dataset covers the years of 2000-2017. When estimating the effect of Chinese aid projects in Africa at a macroeconomic level, primarily two measures are in competition: financial value of aid projects or number of aid projects (Dutta & Williamson, 2016; Gamso, 2021; Dreher et al., 2018; 2021). On the one hand, financial value more accurately captures the magnitude and importance of aid projects, and better lends itself to a measure that takes the overall national economy into account (e.g., "percentage of GDP"). This strategy is employed by some research on media freedom (Dutta & Williamson, 2016; Gamso, 2021). On the other hand, the specifics of the dataset used needs to be considered, in this case AidData. Unfortunately, 39 percent of the aid projects in this dataset lacks information on the financial value of the aid projects. Because of this, the creators of the dataset themselves prefer project counts over financial value for estimating the effect of aid projects (Dreher et al., 2021, p. 148). Considering this, we use Chinese *project counts* as our main independent variable, collected from AidData. When aggregating projects, we exclude projects that were cancelled, or for any other reason were not realized.

To answer research question two – how exposure to Chinese aid through aid project sites affect the local perception of press freedom in Africa – we utilize a geocoded version of the AidData. In this dataset, aid projects are assigned geographical coordinates (longitude and latitude) provided by Bluhm et al. (2018). The precision of this geocoding is graded from 1 (exact location, example: physical structure) to 8 (least precise, example: a capital city). We limit the sample to include projects with precision-coding 1, 2 or 3. Score 3 corresponds to a second-order administrative division (AMD2), or analogous to it (i.e., municipality or commune). Which precision-code to include in the sample has varied within previous research: Isaksson and Kotsadam (2018a; 2018b; 2020) restrict their sample to include only projects with precision code 1-2 while Cha (2020) uses a broader sample of projects ranging the precision code from 1-4. We also use geocoded data on aid projects from the World Bank. This dataset is also gathered from AidData and follows the same method for precision-coding. Naturally, we only include precision scores 1-3 from this dataset as well. We use the latest existing *geocoded* dataset, covering the years between 2000-2014. To ease a comparison with Western aid (DAC-members ODA), we also restrict our study to only use "ODA-like" Chinese aid. After this selection process, we obtain 371 unique Chinese aid project sites across Africa.

### 4.1.2 Varieties of Democracy (V-Dem) – Dependent variable (Part I)

We use the government censorship measure from the Varieties of Democracy (V-Dem, for short) dataset as our main dependent variable. Specifically, this measure captures governmental efforts to censor media (this variable will also be referred to as a measure of "media freedom" going forward) (Coppedge et al., 2022a). In short, V-Dem is a research institute which handles one of the most established data collections in comparative politics, covering characteristics of governments worldwide. The measure used ranges from -5 (highest frequency of governmental efforts to censor media) to 5 (lowest frequency of censorship, where those who participate in it receive punishment). The V-Dem dataset is a panel dataset, covering levels of censorship in 52 African countries across time. The period utilized in this thesis span years 2000-2017 (Coppedge et al., 2022b).

#### 4.1.2.1 Control Variables for Part I

The list of control variables for Part 1, exploring the aggregate effects of Chinese aid on press freedom, cover several different data sources. To begin with, one important determinant for press freedom is regime type (Dutta and Williamson, 2016; Gamso, 2021). In our context, authoritarian regimes may respond more strongly in terms of media censorship in comparison with democracies. At the same time, more authoritarian governments also start at higher levels of censorships (worse press freedom), which would suggest larger potential effects in more democratic regimes. To account for this aspect of regime types, we incorporate the polity2 variable from Marshall et al., (2016). We also include GDP per capita to control for the fact that demands on freedoms likely increase as population wealth increases. This link is also supported by previous research (Djankov et al., 2003; Dutta and Roy, 2009).

An important aspect of isolating the effect of aid is to single it out from other international monetary flows. The regression does this in two ways: controlling for foreign direct investment and international trade with China. Foreign direct investment (FDI, as percentage of GDP) is controlled for using data from the World Bank, with evidence also suggesting FDI to be an important determinate for media freedom (Dutta and Roy, 2009). To control for trade involvement with China, data from the Correlates of War database (Barbieri et al., 2009; Barbieri and Kesh, 2016) is used. Specifically, we create a measurement of the share of a country's total export that goes to China. The data utilized span years 2000-2014. In addition to isolating monetary flows, evidence also suggest Chinese trade dependency

influences governmental efforts to censor media (Gamso, 2021), which further supports its inclusion as a control variable.

As previously mentioned, theory suggests state characteristics to be an important determinate for levels of freedom (Siebert et al., 1956; Dutta and Williamson, 2013; 2016; Vaca-Baqueiro, 2017; Gamso. 2021). To account for this, two controls are incorporated into the regression. The first is retrieved from Linz and Staton (2016) and regards judicial independence. This measure captures the degree to which courts within the country are to be considered as independent, and thus able to oppose and control political leader's in their efforts to limit the rights of the public. This function of judicial institutions has proven important for protecting freedom of press and freedom of speech (Gibler and Randazzo, 2011; Crabtree and Nelson, 2017). The second control for state characteristics pertains to the level of checks and balances within the political system. Research suggest more extensive checks and balances can work to protect media freedoms from political leader's initiatives to increase censorship (Kellam and Stein, 2016). For this measure, the Database of Political Institutions (Beck et al., 2001; Cruz et al., 2020) is utilized. As mentioned in Section 3.1, corruption influences levels of press freedom. We use the political corruption index from V-dem to control for this.

Furthermore, Egorov, Guriev and Sonin (2009) theorize that the evaluation of media freedom differs among authoritarian states depending on natural resource access. For empirical confirmation the authors focus primarily on oil resources, finding evidence for the proposition that media freedom levels are lower in dictatorships where oil resources are larger. In line with this finding, we control for oil rents (as percentage of GDP) using data from the World Bank. Lastly, some research suggests the effects on media freedom from financial ties to China could be mediated by regime type (Gamso, 2021). Considering this, an interaction term between the main independent variable (measure of Chinese aid) and the polity2 (regime type) variable are included in the regression.

### 4.1.3 Afrobarometer – Dependent variable (Part II)

To study research question two, we use survey data from the Afrobarometer. Specifically, we use round 5, covering 34 African countries and spanning years 2011-2013. We use this round since it is the latest available version of the Afrobarometer which corresponds with the years covered in AidData. In total, the dataset contains 53977 observations. The data is geocoded by BenYishay et al. (2017), assigning longitude and latitude coordinates to respondents answering the survey. We then geographically match the geocoded data from AidData (on Chinese project

sites) with the geocoded data from the Afrobarometer (on location of respondents). In accordance with previous applications of this method (Knutsen et al, 2016; Isaksson and Kotsadam, 2018a:2018b:2020), we cluster the respondents of the Afrobarometer to nearby villages and neighborhoods and match them to nearby Chinese aid project sites.

We use five survey questions for our main analysis. The first asks whether the media should be able to operate without government control (labeled "Role of Gov." in regression outputs). The second asks whether the media should investigate all mistakes or corruption cases of the government, or if too much investigation only harms the country (labeled "Purpose of Media" in regression outputs). Both questions are posed as statements to which the respondent answers in degrees of agreement (ranging from simply agreeing to strongly agreeing with either statement). We use these questions to test our first hypothesis of the second part, Hypothesis 2a. The third question asks the respondent to pick out what they consider to be the most important characteristic of a democracy (labeled "Charac. of a Dem." in regression outputs). There are four alternatives on this list, one of which states medias freedom to criticize the government. We use this question to test Hypothesis 2b. Question four asks how often the respondent thinks media abuses its freedom by being untruthful (labeled "Abuse of Truth" in regression outputs). The answer ranges from "Never" to "Often" on a 4-point scale. Question five, lastly, asks how effective the media is in revealing government mistakes and cases of corruption (labeled "Inefficiency of Media" in outputs). The answers range from "Not at all effective" to "Very effective" on a 4-point scale. These questions are used to test Hypothesis 3a and 3b. Table A2a-c (see in Appendix: Section 1) presents detailed descriptions of all survey questions used, as well as their possible answers. All questions have been re-coded as dummy variables for the main analysis (see Table A3 in Appendix: Section 2), with ordinal scale measures kept as a robustness check. To avoid assigning beliefs or opinions onto respondents who answered: "Don't know" or "Refuse to answer", we exclude these in our regressions.

#### 4.1.3.1 Control Variables for Part II

We also use several control variables from the Afrobarometer in accordance with similar previous research (Isaksson and Kotsadam, 2018a:2018b:2020; Knutsen et al., 2016; Cha 2020). These controls cover basic background characteristics of respondents: age, age-squared, gender, level of education, as well as residency (living in an urban or rural area). Furthermore, the effect of an increase in Chinese presence – via aid projects – on local attitudes and beliefs could be conditioned on pre-conceived attitudes towards China. In other words, whether one views China more favorably might influence the interpretation of increases in Chinese

influence. To explore this, we use a question from the Afrobarometer asking the respondent to place China on a scale from completely democratic (0) to completely undemocratic (10) (see Table A2c in Appendix: Section 1).

## 4.2 Methodology

We employ two strategies to answer our research questions. Each of which constitute a distinct part (Part I and Part II) of the overall thesis. The first part aims to answer research question one, and the second part deals with question two. They represent two dimensions of the grander project of analyzing the impacts of Chinese aid in Africa. The aim is for them to work complementary in the later discussion. The parts will now be dealt with in turn. Briefly, the first part utilizes cross-national panel data to examine the aggregate effects of Chinese aid on levels of press freedom. The second part then turns to potential local effects of Chinese aid, exploring the impact of proximity to Chinese aid projects sites on citizens' beliefs and attitudes on matters of freedom of press. This part instead employs a spatial-temporal strategy on sub-national, geographically coded data. Considering all aspects, the ambition is a fuller understanding of the impact of Chinese aid on press freedom in Africa.

### 4.2.1 Part I – Cross-National: Aggregate effects

We start off with the cross-country analysis. The aggregate effects of Chinese aid on levels of press freedom in recipient countries will be estimated using a linear regression model. Previous research on the aggregate effects of foreign aid on press freedom employ both ordinary least squares and IV-regression models (Dutta and Williamson, 2016; Gamso 2021). For this part of our estimations, we use a linear fixed effect model. While utilizing the available tactics for minimizing endogeneity concerns, this model will primarily focus on capturing results of correlation.

#### 4.2.1.1 Estimation Strategy

The aim of this estimation is to capture the effects of Chinese aid on the press freedom of recipient countries. Using panel data covering the years between 2000-2017, our reduced form specification takes the following form:

$$PF_{i\,t} = \alpha_0 + \alpha_1\, Projects_{i\,t\text{-}1} + \gamma\, X_{i\,t\text{-}1} + \eta + \delta + \epsilon \qquad (1)$$

where PF represent the press freedom score measured by V-Dem for country *i,* at time *t,* and Projects reflects the number of aid projects in country *i* in the previous year. All independent variables are lagged one year (t-1) to account for a probably delay in effect (Clemens et al., 2012). X is a vector of control variables: including political aspects (regime type, judicial independence, checks and balances), economic aspects (GDP per capita, Oil rents), as well as other relevant monetary flows (such as FDI, export and import to China, and World Bank aid). $\eta$ and $\delta$ further controls for country- and year fixed effects, and $\epsilon$ is an error term. We will use a stepwise approach for incorporating the list of controls.

#### 4.2.1.2 Endogeneity Concerns and Robustness Analysis

There are considerable endogeneity concerns within this specification. One of which pertains to the likelihood of omitted explanatory variables. Explaining the reasons for any country's level of press freedom is a complicated matter. The list of possible determinants is long, and although we incorporate many of the controls suggested by previous literature (Djankov et al., 2003; Dutta and Roy, 2009; Egorov et al., 2009; Gibler and Randazzo, 2011; Dutta and Williamson, 2016; Kellam and Stein, 2016; Crabtree and Nelson, 2017; Gamso, 2021), the possibility of omitted variables is non negligible. Another concern is the potential selection effect of Chinese aid, where some countries are more likely to receive aid than others. Indeed, research on Chinese aid allocation does suggest such an effect (Guillon and Mathonnat, 2020). This unobserved set of characteristics governing the distribution of Chinese aid would pose a threat to any causal claims from this model (Part II of this thesis will introduce a method for handling this probable selection effect). There is also a concern of significant reverse causality in this specification, where countries with less press freedom receive more aid than their counterparts. To get a sense of this aspect of endogeneity, we run the models – stepwise adding the list of controls – with Chinese aid as the dependent and press freedom as the independent variable. In this part of the thesis – investigating aggregated effects of Chinese aid – it is worth highlighting the econometric hardships of capturing causal effects. As we cannot be sure we have overcome these endogeneity concerns it is important to highlight that we view results as indicating correlations. We believe this is still important as there is no existing research on this question.

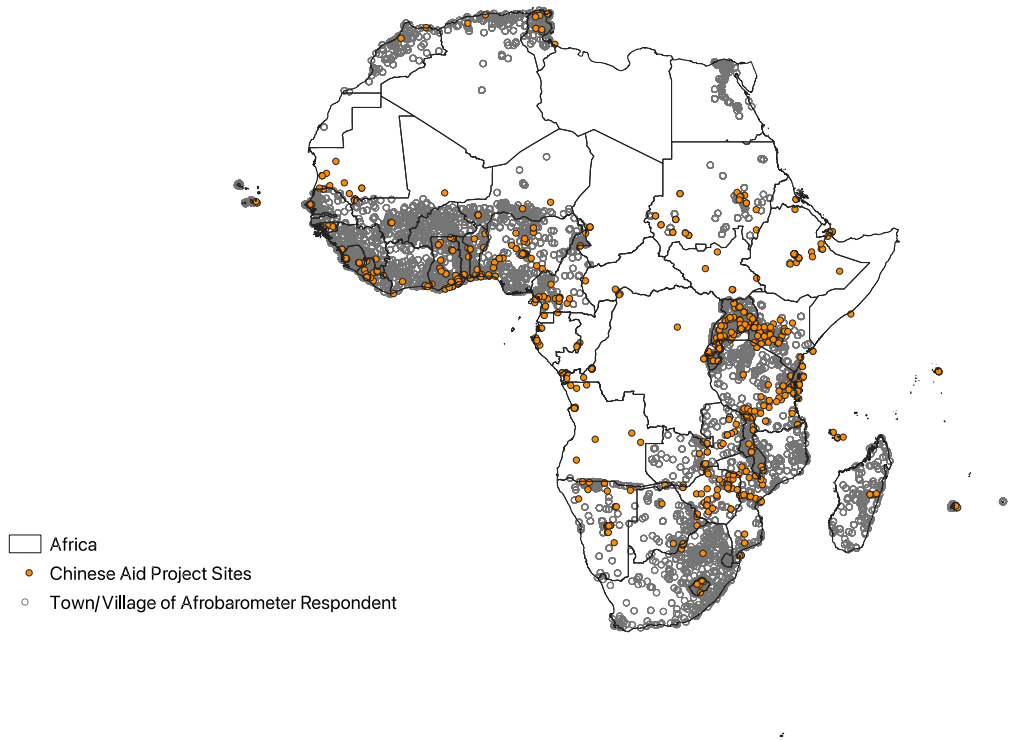
Moreover, we perform several other robustness checks. First, we estimate an alternative model using the Freedom House measure of media freedom as the dependent variable. This tests whether the results from our main specification are robust across other measures of media freedom. In other words, to make sure the results are not limited to a specific

measure of media freedom. Second, we construct an alternative measure of our main independent variable. This measure instead takes the financial value of Chinese aid projects and calculates the percentage of this aid in terms of the recipient's country's GDP. As previously mentioned, this measure carries some flaws, most poignantly in the amount of missing financial value data. Despite this, we consider the measure a good robustness check for our chosen 'project count' measure. Lastly, as argued by Clemens et al. (2016) one must take into consideration the timing when measuring the effect of aid. Hence, we vary the delay to account for the likelihood that some effects take longer before exhibiting a noticeable effect, including variations with both a three and a five-year lag.

### 4.2.2 Part II – Sub-National: Local Effects

The data from the Afrobarometer is not a panel, excluding the possibility of following a specific location (cluster of individuals surveyed by the Afrobarometer) over a period of time, comparing effects before and after Chinese entry. Instead, we will use a spatial-temporal strategy in line with Knutsen et al. (2016) and Isaksson and Kotsadam (2018a; 2018b; 2020). This strategy utilizes the geocoding and time variation in the datasets in the following way. First, AidData provides geographical information of the locations where China has ongoing aid projects. Additionally, it also contains information of the locations for future aid projects (i.e., geographic locations where China has made the decision to – in the future – start up aid project sites). We are then able to match this geographically coded AidData with the geographically coded Afrobarometer and estimate the effects of proximity to Chinese aid project sites on the beliefs about press freedom.

**FIGURE 2: Chinese Aid Project Sites**




Note: Geographically matched spatial from the Afrobarometer and AidData

The response options for the survey questions we have decided to use are mostly structured as nominal data, with the options reflecting different opinions. We are mainly interested in considerable shifts in response patterns rather than the precise answer of the respondent. Put differently, our main interest lies in whether the believed level of press freedom has changed because of Chinese entry, not necessarily to which degree. Hence, we code the dependent variables as dummy-variables (see Appendix: Section 1, Table A2a-c, and Table A3 for more details). Possible weaknesses of this categorization will be explored and discussed further in Section 4.2.2.3.

#### 4.2.2.1 Choosing a cut-off

Deciding on an appropriate cut-off distance is a significant decision in this estimation strategy. If the cut-off is placed too far from the project site, it increases the probability of not capturing the desired effect due to attenuation bias (i.e., we underestimate the effect of the project site). Instead placing the cut-off too narrow could also create estimation problems, reducing the size

of the treatment group. Studying the local effects of mining on corruption, Knutsen et al. (2016) argues that the optimum cut-off distance is at 50 km. In their reasoning, travel of more than 50 km is deemed too impractical for occurring on any regular basis, suggesting an appropriate cut-off distance to be placed at < 50 km.

However, the cut-off distance should also be informed by the nature of the dependent variable. On the one hand, a mechanism previously outlined revolve around visual exposure to – and interaction around – aid project sites, and while 50 km is possible to cover with a car, such trips are presumably rather rare. Consequently, 25 km may instead be a distance more frequently covered by citizens. With more frequent trips, and more routine exposure to project sites, the possibility of following the construction of a hospital or school funded by Chinese aid (Cha 2020). On the other hand, another mechanism outlined revolve around the local spread of information, either via news coverage or mouth-to-mouth. An example here could be news of infrastructure being built with Chinese funding. In terms of this mechanism, 25 km could be considered too narrow, leaving out part of the effect. Considering this tension between the mechanisms, we choose 50 km as the cut-off for our main analysis – consistent with previous literature (Knutsen et al. 2016; Isaksson and Kotsadam 2018a; 2018b; 2020) – while including both 25 km and 75 km as alternative cut-offs in the robustness section (see Section 4.2.2.3).

Furthermore, when an individual is said to be living near a project site, what is meant is that the center of the cluster that this individual belongs to is less than < 50 km away from at least one project site. Approximately 46 percent of our observations lives close to a Chinese aid project site (see Table 1). Of those, most of the respondents live close to an *ongoing* project site (see Table 2).

**TABLE 1: Respondents live near project site**

| | Freq. | Percent | Cum. |
|---|---|---|---|
| Respondent's not living close | 29185 | 54.07 | 54.07 |
| Respondent's living close | 24792 | 45.93 | 100.00 |
| Total | 53977 | 100.00 | |

Note: Respondents living close (< 50 km) or not living close to a Chinese aid project site. A dummy variable taking 0 if the respondent is not living close to a Chinese aid project site and 1 if the respondent lives close to a Chinese aid project site.

**TABLE 2: Ongoing and projected**

| *Ongoing* | *Projected* | | Total |
|---|---|---|---|
| | Respondent's not living close | Respondent's not living close | |
| Respondent's not living close | 32453 | 2200 | 34652 |
| Respondent's living close | 19325 | 0 | 19325 |
| Total | 51778 | 2200 | 53977 |

Note: Given that the respondent lives close (< 50 km) to a Chinese aid project site: Ongoing represents that this project site is active and Projected reflects if this project site is projected.

#### 4.2.2.2 Estimation strategy

The overall aim of this section is to capture local effects of Chinese aid projects on citizens' attitudes and beliefs. Specifically, the attitudes and beliefs surrounding the truthfulness, efficiency, and role of the media – as well as more abstract aspects of press freedoms' role in a democracy. To do this, our reduced form specification takes the following form:

$$Y_{ivt} = \beta_0 + \beta_1 \, ongoing + \beta_2 \, projected + \theta_s + \tau_t + \rho C_{it} + \varepsilon_{ivt} \qquad (2)$$

Where $Y_{ivt}$ represents the belief of individual *i*, in cluster *v* at time *t*. *Ongoing* is a dummy variable reflecting whether the individual lives near (< 50 km) any ongoing project site at the time of answering the Afrobarometer survey. *Projected* is a dummy variable reflecting whether the individual lives near (< 50 km) any expected Chinese aid project sites. The reference category consists of individuals *not* living close (> 50 km) to any – either ongoing or projected – project sites. $\beta_1$ captures the effect on attitudes associated with living near (< 50 km) an *ongoing* Chinese project site, while $\beta_2$ captures the effect associated with living near (< 50 km) a *projected* Chinese project site. Further, $\theta$ and $\tau$ represent dummy variables for country- and year fixed effects, $\rho C_{it}$ is vector of individual background variables, and $\varepsilon_{ivt}$ is the error term.

Utilizing the spatial-temporal strategy, the main variable of interest becomes the difference between the two coefficients ($\beta_1 - \beta_2$) where $\beta_2$ in effect captures the spatial selection bias of which locations get turned into Chinese aid project sites and which do not. Thus, by subtraction ($\beta_1 - \beta_2$), we get a difference-in-difference type estimator that captures the local effect of Chinese presence on attitudes and beliefs. In this subtraction, we control for

the unobserved characteristics that influence the Chinese selection process of where to implement aid project sites.

When the year of an aid site being establish overlap with the year the Afrobarometer performs the survey, the site is placed in the 'ongoing' category. This group of observations could belong to the 'completed' category, depending on the date during the year, the two events occur (aid site implemented and Afrobarometer survey). In any case, the total number of observations in this group – of which some may be misplaced – only amounts to 247, likely having a negligible on the following regressions.

To gain perspective on whether the potential effects of Chinese aid not just capture effects of foreign aid more generally, we compare the local effects of Chinese aid with the local effects of World Bank aid. We use the same geo-matching approach as with Chinese aid, producing the same measures of proximity to ongoing and projected World Bank aid project sites. Further on this point of comparison, we target a subsample of respondents who only live close to Chinese aid project sites, and not to any World Bank aid projects.

Lastly, an important aspect of analyzing the difference in perceptions between those who live near (and those who do not) Chinese aid project sites may be already existing perceptions of China. These pre-existing attitudes towards China might govern the direction of the effect on perceptions of press freedom. For example, the hypothesized negative effect on press freedom perceptions may be absent (or, indeed, even positive) for the group of respondents who hold favorable views towards China. In this spirit, we analyze the subsample of respondents who hold favorable views towards the Chinese state at the time of the survey. To do this, we utilize the Afrobarometer's question on whether China is considered democratic. The scale of answers spans 0-10 (0 equals completely undemocratic, 10 completely democratic). From this, we create a dummy-variables with scores 0-4 representing undemocratic attitudes and 6-10 democratic (score 5 is considered 'neutral', and thus left out of this subsample analysis). We then split the data and run conditional regressions for both subsample groups, ultimately comparing the effects both between the subsamples, as well as to the overall sample. Splitting the data accordingly it is analogous with running a regression with interaction term included (Le 2016). However, this method of splitting the data is not flawless. Hence, to test the heterogeneity of the two subsamples and the validity of the models, we change the specification of our main analysis to include an interaction term of attitudes toward China and living close to an ongoing or projected Chinese aid project site (this is presented in Appendix: Section 3, Table A20). We then perform a post-estimation analysis to measure if the effect of the interaction; whether respondents who hold favorable views towards

China and lives close to either an ongoing or projected Chinese aid project site significantly differs in their beliefs on media freedom from the respondents who hold non-favorable views towards China and lives close to either an ongoing or projected Chinese aid project site. We also run these conditional regressions using ordinal scale dependent variables instead of the primary dummies (see Appendix: Section 3, Table A22).

#### 4.2.2.3 Endogeneity Concerns and Robustness Checks

While the estimation strategy employed do offer a creative approach to handle the likely selection bias in Chinese placement of aid project sites, a key assumption in this method is that the selection process itself *does not change over time*. In other words, while we can control for the biased selection of where project sites are established, we cannot control for this selection – and the criteria guiding it – to change over time. Thus, our conclusions only hold under the assumption that no important changes in the selection process of aid sites occurred during the period covered by our data (2000-2014). For example, if Chinese aid project sites has tended to be built in locations often governed by more corrupt government officials, this bias would be controlled for in the present estimation strategy. However, if a hypothetical shift of focus occurred in 2007, where another parameter (or set of parameters) governed the placement of aid sites from 2007 onwards, this would produce an endogeneity bias in the present model. Chinese aid allocation patterns have been argued to exhibit certain characteristics (Guillon and Mathonnat, 2020), however, within the framework of this study this constancy will be assumed rather than tested, and thus simply established here as an endogeneity concern.

Another concern within the model is the survey data underlying the dependent variables. While the Afrobarometer is a well-established source for similar research enquiries (Isaksson and Kotsadam, 2018a; 2018b; 2020; Cha, 2020), a note on the inherent flaws of survey data is appropriate. In this study specifically, the most important disadvantage is that response options may be evaluated differently across respondents (Keusch et al., 2021). This variation is most obvious in abstract questions, in our case, for example, the priority of press freedom within a democracy (Q45 in table A2b found in Appendix: Section 1). At the same time, the question on whether media is thought of as truthful (Q52a in table A2c found in Appendix: Section 1) is more concrete in nature, alleviating some of this interpretive variance among respondents. On this point of flaws in survey data, it is worth repeating that an underlying idea in this thesis is to explore the effect of Chinese aid on media freedom along two dimensions: both institutional degree (covered in part one) and beliefs affected locally. Thus, the attitudes and beliefs collected from the Afrobarometer are not thought of as a proxy

for actual, institutional, levels of press freedom. Rather, the attitudes and beliefs are themselves of interest in this study.

To test the sensitivity of our model, and further understand the effects captured by it, we perform five robustness checks. The first concerns the selection of the cut-off distance. To control for the model sensitivity to differing cut-off's, we perform the regressions both with 25 km and 75 km (to be compared with the 50 km cut-off in the main analysis). Beyond checking whether the results are robust across other distances, this robustness check also present an opportunity to develop the understanding of potential mechanisms.

Second, it is unlikely that the effect a Chinese aid project site on local perceptions is instantaneous – previous research highlights the importance of the timing of aid (Clemens et al., 2012). Additionally, beliefs and attitudes reasonably take time to form and change. To investigate the extent of this potential delay, we perform two robustness checks that include project duration as a control. The first as a continuous measure (i.e., the number of years the project site has been active), and the second as duration dummies. The duration variables measure the time the project has been on a particular site.

Third, we check for a lingering effect of past project sites. To do this, we control for proximity to completed project sites. These sites are geographical locations of projects that were completed before the Afrobarometer survey took place in any given town or village. The variable is estimated using the same method as for the main analysis, utilizing geographical data to create variables stating proximity to Completed and Projected Chinese aid project sites. This group of respondents is dropped from the main analysis to not muddle the effect of the diff-in-diff estimator. However, it is still reasonable that effects of project sites remain after project completion, and this effect is further analyzed in this robustness check.

Fourth, we test the fit of our linear probability model by comparing it to an alternative, non-linear, probit specification. The primary purpose is to check which probability model fits the data best. We also vary the dependent variable measure, comparing the primary dummy variables with ordinal scale measures.

Lastly, we would have liked to advance the analysis by investigating the effect of different types of aid within specific sectors. For example, to see if sectors consisting of more frequent construction projects – and therefore opening more of the exposure and interaction channels of effect – yield different regression results in their effect on citizen attitudes than aid projects that opens more of the media coverage channel. Unfortunately, the data does not contain the necessary distinctions between types of aid projects that would allow such analysis. For example, there is no categorization of aid specifically aimed at the construction of buildings

(as in, for example, schools or hospitals). Rather, the construction of schools is placed in the broad "Education" sector while the construction of hospitals is placed in the "Health" sector. This leaves the wish to estimate the effect of the unifying quality of aid resulting in the construction of buildings (in this case spanning two different sectors) unfulfilled. Nonetheless, we carry out our main regression in the three largest sector categories: Education, Health, and Transport and Storage.

# 5 Results and Findings

## 5.1 Part I – Cross-National: Aggregate effects

Table 3 presents a stepwise estimation of the aggregate effects of Chinese aid projects on governmental efforts to censor media. Each column in the table represents a regression model that has V-Dem's measure of governmental efforts to censor media as the dependent variable. Model 1 (column 1) constitute the baseline regression, excluding any control variables. As such, Model 1 estimates the effect of the number of Chinese aid projects on the recipient country's governmental efforts to censor its media. Model 2-5 (column 2-5) then successively builds the number of controls, resulting in Model 5 (column 5) as the final model.

**TABLE 3: Part I - Results**

| Dep. variable: | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Gov. censor media | Model 1 | Model 2 | Model 3 | Model 4 | Model 5 |
| Projects | -0.002 | -0.031** | -0.025 | -0.023 | -0.022 |
| | (0.006) | (0.014) | (0.015) | (0.015) | (0.015) |
| Regime type | | -0.091*** | -0.077** | -0.041* | -0.041* |
| | | (0.030) | (0.033) | (0.024) | (0.024) |
| Projects # Regime type | | 0.003** | 0.002 | 0.002 | 0.002 |
| | | (0.001) | (0.002) | (0.002) | (0.002) |
| FDI | | | -0.000 | -0.001 | -0.001 |
| | | | (0.003) | (0.003) | (0.003) |
| Export | | | 0.000 | -0.000 | 0.000 |
| | | | (0.002) | (0.002) | (0.002) |
| Import | | | 0.006 | 0.001 | 0.001 |
| | | | (0.012) | (0.011) | (0.011) |
| GDP per capita (log) | | | -0.342 | -0.469 | -0.537 |
| | | | (0.399) | (0.360) | (0.409) |
| ODA | | | 0.009 | 0.010 | 0.010 |
| | | | (0.016) | (0.015) | (0.014) |
| Judicial independence | | | | 5.101** | 5.096** |
| | | | | (2.112) | (2.095) |
| Checks and balances | | | | -0.060* | -0.061* |
| | | | | (0.032) | (0.032) |
| Corruption | | | | -2.309*** | -2.332*** |
| | | | | (0.766) | (0.761) |
| Oil rents | | | | | -0.001 |
| | | | | | (0.008) |
| Observations | 817 | 761 | 529 | 510 | 504 |
| R-squared | 0.001 | 0.113 | 0.102 | 0.240 | 0.242 |
| Number of countries | 52 | 48 | 41 | 40 | 39 |

Note: The regression controls for country and year fixed effects. All independent and control variables are lagged one year (t-1). Projects: Number of projects, Regime type: Measured through Polity IV's variable Polity2, FDI: Foreign Direct Investments, Export: Export to China as percentage of total export, Import: Import from China as a percentage of total import, GDP per capita: Gross Domestic Product per Capita collected from World Bank (Constant 2017), ODA: Official Development Assistance (ODA) as percentage of GDP from OECD-DAC, Judicial independence collected by Harvard Dataverse, Checks measuring checks and balances within the political system collected from Database of Political Institutions, Corruption: Political Corruption Index measured by V-Dem's variable *v2x_corr*, Oil rents: collected from the World Bank. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

The *Projects* coefficient is the main independent variables of interest, capturing the effect of one additional Chinese aid project. *Regime type* and its interactive effect on Projects are added as the first set of controls (see Model 2). Regime type is negative and statistically significant, indicating that when the number of Chinese aid projects is zero, there is a negative relationship between regime type and media freedom. With the variable coding in mind, this means that when the number of Chinese aid projects is zero, more authoritarian regimes are associated with greater governmental efforts to censor media. This is in line with similar model applications (Dutta and Williamson, 2016; Gamso, 2021). Model 3 (column 3) controls for

*Foreign Direct Investment* (FDI), *Official Development Aid* (ODA) from the OECD, *income levels* (logged GDP per capita) as well as *Chinese trade dependency* (import and export to China) none of which are statistically significant. Model 4 (column 4) further controls for institutional aspects: *Judicial independence*, *checks and balances* within the political system as well as *corruption*, and all three controls are statistically significant. This indicates that an increase in judicial independence – improved capacity for courts and judges to perform their duties without governmental control – is associated with an increased media freedom. As are corruption, which indicates that increased political corruption is associated with a decreased media freedom. Lastly, Model 5 (column 5) adds oil rents to the list of controls.

In the final model (column 5), after the necessary control variables are added, we find no statistically significant effect of number of projects nor on the interaction term number of projects and regime type. These results suggest Chinese aid projects is not associated with an impact on media freedom among African aid recipients. Our results stand in contrast to findings by Dutta and Williamson (2016) who found a statistically significant positive effect of foreign aid on media freedom. It could be, as we theorize, that Chinese aid characteristics effectively cancel out the positive effects generally ascribed to foreign aid, producing the statistically insignificant relationship we see here. All models suffer from relatively low R2, indicating that the degree of explanation in our model estimates are low. On the other hand, the low R2 capture the complexity of measuring media freedom and the variation across countries. Even though we include several of the control variables found in previous research (Gamso 2019; 2020, Dutta and Williamson 2016, Marshall et al., 2016; Siebert et al., 1956; Dutta and Williamson, 2013; 2016; Djankov et al., 2003; Dutta and Roy, 2009). Overall, our results cannot confirm Hypothesis 1, that African countries housing more Chinese aid projects are associated with lower levels of media freedom.

### 5.1.1 Sensitivity Analysis

As additional robustness checks beyond adding control variables, we perform several alternations of our models to further test the robustness of the models. The results of our sensitivity analysis are found in Appendix: Section 2, also the stepwise column distribution from the main results, are the same throughout the sensitivity analysis. Firstly, in table A8, we re-estimate the models and change the independent variable. Instead of measuring the impact of an additional Chinese aid project on media freedom we now estimate how an increase in the *share* of aid received in relation to the country's GDP, affects the media freedom in the

recipient country. Similar as the main analysis, the majority of the variables are statistically insignificant. However, even regime type, which was statistically significant at various level for all models are now statistically insignificant for model 4-5 (column 9-10). As discussed briefly earlier, the number of Chinese aid projects where the magnitude of the project has been identified amounts to approximately 61 percent of the data. Hence, the fact that we observe less statistical significance in comparison to when we used the number of projects, may be due this lack of sufficient information regarding the magnitude of Chinese aid projects.

Next, we change the dependent variable. Instead of using V-dem's measure of media freedom we now use a similar measurement provided by Freedom House. The independent variable from the main analysis (number of projects) is kept and the results of this is found in Table A9 in Appendix: Section 2. The results reflect the estimations from the main analysis where Projects is not statistically significant in any model. Likewise, regarding the level of statistical significance for the interaction term Projects # Regime Type. However, Regime type is statistically significant at a higher level in comparison with the main analysis which confirms the results form Table 3, that more authoritarian regimes are associated with greater governmental efforts to censor media given that there exist no project sites in the country. We can make similar conclusions with the judicial independence variable.

On the risk of reverse causality bias, we test the associated impact of governmental efforts to censor media on the number of Chinese aid projects in the country (see Table A10 in Appendix: Section 2). We find no effect that increasing efforts to censor would affect the number of projects in a country (i.e., countries with higher respectively lower press freedom score would affect the Chinese selection of where to implement a project site). Regime type is negative and statistically significant for model 2 (column 17) indicating that when the press freedom score is zero, the probability of an additional project site decreases as the regime becomes more authoritarian. This effect diminishes when more controls are added – such as GDP per capita (column 18-20) – which suggests that whether a regime is authoritarian is not as important as for example GDP per capita, in the Chinese aid selection process.

When we vary the delay of our estimates and controls in the main model (see Table A11 and A12), adding a three-year and five-year lag respectively, the results do not change. Even though we change the timing of the Chinese aid, we still do not find any evidence that the number of projects an African country holds would be associated with the domestic press freedom.

## 5.2 Part II – Sub-National: Local Effects

The second part of our analysis measures the effect of Chinese aid on a sub-national level and in more detail, the perceived level of press freedom among the citizens of countries who live near Chinese aid projects. The results are presented in Table 4 and every column represents a different question from the Afrobarometer.

**TABLE 4: Part II – Results**

| Dependent variables | (1) *Role of Gov.* | (2) *Purpose of Media* | (3) *Charac. of a Dem.* | (4) *Abuse of Truth* | (5) *Inefficiency of Media* |
|---|---|---|---|---|---|
| Ongoing | -0.007 | -0.029*** | 0.016*** | 0.027*** | -0.044*** |
| | (0.005) | (0.004) | (0.004) | (0.005) | (0.004) |
| Projected | 0.020 | -0.078*** | 0.096*** | -0.045*** | -0.085*** |
| | (0.012) | (0.010) | (0.011) | (0.013) | (0.011) |
| Diff-in-Diff. ($ß_1 - ß_2$) | -0.0269 | 0.0485 | -0.0796 | 0.0720 | 0.0411 |
| Observations | 49,220 | 49,144 | 48,328 | 44,615 | 46,790 |
| R-squared | 0.007 | 0.012 | 0.005 | 0.022 | 0.017 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 39.80 | 68.31 | 25.88 | 113.4 | 88.38 |
| Prob >F | 0 | 0 | 0 | 0 | 0 |

Note: All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Beginning with the Role of Government (column 1), our results suggest no statistically significant effect of ongoing project sites. However, when measuring the impact of Chinese aid on the Purpose of the Media (column 2), both ongoing and projected is highly statistically significant and the difference between the coefficients is positive. With the coding in mind, this indicates that the probability that the respondent agrees with Statement 2 (too much reporting on negative events, like government mistakes and corruption, only harms the country) increases after the implementation of Chinese aid project sites. Hence, if a respondent lives close to an ongoing project site, the probability that the respondent believes that the media *should not* investigate the government for mistakes and corruption is *higher* than for the respondents who lives close to a projected Chinese aid project site. This result could be interpreted as a change in the respondent's belief regarding the purpose of media after the implementation of Chinese

aid project site. Evaluated together (column 1 and 2), the results on how exposure to Chinese aid project sites affect attitudes in favor of governments' control of media, and its right to intervene in the news feed, are mixed. This result is only partially in line with Hypothesis 2a. Column (3) measures the probability that the respondent chose "Free Media" as one of the most important characteristics of a democracy among the list of options. Like previously, the effect of ongoing and projected is statistically significant. The difference-in-difference type estimate (the difference between the ongoing and projected estimates) for column (3) is negative. This suggests that the probability of choosing "Free Media" decreases after the implementation of a local Chinese aid project site. In other words, that medias right to freely criticize the government is considered less important after the implementation. This result is consistent with Hypothesis 2b. Regarding what we have defined as the Abuse of Truth (column 4), both of our main independent variables (ongoing and projected) are statistically significant. The difference-in-difference type estimate is positive. This implies that respondents who live near an ongoing project site believe the media abuses its freedom – by publishing things that are not true – more often. This result is consistent with Hypothesis 3a: that Chinese presence, in the form of aid project sites, negatively affect local perceptions of the quality of the media in terms of truthfulness. Lastly, column (5) presents the estimation of how Chinese aid project sites affect the perceived level of media effectiveness. As seen in column (5), the estimates of interest are highly statistically significant and negative, and by observing the difference between ongoing and projected, the results indicates that the perceived level of media effectiveness declines after China's entry (i.e., the media has become more inefficient to reveal governmental mistakes and corruption). This result confirms Hypothesis 3a: that Chinese presence, in the form of aid project sites, negatively affect local perceptions of the quality of the media in terms of efficiency.

To add perspective to the effects of Chinese aid, we compare the effects of living close to a Chinese aid project site with the effects of living close to an aid project site implemented by the World Bank. We utilize the fact that the data from AidData is classified according to OECD's DAC-classification of aid and compare the Chinese "ODA-like" project sites with ODA project sites form the World Bank. This is done in Table 5: Panel A. The columns are paired for each Afrobarometer question, alternating between the effect of Chinese aid and World Bank aid. Hence, the results of Chinese aid project site found in Table 4 reoccur in Table 5: Panel A.

| **TABLE 5: Part II Comparison** | *Role of Gov.* | | *Purpose of Media* | | *Charac. of a Dem.* | | *Abuse of Truth* | | *Inefficiency of Media* | |
|---|---|---|---|---|---|---|---|---|---|---|
| VARIABLES | (1) Chinese Aid | (2) World Bank Aid | (3) Chinese Aid | (4) World Bank Aid | (5) Chinese Aid | (6) World Bank Aid | (7) Chinese Aid | (8) World Bank Aid | (9) Chinese Aid | (10) World Bank Aid |
| *PANEL A:* | | | | | | | | | | |
| Ongoing | -0.007 | -0.019*** | -0.029*** | -0.078*** | 0.016*** | 0.045*** | 0.027*** | -0.018*** | -0.044*** | -0.122*** |
| | (0.005) | (0.005) | (0.004) | (0.005) | (0.004) | (0.004) | (0.005) | (0.006) | (0.004) | (0.005) |
| Projected | 0.020 | -0.003 | -0.078*** | -0.081*** | 0.096*** | 0.013 | -0.045*** | 0.058*** | -0.085*** | -0.028* |
| | (0.012) | (0.016) | (0.010) | (0.014) | (0.011) | (0.012) | (0.013) | (0.017) | (0.011) | (0.016) |
| Diff-in-Diff ($ß_1 - ß_2$) | -0.0269 | -0.0161 | 0.0485 | 0.00264 | -0.0796 | 0.0327 | 0.0720 | -0.0760 | 0.0411 | -0.0943 |
| Observations | 49,220 | 49,220 | 49,144 | 49,144 | 48,328 | 48,328 | 44,615 | 44,615 | 46,790 | 46,790 |
| R-squared | 0.007 | 0.007 | 0.012 | 0.016 | 0.005 | 0.005 | 0.022 | 0.022 | 0.017 | 0.026 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| F test | 39.80 | 40.71 | 68.31 | 86.73 | 25.88 | 31.01 | 113.4 | 110.4 | 88.38 | 131.8 |
| Prob >F | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| *PANEL B:* | | | | | | | | | | |
| Ongoing | -0.011 | -0.034*** | -0.035*** | -0.104*** | 0.008 | 0.048*** | 0.031*** | 0.023** | -0.048*** | -0.119*** |
| | (0.009) | (0.009) | (0.008) | (0.009) | (0.007) | (0.007) | (0.009) | (0.010) | (0.008) | (0.009) |
| Projected | -0.002 | 0.001 | -0.101*** | -0.104*** | 0.113*** | 0.016 | 0.080*** | 0.150*** | -0.089*** | -0.047* |
| | (0.024) | (0.029) | (0.020) | (0.026) | (0.023) | (0.022) | (0.025) | (0.029) | (0.021) | (0.028) |
| Diff-in-Diff ($ß_1 - ß_2$) | -0.0085 | -0.0347 | 0.0659 | 0.006 | -0.105 | 0.0318 | -0.0490 | -0.127 | 0.0411 | -0.0716 |
| Favorable views towards China | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 14,701 | 14,701 | 14,665 | 14,665 | 14,679 | 14,679 | 14,166 | 14,166 | 14,618 | 14,618 |
| R-squared | 0.008 | 0.009 | 0.014 | 0.021 | 0.007 | 0.007 | 0.015 | 0.016 | 0.020 | 0.029 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| F test | 13.19 | 14.63 | 22.99 | 33.87 | 10.43 | 12.92 | 24.52 | 25.70 | 34.08 | 48.52 |

Note: Panel A: Compares Chinese aid project sites with World Bank project sites. Panel B performs the regression within the subsample among the respondents that considers China to be democracy. All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Panel A, column (2) measures the effect of an additional World Bank project site on the attitudes surrounding the Role of Government, with the same interpretation made previously. In this case, both ongoing and projected are statistically significant and the difference-in-difference type estimate is negative. This implies that the implementation of a World Bank project site is associated with a decreased probability that the respondent agrees with Statement 2 (The government should have the right to prevent the media from publishing things that it considers harmful to society) if the respondent lives closely to an ongoing World Bank project site. In column (4), where we measure the beliefs of media's purpose, both regressors are statistically significant and the difference between them is positive, suggesting that proximity to a World Bank aid project site increases the probability that the respondent agrees with Statement 2 (too much reporting on negative events, like government mistakes and corruption, only harms the country). Thus, the direction of the effect is the same for World Bank aid and Chinese aid in terms of attitudes on the Role of Government and the Purpose of Media.

In column (6), both ongoing and projected is statistically significant, with the difference-in-difference type estimate being negative. This indicates that respondents in proximity to World Bank aid project sites are more likely to choose "Free Media" when asked for the most important characteristic of a democracy. This result stands in contrast to the effect of proximity to a Chinese aid project site (column 5), where the difference-in-difference type estimate instead is negative. When measuring the effect of World Bank project sites on the "Abuse of truth" (column 8), both ongoing and projected is statistically significant and the difference-in-difference type estimator is negative. This implies that respondents living close to a World Bank project site believe the media abuses its freedom – by publishing things that are not true – *less* often (i.e., the respondents believe the media to be more truthful after the implementation of the World Bank project site). This result, as with the Characteristics of a Democracy (column 5-6), stands in direct contrast to the registered effect of a Chinese aid project site (column 7). In terms of the media's efficiency to reveal government mistakes and cases of corruption (column 10), the effect of ongoing and projected is statistically significant and the difference-in-difference type estimate is negative. This indicates that respondents close to a World Bank project site are less likely to view the media as inefficient. Again, this effect of proximity to a World Bank aid project site is the opposite of the effect of a Chinese aid project site (column 9). Thus, the effect of Chinese aid stands in direct contrast to the effect of World Bank aid in three of the five measures: Characteristic of a Democracy, Abuse of Truth, and Inefficiency of Media (column 5-10).

Table 5: Panel B present regressions of our main model specification within the subsample of respondents who hold favorable views toward China. This favorable view is captured by considering China to be a democracy and is represented by a dummy variable. The results of column (1)-(2) does not suggest any considerable changes between the subsample and the full sample. The same is true for column (3)-(4), with the direction of the difference-in-difference type estimates staying the same, and marginal changes in magnitude. Column (5)-(6) show the same trend as previous columns (1)-(4), with identical direction of effect and slight changes in magnitude – only here some significance is also lost (in column 5). However, column (7)-(8) breaks the trend of similarity. Here, the effect of proximity to a Chinese aid project site differs notably between the subsample and the full sample estimations. The results are statistically significant in both cases. In the full sample the estimates suggest an increase in the probability that the respondent perceives the media to abuse its freedoms more often through publishing things that are not true. But when regressed on the subsample, we observe a decreased probability that the respondent perceives the media to abuse its freedoms. This shift is not noticeable when an additional project site is implemented by the World Bank, where we instead observe the opposite. In column (9)-(10), estimating the media efficiency of revealing government mistakes and corruption, the trend of similarity returns. The results suggest that respondents who hold more favorable views towards the Chinese state also perceives the media to be more inefficient in revealing government mistakes.

### 5.2.1 Sensitivity Analysis

We perform several robustness checks and alternative specifications to explore potential endogeneity bias. All the details from these sensitivity tests can be found in the Appendix: Section 3. Firstly, we change our model from a linear to a non-linear probability model (probit model) which is presented in Table A13. The columns represent the different Afrobarometer questions, and the estimations are transformed to average marginal effects to ease the interpretation and comparison. Overall, the results from the probit model resembles the findings in our main results for Part II (Table 4), suggesting that our OLS is sufficient and robust to measure the effects of Chinese aid project sites on local beliefs and attitudes.

Next, instead of using dummy coded version of the Afrobarometer responses as our dependent variable, we use a LPM model to estimate the ordinal scaled response options to control if the coefficients changes when the replies are more nuanced (see Table A14). As with the alternative probit model (Table A13) and besides that statistically significance level for

projected diminishes for two out of five columns (column 1 and 4), the direction of the estimates is the same when comparing with the main analysis. The magnitudes vary slightly; for example, the probability that the respondent believes that too much reporting on negative events, like government mistakes and corruption, only harms the country (column 2) increases after the implementation of Chinese aid project sites when we use ordinal scaled compared to dummies. Overall, the sensitivity check confirms the estimates from the main analysis.

Since the effect of a project site might not happen instantaneously, we control for the duration of ongoing project sites in Table A15a and A15b. In Table A15a we use a continuous variable representing duration, where an increase with one unit reflects an additional year the project site has been active at the time of the Afrobarometer interview. In each column of Table A15a, duration (independent variable) is regressed on the main dependent variable, with the standard list of controls. The effect of an additional year is statistically significant and negative for all columns except column (3), Characteristics of a Democracy. This indicates that duration matters. The likelihood of respondents agreeing with Statement 2 in both Role of Government (column 1) and Purpose of Media (column 2) decreases as duration increases. Similarly, for Abuse of Truth (column 4), the probability that the respondent perceives the media to abuse its freedom by printing things that are not true decreases. As does the probability that the respondent perceives the media to be inefficient (column 5). Further on the effect of duration time, we test for the effect of dummy versions of the duration variable (see Table A15b). The results when using duration-dummies are very similar (see Table A15a): we observe a negative effect from increases in duration (i.e., the number of years that the site is active). However, it is worth noting that the duration variables do not control for systematic differences within different groups of duration; for example, if projects of shorter duration consistently differ in characteristics from projects of longer duration.

In Table A16a and A16b we modify the cut-off distances in our model. We do this to check for the sensitivity of distance specification (see discussion in section 4.2.2.1 and section 4.2.2.3). As before, the dependent variable in each column represents questions from the Afrobarometer and we alternate the cut-off distance from 25 km to 75 km. In Table A16a the cut-off distance is set at 25 km and the regressor ongoing are statistically significant at the 1 percent level for all columns. The direction of the difference-in-difference estimator is consistent with the main estimates, which indicate that the findings in Table 4 are robust for narrowing the distance to Chinese project sites. When increasing the cut-off distance to 75 km (see Table A16b), the difference between the estimates ongoing and projected in column (2) respectively 5 shifts and change direction, while also losing some degree level of significance.

This shift in direction and drop in level of significance may indicate that when increasing to 75 km cut-off distance, the risk of omitted variable bias increases, and the model starts including other factors which disturb the estimation (i.e., attenuation bias).

As mentioned earlier (see section 4.2.2.3), we are not able to distinguish between different types of aid projects (e.g., longer construction projects or direct cash payments), however, we can distinguish among the different sectors receiving aid. Some of these sectors are very small, with few aid projects in total. To avoid analysis based on too few observations, we focus solely on the three largest sectors: "Education", "Health", and "Transport and Storage". All sector specific regressions can be found in Table A17a-c in the Appendix: Section 3. First, in Table A17a, we limit the sample to only include aid in the "Education" sector. The results lack statistical significance overall (compared to the main results), suggesting no existing relationship. However, column (4) (Abuse of Truth) indicate similar results as our main analysis. Second, Table A17b covers the subsample on the "Health" sector. But as with "Education", the regressions in Table A17b lacks the desired level of statistical significance, cautioning any conclusions. Last, we examine the "Transport and Storage" sector (see Table A17c). Column (2) and column (5) confirms the findings of the main analysis, with statistically significant estimates for ongoing and projected and the same direction of the diff-in-diff estimator. Overall, the results indicate that our suspicion holds true: the fact that types of aid is spread out in all sectors of aid muddle the effect.

We also explore the potential lingering effect of living near past project sites (i.e., locations which have been, but is not – at the time of the Afrobarometer interview – active sites). To do this, we utilize the spatial-temporal strategy (in the same way as in the main analysis) to compute the effect of living near completed Chinese project sites (see Table A18 in Appendix: Section 3). The results indicate that there is an effect of project sites that stays after the project is completed and closed. Notably, column (3) (Charac. of Dem.) and column (5) (Inefficiency of Media) show statistically significant results, where the direction of the effect is the same as in the main analysis. Although column (4) (Abuse of Truth) lost some statistical significance in this check, it has shifted direction of effect. In other words, respondents living near completed Chinese project sites are less likely to perceive the media to abuse its freedoms by printing things that are not true. This can be clearly contrasted with the main result, in which the opposite appears to be true.

Furthermore, we target a subsample of respondents living within a 50 km radius to only a Chinese project site, and not close to any World Bank project site. This could make for an interesting group of comparison, seeing as there is a significant overlap between the two

categories in the main comparison (proximity to Chinese aid and proximity to World Bank aid). This could also help to gain understanding of the isolated effect of Chinese aid projects (see Table A19). However, the subsample regression shows a loss of significance, advising caution in interpretation. For column (3) (Characteristics of a Democracy), we note an increase in the probability that the respondent chooses "Free media" as an option if they live in the proximity to an ongoing project site. This result stands in contrast to our main analysis. Column (5) (Inefficiency of Media) is similar, suggesting that the implementation of a Chinese aid project site decreases the probability that the respondent perceives the media to be inefficient, which differs from our main results in Table 4.

Lastly, in Table 5: Panel B, we split the data and analyze a subsample of respondents who hold favorable views toward China to see how this may influence the direction of the effect. To complement this subsample analysis, we also carry out a regression on the subsample holding more hostile views towards China (non-favorable views towards China or in other words considering China to be more authoritarian) (see Table A20 in Appendix: Section 3). The result from the subsample regression (where we estimate the main model on only those who hold non-favorable views towards China), most results are statistically insignificant. As discussed briefly in Section 4.2.2.2, this method of splitting the data is not flawless. To control the validity of our split-sample analyses, we also run the main specifications with an added interaction term controlling for the attitudes towards China when living close to an ongoing and a projected Chinese aid project site. We then conduct t-tests of the interaction term between the respondents who holds favorable views towards China (i.e., believes China to be a democracy) and lives close to an ongoing or projected project site (see Appendix: Section 3, Table A13). From the results of the post-estimation analysis, we can note that the two subsample groups (i.e., respondents holding favorable and respondents holding non-favorable views towards China), does not differ significantly in three out of five regression models. Therefore, we cannot reject the null hypothesis that respondents perceive media freedom differently depending on their attitudes towards China. This weakens our split-sample analysis, as we cannot exclude the possibility of omitted variables bias in the subsample analyses.

Even though the post-estimation results are mostly statistically insignificant, it still provides an interesting analysis when observing the students t-test for Inefficiency of Media (column 5 of Table A20). When observing the post-estimation of respondents with non-favorable and favorable views towards China while living close to an ongoing Chinese aid project site, the two groups significantly differ. These results suggest that the Chinese aid

project sites influence the perceptions of media inefficiency differently depending on the respondents' attitudes towards China.

We also test the conditional regression using an ordinal scale measure instead of a dummy (see Table A21 in Appendix: Section 3). This ordinal measure spans from -5 (completely undemocratic) to 5 (completely democratic). The results from this change of measure does not differ from our main analysis, apart from the magnitudes of the main coefficients decreasing slightly.

# 6 Analysis and Discussion

Two questions have guided this thesis. The first pertained to the aggregate effects of Chinese aid on recipient countries' levels of press freedom. The second focused instead on local, sub-national, effects on the attitudes and beliefs of people most exposed to the Chinese aid. Part one and two will first be discussed separately, leaving an assessment of their joint meaning for the later part of the discussion.

Starting with the first research question. The main takeaway from the results of part one was a statistically insignificant relationship between Chinese aid and media freedom. Potential reasons for this weak connection have been raised in section 4.2.1.2 (Endogeneity Concerns and Robustness Analysis), with central issues of selection bias and omitted variables. Part II – on local effects of Chinese aid – later tries to neutralize this selection bias. On the one hand, these biases might explain part of the statistically insignificance observed in the results. A selection bias from the likely non-random allocation of Chinese aid across countries in Africa, and an omitted variable bias ultimately stemming from the complex task of explaining press freedom variation. While hard to explain in theory, being even harder to pinpoint empirically (Dutta and Williamson, 2016). On the other hand, the statistically insignificant results might simply imply a lack of relationship between Chinese aid and press freedom. The statistically insignificant results are consistent across all our robustness checks, and any strong, and politically impactful relationship between Chinese aid and governmental censorship should reasonably have been at least indicated by the results. This does not exclude the possibility of *any* significant link, but if such a relationship does exist, our results suggest it to be – on an aggregate, cross-national level of analysis – of no great magnitude.

The lack of evidence for a negative relationship between Chinese aid and press freedom (on an aggregate level) stand in contrast to suggestions by Gamso (2021) about China potentially exporting norms of media censorship to its economic allies. However, just as with the literature establishing a link between foreign aid and press freedom (Dutta and Williamson,

2016), Gamso (2021) examines a larger sample than we do, covering most nations worldwide. Narrowing down the sample to only include Africa could also explain part of the absence of statistical significance in our aggregate model.

Shifting focus to the second research question, results were very interesting. Across the two measures of media quality, we see a significant negative effect of living near Chinese aid project sites. First, respondents near Chinese aid project sites are more likely to think the media "abuses its freedom by saying or printing things that are not true". Second, they are also more likely to think that the media is inefficient in "uncovering government mistakes and cases of corruption". Both effects are in accordance with our hypotheses (hypothesis 3a and 3b) and remain robust and significant through *all* robustness and sensitivity tests. These negative effects could be explained as a reaction to Chinas own local media coverage of their aid projects. As discussed in Section 3.2, several papers (Nelson, 2013; Zhang and Mwangi, 2016; Zhang et al., 2016; Wassermann, 2012; 2016) note considerable efforts by China to expand its broadcasting footprint on the African continent. An increasing skepticism towards media could reflect an increased skepticism towards the Chinese media operating in Africa. Just on its own, this point could be considered speculative. However, the subsample analyses (see Table 5 in results and A19 in Appendix: Section 3) of respondents holding favorable views towards the Chinese state (i.e., believe China to be a democracy) support this channel of effect. These respondents are more likely to perceive the media to be truthful than both the overall sample and the subsample holding more hostile views towards China (i.e., considering China authoritarian). In other words, this suggests more skeptic respondents might view China's own media coverage of aid projects as propaganda, deeming media as a whole to be more untruthful, while respondents with favorable attitudes toward China 'buys' the information (or view it as non-problematic) to a larger degree. This would also indicate a use of foreign aid to promote a positive picture of Chinese presence in Africa, in accordingly with previous literature (Radelet, 2006; Wellner et al., 2022).

Without the possibility of directly investigating the details of media coverage, we can test the channel of how the effects spread by alternating the cut-off distance between 25 km to 50 km (see Table A17a in Appendix: Section 3). This would, in theory, lessen the channel of worker interaction but still capture the channel of media coverage. When fluctuating the proximity of living close to project sites form 50 km to 25 km, the magnitude of the project sites impacts decrease. This implies that the effect of project sites is spread more through media coverage than mouth-to-mouth among the local population.

Another of our main findings comes from the comparison between Chinese aid and World Bank aid (see Table 5). The local effects of Chinese aid stand in *direct contrast* to the local effects of World Bank aid. This difference of effects aligns with previous research on the negative local effects of Chinese aid (Isaksson and Kotsadam 2018a:2018b:202; Cha, 2020), pointing towards Chinese aid being systematically different in its effects when compared to World Bank aid.

Another finding is that normative attitudes among the local population is affected by proximity to Chinese project sites. Specifically, the results indicate that respondents living near Chinese aid project sites are less likely to answer "media's right to freely criticize the government" when asked about the most important characteristic of a democracy, while also being more likely to think that the government should have the right to intervene in the work of media. Put differently, normative attitudes on the role and importance of free media appear to differ among people living near Chinese aid project sites. This difference is in the hypothesized direction (hypothesis 2a and 2b). The change in normative attitudes could be driven by an increase in cultural exchange around aid project sites. For example, in the interaction between the local population and Chinese workers. However, the results on the local effects on normative attitudes are not as robust as the ones on quality of media. Because of this, we do not suggest any extensive spread of normative attitudes.

Lastly, combining part one and part two of this thesis sets up interesting points of analysis. Overall, our aggregate model does not pick up any significant negative effects of Chinese aid on press freedom. At the same, our local effects model does. On the one hand, this discrepancy could reflect a lack of conscious agenda in Chinese aid giving, where shifts in attitudes are explained by prejudice towards China. On the other hand, the discrepancy between the aggregate and local models could reflect a subtlety in the negative effects of Chinese aid, which is only visible when examining more granular data. Juxtaposing country rankings (V-Dem) with citizen attitudes also helps to highlight the real-world connection between the two data sources. While country rankings could be considered to portray a more accurate picture of actual levels of press freedom, the role of citizen attitudes and perceptions should not be understated. Indeed, if there is no perceived need for change (in aspects such as press freedom rights), actual change is unlikely to happen.

# 7 Conclusion

The last decades increase in Chinese presence worldwide has given rise to a debate on the impact and consequences of Chinese expansion. In this thesis, we have tested if Chinese aid

has an impact on the level of press freedom in Africa. Both at an aggregated level, measuring press freedom as an index-score provided by V-Dem, and at a sub-national level, measuring the difference in belief of media freedom among individuals who live close to Chinese aid project sites. To answer research question one, we constructed a linear fixed effect model at the cross-national level, using the number of Chinese aid project to capture effect on V-Dem's press freedom index. However, our results from Part I showed no evidence that countries housing more Chinese aid projects also experienced lower press freedom at an aggregated level.

Answering research question two, we shifted focus and analyzed the effect of Chinese aid projects at a sub-national level, estimating differences in local beliefs of press freedom. We measured the local effect of Chinese aid project sites through comparing the perceived level of press freedom among the respondents living close to ongoing project sites with the perceived level of press freedom among the individuals living in the proximity of locations that, at the time of the Afrobarometer interview, had not yet been turned into a project site. Since project sites were going to be built on the locations, we utilized this fact to reduce the selection bias inherent in Chinese aid project site allocation. Our results from Part II were not only statistically significant but also robust when testing for several model specifications. The results suggested that the implementation of Chinese aid project sites in Africa was associated with an increased probability that the respondent believed the media to be more untruthful and less efficient in revealing government mistakes and corruption. Hence, the shift in perceptions could be interpreted as that the local belief of the quality of media worsened after the implementation of a Chinese project site. To add perspective, we then compared Chinese aid with World Bank aid, with the two donors showing remarkably different effects on the local perceptions of the media. These findings are in line with previous research (Isaksson and Kotsadam, 2018a; 2018b; 2020; Cha, 2020; Iacoella et al., 2021) highlighting negative tendencies of Chinese aid in its local effects, ultimately pressing on the importance of careful evaluation of China as an aid donor.

To our knowledge, there is no other research analyzing the effects of Chinese aid on either national level of press freedom in Africa (Part I), or on local perceptions of press freedom in Africa (Part II). The results presented in this thesis is therefore the first of its kind. For future research to consider, we believe that a more thorough sector analysis will contribute with valuable insight.

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# 9 Appendix

## 9.1 Section 1 – Pre-estimation

**TABLE A1: List of countries**

| Recipient name | Region | Years | Recipient name | Region | Years |
|---|---|---|---|---|---|
| Algeria | North Africa | 2000-2017 | Liberia | West Africa | 2000-2017 |
| Angola | Mid Africa | 2000-2017 | Libya | North Africa | 2000-2017 |
| Benin | West Africa | 2000-2017 | Madagascar | East Africa | 2000-2017 |
| Botswana | South Africa | 2000-2017 | Malawi | East Africa | 2000-2017 |
| Burkina Faso | West Africa | 2000-2017 | Mali | West Africa | 2000-2017 |
| Burundi | East Africa | 2000-2017 | Mauritania | West Africa | 2000-2017 |
| Cameroon | Mid Africa | 2000-2017 | Mauritius | East Africa | 2000-2017 |
| Cabo Verde | West Africa | 2000-2017 | Morocco | North Africa | 2000-2017 |
| Cen. African Rep. | Mid Africa | 2000-2017 | Mozambique | East Africa | 2000-2017 |
| Chad | Mid Africa | 2000-2017 | Namibia | South Africa | 2000-2017 |
| Comoros | East Africa | 2000-2017 | Niger | West Africa | 2000-2017 |
| Dem. Rep. of the Congo | Mid Africa | 2000-2017 | Nigeria | West Africa | 2000-2017 |
| Rep. of the Congo | Mid Africa | 2000-2017 | Rwanda | East Africa | 2000-2017 |
| Cote D'Ivoire | West Africa | 2000-2017 | Sao Tome & Principe | Mid Africa | 2000-2017 |
| Djibouti | East Africa | 2000-2017 | Senegal | West Africa | 2000-2017 |
| Egypt | North Africa | 2000-2017 | Seychelles | East Africa | 2000-2017 |
| Equatorial Guinea | Mid Africa | 2000-2017 | Sierra Leone | West Africa | 2000-2017 |
| Eritrea | East Africa | 2000-2017 | South Africa | South Africa | 2000-2017 |
| Ethiopia | East Africa | 2000-2017 | Sudan | North Africa | 2000-2017 |
| Gabon | Mid Africa | 2000-2017 | Swaziland | South Africa | 2000-2017 |
| Gambia | West Africa | 2000-2017 | Tanzania | East Africa | 2000-2017 |
| Ghana | West Africa | 2000-2017 | Togo | West Africa | 2000-2017 |
| Guinea | West Africa | 2000-2017 | Tunisia | North Africa | 2000-2017 |
| Guinea-Bissau | West Africa | 2000-2017 | Uganda | East Africa | 2000-2017 |
| Kenya | East Africa | 2000-2017 | Zambia | East Africa | 2000-2017 |
| Lesotho | South Africa | 2000-2017 | Zimbabwe | East Africa | 2000-2017 |

**TABLE A2a: Afrobarometer Questionnaire**

| Definitions | Inquiries | Response options | N.Q. | Year | Countries | Number of observations |
|---|---|---|---|---|---|---|
| **Role of Government** | **Which of the following statements is closest to your view?** Choose Statement 1 or Statement 2.<br><br>*Statement 1: The media should have the right to publish any views and ideas without government control.*<br><br>*Statement 2: The government should have the right to prevent the media from publishing things that it considers harmful to society.* | Agree strongly with statement 1 (media right)<br><br>Agree with statement 1 (media right)<br><br>Agree strongly with statement 2 (government right)<br><br>Agree with statement 2 (government right)<br><br>Agree with neither<br><br>Don´t know | Q20 | 2011/2013 | Algeria, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Côte d´Ivoire, Egypt, Ghana, Guinea, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritius, Morocco, Mozambique, Namibia, Niger, Nigeria, Senegal, Sierra Leone, South Africa, Sudan, Swaziland, Tanzania, Togo, Tunisia, Uganda, Zambia, Zimbabwe | 51.754 |
| **Purpose of Media** | **Which of the following statements is closest to your view?** Choose Statement 1 or Statement 2**.**<br><br>*Statement 1: The news media should constantly investigate and report on government mistakes and corruption.*<br><br>*Statement 2: Too much reporting on negative events, like government mistakes and corruption, only harms the country.* | Agree strongly with statement 1<br><br>Agree with statement 1<br><br>Agree strongly with statement 2<br><br>Agree with statement 2<br><br>Agree with neither<br><br>Don´t know | Q38 | 2011/2013 | Algeria, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Côte d´Ivoire, Egypt, Ghana, Guinea, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritius, Morocco, Mozambique, Namibia, Niger, Nigeria, Senegal, Sierra Leone, South Africa, Sudan, Swaziland, Tanzania, Togo, Tunisia, Uganda, Zambia, Zimbabwe | 51.577 |

## TABLE A2b: Afrobarometer Questionnaire

| | | | | | | |
|---|---|---|---|---|---|---|
| Characteristics of a democracy | Which one of these things would you choose as the most essential characteristic of democracy? | Government ensures law and order<br><br>Media is free to criticize the things government does<br><br>Government ensures job opportunities for all<br><br>Multiple parties compete fairly in elections<br><br>None of these<br><br>Don't know | Q45 | 2011/2013 | Algeria, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Côte d´Ivoire, Egypt, Ghana, Guinea, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritius, Morocco, Mozambique, Namibia, Niger, Nigeria, Senegal, Sierra Leone, South Africa, Sudan, Swaziland, Tanzania, Togo, Tunisia, Uganda, Zambia, Zimbabwe | 51.563 |
| Truthfulness | In your opinion, how often, in this country:<br><br>*Does the news media abuse its freedoms by printing or saying things it knows are not true?* | Never<br><br>Rarely<br><br>Often<br><br>Always<br><br>Don't know | Q52a | 2011/2013 | Algeria, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Côte d´Ivoire, Egypt, Ghana, Guinea, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritius, Morocco, Mozambique, Namibia, Niger, Nigeria, Senegal, Sierra Leone, South Africa, Sudan, Swaziland, Tanzania, Togo, Tunisia, Uganda, Zambia, Zimbabwe | 51.754 |

**TABLE A2c: Afrobarometer Questionnaire**

| | | | | | | |
|---|---|---|---|---|---|---|
| Effectiveness | In this country, how effective is the news media in revealing government mistakes and corruption?<br><br>*How effective the news media reveals government mistakes and corruption?* | Very effective<br><br>Somewhat effective<br><br>Not very effective<br><br>Not at all effective<br><br>Don't know | Q53 | 2011/2013 | Algeria, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Côte d´Ivoire, Egypt, Ghana, Guinea, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritius, Morocco, Mozambique, Namibia, Niger, Nigeria, Senegal, Sierra Leone, South Africa, Sudan, Swaziland, Tanzania, Togo, Tunisia, Uganda, Zambia, Zimbabwe | 51.561 |
| Attitudes | On a scale between 0 and 10, where 0 means completely undemocratic and 10 means completely democratic, where would you place each of the following countries, or haven't you heard enough to say:<br><br>*People's Republic of China?* | 0 - 10<br>Don't know | Q47a | 2011/2013 | Algeria, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Cape Verde, Côte d´Ivoire, Egypt, Ghana, Guinea, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritius, Morocco, Mozambique, Namibia, Niger, Nigeria, Senegal, Sierra Leone, South Africa, Sudan, Swaziland, Tanzania, Togo, Tunisia, Uganda, Zambia, Zimbabwe | 51.587 |

**TABLE A3: Re-Coding of Questionnaires**

| Labels | Question | Code | Response options |
|---|---|---|---|
| Role of Gov. | Q20 | 0 | Strongly agree with statement 1<br>Agree with statement 1 |
| | | 1 | Strongly agree with statement 2<br>Agree with statement 2 |
| | | . | Agree with neither<br>Do not know |
| Purpose of media output | Q38 | 0 | Strongly agree with statement 1<br>Agree with statement 1 |
| | | 1 | Strongly agree with statement 2<br>Agree with statement 2 |
| | | . | Agree with neither<br>Do not know |
| Characteristic of a democracy | Q45 | 0 | All other response options |
| | | 1 | Media is free to criticize the things government does |
| | | . | Neither<br>Do not know |
| Abuse of truth | Q52a | 0 | Never<br>Rarely |
| | | 1 | Often<br>Always |
| | | . | Agree with neither<br>Do not know |
| Inefficiency of media | Q53 | 0 | Very effective<br>Somewhat effective |
| | | 1 | Not very effective<br>Not at all effective |
| | | . | Neither<br>Do not know |

**TABLE A4: Respondents live near project site (25 km)**

| Near (< 25 km) | Freq. | Percent | Cum. |
|---|---|---|---|
| Respondent's not living close | 39039 | 72.33 | 72.33 |
| Respondent's living close | 14938 | 27.67 | 100.00 |
| Total | 53977 | 100.00 | |

Note: Respondents living close (< 25 km) or not living close to a Chinese aid project site. A dummy variable taking 0 if the respondent is not living close to a Chinese aid project site and 1 if the respondent lives close to a Chinese aid project site.

**TABLE A5: Respondents live near project site (75 km)**

| Near (< 75 km) | Freq. | Percent | Cum. |
|---|---|---|---|
| Respondent's not living close | 23473 | 43.49 | 43.49 |
| Respondent's living close | 30504 | 56.51 | 100.00 |
| Total | 53977 | 100.00 | |

Note: Respondents living close (< 75 km) or not living close to a Chinese aid project site. A dummy variable taking 0 if the respondent is not living close to a Chinese aid project site and 1 if the respondent lives close to a Chinese aid project site.

**TABLE A6: Ongoing and Projected (25 km)**

| *Ongoing* | *Projected* | | Total |
|---|---|---|---|
| | Respondent's not living close | Respondent's not living close | |
| Respondent's not living close | 39507 | 1434 | 40941 |
| Respondent's living close | 13037 | 0 | 13037 |
| Total | 52544 | 1434 | 53978 |

Note: Given that the respondent lives close (< 25 km) to a Chinese aid project site: *Ongoing* represents that this project site is active and *Projected* reflects if this project site is projected.

**TABLE A7: Ongoing and Projected (75 km)**

| *Ongoing* | *Projected* | | Total |
|---|---|---|---|
| | Respondent's not living close | Respondent's not living close | |
| Respondent's not living close | 25822 | 2820 | 28642 |
| Respondent's living close | 25336 | 0 | 25336 |
| Total | 51158 | 2820 | 53978 |

Note: Given that the respondent lives close (< 75 km) to a Chinese aid project site: Ongoing represents that this project site is active and Projected reflects if this project site is projected.

## 9.2 Section 2 – Sensitivity Analysis Part 1

**TABLE A8: Robustness Part I – AID/GDP as independent variable**

| VARIABLES | (6) Model 1 | (7) Model 2 | (8) Model 3 | (9) Model 4 | (10) Model 5 |
|---|---|---|---|---|---|
| Aid | 0.000897 | -0.043 | -0.012 | -0.028 | -0.026 |
| | (0.0127) | (0.032) | (0.019) | (0.024) | (0.024) |
| Regime type | | -0.078*** | -0.067** | -0.031 | -0.031 |
| | | (0.029) | (0.031) | (0.023) | (0.023) |
| Aid # Regime type | | 0.004 | 0.002 | 0.003 | 0.003 |
| | | (0.003) | (0.002) | (0.002) | (0.003) |
| FDI | | | -0.001 | -0.002 | -0.001 |
| | | | (0.003) | (0.003) | (0.004) |
| Export | | | -0.000 | -0.000 | -0.000 |
| | | | (0.002) | (0.001) | (0.001) |
| Import | | | 0.006 | 0.002 | 0.002 |
| | | | (0.012) | (0.011) | (0.012) |
| GDP per Capita (log) | | | -0.364 | -0.458 | -0.533 |
| | | | (0.396) | (0.369) | (0.418) |
| ODA | | | 0.006 | 0.008 | 0.008 |
| | | | (0.016) | (0.014) | (0.014) |
| Judicial independence | | | | 4.984** | 4.991** |
| | | | | (2.095) | (2.078) |
| Checks and balances | | | | -0.065* | -0.066* |
| | | | | (0.033) | (0.033) |
| Corruption | | | | -2.324*** | -2.347*** |
| | | | | (0.787) | (0.782) |
| Oil rents | | | | | -0.001 |
| | | | | | (0.008) |
| Observations | 774 | 720 | 529 | 510 | 504 |
| R-squared | 0.000 | 0.102 | 0.094 | 0.232 | 0.235 |
| Number of countries | 50 | 46 | 41 | 40 | 39 |

Note: The regression controls for country and year fixed effects. All independent and control variables are lagged one year (t-1). Aid: financial value of Chinese aid divided by GDP. Regime type: Measured through Polity IV's variable Polity2, FDI: Foreign Direct Investments, Export: Export to China as percentage of total export, Import: Import from China as a percentage of total import, GDP per capita: Gross Domestic Product per Capita collected from World Bank (Constant 2017), ODA: Official Development Assistance (ODA) as percentage of GDP from OECD-DAC, Judicial independence collected by Harvard Dataverse, Checks measuring checks and balances within the political system collected from Database of Political Institutions, Corruption: Political Corruption Index measured by V-Dem's variable *v2x_corr*, Oil rents: collected from the World Bank. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A9: Robustness Part I – Freedom House**

| VARIABLES | (11) Model 1 | (12) Model 2 | (13) Model 3 | (14) Model 4 | (15) Model 5 |
|---|---|---|---|---|---|
| Projects | 0.0131 | 0.0207 | -0.172 | -0.0543 | -0.0308 |
| | (0.0624) | (0.172) | (0.186) | (0.157) | (0.166) |
| Regime type | | 1.275*** | 1.327** | 1.228*** | 1.238*** |
| | | (0.459) | (0.517) | (0.449) | (0.448) |
| Projects # Regime type | | 0.00437 | 0.0140 | 0.00352 | 0.00150 |
| | | (0.0155) | (0.0185) | (0.0164) | (0.0174) |
| FDI | | | -0.0643** | -0.0436 | -0.0370 |
| | | | (0.0270) | (0.0293) | (0.0367) |
| Export | | | -0.0378 | -0.0440* | -0.0411 |
| | | | (0.0263) | (0.0257) | (0.0273) |
| Import | | | 0.102 | 0.141* | 0.143* |
| | | | (0.0833) | (0.0716) | (0.0726) |
| GDP per capita (log) | | | 1.874 | 3.855 | 2.856 |
| | | | (4.764) | (3.887) | (4.844) |
| ODA | | | 0.141 | -0.0251 | -0.0323 |
| | | | (0.152) | (0.140) | (0.142) |
| Judicial independence | | | | -52.06** | -52.05** |
| | | | | (20.67) | (20.54) |
| Checks and balances | | | | 0.0877 | 0.0967 |
| | | | | (0.734) | (0.742) |
| Corruption | | | | 17.63* | 17.40* |
| | | | | (9.151) | (9.284) |
| Oil rents | | | | | 0.0225 |
| | | | | | (0.0441) |
| Observations | 732 | 681 | 500 | 481 | 475 |
| R-squared | 0.000 | 0.178 | 0.217 | 0.350 | 0.345 |
| Number of countries | 49 | 46 | 39 | 38 | 37 |

Note: Country and year fixed effects, likewise the independent and control variables are lagged one year (t-1). Projects: Number of projects, Regime type: Measured through Polity IV's variable Polity2, FDI: Foreign Direct Investments, Export: Export to China as percentage of total export, Import: Import from China as a percentage of total import, GDP per capita: Gross Domestic Product per Capita collected from World Bank (-Constant 2017), ODA: Official Development Assistance (ODA) as percentage of GDP from OECD-DAC, Judicial independence collected by Harvard Dataverse, Checks measuring checks and balances within the political system collected from Database of Political Institutions. Corruption: Political Corruption Index measured by V-Dem's variable *v2x_corr*. Oil rents: collected from the World Bank. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A10: Robustness Part I - Reverse Causality**

| VARIABLES | (16) Model 1 | (17) Model 2 | (18) Model 3 | (19) Model 4 | (20) Model 5 |
|---|---|---|---|---|---|
| Censorship effort | -0.0996 | -1.552 | -0.243 | 0.268 | 0.296 |
| | (0.672) | (1.413) | (0.809) | (0.875) | (0.841) |
| Regime type | | -0.467*** | -0.215* | -0.226** | -0.208* |
| | | (0.134) | (0.110) | (0.106) | (0.106) |
| Censorship effort # Regime type | | 0.0898 | 0.00772 | -0.0148 | -0.0201 |
| | | (0.120) | (0.0760) | (0.0803) | (0.0769) |
| FDI | | | 0.00227 | -0.00316 | -0.00357 |
| | | | (0.0143) | (0.0137) | (0.0153) |
| Export | | | 0.0340 | 0.0325 | 0.0265 |
| | | | (0.0298) | (0.0290) | (0.0297) |
| Import | | | 0.0918 | 0.0958 | 0.0996 |
| | | | (0.0588) | (0.0602) | (0.0617) |
| GDP per Capita (log) | | | 7.503*** | 7.571*** | 8.351*** |
| | | | (2.280) | (2.318) | (2.683) |
| ODA | | | 0.119 | 0.0831 | 0.0635 |
| | | | (0.0776) | (0.123) | (0.115) |
| Judicial independence | | | | 1.150 | 2.005 |
| | | | | (8.345) | (8.505) |
| Checks and balances | | | | 0.380 | 0.426 |
| | | | | (0.577) | (0.566) |
| Corruption | | | | 6.234 | 6.844* |
| | | | | (3.805) | (3.651) |
| Oil rents | | | | | 0.158*** |
| | | | | | (0.0407) |
| Observations | 816 | 728 | 538 | 520 | 513 |
| R-squared | 0.000 | 0.026 | 0.177 | 0.188 | 0.201 |
| Number of countries | 52 | 48 | 41 | 40 | 39 |

Note: Country and year fixed effects, likewise the independent and control variables are lagged one year (t-1). Censorship effort: Press Freedom score measured through V-Dem's v2mecenfm variable, Regime type: Measured through Polity IV's variable Polity2, FDI: Foreign Direct Investments, Export: Export to China as percentage of total export, Import: Import from China as a percentage of total import, GDP per capita: Gross Domestic Product per Capita collected from World Bank (-Constant 2017), ODA: Official Development Assistance (ODA) as percentage of GDP from OECD-DAC, Judicial independence collected by Harvard Dataverse, Checks measuring checks and balances within the political system collected from Database of Political Institutions. Corruption: Political Corruption Index measured by V-Dem's variable *v2x_corr*. Oil rents: collected from the World Bank. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A11: Robustness Part I – Three years lag**

| VARIABLES | (21) Model 1 | (22) Model 2 | (23) Model 3 | (24) Model 4 | (25) Model 5 |
|---|---|---|---|---|---|
| Projects | -0.005 | -0.021* | -0.006 | -0.006 | -0.004 |
| | (0.006) | (0.012) | (0.013) | (0.013) | (0.014) |
| Regime type | | -0.048* | -0.031 | 0.009 | 0.009 |
| | | (0.028) | (0.027) | (0.020) | (0.020) |
| Projects # Regime type | | 0.002 | 0.000 | 0.000 | 0.000 |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| FDI | | | 0.002 | 0.001 | 0.001 |
| | | | (0.002) | (0.002) | (0.003) |
| Export | | | -0.002 | -0.002 | -0.002 |
| | | | (0.002) | (0.002) | (0.002) |
| Import | | | 0.005 | 0.000 | 0.000 |
| | | | (0.006) | (0.006) | (0.006) |
| GDP per Capita (log) | | | -0.225 | -0.330 | -0.396 |
| | | | (0.332) | (0.311) | (0.364) |
| ODA | | | 0.003 | 0.004 | 0.004 |
| | | | (0.012) | (0.012) | (0.012) |
| Judicial independence | | | | 5.454*** | 5.460*** |
| | | | | (1.877) | (1.848) |
| Checks and balances | | | | -0.028 | -0.028 |
| | | | | (0.037) | (0.037) |
| Corruption | | | | -1.649** | -1.667** |
| | | | | (0.799) | (0.790) |
| Oil rents | | | | | 0.001 |
| | | | | | (0.006) |
| Observations | 818 | 761 | 529 | 510 | 504 |
| R-squared | 0.003 | 0.031 | 0.026 | 0.129 | 0.130 |
| Number of countries | 52 | 48 | 41 | 40 | 39 |

Note: The regression controls for country and year fixed effects. All independent and control variables are lagged three years (t-3). Projects: Number of projects, Regime type: Measured through Polity IV's variable Polity2, FDI: Foreign Direct Investments, Export: Export to China as percentage of total export, Import: Import from China as a percentage of total import, GDP per capita: Gross Domestic Product per Capita collected from World Bank (Constant 2017), ODA: Official Development Assistance (ODA) as percentage of GDP from OECD-DAC, Judicial independence collected by Harvard Dataverse, Checks measuring checks and balances within the political system collected from Database of Political Institutions, Corruption: Political Corruption Index measured by V-Dem's variable *v2x_corr*, Oil rents: collected from the World Bank. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A12: Robustness Part I – Five years lag**

| VARIABLES | (26) Model 1 | (27) Model 2 | (28) Model 3 | (29) Model 4 | (30) Model 5 |
|---|---|---|---|---|---|
| Projects | -0.004 | -0.013 | -0.006 | -0.004 | -0.003 |
| | (0.005) | (0.012) | (0.017) | (0.016) | (0.016) |
| Regime type | | -0.021 | -0.015 | 0.021 | 0.021 |
| | | (0.027) | (0.026) | (0.018) | (0.018) |
| Projects # Regime type | | 0.001 | 0.001 | 0.001 | 0.001 |
| | | (0.001) | (0.002) | (0.002) | (0.002) |
| FDI | | | 0.002 | 0.000 | 0.001 |
| | | | (0.002) | (0.002) | (0.002) |
| Export | | | -0.002 | -0.002 | -0.002 |
| | | | (0.002) | (0.002) | (0.002) |
| Import | | | -0.001 | -0.004 | -0.004 |
| | | | (0.006) | (0.006) | (0.006) |
| GDP per Capita (log) | | | -0.109 | -0.203 | -0.236 |
| | | | (0.401) | (0.413) | (0.503) |
| ODA | | | 0.010 | 0.009 | 0.009 |
| | | | (0.012) | (0.012) | (0.012) |
| Judicial independence | | | | 3.720* | 3.740* |
| | | | | (2.002) | (2.007) |
| Checks and balances | | | | -0.008 | -0.008 |
| | | | | (0.041) | (0.041) |
| Corruption | | | | -1.361* | -1.373* |
| | | | | (0.709) | (0.700) |
| Oil rents | | | | | 0.001 |
| | | | | | (0.006) |
| Observations | 767 | 714 | 529 | 510 | 505 |
| R-squared | 0.002 | 0.007 | 0.010 | 0.066 | 0.066 |
| Number of countries | 51 | 48 | 41 | 40 | 39 |

Note: The regression controls for country and year fixed effects. All independent and control variables are lagged five years (t-5). Projects: Number of projects, Regime type: Measured through Polity IV's variable Polity2, FDI: Foreign Direct Investments, Export: Export to China as percentage of total export, Import: Import from China as a percentage of total import, GDP per capita: Gross Domestic Product per Capita collected from World Bank (Constant 2017), ODA: Official Development Assistance (ODA) as percentage of GDP from OECD-DAC, Judicial independence collected by Harvard Dataverse, Checks measuring checks and balances within the political system collected from Database of Political Institutions, Corruption: Political Corruption Index measured by V-Dem's variable *v2x_corr*, Oil rents: collected from the World Bank. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

## 9.3 Section 3 – Sensitivity Analysis Part 2

**TABLE A13: Robustness Part II - Probit**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of a Dem. | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| Ongoing | -0.007 | -0.041*** | 0.016*** | 0.027*** | -0.044*** |
| | (0.005) | (0.004) | (0.004) | (0.005) | (0.005) |
| Projected | 0.019 | -0.071*** | 0.086*** | -0.046*** | -0.090*** |
| | (0.012) | (0.011) | (0.009) | (0.013) | (0.012) |
| Diff. in Diff. ($ß_1 – ß_2$) | -0.0267 | 0.0298 | -0.0701 | 0.0730 | 0.0455 |
| Observations | 49,220 | 43,598 | 48,328 | 44,615 | 46,790 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Wald chi2 (9) | 349 | 640.7 | 243.4 | 964.7 | 774 |
| Prob > chi2 | 0 | 0 | 0 | 0 | 0 |

Note: Coefficients transformed to marginal effect. All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A14: Robustness Part II – Ordinal Scaled Dependent Variable (LPM)**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of a Dem. | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| Ongoing | -0.021* | -0.077*** | 0.020* | 0.074*** | 0.131*** |
| | (0.011) | (0.010) | (0.011) | (0.009) | (0.009) |
| Projected | 0.021 | -0.194*** | 0.036 | -0.046* | 0.187*** |
| | (0.029) | (0.025) | (0.027) | (0.024) | (0.023) |
| Diff-in-Diff ($ß_1 – ß_2$) | -0.0420 | 0.118 | -0.0163 | 0.120 | -0.0564 |
| Observations | 49,220 | 49,144 | 48,328 | 44,615 | 46,790 |
| R-squared | 0.007 | 0.015 | 0.002 | 0.021 | 0.023 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 38.75 | 82.38 | 8.442 | 111.7 | 127.1 |
| Prob > F | 0 | 0 | 0 | 0 | 0 |

Note: Dependent variable coded as an ordinal scale. All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A15a: Robustness Part II - Duration**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of a Dem. | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| Duration | -0.009*** | -0.005*** | -0.001 | -0.004*** | -0.010*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Observations | 17,749 | 15,723 | 17,419 | 16,499 | 17,148 |
| R-squared | 0.019 | 0.010 | 0.002 | 0.037 | 0.026 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 43.76 | 20.75 | 4.196 | 80.91 | 54.36 |
| Prob >F | 0 | 0 | 0 | 0 | 0 |

Note: Duration dummy regressed on only ongoing Chinese aid project sites. All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A15b: Robustness Part II – Duration dummy**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of a Dem. | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| Duration: 1-2 years | -0.054*** | -0.026** | 0.006 | 0.048*** | 0.052*** |
| | (0.013) | (0.010) | (0.010) | (0.013) | (0.013) |
| Duration: 3-4 years | -0.076*** | -0.034*** | 0.045*** | -0.012 | -0.060*** |
| | (0.011) | (0.009) | (0.009) | (0.011) | (0.010) |
| Duration: 5-6 years | -0.134*** | -0.042*** | 0.019** | -0.033*** | -0.082*** |
| | (0.010) | (0.009) | (0.009) | (0.011) | (0.009) |
| Duration: 7 years < | -0.051*** | -0.041*** | -0.019** | -0.028** | -0.050*** |
| | (0.012) | (0.009) | (0.009) | (0.012) | (0.011) |
| Observations | 17,749 | 15,723 | 17,419 | 16,499 | 17,148 |
| R-squared | 0.025 | 0.011 | 0.004 | 0.039 | 0.032 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 42.49 | 15.93 | 6.690 | 61.64 | 47.47 |
| Prob >F | 0 | 0 | 0 | 0 | 0 |

Note: Duration dummy regressed on only ongoing Chinese aid project sites. All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A16a: Robustness Part II - Cut-off 25 km**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of a Dem. | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| *Cut-off 25* | | | | | |
| Ongoing25 | -0.014** | -0.037*** | 0.014*** | 0.032*** | -0.038*** |
| | (0.005) | (0.004) | (0.004) | (0.006) | (0.005) |
| Projected25 | 0.024 | -0.062*** | 0.103*** | 0.004 | -0.081*** |
| | (0.015) | (0.011) | (0.014) | (0.016) | (0.013) |
| Diff. in Diff. ($ß_1 - ß_2$) | -0.0380 | 0.0249 | -0.0896 | 0.0278 | 0.0422 |
| Observations | 49,220 | 43,598 | 48,328 | 44,615 | 46,790 |
| R-squared | 0.007 | 0.015 | 0.005 | 0.022 | 0.016 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 40.32 | 68.14 | 23.54 | 111.6 | 81.96 |
| Prob >F | 0 | 0 | 0 | 0 | 0 |

Note: *Ongoing25* respectively *Projected25* represents if the respondent lives in the proximity of 25 km to an ongoing or projected project site. All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A16b: Robustness Part II - Cut-off 75 km**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of a Dem. | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| *Cut-off 75* | | | | | |
| Ongoing75 | 0.000 | -0.038*** | 0.017*** | 0.019*** | -0.052*** |
| | (0.005) | (0.004) | (0.004) | (0.005) | (0.004) |
| Projected75 | 0.032*** | -0.057*** | 0.079*** | -0.045*** | -0.060*** |
| | (0.011) | (0.009) | (0.010) | (0.012) | (0.010) |
| Diff. in Diff. ($ß_1 - ß_2$) | -0.0314 | 0.0193 | -0.0622 | 0.0642 | 0.00857 |
| Observations | 49,220 | 43,598 | 48,328 | 44,615 | 46,790 |
| R-squared | 0.007 | 0.015 | 0.005 | 0.022 | 0.017 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 40.01 | 69.82 | 24.74 | 112 | 90.40 |
| Prob >F | 0 | 0 | 0 | 0 | 0 |

Note: *Ongoing75* respectively *Projected75* represents if the respondent lives in the proximity of 75 km to an ongoing or projected project site. All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A17a: Robustness Part II – Sector Analysis EDUCATION**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of a Dem. | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| Ongoing | 0.016 | -0.018* | 0.006 | 0.068*** | -0.020 |
| | (0.014) | (0.011) | (0.012) | (0.015) | (0.012) |
| Projected | -0.068** | -0.024 | 0.043* | -0.061** | -0.056*** |
| | (0.026) | (0.020) | (0.025) | (0.026) | (0.021) |
| Diff. in Diff. ($ß_1 - ß_2$) | 0.0844 | 0.00550 | -0.0370 | 0.128 | 0.0366 |
| Observations | 5,932 | 5,193 | 5,803 | 5,469 | 5,653 |
| R-squared | 0.003 | 0.008 | 0.004 | 0.036 | 0.017 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 2.176 | 3.633 | 2.492 | 22.81 | 10.73 |
| Prob >F | 0.0208 | 0.0208 | 0.0208 | 0.0208 | 0.0208 |

Note: All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A17b: Robustness Part II – Sector Analysis HEALTH**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of a Dem. | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| Ongoing | -0.015 | -0.030*** | 0.001 | -0.029** | -0.020 |
| | (0.014) | (0.011) | (0.011) | (0.013) | (0.012) |
| Projected | 0.064*** | -0.069*** | 0.060*** | -0.039* | -0.007 |
| | (0.020) | (0.016) | (0.017) | (0.021) | (0.019) |
| Diff. in Diff. ($ß_1 - ß_2$) | -0.0789 | 0.0388 | -0.0595 | 0.00995 | -0.0130 |
| Observations | 9,192 | 8,114 | 9,031 | 8,284 | 8,755 |
| R-squared | 0.017 | 0.024 | 0.005 | 0.049 | 0.005 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 18.18 | 20.02 | 4.543 | 51.52 | 3.486 |
| Prob >F | 0 | 0 | 0 | 0 | 0 |

Note: All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A17c: Robustness Part II – Sector Analysis TRANSPORT AND STORAGE**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of a Dem. | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| Ongoing | -0.034*** | -0.040*** | 0.006 | 0.015 | -0.104*** |
| | (0.013) | (0.011) | (0.011) | (0.013) | (0.012) |
| Projected | -0.052** | -0.086*** | 0.172*** | -0.008 | -0.125*** |
| | (0.024) | (0.018) | (0.024) | (0.025) | (0.022) |
| Diff. in Diff. ($ß_1 - ß_2$) | 0.0188 | 0.0463 | -0.166 | 0.0237 | 0.0208 |
| Observations | 8,160 | 7,440 | 8,029 | 7,515 | 7,768 |
| R-squared | 0.005 | 0.008 | 0.016 | 0.008 | 0.025 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 4.571 | 6.880 | 12.40 | 6.195 | 22.55 |
| Prob >F | 0 | 0 | 0 | 0 | 0 |

Note: All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A18: Robustness Part II – Completed Projects**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of a Dem. | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| Completed | -0.023*** | -0.038*** | 0.030*** | -0.033*** | -0.049*** |
| | (0.006) | (0.005) | (0.005) | (0.006) | (0.006) |
| Projected | -0.040* | -0.028 | 0.059*** | -0.017 | -0.050*** |
| | (0.021) | (0.018) | (0.019) | (0.021) | (0.019) |
| Diff-in-Diff ($ß_1 - ß_2$) | 0.0172 | -0.0106 | -0.0289 | -0.0157 | 0.002 |
| Observations | 37,554 | 33,409 | 36,879 | 33,839 | 35,566 |
| R-squared | 0.006 | 0.016 | 0.005 | 0.017 | 0.015 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 23.55 | 57.29 | 19.51 | 65.17 | 61.14 |
| Prob >F | 0 | 0 | 0 | 0 | 0 |

Note: Completed – Respondents who lives in the proximity to a completed project site. Projected – Respondent who has *not* lived close any project site in the past but lives in the proximity to a projected Chinese aid project site. All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A19: Robustness Part II – Only close to Chinese aid projects**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of a Dem. | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| Ongoing | 0.014 | 0.017 | -0.031*** | 0.136*** | 0.081*** |
| | (0.012) | (0.011) | (0.008) | (0.013) | (0.012) |
| Projected | 0.182*** | -0.059 | 0.017 | -0.182*** | -0.060 |
| | (0.052) | (0.047) | (0.040) | (0.052) | (0.062) |
| Diff-in-Diff ($ß_1 - ß_2$) | -0.168 | 0.0760 | -0.0486 | 0.319 | 0.140 |
| Observations | 12,505 | 10,896 | 12,324 | 11,059 | 11,702 |
| R-squared | 0.010 | 0.020 | 0.005 | 0.022 | 0.041 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 13.67 | 24.04 | 6.792 | 29.45 | 61.87 |
| Prob >F | 0 | 0 | 0 | 0 | 0 |

Note: Respondents only lives close to a Chinese ongoing or projected aid project site. Regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A20: Robustness Part II – Interaction term**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of a Dem. | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| Ongoing | -0.013 | -0.022** | 0.007 | 0.035** | -0.051*** |
| | (0.014) | (0.011) | (0.012) | (0.014) | (0.013) |
| Projected | -0.005 | -0.054** | 0.098*** | -0.071* | -0.095*** |
| | (0.036) | (0.026) | (0.034) | (0.037) | (0.031) |
| Favorable views to China | 0.006 | 0.040*** | -0.028*** | -0.024** | -0.008 |
| | (0.010) | (0.008) | (0.008) | (0.010) | (0.009) |
| Ong. # Fav. China | 0.003 | -0.025* | 0.005 | -0.007 | 0.000 |
| | (0.016) | (0.013) | (0.013) | (0.016) | (0.014) |
| Proj. # Fav. China | 0.020 | -0.056* | 0.011 | 0.137*** | 0.002 |
| | (0.043) | (0.030) | (0.040) | (0.044) | (0.037) |
| Observations | 22,392 | 20,033 | 22,370 | 21,693 | 22,324 |
| R-squared | 0.008 | 0.015 | 0.006 | 0.015 | 0.023 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 15.16 | 24.97 | 11.38 | 28.25 | 44.94 |
| Prob >F | 0 | 0 | 0 | 0 | 0 |
| *Student's t-tests:* | | | | | |
| Ong. = Ong. # Fav. China | 0.574 | 0.910 | 0.952 | 0.147 | 0.050** |
| Proj. = Proj. # Fav. China | 0.737 | 0.976 | 0.220 | 0.007*** | 0.140 |
| Fav. China = Ong. Fav. China. | 0.883 | 0.000*** | 0.0835 | 0.489 | 0.711 |

Note: Ongoing – Respondents who lives in the proximity (< 50 km) to an ongoing Chinese aid project site. Projected – Respondents who lives in the proximity (< 50 km) to a projected Chinese aid project site. Favorable views to China – Dummy variable representing if the respondent believes China to be a democracy. All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**TABLE A21: Robustness Part II – Ordinal scaled attitudes**

| VARIABLES | (1) Role of Gov. | (2) Purpose of Media | (3) Charac. of Democracy | (4) Abuse of Truth | (5) Inefficiency of Media |
|---|---|---|---|---|---|
| Ongoing | -0.011 | -0.034*** | 0.011* | 0.030*** | -0.050*** |
| | (0.007) | (0.006) | (0.006) | (0.007) | (0.006) |
| Projected | 0.009 | -0.091*** | 0.108*** | 0.028 | -0.093*** |
| | (0.019) | (0.016) | (0.018) | (0.020) | (0.017) |
| Attitudes | 0.001 | 0.003*** | -0.003*** | -0.005*** | -0.001 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Diff-in-Diff ($ß_1 - ß_2$) | -0.0201 | 0.0579 | -0.0969 | 0.00113 | 0.0424 |
| Observations | 22,392 | 22,363 | 22,370 | 21,693 | 22,324 |
| R-squared | 0.008 | 0.011 | 0.006 | 0.015 | 0.023 |
| Country FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| F test | 18.13 | 25.71 | 13.18 | 34.03 | 53.88 |
| Prob >F | 0 | 0 | 0 | 0 | 0 |

Note: Performs the regression within the subsample among the respondents that considers China to be democracy measured through *Attitudes* (ranging from 0 completely undemocratic to 10 completely democratic). All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

**TABLE A22:**
**Part II – Comparison of Attitudes**

| | *Role of Gov.* | | *Purpose of Media* | | *Charac. of a Dem.* | | *Abuse of Truth* | | *Inefficiency of Media* | |
|---|---|---|---|---|---|---|---|---|---|---|
| Is China a Democracy? | (1) Yes | (2) No | (3) Yes | (4) No | (5) Yes | (6) No | (7) Yes | (8) No | (9) Yes | (10) No |
| Ongoing | -0.011 | -0.046 | -0.035*** | -0.027** | 0.008 | 0.011 | 0.031*** | 0.035** | -0.048*** | -0.053*** |
| | (0.009) | (0.032) | (0.008) | (0.011) | (0.007) | (0.012) | (0.009) | (0.015) | (0.008) | (0.013) |
| Projected | -0.002 | -0.029 | -0.101*** | -0.054** | 0.113*** | 0.100*** | 0.080*** | -0.068* | -0.089*** | -0.095*** |
| | (0.024) | (0.085) | (0.020) | (0.026) | (0.023) | (0.034) | (0.025) | (0.037) | (0.021) | (0.031) |
| Diff-in-Diff ($ß_1 - ß_2$) | -0.00849 | -0.0172 | 0.0659 | 0.0271 | -0.105 | -0.0891 | -0.0490 | 0.103 | 0.0411 | 0.0425 |
| Observations | 14,701 | 5,484 | 14,665 | 4,919 | 14,679 | 5,474 | 14,166 | 5,350 | 14,618 | 5,498 |
| R-squared | 0.008 | 0.006 | 0.014 | 0.006 | 0.007 | 0.005 | 0.015 | 0.019 | 0.020 | 0.029 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| F test | 13.19 | 3.509 | 22.99 | 3.165 | 10.43 | 5.583 | 24.52 | 11.45 | 34.08 | 16.72 |
| Prob >F | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |

Note: Dependent variable: The different Afrobarometer questions. Performs the regression within the subsample among the respondents that considers China to be democracy respectively among the respondents that do *not* consider China to be a democracy. All regressions controls for individual characteristics: age, age^2, gender, level of education and rural/urban residency. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1