China and the World Bank – How Contrasting Development Approaches Affect the Stability of African States

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China's development model challenges the approaches of traditional donors like the World Bank (WB). While some see this mostly as a chance, Chinese aid specifically and aid in general are also suspected of undermining developing countries' stability for various reasons. To examine the effect of aid on stability thoroughly, we define stability as a continuum ranging from outright over social conflict to attitudes about democracy. We find no evidence that either WB or Chinese aid increases conflict in Africa using a comprehensive set of georeferenced aid projects and sub-national stability measures. Those results are robust and hold across different types of outright conflict, but also for social conflict. Overall, WB aid correlates more strongly with a reduction of conflict than Chinese aid. Moreover, WB aid is associated with a more positive attitude about democracy, while Chinese aid is related to an increased acceptance of authoritarian models.

Keywords: Development Models, Development Aid, Stability, Conflict, Attitudes, World Bank, China, Africa, Geolocation
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1. Introduction

Global poverty rates decreased considerably over the last decades, most strongly in Asia, but many African states still lag behind (World Bank, 2020). In particular conflict-prone states plagued by reoccurring violence are often labeled the "new frontier of development."[1] The international community proposes to focus even more on those difficult cases to achieve the Sustainable Development Goals (UN, 2021). But there are also critical voices and studies suggesting that aid might increase the likelihood of conflict, and thus itself constitutes an obstacle to development (e.g., Collier and Hoeffler, 2002; Nunn and Qian, 2014).

The situation is further complicated by emerging donors, particularly China (e.g., Dreher and Fuchs, 2015; Dreher et al., 2018), challenging the predominance of traditional donors as part of a "new scramble for Africa" (The Economist, 2019). Some perceive Chinese aid as a crucial step forward that brings growth and stability to Africa. Others regard it as a big risk that narrowly focuses on Chinese self-interest and enriches local elites (Dreher et al., 2019; Anaxagorou et al., 2020), fosters conflict (Kishi and Raleigh, 2016) and exports authoritarianism (Hackenesch and Bader, 2020). We take these big geopolitical changes as a reason to revisit the potential impact of aid on stability by systematically contrasting the Chinese approach to development with that of the World Bank (WB), an important traditional donor.

Moving beyond the partly subjective public rhetoric, we argue that Chinese foreign aid needs to be considered with all its nuances. China's "no strings attached" approach to development differs sharply from the expert-driven, conditional approach of traditional donors like the WB and many other Western OECD's Development Assistance Committee (DAC) members. Both donors are interested in growth, but while the WB regards democracy, transparency, and human rights as critical to prosperity, the Chinese model highlights social and political stability as the key ingredient to development. China's economic growth and stability-oriented perspective might, thus, be seen as a contrast to the WB's rule- and expert-based democratic perspective, but their impact on stability is complex. Even if China's motive would be mere self-interest, China also has an incentive to protect its investments and its workers in Africa. Both donors will try to stop recipient governments from engaging in conflicts that they deem avoidable or unnecessary, and given their size, have some leeway over recipient governments.

At the same time, when defining stability more broadly than just focusing on outright conflicts, China is likely to build on its own domestic development experience, which combines growth with an autocratic and stability-oriented rule. Therefore, there are good reasons to believe China would be more willing to accept recipient governments' use of autocratic policies and non-lethal repression to enhance stability, while the WB emphasizes democracy and humanitarian values more strongly. We carve out the most important conceptual differences between the two donors and their potential effect on state stability.

To compare the impact of these approaches, one requires a holistic definition of stability. For this purpose, this paper defines stability as a broad continuum ranging from outright conflicts with at least a certain number of Battle-related Deaths to lower-level social conflict events like citizen protests and government repression, as well as attitudes related to stability. To investigate the complex relationship empirically, we link detailed georeferenced datasets on development projects by China (Strange et al., 2017; Dreher et al., 2019) and the WB (AidData, 2017) with georeferenced measures of stability at the sub-national level in Africa. Our dataset allows us to match the location of aid projects and conflicts more precisely

[1] See The Economist (2017).

than earlier studies. This way, it flexibly enables us to eliminate potential biases arising from, for instance, unobserved conflict trends, region-specific time-invariant factors, and country level time-varying factors. To avoid the risk of overlooking a conflict-increasing effect by eliminating too much variation, we show a wide range of fixed effects specifications introducing control variables step by step.

Our results show that when using the more precise sub-national data, there is no support for the hypothesis that aid by either donor fuels conflict, on average. Once we account for geographical heterogeneity and conflict history with fixed effects for sub-national regions (region FE), the coefficients for the WB are all negative, quite stable in magnitude, and mostly statistically significant. A one standard deviation change in aid is associated with about a 1.5 - 2.0 percentage points lower conflict likelihood. When studying China, we also find mostly negative but insignificant coefficients. There are no signs of strong selection effects. A sensitivity analysis using the procedure by Oster (2019) provides confidence intervals that comprise negative to neutral effects only.

We move beyond outright conflict by differentiating to what extent state and non-state actors are involved. Again, there is no sign of a conflict-fueling effect for any of the actors. WB aid is even associated with a significantly lower likelihood of conflicts between state and non-state actors and non-state actor violence against civilians. We also find no positive effects on social conflict like demonstrations, riots, strikes, and on government repression for both donors. When considering how aid may change attitudes, Afrobarometer survey responses suggest that both donors have different effects on measures of security, democratic norms, and perceptions of government behavior. While WB aid is linked to higher perceived security and stronger support for democratic values, Chinese aid tends to be associated with a stronger emphasis on rule-following behavior and a higher acceptance of autocratic regimes.

This paper contributes in several ways to a better understanding of the role of donors in influencing recipient countries' stability and the channels and mechanisms linking aid to various types of conflict. We combine the strengths of existing approaches at the country level (e.g., Bluhm et al., 2021; Nielsen et al., 2011; Nunn and Qian, 2014), with the advantages of studies focusing on sub-national aid data in specific sectors in selected countries (e.g., Berman et al., 2011; Child, 2018; Sexton, 2016; Van Weezel, 2015). The aim is to deliver the best possible compromise between using micro-data, which allows us to control for many unobservable factors, and estimating externally valid results for more than one country. Truly randomly allocated aid projects in individual countries possess a higher internal validity (Crost et al., 2016), but their findings could be driven by the particular country context or the specific type of aid. We consider a broad set of all aid-eligible African countries so that our results can be meaningfully interpreted beyond the context of an individual country.

Besides using new data and providing more precise estimates about the effect of aid on more comprehensive measures of stability, we shed some light on the hopes and fears associated with emerging donors (Asmus et al., 2020; Fuchs and Vadlamannati, 2013). In particular, China's increased global engagement, like the Belt and Road Initiative and the intense China-Africa Cooperation, is one of the crucial geopolitical changes in the last two decades. It will continue to create tensions in the future. Existing papers have focused on outright conflict or the impact of aid on attitudes towards China (Eichenauer et al., 2021; Blair et al., 2021; Müller, 2021), but a comprehensive picture of the impact of Chinese aid on stability defined in a broad sense is missing. The WB provides a good contrast as a prototypical example of a traditional, multilateral donor that accounts for democracy and humanitarian values. Comparing the two donors, we aim to paint a nuanced picture of differing approaches to development and their relationship with stability in an encompassing way.

2. Theoretical Considerations and Related Literature

We adopt a holistic definition of stability. The whole spectrum ranges from outright lethal conflicts to conflict between particular actors, up to lower-level social conflict like citizen protests against governments and government repression against its citizens. Beyond actual conflict events, citizens' attitudes provide an idea about stability and its underlying factors in a region, for instance, beliefs about the quality and fairness of democratic processes or rule-following behavior.

Several aspects of aid delivery could influence its effects on stability. First, aid may induce growth and raise incomes. This could affect stability by increasing the opportunity costs of conflict. In this regard, the aid effectiveness literature converges towards either a null effect (Doucouliagos and Paldam, 2009) or minor positive effects (Galiani et al., 2017) of aid on growth. There is some evidence that China's less bureaucratic processes with quicker implementation times and emphasis on economic "mutual benefits" (Asmus et al., 2020; Humphrey and Michaelowa, 2019) foster growth (Dreher et al., 2021), potentially even more than the established WB approach.

Second, by providing successful infrastructure projects or better training of bureaucrats, aid can enhance state capacity. If used wisely, e.g., to enforce the rule of law impartially, this may result in a "virtuous circle" of better state capability (Levi et al., 2009), conflict reduction (Berman et al., 2011), and fewer reasons to protest. If state agents exploit their increased capacity to enrich themselves, favor some groups over others, or weaken political opponents (Wig and Tollefsen, 2016), this higher capacity can also be associated with or trigger protests. At the same time, the higher state capacity could enable governments to repress civic engagement and curb protests (Besley and Persson, 2011).

Third, aid, like other resource-related income shocks, may have crucial distributional aspects (e.g., Berman et al., 2017; Dube and Vargas, 2013; Gehring et al., 2018). Similar to bilateral aid, WB projects appear to follow political motives in its allocation at the country level (e.g., Dreher et al., 2009) and not necessarily targeting the poorest within countries (Briggs, 2018; Öhler et al., 2019). At the same time, Chinese projects seem to reduce inequalities in economic activity within countries (Bluhm et al., 2020). By contrast, Isaksson and Kotsadam (2018a) suggest that Chinese engagement is associated with higher local corruption and a reduction in trade union membership (Isaksson and Kotsadam, 2018b). Moreover, Dreher et al. (2019) find that Chinese projects are more likely to benefit leaders' birth regions. Both could increase individual inequalities and induce conflict (Østby, 2008).

Fourth, aid projects could influence stability through conditions imposed by the donors and the sensitivity of the donor towards local culture and context. Like many other traditional Western donors, the WB often imposes conditions regarding governance, equality, anti-discrimination, etc., and requires specific processes in aid-receiving countries. The Bank is also considered to be a global leader in "conflict-sensitive programming" (Van der Windt and Humphreys, 2016; World Bank, 2011), based on the idea that conflict management can mitigate conflict (Gonzalez and Neary, 2008). This involves the identification of conflict escalators using a detailed Conflict Analysis Framework (CAF) (Wam and Sardesai, 2006) and Operational Procedures (World Bank, 2001; Bannon, 2010) are supposed to help WB staff to understand and cope with country-specific sources of conflicts.

Due to public pressure, the WB might also be more likely to withdraw aid if a government engages in excessive violence (see,e.g., Tir and Karreth, 2018). It uses an independent "Inspection Panel" to investigate complaints about human rights abuses or local conflict related to projects (Zvogbo and Graham, 2020). The aim is to build trust and social cohesion in post-conflict and conflict-affected countries (Bannon, 2010). This approach includes projects focusing on community-driven development and capacity building with regard to account-

ability and public service delivery, among others. The Kecamatan Development program in Indonesia, for instance, attempted to reduce protests via transparency through a particularly participatory approach (Gibson and Woolcock, 2005; Barron et al., 2011).

To the best of our knowledge, China, with its "no strings attached"-policy officially does not have an analogous set of policies, institutions, or operational tools in place to encourage conflict-sensitive development programming.[2] Yet, it is implausible that China would be neutral about or even welcome avoidable conflicts. This would endanger their existing investments and the safety of a large number of Chinese workers in Africa (officially 182,745 by 2019).[3] Hence, the Guardian postulates that unofficially "Chinese aid to Africa is going to come with all sorts of strings attached, despite the "no-conditionality" rhetoric" (The Guardian, 2012).

There are also ideological reasons why China is interested in avoiding conflict. Stability is a crucial part of the domestic Chinese development model. In a speech at the 2008 Communist Party's National Congress, Hu Jintao mentioned the word stability 21 times (freedom did not appear a single time) (see Beijing Review (2009). The party also portrays the country as a "rock of stability" for the world.[4] The downside of this focus on stability is that China is accused of financially supporting repressive governments in Africa and "exporting repression" to recipient countries (Kishi and Raleigh, 2016). Chinese aid was also linked to more corruption (Brazys et al., 2017; Isaksson and Kotsadam, 2018a) and weaker labor unions (Isaksson and Kotsadam, 2018b). In that respect, Chinese soft power is perceived as a tool to promote an authoritarian "Beijing consensus" (Halper, 2010).

China is keen on spreading its development model and emphasizes its advantages via a concerted public diplomacy approach (Bailard, 2016). Chinese development projects can require that partners broadcast Chinese radio or TV to win African "hearts and minds" (Zhu et al., 2019). For instance, a radio station set up in Kenya reserves a specified amount of hours to promote Chinese culture and values, China supplies textbooks for schools in Liberia, Ghana, and Tanzania, and organizes cultural events in South Africa (LA Times, 2017). Cultural centers aim at spreading Chinese culture and values. This is not good or bad per se; Western donors and the WB are engaging in similar efforts. The motivation to do so may be mere self-interest or the honest conviction that the respective development model is the best to raise developing countries out of poverty.

We will not be able to test all those individual channels directly. However, the comparison reveals crucial differences in the respective approaches to development in many dimensions. Those heterogeneities might translate into different direct and indirect effects on stability. For instance, while aid by either actor does not necessarily have to induce lethal violence, there might be differential effects on social conflict or local attitudes. We, thus, consider stability as a continuum taking into account several sub-national measures.

3. Data

3.1. Aid Data: World Bank and China

We consider all African countries with more than one million inhabitants on the OECD's DAC list of ODA recipients in 1995, the initial year of our sample period. We focus on

[2] China only established its "China International Development Cooperation Agency" with a centralized evaluation mandate in 2018 (see Janus (2018) on "Next Steps for China's New Development Agency").

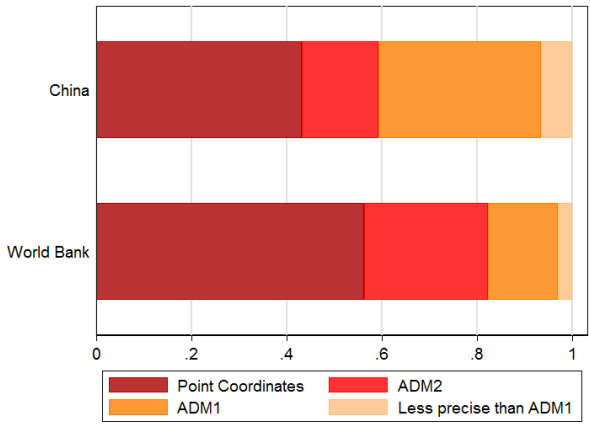
[3] See Cook et al. (2016) and SAIS-CARI (2021).

[4] *The Economist*, last accessed 31.01.2019.

disbursements by the International Development Association (IDA), the WB's arm for development aid. For China, we use Strange et al.'s 2017 dataset on Chinese ODA-like commitments, georeferenced by Dreher et al. (2019).[5] The data reports commitments based on announcements as the Chinese government does not publish fine-grained disbursement data. It considers all financial flows that qualify as aid by having a significant concessionary component.[6] Both donors are active in most of Africa: the WB in 35 and China in 41 countries. There is a significant overlap in recipient countries, but Humphrey and Michaelowa (2019) find no evidence of one donor systematically affecting each other's allocation choices.

Our main unit of analysis is the country-region-year, with regions as the unit of analysis referring to the first-level administrative division (ADM1: "regions," "provinces," or "states") (data from Hijmans et al., 2010). This level allows us to consider a substantial sub-national variation while still capturing over 90% of the overall project spending by China and the WB (see Figure 1).[7] This administrative level is also highly relevant for aid allocation. Many projects are assigned to specific regions, and the regional governments can influence how the funds are spent.

Figure 1: Disbursement/Commitment Shares by Geocoding Precision



Notes: Based on WB and Chinese project aid data from Strange et al. (2017) and Dreher et al. (2019).

Assigning aid projects to regions involves multiple steps. Precisely georeferenced projects and those with information about the first or second-level administrative units are intersected with the first-level division shapefile. Since most projects have several project locations, we assume that aid is distributed equally across locations, following Dreher and Lohmann (2015). For example, a project has ten project sites, with four locations in region A and six in region B. Hence, 40% of the project volume would be assigned to A and 60% to B. This procedure ignores projects with less precise locations. Those are mostly direct support

[5]Those data were compiled using, among others, the TUFF methodology, which covers a broad set of quality and triangulation steps. Due to the partial reliance on media, politically controversial and potentially more conflict-prone projects may be under-reported in regimes with low press freedom (Kilby, 2017). While this may induce a downward bias when using conflict as the outcome, descriptive statistics in Table A5, as well as the evidence in Dreher et al. (2019), suggest no such bias.

[6]We exclude other official finance (OOF) flows as they lack a development focus. Similarly, the WB's International Bank for Reconstruction and Development (IBRD) also provides development finance in the form of loans with interest rates closer to market rates.

[7]Lower level administrative regions (ADM2) would only capture between 60 and 80% because not all projects allow this precise coding. Using smaller grid cells would require solely relying on projects with exact data on latitude and longitude, being only available for about 50% for the WB and less than 50% for China.

for governments, but their average effect would be captured by country-year fixed effects. Appendix Section A provides more details.[8]

Table 1 compares aid projects by the two donors that we can assign to the ADM1 level. WB disbursements sum up to USD 29.4 billion, distributed over 1,472 projects in 25,041 locations in Africa. In Africa, Chinese aid amounts to USD 13.2 bn, from 333 projects in 1,308 locations. Hence, the WB finances more projects than China, and each project tends to have more project locations. China finances fewer projects but spends almost twice as much per project and nearly ten times as much per project location. Even though project characteristics differ by donor, both have a comparable propensity to engaging in regions with ongoing conflict events. It is also not the case that Chinese aid is committed predominantly to autocratic states (based on the democracy indicator of Bjørnskov and Rode 2019). Finally, about 77% of the recipient countries receive at least some aid from both donors.

Table 1: Donor Comparison – WB vs. China

	WB IDA	China
Total aid after cleaning (M$)	29357	13167
Active in No of countries	35	41
No of projects coded	104	206
No of project locations	1,434	612
Mean per Project (M$)	282.28	63.92
Mean per Location (M$)	20.47	21.51
Average No of Locations/Project	14	3
Recipient Region w. conflict (%)	9.87	9.09
Share of Aid to Democracies (%)	36.02	48.27
Countries w. Aid of Both Donors (%)	76.74	76.74

Notes: Aid measured in constant 2011 USD. Democracies are defined based on Bjørnskov and Rode (2019).

3.2. Stability Measures

To measure outright conflict, we follow the literature and create a binary *conflict incidence* measure based on the number of Battle-related Deaths (BRD). The data is taken from the Uppsala Conflict Data Program's (UCDP) Georeferenced Event Dataset (GED) (Croicu and Sundberg, 2015). GED provides a reliable and comprehensive source of georeferenced conflict events based on media and NGO reports and secondary sources like field reports and books. It also includes information about the type of conflict and the involved groups.[9]

Table 2 shows descriptive statistics for all stability measures. Figure 2b shows a map with all conflict events in our sample period, distinguishing between conflict with less than 5, 5 to 25, and more than 25 BRD. Country level studies usually use 25 or 1000, but at the smaller

[8]23% of Chinese projects focus on one location, while 95% of WB projects have more than one location. Formally, we distribute aid according the following equation: $Aid_{pijt} = \frac{Aid_{pit}}{\int Locations_{pi}} * \int Locations_{pj}$, where p is the project, i is the country, j is the region, and t is the period for which we estimate the allocation shares. For robustness, Table A22 displays the main results using population weights instead.

[9]Alternatives are the ACLED and PRIO datasets, which rely on similar primary data as UCDP. One issue with PRIO Gridded data is that neighboring cells in a 50km radius are also coded as conflict-affected, which may lead to erroneous conflict coding of neighboring administrative and ethnic regions (Tollefsen et al., 2012). ACLED is broader in coverage than UCDP data but is criticized for its partly ambiguous inclusion criteria and vague geocoding (Eck, 2012).

first-order sub-national level, 5 BRD per country-region-year is a reasonable threshold. We also code whether an outright conflict was a two-sided fight between government-related groups and non-state actors (rebels) or a one-sided action by either of those sides against civilians. For ease of reading the following results, all incidence measures are coded as 0 if there was no conflict and 100 if there was one.

We use the Social Conflict Analysis Database (SCAD, Salehyan et al. 2012) to measure social conflict and repression. A binary indicator for protests, riots, or demonstrations would take the value of 100 if there were at least one event in either of the categories. *Government repression* includes reactive and pro-active government repression. Examples include tear gas use against demonstrators but also increased surveillance activities like in Niger, where "after conducting one month of surveillance, the government arrested 9 military officers said to be planning a coup." Finally, we use selected questions from Afrobarometer Data (2018) to measure perceptions of security, democratic norms, and attitudes (see Table A8 for details).

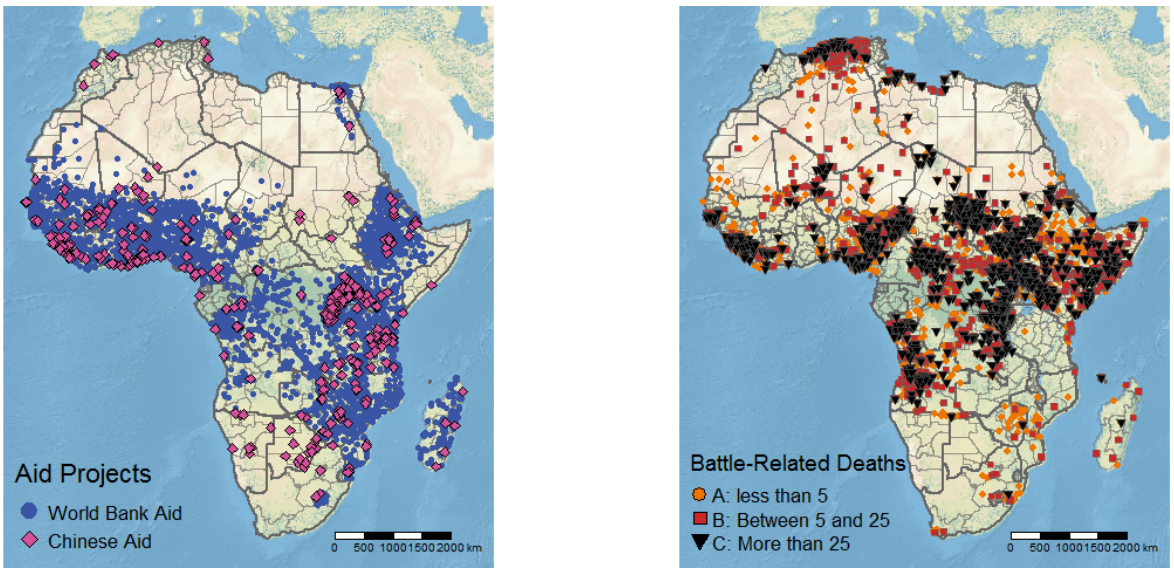
Table 2: Descriptive Statistics - ADM1 Regions

	Mean	SD	Min	Max
World Bank Aid	2,240,340.4	8,991,908.8	0.0	488,643,177.8
ln(WB Aid)	5.5	9.3	-4.6	20.0
Chinese Aid	1,391,272.1	22,843,119.5	0.0	900,000,000.0
ln(Chinese Aid)	-3.7	4.0	-4.6	20.6
Riots, Strikes, Demonstrations in Perc.	13.6	34.3	0.0	100.0
Repression Incidence in Perc.	7.2	25.9	0.0	100.0
Conflict Incidence in Perc.	11.6	32.1	0.0	100.0

Notes: Descriptive statistics for our main variables. We use the log of (aid +0.01), e.g., adding 1 cent of a USD, equaling approximately -4.6 if aid is zero.

Figure 2: Descriptive Statistics - Aid and Conflict across Africa

(a) (b)



Notes: Figure 2a depicts Chinese (2000-2012) and WB (1995-2012) development aid based on AidData (2017) and Dreher et al. (2019). Figure 2b depicts conflict (1996-2014) based on Croicu and Sundberg (2015). Borders refer to countries (thick line) and first-level administrative divisions (thin line).

3.3. Control Variables

We estimate specifications with control variables, which consider the most important aspects highlighted in the previous literature. However, due to the bad control problems prevalent in the literature, we use potentially endogenous contemporary controls only in selected specifications. Initial regional development is proxied using nighttime light (Henderson et al., 2012). To scale the potential for conflict for regions of different size (Hegre and Sambanis, 2006), we use population data from the Gridded Population of the World dataset (CIESIN, 2016). From the PRIO gridded data (Tollefsen et al., 2012), we calculate area-weighted regional averages for several natural resource indicators, including oil, gold, gemstones, and narcotics, as well as measures of temperature and precipitation that can be linked to conflict (Miguel et al., 2004). Table A9 gives a more detailed overview of all variables and sources.

Table 2 provides summary statistics. The final sample comprises 728 ADM1 regions in 45 countries. WB aid is, on average, higher per region-year than Chinese aid: USD 2.2 million versus USD 1.4 million. Figure 2a illustrates that both donors are active in a large number of countries and regions. Figure 2b reveals sufficient cross-sectional variation in conflict events across as well as within countries to estimate a demanding FE model. While the information for aid disbursements by the WB's IDA is available from 1995 to 2012, information on Chinese aid commitments in Africa is constrained to the years 2000 to 2012. We conduct our main analyses separately for each donor to exploit that the WB data are available for a longer sample period. However, we explore joint regressions for sensitivity analyses.

4. Empirical Strategy

There are various challenges for identification. Self-evidently, the WB and China do not allocate aid projects randomly. The main challenge is the potential strategic behavior of donors where and when to disburse aid. If donors could anticipate whether there will be a conflict in a certain region in the next year, and if that influences their strategic decisions, this would bias our estimates. If donors would be more likely to go into regions with a high-conflict likelihood, this would bias the effect of aid on conflict upwards. If they would rather select out of conflict regions, a downward bias would be the result.

Figures 2a and 2b help to understand how our identification approach utilizes the sub-national data and conditions step by step on more and more observables and unobservables through various fixed effects, time trends, and covariates. First, country level data may obfuscate the links between regional aid receipts and the occurrence of conflict. Angola, for instance, receives more aid projects in regions that also experience more conflict. In contrast, the regions in Sudan that often receive aid are not the ones that experience conflict. Sub-national data helps to more precisely link aid and stability. Second, the correlation between aid and conflict is affected by unobserved region-specific factors that can make both receiving aid projects and conflict more likely. Region fixed effects eliminate time-invariant differences that could affect both the independent and dependent variables. Third, events at the country-year level - like a political regime change - could affect conflict and coincide with changes in aid allocation, inducing a spurious correlation. While country-times-year (henceforth country-year) fixed effects can eliminate the effect of such events, those are very restrictive specifications, which may eliminate too much variation and falsely conclude that there is no conflict-fueling effect of aid. Thus, we eliminate biasing variation step by step to assess the direction of a possibly remaining bias transparently.

Specifically, we start with the cross-country correlations between aid and conflict using

$$C_{c,t} = \beta_1 A_{c,t-1/t-2} + \delta_c + \tau_t + \epsilon_{c,t}, \tag{1}$$

where $C_{c,t}$ is our conflict indicator of interest in country c, and year t. $A_{c,t-1/t-2}$ is the log of per capita aid. We consider aid variables in standard deviations to make them comparable across donors. Note, that for data reasons we use WB aid *disbursements* but Chinese aid *commitments*. We lag WB aid disbursements by one year. Chinese aid commitments are on average disbursed about one year later (see Dreher et al., 2019, 2021), which is why we use a lag of two years for China. Thus, the timing that we assume for aid to potentially affect conflict is the same for both donors.[10]

We then move our analysis to the level of sub-national regions i:

$$C_{i,c,t} = \beta_1 A_{i,c,t-1/t-2} + \delta_i + \tau_t + \Delta_i T + X'^{Ex}_{i,c,t}\beta_2 + \epsilon_{i,c,t}, \tag{2}$$

$$C_{i,c,t} = \beta_1 A_{i,c,t-1/t-2} + \delta_i + \Delta_i T + X'^{Ex}_{i,c,t}\beta_2 + \kappa_{c,t} + \epsilon_{i,c,t}, \tag{3}$$

where our two baseline empirical specifications include time and region fixed effects, δ_i and τ_t. Furthermore, we add linear time trends $\Delta_i T$ that are allowed to differ for each country-region to control for any differing linear conflict trends across regions. Including country-year fixed effects $\kappa_{c,t}$ asks a subtly different question: conditional on whether the whole country is involved in a conflict or not in a particular year, how did previous aid receipts affect the conditional likelihood of a particular region to be also in conflict? For that reason, the following sections always consider one specification without (eq. 2) and one with country-year fixed effects (eq. 3).

We distinguish between three types of control variables. First, exogenous controls such as climatic shocks. Second, we account for the effect of time-invariant controls such as elevation or ruggedness of terrain by interacting those controls with year dummies. These first two sets of controls are contained in $X^{Ex}_{i,c,t}$, as they are not prone to constitute bad controls. Third, we twice lag potentially "bad controls" like nighttime light (as a proxy for economic activity), or population, $X^{End}_{i,c,t-2}$, which can be affected directly by aid projects. Using "pre-determined" values solves the bad control issue only if we assume sequential exogeneity. For that reason, those variables are not included in our baseline equations 2 and 3. They are only used in one specification as an additional test. Finally, the error term is denoted as $\epsilon_{ir,t}$.

Standard errors are two-way clustered at both the country-year and the regional level (Cameron et al., 2011). This allows for arbitrary correlation within a country and year, which is important since conflicts often have a strong spatial component and tend to spill over to other regions. Also, allowing for correlation within a region over time is important as conflict also tends to exhibit strong persistence.

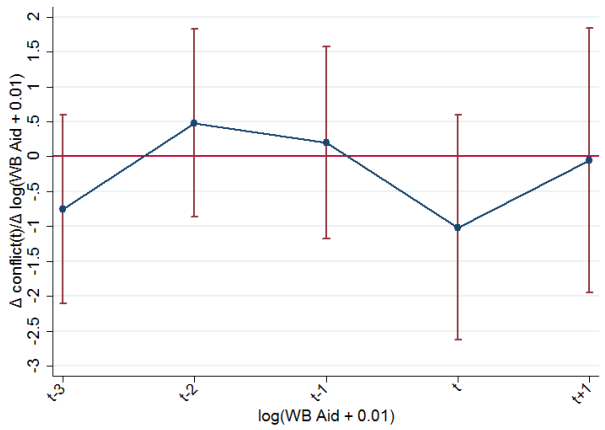
The remaining challenge is thus not the "where," but the "when" of strategic aid allocation. Given ambiguous predictions from the literature, we do not have a clear theoretical prior if the net effect of a higher expected future conflict likelihood on aid disbursements would be positive or negative for either donor. Conflicts might increase the need for aid to prevent or mitigate that conflict, but they also affect the success and costs of aid projects (Chauvet et al., 2010). Hence, besides trying to account for conflict dynamics in some specifications, we begin by assessing whether signs of such anticipation effects can be found in the data.

In the first step, Figure 3 shows results from lead and lag regressions. We simultaneously regress our conflict measure on one lead, the contemporaneous value, and three lag terms of aid. For the WB, there are no signs of problematic pre-trends. For China, the lead

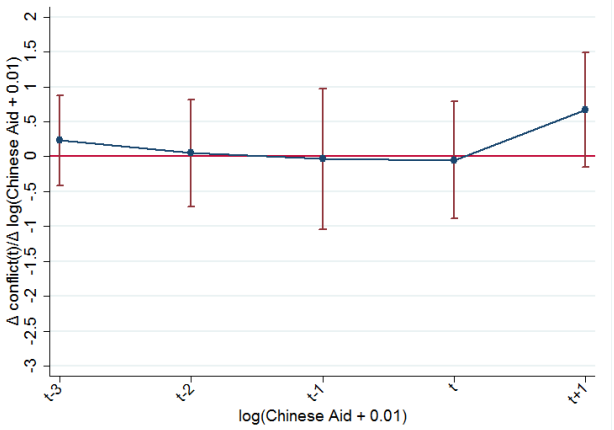
[10] AidData cannot distinguish exactly how much money from the Chinese commitments is disbursed in a particular year for most projects. However, Dreher et al. (2021) suggest that one year fits the data best, for observations where the information exists .

Figure 3: Pre- and Post-Trends

(a) Lags and Leads of World Bank Aid



(b) Lags and Leads of Chinese Aid



Notes: We jointly consider three lags and an additional lead for a standard deviation of $ln(aid + 0.01\,\text{USD})$. Figure (a) plots the coefficients for the WB and Figure (b) for China. Analogous to eq. 2, estimates consider sub-national region and year fixed effects, time trends, as well as a set of exogenous controls and exogenous controls interacted with year fixed effects.

term has a positive sign, suggesting that the donor might select into more conflict-prone regions. Empirically, this suggests that, if anything, the estimate for China in the following regressions might be somehow upward biased.[11]

5. Results

5.1. Main Results: Outright Conflicts

To ease comparison with the existing literature, Table 3 starts with three country level specifications that include only country and year fixed effects. We aim to show that our results are neither driven by our allocation decisions regarding aid nor the chosen conflict threshold. Columns 1 and 2 use a threshold of 25 Battle-related Deaths (BRD). Column 3 moves the threshold to 5 BRD, which we also use for the sub-national analysis. Column 1 also includes project aid that has no geocoordinates allowing an assignment to the ADM1 level (see Figure 1). Column 2 then switches to using only aid with sufficient precision in the georeferences, as in the sub-national specifications that follow.

These decisions partly make a difference at the country level. For the WB, all three coefficients are negative; however, the sign changes between specifications for China. Nonetheless, the correlations are all insignificant. One important limitation is that we do not analyze general budget aid directly paid to the central government. Budget aid might be particularly problematic given that it is untied and more fungible than project aid. We can observe that there is no evidence for either donor's project aid fostering conflict at the country level.

Our main analysis at the sub-national level begins in column 4 with simple correlations, accounting only for country and year fixed effects (FE). This allows readers to evaluate a potential trade-off between eliminating bias and losing substantial variation. We add more restrictive fixed effects, time trends, and different categories of control variables step by step. The most important step, and the big advantage of the sub-national data, is adding region fixed effects in column 6. They capture region-specific, time-invariant attributes that can

[11] Appendix Table A19 shows similar results with country-year FE.

explain heterogeneity and conflict patterns within countries. In addition, we add region-specific linear time trends and exogenous time-invariant regional characteristics interacted with year dummies to capture their potentially time-varying influence. What is more, with country-year FE, the remaining variation is solely due to differences in aid distribution across regions within country-years. Hence, among other potentially omitted variables, this is conditioning on whether the country as a whole experiences a conflict. Finally, we add potentially endogenous controls as a robustness test in column 9.

For the WB, we find that the coefficient in the initial sub-national specification is close to zero (column 4). The estimates with more restrictive specifications are strictly negative. As expected, the most important step seems to be the use of region fixed effects to control for the large sub-national heterogeneity. Once those FE are included, the magnitude of the coefficients suggests that a one standard deviation change in log WB aid is associated with a decrease in the conflict likelihood between 1.64 and 1.98 percentage points. Compared to the baseline likelihood of 12 percentage, this is a substantial reduction.

For China, there are two positive, although insignificant, correlations at the country level. However, the coefficients turn negative in all specifications using sub-national data that allows for more precise identification. Again, adding region FE makes a substantial difference and turns the coefficient substantially more negative. This highlights the advantages of using the sub-national data, which allows us to control for important differences within countries. The overall pattern is comparable to the WB, but the coefficients suggest a less negative relationship with conflict. A one standard deviation change in log Chinese aid is associated with a decrease in the conflict likelihood of between 0.14 to 0.44 percentage points. Coefficients become smaller and turn insignificant once we add regional trends and exogenous controls, constituting a rather precisely estimated null effect. By being transparent about all those choices, Table 3 introduces control variables and fixed effects step by step. It is reassuring that there is no sign of a conflict-inducing effect for either WB or Chinese projects once we account for region fixed effects, which seems an uncontroversial and reasonable choice. If there would be influential omitted variables biasing the prior estimations, the change in our estimates across more and less restrictive specifications should give us an idea about the extent of a potentially remaining bias. A procedure championed by Oster (2019) uses that information together with the R^2 values to compute which parameter estimates could potentially be associated with aid as our variables of interest. In slightly simplified terms, it provides a confidence interval accounting for uncertainty regarding the completeness of the specification and data. For the WB, this interval ranges from -3.29 to 0.01, for China from -0.18 to -0.09. Hence, there are no indications that other unobserved factors would move the results towards a positive coefficient of an economically meaningful magnitude and statistical significance in both cases.

Table 3: Main Results - Aid and Conflict Likelihood

	Country Level			Sub-national Level (ADM1)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: WB Aid									
ln(World Bank Aid $_{t-1}$)	-1.5913	-0.3027	-2.5929	0.0097	-0.4623	-1.9838***	-1.4983**	-1.6516**	-1.6367**
	(4.3837)	(4.2432)	(3.6482)	(0.7235)	(0.6369)	(0.6136)	(0.6684)	(0.7603)	(0.8331)
N	836	836	836	13104	13104	13104	13050	13050	11699
R-squared	0.522	0.522	0.541	0.167	0.233	0.440	0.521	0.587	0.591
Identified β-set (Oster)						[-3.29;0.01]			
Panel B: Chinese Aid									
ln(Chinese Aid $_{t-2}$)	0.5251	-1.9316	0.4086	-0.0935	-0.0104	-0.4376*	-0.2624	-0.1392	-0.1770
	(1.5761)	(1.3596)	(1.4157)	(0.2829)	(0.2579)	(0.2298)	(0.2915)	(0.3547)	(0.3683)
N	792	792	792	9464	9464	9464	8700	8700	8254
R-squared	0.525	0.526	0.538	0.170	0.233	0.477	0.583	0.633	0.632
Identified β-set (Oster)						[-0.18;-0.09]			
Aggregation Level	Country	Country	Country	ADM1	ADM1	ADM1	ADM1	ADM1	ADM1
Focus on Georeferenced Aid	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Conflict Threshold (BRD)	≥ 25	≥ 25	≥ 5	≥ 5	≥ 5	≥ 5	≥ 5	≥ 5	≥ 5
Country FE	Yes	Yes	Yes	Yes	Yes	–	–	–	–
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Time Trends	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Region FE	No	No	No	No	No	Yes	Yes	Yes	Yes
Exogenous Controls	No	No	No	No	No	No	Yes	Yes	Yes
Exogenous Controls × Year FE	No	No	No	No	No	No	Yes	Yes	Yes
Linear Regional Trends	No	No	No	No	No	No	Yes	Yes	Yes
Lagged Endogenous Controls	No	No	No	No	No	No	No	No	Yes
Country × Year FE	No	No	No	No	No	No	No	Yes	Yes

Notes: The dependent variable is a binary conflict incidence indicator (in columns 1-2: 100 if BRD≥25, 0 if BRD<25; in columns 3-9: 100 if BRD≥5, 0 if BRD<5). The treatment variable is the standard deviation of $ln(aid + 0.01\,\text{USD})$. Columns 1-3 report country level results, whereas columns 4-9 refer to the sub-national level. The identified β-set builds on the approach on coefficient stability by Oster (2019). The sub-national sample includes first-order administrative regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Standard errors are in parentheses, two-way clustered at the country-year and regional level. Due to the lag structure, conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014 . FE and Time Trends refer to both Panel A and B. Time Trends include linear and squared country-specific time trends. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

5.2. Results: Types of Conflict

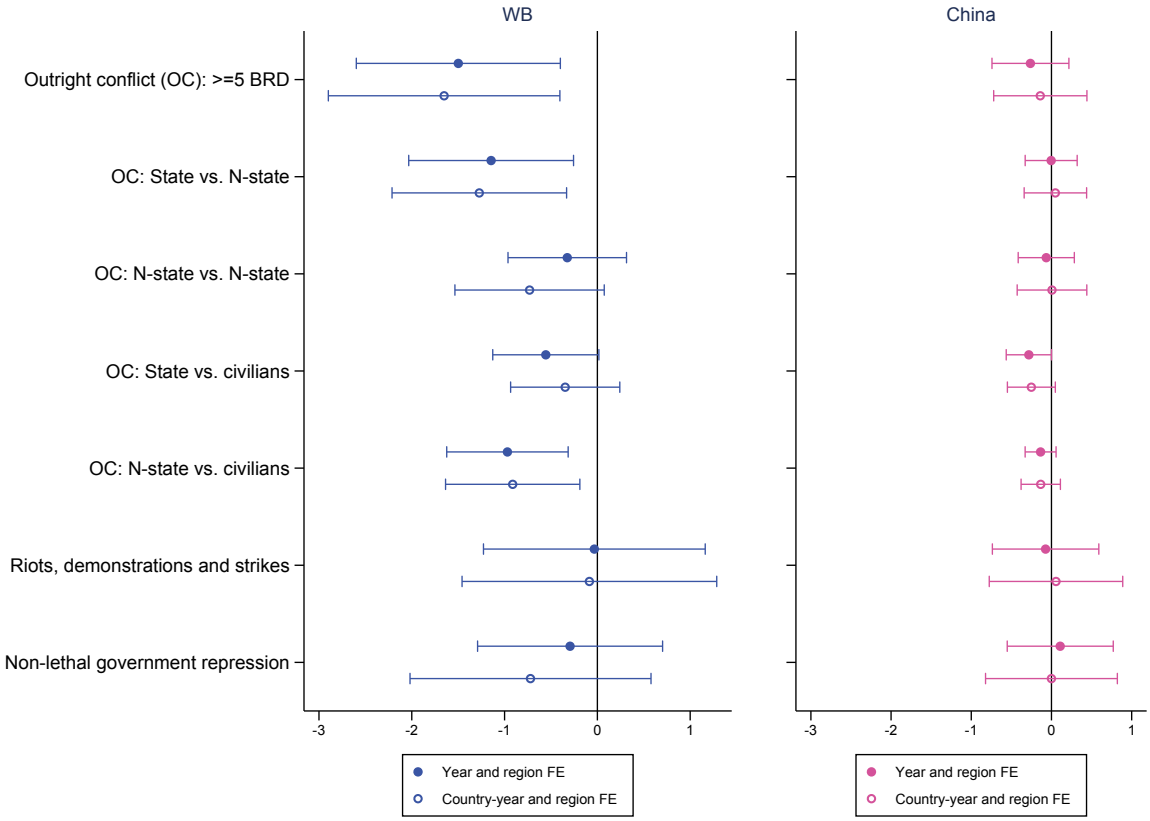
This and the following section use the restrictive specifications with and without country-year FE (column 6 and 7 in Table 3). Figure 4 shows results for four different types of conflict: (i) two-sided conflict action by governments against non-state groups (e.g., insurgents), (ii) two-sided conflict between two non-state groups, and one-sided conflict against civilians by the (iii) government or (iv) non-state groups. We also consider the effect on social conflicts (Salehyan et al., 2012) with our first indicator measuring whether at least one demonstration, riot, or strike took place. Second, we consider non-lethal repression since several reports associate China with repressing opposition and civilians to reduce social conflict (e.g., Kishi and Raleigh, 2016).

One of the most important results is that we find no sign of a conflict-fueling effect for even a single outcome or specification for neither donor. This is not just due to potentially imprecise estimates and wide confidence intervals, but all point estimates are close to zero or negative. Hence, even when looking in more detail at conflicts and often-overlooked social conflict, we find no evidence that aid systematically triggers violence. When compared across both donors, the pattern resembles the prior results. WB aid tends to be associated with a lower conflict likelihood, and Chinese aid shows a consistent null effect.

Specifically, WB aid is associated with reducing government violence against non-state actors and of conflict between non-state actors and civilians. This is plausible because the WB is known to punish human rights violations by governments. An example is suspended aid payments in Indonesia to push the government towards finding peaceful bargaining solutions (Tir and Karreth, 2018). The latter results could be an indication that the conflict-sensitive programming efforts of the Bank are actually paying off. There is no significant coefficient regarding social conflict and a small but insignificant negative coefficient for government repression. Hence, the WB seems more successful in affecting outright conflict than social conflict.

All results for Chinese aid are indistinguishable from zero. Theoretically, the average null could be hiding crucial heterogeneity with potentially positive conflict-reducing effects on one type of conflict but detrimental effects on other types, and we find no evidence for that. Moreover, we can provide no evidence for such an effect at odds with the popular repression hypothesis. This is contradicting reports about increasing protests against the presence of Chinese businesses (Wegenast et al., 2019) and investment (Iacoella et al., 2021). However, note that reasons could be the difficulty of capturing repression events, such as our data ending in 2014, or identification problems. Compared to the other estimates, the confidence intervals on social conflict are relatively wide, and it seems a worthwhile endeavor for future research to evaluate these plausible claims with more data and other methods.

Figure 4: Fixed Effects Regressions - Type of Conflict



Notes: The figure shows coefficient plots of individual FE regressions of our binary conflict incidence indicator (100 if BRD≥5, 0 if BRD<5) on aid. Aid is measured in standard deviations of $ln(aid + 0.01\,\text{USD})$. Hence, the coefficients reflect the effect of a one standard deviation change in WB/Chinese aid. The sample includes first-order sub-national regions in African countries for the 1995-2012 (WB) and 2000-2012 periods (China). Due to the lag structure, conflicts are considered for the WB from 1996 to 2013 and for Chinese aid from 2002 to 2014. Exogenous (time-varying) controls are included in all regressions. The Time Trends included consist of linear and squared country-specific time trends as well as linear regional time trends. "State vs. N-State" refers to state-based violence against non-government actors, "N-State vs. N-State" refers to non-government violence against other organized non-state groups. "State vs. Civilians" and "N-State vs. Civilians" refer to one-sided violence versus civilians by the government and non-government actors, respectively. The categories are mutually exclusive. "Riots, Demonstrations, and Strikes" and "Non-lethal Government Repression" are binary protest and government repression incidence indicators, taking on the value of 100 if there was at least one event in the respective category. Table A20 provides the full regression results. 90% confidence intervals are based on standard errors, clustered two-way at the country-year and regional levels. Full results are displayed in the Appendix.

5.3. Results: Attitudes

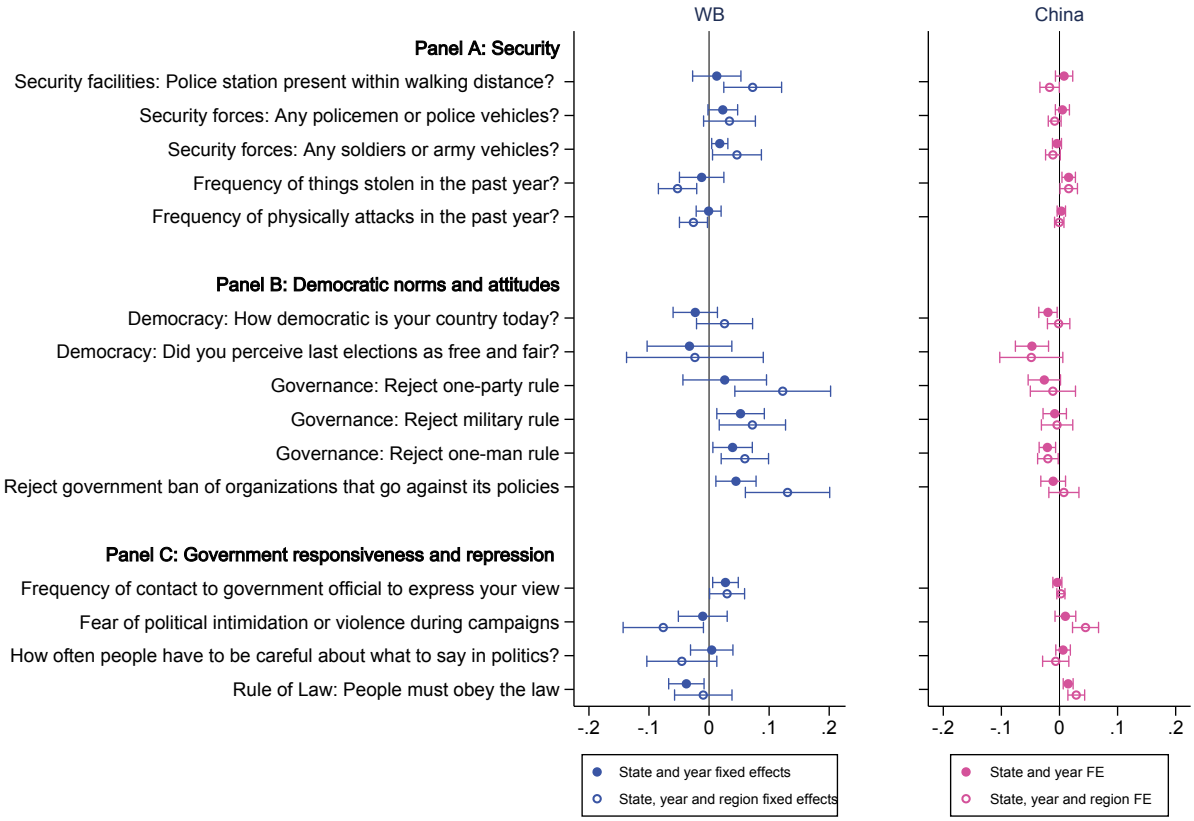
We use georeferenced Afrobarometer data to assess the plausibility of our results and investigate whether there is any indication that focusing only on conflict events might be hiding important changes that are not directly causing more conflict in the short term. To do so, we match data from all Afrobarometer waves to the regions and years in our sample and compute the region-year level average of each question we use (details in Table A7). Since the resulting dataset is an unbalanced panel with gaps, we use less restrictive sets of fixed effects than in our main specifications. Figure 5, thus, plots the coefficients from individual regressions of selected relevant questions on WB and Chinese aid: model 1 uses country and time FE, model 2 region and time FE.

The results are grouped into three categories. Panel A refers to questions signaling the presence of state security forces as a measure for state capacity within the area and the ability to maintain a monopoly of violence. Moreover, we use two questions asking whether respondents or their families were the victims of robbery or physical attacks in the past year. The results suggest that the WB engagement is associated with increased security forces and reduced crimes. There is no such increase for China.

Panel B examines democratic norms and attitudes. The results are not causal, but differences stand out that reflect both donors' distinct approaches. There are indications that the perception of democracy, and the fairness of elections, deteriorate in regions with Chinese aid projects. The WB seems to have a consistently positive relationship with democratic norms and a neutral to positive association with stability. Respondents are more likely to reject one-party rule, military rule, and one-man rule, which is not the case for China. With the coefficients being consistently significant in both models regarding one-man rule, respondents are less likely to reject these authoritarian governance forms.

Panel C examines how the government interacts with its citizens and its use of repression. In regions with more WB aid, people report being more apt to contact their government officials and express their views frequently. The fear of political intimidation or violence is lower with WB aid but higher in regions with Chinese aid activities. A final result stands out: In regions with more Chinese aid, respondents state more often that people must always obey the law, matching the social stability model propagated by the Chinese government. Hence, both donors' approach to influence local norms seems successful to some extent. WB aid correlates with a better perception of government-provided security, stronger democratic norms, and a higher willingness to interact with the government. For China, we observe a positive association with acceptance of autocratic norms and signs of deterioration of local democracy, together with a stronger emphasis on rule-following behavior. Although the results represent only conditional correlations, they seem plausible since the results correspond to the donors' different development models and norms.

Figure 5: Fixed Effects Regressions on Mechanisms using Afrobarometer for WB and China



Notes: The figure shows coefficient plots along with 90% confidence intervals of individual OLS regressions of the respective questions from Afrobarometer on the standard deviation of $ln(aid + 0.01\,\text{USD})$. All outcome measures were standardized, setting the mean to zero. Respondents were matched to the ADM1 regions using the provided geocoordinates. Afrobarometer surveys were conducted in 1999-2015 for a varying number of 12 to 36 countries, resulting in an unbalanced panel with uneven gaps between years. Full results are displayed in the Appendix.

5.4. Sensitivity

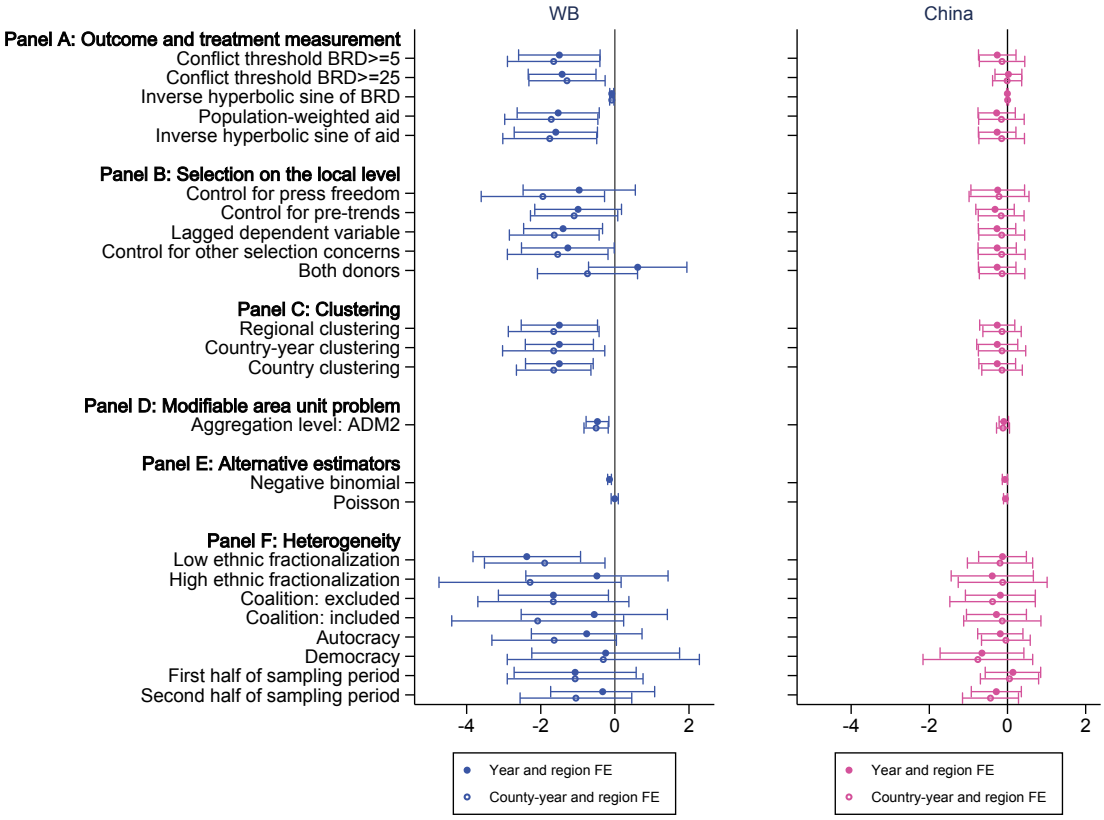
We evaluate several potential threats to identification and run further robustness tests. We acknowledge the remaining uncertainty but assess if any plausible variation in our set-up drastically changes the coefficients. Such a change could signal that even our comprehensive fixed effects analysis makes choices that lead us to overlook a conflict-fueling effect of aid.

Panel A uses different outright conflict indicators. We employ a higher threshold for the binary indicator or continuous measures of Battle-related Deaths (using the inverse hyperbolic sine transformation to address zero observations as in Bellemare and Wichman (2020)). None of those suggests a systematic conflict-fueling effect. We also consider adjustments to our treatment variable. Allocating aid based on population instead of locations also makes little difference, as does considering the inverse hyperbolic sine transformation of aid.

In panel B, we further investigate the sensitivity towards local selection effects. We try to account for a potential reporting bias in the media coverage of Chinese aid projects by controlling for press freedom. We also control directly for pre-trends, include a lagged dependent variable, and jointly consider both donors in a regression. In those regressions, we find an insignificant positive coefficient for WB aid, which, however, turns insignificant with country-year FE. Other papers, considering both donors, do identify different sub-national selection patterns regarding political participation (Isaksson and Kotsadam, 2018b),

initial income proxied by nighttime lights (Isaksson and Kotsadam, 2018a), and incumbency (Knutsen and Kotsadam, 2020). Overall, we find no evidence that the estimates are sensitive to further local selection bias. Using different levels to cluster standard errors in panel C barely changes the confidence interval. Furthermore, we address the modifiable area unit problem in panel D to consider if we find a conflict-fueling effect with a lower aggregation level. This is not the case. We also use non-linear specifications, a Poisson, and a negative binomial estimator, in Panel E. Both also suggest no conflict-fueling effect. Panel F considers heterogeneity along some key dimensions from the aid and conflict literature, including ethnic fractionalization and power relations, political institutions (Kersting and Kilby, 2014), and potential shifts in donors' conflict sensitivity across time. Consistent with previous results, the main finding of coefficients ranging between a negative and a neutral effect remains unchanged.

Figure 6: Robustness Tests



Notes: The figure shows coefficient plots along with 90% confidence intervals of individual OLS regressions of the respective outcomes on the standard deviation of $ln(aid + 0.01\,\text{USD})$. Dependent variables refer to a conflict incidence indicator that ranges from 0 to 100, except for Battle-related Deaths (BRD) being reported in standard deviations. Full results are displayed in the Appendix.

Finally, we also consider an instrumental variable approach building on Nunn and Qian (2014), Lang (2021), Gehring and Lang (2020), and Bluhm et al. (2020). Appendix Section Section B.1 provides results for our main specification and discusses sensitivity. Given the recent critique of Bartik-style IVs, we do not consider the IV as our main estimation strategy but as another piece of evidence. In line with the prior results, which suggested no problematic selection effects, the IV results also signal no conflict-fueling effect.

6. Conclusion

Conflict, particularly in African states, is seen as the major challenge for development, and development aid is an important tool to foster such development. At the same time, various theoretical arguments and empirical studies suggest that aid might, unfortunately, contribute to more rather than less conflict. This question is discussed even more controversially regarding China as an emerging donor, which constantly increases the range and extent of its development projects in Africa.

To address this issue, we compare China to a donor that represents the traditional, Western approach to development – the World Bank. China is the major emerging donor, emphasizing mutual economic benefits without official economic or political conditions for recipient governments and has no specific guidelines to manage potential conflict risks (Asmus et al., 2020; Hernandez, 2017). In contrast, the WB is a traditional, multilateral donor that emphasizes human rights conditions, expert knowledge, and engages explicitly in conflict-sensitive programming. We estimate the effects of those different development approaches using georeferenced aid projects and a comprehensive set of stability measures, ranging from outright conflict to social conflict and attitudes.

Our results show no signs of a conflict-fueling effect for either donor once we condition on region fixed effects. Instead, the results suggest that the WB is generally associated with reducing specific types of outright conflict, and Chinese aid has a net-zero effect on all indicators. When considering social conflicts, we find no significant relationship for either donor. From assessing attitudes, our results suggest that WB aid has a positive association with perceived safety, democratic norms, and democratic values. Chinese aid is linked with attitudes related to stability, such as a higher adherence to the rule of law and a higher acceptance of autocratic approaches.

Prior results on the aid-conflict relationship differ widely. Our paper contributes to the knowledge by providing, as we hope, the most comprehensive analysis of the effect of development aid projects on a wide range of stability measures in a multi-country analysis at the sub-national level this far. Given the difficulty of establishing causality, we discuss the direction of theoretical biases, are transparent about our choices, and examine the sensitivity of the results towards different approaches. While the precise magnitude of the coefficients could differ, none of our analyses indicate that Chinese projects fuel conflict and most suggest a conflict reduction for WB aid projects.

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