China's Foreign Aid: Political Determinants, Economic Effects*

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Abstract:

The efficacy of foreign aid, especially when given to satisfy the objectives of the donor country, is highly controversial. I study this question in the context of Chinese infrastructure aid, which has received much attention from policy makers. I build a novel project and firm-level dataset to identify determinants of Chinese aid and its economic consequences for recipient countries. I document that when there is local labor unrest in China, contracts for Chinese aid projects are allocated to large state-owned firms in the area and employment by these firms increases. Connections between these firms and other countries mean that China's response to domestic unrest also affects the allocation of Chinese aid to other countries. I exploit the variation in countries' receipt of aid caused by the timing and spatial variation in local labor unrest in China to develop an instrument for identifying the causal effects of Chinese aid on recipients. I find large positive effects on GDP, capital formation, consumption and employment.

Keywords: foreign aid, development finance, political economy, political stability, labor unrest, Chinese economy.

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1. Introduction

Governments of rich countries have transferred over 3.5 trillion dollars in foreign aid to poorer countries over the last decades (World Bank, 2020). The efficacy of foreign aid for poor countries is highly controversial (e.g., Easterly (2003), Sachs (2006)). Several studies have shown that donors allocate bilateral aid out of political or economic self-interest rather than to alleviate poverty (e.g., Alesina and Dollar (2000), Kuziemko and Werker (2006)). Existing cross-country studies, focusing on OECD donors, have regularly failed to find evidence of a positive effect of bilateral aid, and in some cases, have even found that such aid can harm recipients (e.g., Nunn and Qian (2014)). Many policymakers and several well-known papers suggest that the efficacy of bilateral aid can be improved if it is conditioned on the recipient country having good institutions (e.g., Burnside and Dollar (2000), Svensson (2000)). Amidst this debate, China has recently emerged as one the largest and most controversial aid donors. Since 2000, China has provided 843 billion dollars in official finance to developing countries, now outspending the U.S. (AidData, 2021).[1] The Chinese aid allocation process is non-transparent, poorly understood, and well-known for ignoring the traditional OECD standards of aid conditionality. Most Chinese aid is in the form of loans for hard infrastructure (e.g., roads, ports), supplied by Chinese firms and workers. The characteristics of China's aid have led to much speculation but little concrete evidence about its objectives and efficacy.

This paper studies political objectives that drive Chinese infrastructure aid and its economic impact on recipients. Several empirical challenges have hindered progress on these questions. Lack of transparency poses the first challenge. The Chinese government does not publish detailed data on its aid and the *de facto* process behind its allocation is poorly understood. Causal identification of the effects of aid on recipients (in any context) poses the second challenge. Donors may allocate aid based on recipient outcomes, such as levels of income or future growth potential, and there may be unobserved joint determinants of aid receipt and recipient outcomes, such as the quality of the recipients' institutions.

The primary goal of this study is to address these challenges by constructing a novel dataset linking unofficial project-level aid data with administrative firm-level data from China, including detailed customs and tax records, covering 2005–2015. To the best of my knowledge, this is the first comprehensive panel dataset of contractors involved in Chinese foreign aid projects. The granularity of the data provide variation which I use to understand the process of Chinese aid allocation at a micro level and to identify its causal impact on recipient countries. My study proceeds in several steps.

[1] This paper uses the terms *aid* and *official finance* interchangeably. Aid includes all bilateral finance by the Chinese government to governments of developing countries, including official development assistance (ODA), including grants and concessional loans, as well as other official finance (OOF), including less concessional financing. All findings in the paper hold when restricting the sample to aid that meets the OECD criteria for ODA / aid. See the Background section for more details and discussion.

First, I identify an important driver of Chinese aid. Anecdotal evidence suggests that a key goal of China's aid is to secure stable employment for Chinese workers by generating acyclical demand for Chinese goods and labor.[2] Consistent with this, China provides aid not as cash but in the form of goods and services directly supplied by Chinese firms. The Chinese government views stable employment of its citizens as a key condition for domestic political stability, its paramount policy goal (State Council, 2021). I exploit variation over space and time in threats to political stability within China to provide evidence for this motive. Specifically, I test whether the Chinese central government allocates foreign aid contracts to large firms it directly controls to provide employment in prefectures which experienced labor unrest in the previous year. Local unrest lagged by one year is plausibly unrelated to recipient countries' demand for aid, conditional on firm and year fixed effects as well as other leads and lags of unrest. My specification thus avoids bias from reverse causality and joint determinants of aid and unrest at the country level. I conduct a variety of tests to address remaining confounders.

I find that on average, an additional unrest event per million inhabitants in a prefecture leads to 0.1 additional contracts allocated to each central state-owned firm in the prefecture in the following year. Other leads and lags of unrest have no effect. The cumulative effect is sizeable: A back-of-the-envelope calculation shows that a one standard deviation increase in yearly unrest in Beijing leads to the additional allocation of 5.5 aid projects with a total value of USD 1.7 billion to firms in the prefecture, as much as 40% of Beijing's yearly public security spending (Zenz, 2018). Employment by central state-owned firms increases by 3%. I find no such effect for aid contractors not controlled by the central government (i.e., not designated to address local unrest). This goes against the possibility that the increases in aid contracts and employment in centrally controlled firms are confounded by economic shocks. I also document that the results are driven by prefectures whose local governments are constrained in their ability to address domestic instability directly by using public expenditure.

Second, I examine the impact of Chinese aid on recipient countries. To address the identification challenge, I construct an instrumental variable for the amount of aid received by a country in a year. The instrument exploits the time and spatial variation in labor unrest in Chinese prefectures, in combination with cross-sectional variation in connections between recipient countries and central state-owned firms in these prefectures. The cross-sectional variation in aid flows to specific countries could be correlated with those countries' levels of income, and changes in aggregate Chinese aid flows over time may be correlated with global economic trends. However, conditional on country and year fixed effects, the timing of prefecture-level unrest shocks is plausibly exogenous to recipient countries. Hence, the

[2] For example, Chinese policymakers in 2009 proposed launching a multi-billion dollar "Marshall Plan" to increase developing countries' purchases of Chinese goods to boost China's export economy and prevent worker unrest (Copper, 2016).

interaction between local unrest in China and recipient countries' exposure to this variation generates a valid IV for the amount of Chinese aid received by a country in a year.[3] I conduct several robustness and falsification tests to support the validity of the exclusion restriction.

I find that a one standard deviation increase in the instrument on average increases the number of aid projects committed to a country one year later by 0.249 (25% of a standard deviation), conditional on country and year fixed effects.[4] In other words, the IV first stage shows that China's political response to domestic unrest strongly influences the allocation of its aid to other countries.

I find large positive effects of instrumented aid on recipients. An additional Chinese infrastructure aid project on average increases GDP per capita by $129 (2.5% of the sample mean) and annual GDP growth by 0.5 percentage points three years after commitment.[5] Interestingly, these estimates are very similar in magnitude to effects of Chinese aid driven by arguably economic, rather than political motives found by a recent study (Dreher et al., 2021).[6] In addition, consistent with the nature of large-scale infrastructure projects, I observe positive effects on government consumption and capital formation. The increases in GDP and government consumption do not necessarily translate into an improvement of the economic well-being of the population *ex ante*. Part of the increases may account for the direct value of the infrastructure projects. One may also be concerned that the economic benefits of the projects are captured by political elites (e.g., Werker, Ahmed and Cohen (2009), Dreher et al. (2019)) or hurt the local population via negative spillovers on local labor markets (e.g., Zhao (2014), Wegenast et al. (2019)), especially since this aid is driven by the interests of the Chinese government and unconditional on recipients' institutions. To investigate this possibility, I examine variables connected to household income: household consumption and local employment. I find large positive effects on these outcomes as well: household consumption increases by 72 dollars per capita on average (2.2% of the sample mean) and

[3] The IV is exogenous since the unrest shocks are conditionally exogenous. Note that the cross-sectional variation in recipient countries' exposure to the prefecture-level shocks can be endogenous. The interaction between endogenous and exogenous variables is exogenous, controlling for the uninteracted endogenous variables. See the paper for more discussion.

[4] I measure a recipient country's propensity to get aid from a prefecture as the fraction of years in which the country receives aid projects supplied by central state-owned firms in the prefecture. Note that the country fixed effects control for the uninteracted, time-invariant propensity of a country to get aid.

[5] The average time between commitment and completion of a Chinese aid project is 2-3 years in the data. I show results for other leads and lags in the paper.

[6] Dreher et al. (2021) examine the effect of Chinese aid on economic growth and components of GDP. They interact a country's probability of receiving any Chinese aid with changes in China's total annual construction materials production and foreign exchange reserves to instrument for aid. My study differs from theirs in several ways. First, they do not examine domestic political determinants of Chinese aid. Second, I examine the effects of Chinese aid on additional outcomes, such as employment and diplomatic relations. Third, I expand their project-level dataset using additional sources and link it to administrative firm data, which allows me to study the political process underlying aid allocation at a micro level, while they rely solely on country-year variation. My granular data allow me to exploit more disaggregated variation, alleviating concerns about spurious correlations between aid-recipient country outcomes and macroeconomic trends.

the local unemployment rate decreases by 0.4 percentage points three years after commitment.

The results of this paper show that a significant fraction of Chinese foreign aid is driven by the Chinese government's political need to moderate domestic unrest. A back-of-the-envelope calculation shows that a one standard deviation increase in the instrument leads to the yearly re-allocation of 34 aid projects worth $9.5 billion in total. Despite this, Chinese aid has positive effects on recipient countries, in ways that benefit governments as well as households. These findings imply that foreign aid allocated according to domestic political needs of the donor country and unconditional on the institutions of the recipient government need not have deleterious effects on recipient populations as suggested by some existing studies. Importantly for policy, the findings also suggest that other aspects, such as the modality of aid, may be more important for determining aid efficacy, especially given that selfless aid is politically unrealistic. That being said, the allocation of Chinese aid may not be optimal from recipient countries' perspectives, and there may be heterogeneity in the effects of aid. See the Conclusion for more discussion.

This paper firstly contributes to the literature on the causes and consequences of foreign aid. The existing evidence of the impact of aid on recipients, with few exceptions focused on OECD donors, is highly mixed and debated (see the reviews by Easterly (2003) and Qian (2015)).[7] One set of studies posits that the modality of aid delivery and quality of recipients' institutions determine the benefits of aid, motivating political conditions attached to aid by Western donors (e.g., Burnside and Dollar (2000)).[8] A different set of studies argues that political or economic goals by donor countries drive foreign aid (see Morgenthau (1962), Werker (2012))[9], raising the concern that donor interests undermine the potential benefits of aid to the populations of recipient countries. Existing studies have typically stopped at showing country-level evidence consistent with economic or foreign policy goals driving aid allocation (e.g., Alesina and Dollar (2000), Kuziemko and Werker (2006)) and shown

[7] On the one hand, various studies have found that aid increases economic growth (Hansen and Tarp, 2000, Sachs, 2006, Galiani et al., 2017, Chauvet and Ehrhart, 2018). On the other hand, several studies have doubted such findings (Easterly, Levine and Roodman, 2004, Roodman et al., 2007, Rajan and Subramanian, 2008, Clemens et al., 2012). Many studies find negative effects of foreign aid on recipient countries, for example, corruption and elite capture (Svensson, 2000, Andersen, Johannesen and Rijkers, 2020), a decline in institutional quality (Djankov, Montalvo and Reynal-Querol, 2008, Ahmed et al., 2016), a decrease in export growth (Rajan and Subramanian, 2011), or conflict (Nunn and Qian, 2014, Crost, Felter and Johnston, 2014, Dube and Naidu, 2015).

[8] The modality of aid describes the way of delivering aid, including its fungibility, procurement, terms of finance and targeting of resources (e.g., project aid vs. technical assistance) (Ouattara and Strobl, 2008, Olken, Onishi and Wong, 2014, Deserranno and Qian, 2020). A different set of studies finds that aid is more effective in countries with higher quality of institutions and policies (Boone, 1996, Burnside and Dollar, 2000, Svensson, 2000, Kilby, 2009).

[9] Alesina and Dollar (2000) find that colonial past and political alliances are major determinants of foreign aid. Other goals by donors include foreign policy goals during the Cold War and the promotion of trade (McKinlay and Little, 1977, Schraeder, Hook and Taylor, 1998), garnering support in U.N. voting (Kuziemko and Werker, 2006) or influencing elections in developing countries (Faye and Niehaus, 2012). Dreher et al. (2018) examine the determinants of Chinese aid allocation, including foreign policy and economic goals.

that such aid is captured by elites or harms recipient populations (e.g., Werker, Ahmed and Cohen (2009), Nunn and Qian (2014)). Consistent with these studies, I find that domestic objectives drive a major donor's aid. However, I show that donor interests need not undermine the benefits of foreign aid to recipients. The scarcity of causal evidence of positive effects is due to the difficulty of developing credible identification strategies and the fact that the impact of aid will naturally vary across contexts. Thus, the finding that any bilateral, politically-motivated aid from a major donor can have a positive effect on the recipient country is an important contribution to the debate. My granular data allow me to exploit variation at more disaggregated levels than previous studies and to unpack the black box of China's aid allocation process, which I use to develop a credible identification strategy. In addition, I contribute to this literature by studying a prominent donor which has been understudied in the aid literature. Compared to OECD donors, much less is known about the determinants, allocation and effects of aid given by non-traditional donors like China.[10]

This paper also contributes to research on how governments use economic policy to address political needs. A vast and longstanding literature in economics speaks to governments' responses to domestic economic instability, including monetary policy (see, e.g., Blanchard and Galí (2010), Galí (2018)), fiscal policy (Blinder, Solow et al., 1973, Barro, 1979, Michaillat and Saez, 2019), trade policy (Melitz and Redding, 2014) and lending to other countries (Horn, Reinhart and Trebesch, 2020).[11] Recent studies find that government employment policies contribute to social stability by moderating domestic political conflict (Blattman and Annan, 2016, Fetzer, 2019, Wen, 2020).[12] My paper shows that foreign aid provides another policy tool for governments in need of securing domestic stability.

The paper proceeds as follows. Section 2 provides background on Chinese foreign aid and the underlying political process. Section 3 shows how China uses foreign aid projects to address domestic unrest. Section 4 examines how the Chinese government's political need to moderate unrest influences its aid allocation to other countries and quantifies the economic impacts of Chinese aid on recipients. Section 5 concludes.

[10] Recent studies have examined Chinese aid's extent (Horn, Reinhart and Trebesch, 2019), allocation within recipient countries (Dreher et al., 2019), motivations (Dreher et al., 2018) and impacts (Bluhm et al., 2018, Isaksson and Kotsadam, 2018, Dreher et al., 2021, Guo and Jiang, 2021).

[11] For China specifically, several recent studies show that the Chinese government responds to domestic economic and social instability by increasing domestic infrastructure investment and credit (see, e.g., Cong et al. (2019)), public employment and welfare payments (Wen, 2020), fiscal transfers and public security spending (Campante, Chor and Li, 2019), and censorship (Qin, Strömberg and Wu, 2017).

[12] Empirical and theoretical work shows that lower levels of income may lead to civil conflict through the opportunity cost channel (e.g., Collier and Hoeffler (1998), Fearon and Laitin (2003), Collier and Hoeffler (2004), Miguel, Satyanath and Sergenti (2004), Dube and Vargas (2013), Bazzi and Blattman (2014).

2. Background

2.1. Potential Determinants and Effects of China's Outward Foreign Aid

China transitioned from a planned economy to a socialist market economy in the 1990s, kicking off a period of export-led economic growth (Song, Storesletten and Zilibotti, 2011). At the same time, large state-owned enterprises (SOEs) in strategic sectors were brought under the direct control of the central government (Hsieh and Song, 2015). With the launch of the "Go Out" policy in the early 2000s, China encouraged its firms to expand internationally in search of new markets and opportunities. The Chinese government has viewed these reforms as crucial to maintaining high employment to secure domestic social and political stability, which it considers key policy goals (State Council, 2021).[13]

At the same time, China started rapidly increasing its foreign aid to developing countries, from less than 5 billion USD yearly in 2000 to almost 40 billion USD yearly in 2014, totaling over 350 billion USD (AidData, 2021). China's rise as a major donor has led to much speculation and some concern about its motives. While the Chinese government itself claims to provide foreign aid to "help recipient countries to strengthen their self-development capacity, enrich and improve their peoples' livelihood, and promote their economic growth and social progress" (State Council, 2011, pg.1), critics suspect less altruistic motives, such as providing opportunities for domestic firms and workers in line with the "Go Out" policy, with ambiguous effects on recipients.[14] Much of the debate stems from a lack of transparency of the Chinese government about the goals and allocation of its aid, as well as differences to Western aid (see Appendix A.1 for an overview):

First, Chinese aid is largely bilateral, raising concerns about China undermining Western donors' efforts (Naim, 2007). Furthermore, China does not impose political conditions related to domestic governance on recipients (State Council, 2011). On the one hand, such countries often have the most urgent need for aid since they are poor and have difficulty obtaining funds on international financial markets or from Western donors (Sun, 2014). On the other hand, this practice is viewed as problematic by Western observers since China provides aid to regimes with poor governance and widespread corruption. Critics argue that this may

[13] The Central Economic Work Conference formulates the State Council's policy goals, including securing domestic stability and advancing the international expansion of the Chinese economy. Central SOEs internalize these policy goals, as becomes evident from their public statements. For example, the 2009 annual report by Dongfeng Motor Group Co., the listed arm of one of the four largest automobile manufacturers and employers in China, states that: *The Central Economic Work Conference set the tone for the economy in 2010 to achieve "steady growth and adjustments". The main tasks include [...] promoting exports and foreign cooperation, and sparing no effort to maintain social stability.* (Dongfeng Motor Group Co. Ltd., 2010) In Section 3, I use systematic text analysis of firms' annual reports to show that central SOEs internalize the government's goal of maintaining domestic social stability.

[14] There may also be other, complementary motives for providing aid to other countries, such as promoting trade, political alliances and access to natural resources (Dreher et al., 2018). I examine such motives in Appendix C.5.

lead to elite capture and helps autocrats stay in power, with detrimental effects on recipient households (Naim, 2007).

Second, Chinese aid is much more often mixed with commercial elements than Western donors' aid: According to AidData (2021) estimates, the United States provided over 90% of its development finance in 2000–2014 ($394.6 billion in total) in the form of Official Development Assistance (ODA), including grants and concessional loans, whereas China provided 77% of its development finance during the same time ($354.3 billion in total) in the form of less concessional loans (OOF). Appendix A.2 provides details and definitions of different types of aid.[15] On the one hand, given that poverty is still widespread in many parts of China itself, handouts to other countries are politically infeasible. On the other hand, the more commercial nature of its aid raises the question whether China prioritizes recipients' interests or its own (Sun, 2014).

Third, China provides the vast majority of its aid in the form of hard infrastructure projects such as roads, pipelines and power plants. The Chinese government claims that this is based on recipient countries' needs and its own historical experience as a developing country (State Council, 2011). At the same time, Chinese aid is often conditional on the majority of goods, services and labor being sourced from China, as recently publicized contracts reveal (Gelpern et al., 2021).[16] On the one hand, the Chinese government claims to do so to "exploit its advantages in technology, equipment and materials, and human resources" to deliver high-quality projects at low cost in a sector in which it has comparative advantages (State Council, 2014, pg.7). Central SOEs in particular attained the expertise and scale to build large-scale infrastructure projects over the course of China's own growth path. On the other hand, skeptics argue that this practice is mainly driven by China's need to reduce domestic surpluses in labor and materials. Some worry that this practice leads to inflated and idle projects, and that the presence of Chinese firms negatively affects local labor markets and businesses (Zhao, 2014, Wegenast et al., 2019).[17]

[15] Chinese aid includes export credits and loans at commercial interest rates (e.g., LIBOR). Even though such instruments do not meet the strict definition of Western foreign aid, they constitute a subsidy from the recipient country's perspective since the donor (China) guarantees the debt and recipients would typically not be able to access credit at the same conditions on international financial markets. All findings in the paper hold when restricting the sample to aid that meets the OECD criteria for ODA. See the Data Section for further discussion.

[16] For example, a concessional loan agreement between the governments of China and Cameroon in 2011 for the construction of 1500 social housing units stipulates that: 85% of the financing must be used for the fulfillment of a commercial contract with *China Shenyang International Economic Technical Cooperation Corporation*; preferential visas must be granted for Chinese workers. See https://docs.aiddata.org/ad4/pdfs/how_china_lends/CMR_2011_172.pdf.

[17] A widely publicized and controversial example is Hambantota Port in Sri Lanka. The port, built by a Chinese state-owned company and financed through a loan by the Chinese government, remained largely unused and plunged Sri Lanka in a public debt crisis. See https://www.nytimes.com/2018/06/25/world/asia/china-sri-lanka-port.html.

2.2. Foreign Aid and Domestic Stability

Chinese central SOEs, which supply many Chinese foreign aid projects, are owned by the central *State-owned Assets Supervision and Administration Commission of the State Council* (SASAC). The SASAC was founded in 2003 to bring SOEs in sectors of national strategic importance under the control of the central government. These "national champions" have a dual role: they are profit-oriented and generate revenues for the government but also help it pursue national policy goals. In particular, they play an important role in securing social (political) stability by providing public employment during times of unrest (Lin, Cai and Li, 1998, Bai, Lu and Tao, 2006).[18]

However, hiring surplus workers is inefficient, especially since domestic demand may be lower during times of unrest. Other measures to address unrest such as fiscal transfers, domestic infrastructure construction or violent repression have other costs and decreasing returns.[19] Foreign aid offers a viable alternative: as explained earlier, aid projects generate acyclical demand for Chinese goods and labor, allowing firms to use their surplus labor. In addition, giving aid to other countries likely benefits China in other ways (e.g., promoting trade and political alliances (Dreher et al., 2018)) and is typically repayable by recipients. Hence, from the Chinese government's perspective, the long-term net return to giving aid may exceed or equal the net return to other, more direct measures aimed at securing domestic stability. Conceptually, it makes sense for the Chinese government to employ different measures to address unrest at the same time until their marginal returns are equalized. This motivates the hypothesis that the Chinese government, through its central SOEs (SASAC firms), uses foreign aid to generate employment for Chinese workers to address domestic unrest.[20]

[18] Social stability is particularly essential to the survival of autocratic regimes like China (Svolik, 2012, Gehlbach, Sonin and Svolik, 2016). Wen (2020) shows that, in contrast to private firms, Chinese state-owned firms increase employment in response to political unrest. By providing a wage income, workers' opportunity cost of protesting increases, decreasing protest participation (Becker, 1968, Dube and Vargas, 2013, Bazzi and Blattman, 2014).

[19] Direct transfers may be susceptible to fraud, seen as unfair and create moral hazard. Domestic infrastructure investment to stimulate employment may lead to unsustainable government debt levels and have decreasing returns, especially given that China already spent vast amounts for this purpose in reaction to the 2008/2009 financial crisis (Cong et al., 2019). Violent repression of unrest by the public security apparatus may further antagonize protesters and the state and increase the risk of uprising (Acemoglu, Ticchi and Vindigni, 2010).

[20] Domestic stability as a motive behind Chinese aid is also supported by compelling anecdotal evidence: For example, Copper (2016) writes on Beijing's reaction to the 2008/2009 financial crisis: "*The unemployment rate went up and China experienced greater economic and social instability. Cutting wages helped China adjust, but it also caused further worker unrest. The government approved more infrastructure projects (roads, railroads, etc.) but that still wasn't enough. One remedy was giving still more foreign assistance in the form of aid. In 2009, the idea was proposed that China launch a $500 billion foreign assistance program in the spirit of the Marshall Plan. It was said that financial aid would increase developing nations' purchases of Chinese goods, thus boosting China's export-oriented economy.*"

2.3. The Political Process Behind China's Aid

The political process underlying China's aid allocation is consistent with China using foreign aid to secure domestic stability. Unlike Western donors, where typically the Ministry of Foreign Affairs oversees foreign aid (Bräutigam, 2011*a*), the Ministry of Commerce (MOFCOM) manages China's aid program (Gu, Chen and Zhang, 2014).[21] In addition to planning and allocating foreign aid projects, MOFCOM's mandate includes formulating policy related to international trade and foreign direct investment, with the task of fostering domestic economic growth and employment stability (Zhang and Smith, 2017). It reports to the State Council, the highest organ of the Chinese central government, which formulates the general aid strategy and provides guidelines.

The *official* aid project selection process begins with recipient governments and Chinese embassies passing on aid project requests to MOFCOM, which then invites bids from a pre-approved list of contractors (Gu, Chen and Zhang, 2014). However, qualitative evidence based on interviews suggests that Chinese contractors also play a substantial role in the creation and allocation of aid projects (Zhang and Smith, 2017). The central SOEs in particular have become highly influential in developing countries since the launch of the "Go Out" initiative in the 2000s, building commercial and political relationships with the governments of the countries they operate in (not the least through foreign aid projects). In addition, the local MOFCOM offices are chronically understaffed and often rely on local subsidiaries of Chinese companies for practical matters. As a consequence, contractors often develop projects in recipient countries they are active in, which they then bring forward to MOFCOM. The company that initiates an aid project typically wins the contract if it is approved.[22] While financing agreements are made between the Chinese and recipient country governments, the Chinese contractor is typically paid directly, leading to a relatively unbureaucratic and fast process (Gallagher et al., 2018, Brautigam and Hwang, 2020). The contractor then delivers the project to the recipient country, using inputs from China. Figure A.2 provides a simplified summary of this process.

In sum, this discussion suggests several testable hypotheses. First, SASAC firms supply

[21] Other important government entities that extend development finance to other countries, especially commercial loans, include the Ministry of Finance, the two policy banks (China Export-Import Bank and China Development Bank), and the large state-owned commercial banks (Bank of China, Agricultural Bank of China, Industrial and Commercial Bank of China, and China Construction Bank). Appendix A.3 gives an overview on the – for the purposes of this paper – most important Chinese government entities and provides further details.

[22] Zhang and Smith (2017) state, based on interviews with Chinese policymakers and company representatives: "The tail of China's aid system often wags the dog, leading to an aid programme dominated by infrastructure projects backed by China Exim [Export-Import] Bank's concessional loan programme. [...] The projects are reverse-engineered to suit the political needs of local politicians and the commercial strategies of Chinese contractors" (pg. 2341).

more aid projects to increase employment in response to an increase in unrest in their home prefecture. Second, when there is an increase in unrest in their home prefecture, SASAC firms supply more aid projects in countries in which they typically implement aid projects. The first two hypotheses imply that China's political need to moderate domestic unrest influences the allocation of its aid to other countries. Third, this variation in China's aid allocation can be exploited to test the hypothesis that foreign aid can promote development even when it is driven by donor interests. Section 3 provides evidence for the first hypothesis. Section 4 provides evidence in favor of the second and third hypotheses.

3. Domestic Unrest and Aid Contract Allocation to Chinese Firms

3.1. Empirical Strategy

This section examines whether and how China uses outward aid to help maintain domestic stability. The main threats to identification are reverse causality as well as joint determination of aid and instability at the country level. For example, a global recession may simultaneously increase the overall level of social unrest in China and aggregate aid flows by China. To address these challenges, I exploit variation over space and time in threats to *local* stability within China and examine the internal allocation of aid projects to domestic firms. Specifically, I test whether local subsidiaries of central SOEs (SASAC firms) in China contract more aid projects to increase employment in response to local unrest. I estimate the following specification at the firm-year level:

$$\text{aid}_{f,t} = \sum_{s}^{S}(\text{unrest}_{p(f),t-s}\beta_s + X'_{f,t-s}\Gamma_s) + \sum_{s}^{S}(\text{unrest}_{p(f),t+s}\beta_s + X'_{f,t+s}\Gamma_s) + \alpha_f + \delta_t + \epsilon_{f,t}, \tag{1}$$

where $\text{aid}_{f,t}$ is the total number or financial value of aid contracts allocated to firm f in year t, and $\text{unrest}_{p(f),t}$ is the number of labor unrest events per million inhabitants in firm f's home prefecture p in year t. $X'_{f,t}$ denotes a vector of control variables at the firm-year or prefecture-year level, which I introduce in Subsection 3.5. α_f and δ_t denote the vectors of firm and year fixed effects (note that firm fixed effects absorb prefecture fixed effects). $\epsilon_{f,t}$ denotes standard errors, which are clustered at the prefecture level in the baseline specification.[23]

A great advantage of this approach is that it allows controlling for year fixed effects. The year fixed effects absorb time-varying variables at the country level. Their inclusion prevents potential omitted variable bias from unobserved joint determinants of the yearly levels of

[23] I cluster standard errors at the prefecture level to account for potential within-prefecture correlation in both unrest and aid over time as well as firms. In robustness checks, I report other standard errors.

total Chinese aid and unrest, including macroeconomic shocks such as a global recession.[24] The firm fixed effects absorb time-invariant firm and prefecture characteristics. Omitted variables that are correlated with both the typical amount of aid contracts allocated to a firm and the number of unrest events in its home prefecture would bias the estimates of β_s. For example, firms in manufacturing hubs may generally experience more labor unrest but also contract more aid projects, biasing β_s upwards. The firm fixed effects prevent such bias.

Given that firms should respond to local unrest rapidly and the aid allocation process is fast, I expect only unrest lagged by one calendar year to have an effect on aid allocation.[25] Hence, the coefficient of interest is β_{-1}: If $\beta_{-1} > 0$, SASAC firms – which implement the government's policy goals – contract more aid projects the year after an increase in local unrest intensity. Nevertheless, I also control for several other leads and lags of unrest to deal with potential reverse causality in the presence of serial correlation.[26] Conditional on firm and year fixed effects, as well as other leads and lags of unrest, lagged local unrest is plausibly unrelated to recipient countries' demand for aid. Hence, $\beta_{-1} > 0$ can be interpreted as evidence for the Chinese government internally allocating foreign aid projects in response to domestic unrest.

The main remaining threats to this interpretation are firm- or prefecture-specific, time-varying shocks that are both correlated with local labor unrest and affect local aid allocation through other channels. For example, a decrease in local demand may decrease local marginal wages, leading to worker unrest. At the same time, lower wages decrease firms' costs, allowing them to bid more aggressively on aid projects.[27] Although the allocation of foreign aid projects would also have a stabilizing effect on the Chinese economy through this channel, it would have a different interpretation than the Chinese government allocating aid projects specifically to address local unrest. I address such alternative interpretations in several ways.

First, I control directly for potential time-varying covariates of unrest and aid allocation at the prefecture- and firm-level. Second, I conduct a placebo check using non-SASAC firms (including purely profit-maximizing private firms), which are also licensed to supply foreign

[24] Existing studies on the drivers of foreign aid from the donor's perspective typically examine macroeconomic push factors. For example, Dreher et al. (2021) use the total yearly availability of construction materials and foreign exchange reserves in China to predict total yearly Chinese aid. This relationship does not have a causal interpretation since there may be other joint determinants of the yearly levels of Chinese aid and these factors, including global macroeconomic shocks.

[25] Unrest should have no effect on aid allocation in the same year since the aid budget is determined in the previous calendar year. In addition, it typically takes several months for aid projects to be drawn up and allocated.

[26] For example, unrest at t-2 may lead to aid allocation at t-1, which in turn decreases unrest at t-1. If aid at t-1 and at t were serially correlated, this would complicate the interpretation of the coefficient of unrest at t-1 on aid at t if unrest at t-2 is not controlled for. Similarly if unrest at t-2 directly affected aid at t and were serially correlated with unrest at t-1.

[27] For example, Campante, Chor and Li (2019) show that negative export shocks lead to labor unrest in China. Almunia et al. (forthcoming) show that a drop in domestic demand increases firm exports through price changes.

aid projects but have no direct incentives to provide employment specifically to address local unrest. We should only see these firms contracting aid projects in response to local unrest if there are unobserved local economic shocks correlated with both local unrest and aid allocation. If we see no effect for such firms, the effect of local unrest on aid allocation to SASAC firms is due to political, rather than purely economic motives. Third, I show corroborating evidence for the proposed mechanism by analyzing firm statements using systematic text analysis, examining effects on employment, and investigating the role of local governments. Finally, a battery of robustness checks addresses other potential econometric concerns.

3.2. Data and Descriptive Statistics

Project-level aid data. I build a comprehensive project-level dataset on China's outward foreign aid in 2005–2015 by combining information from multiple unofficial databases. Appendix B.1 describes the data sources, their methodologies and how I combine them. The dataset includes all known bilateral finance by a Chinese government entity to a government entity of a developing country,[28] including ODA-like finance (Official Development Assistance) and OOF-like finance (Other Official Finance). This includes bilateral grants, in-kind donations, zero-interest loans, concessional loans, loans at commercial rates, buyer credits and export credits. I refer to any of these kinds of official finance as *aid*.[29] All findings in the paper hold when restricting the sample to ODA-like aid only. However, since the financing conditions are unknown for a significant share of projects and the Chinese government adopts a broader definition of aid, the main sample does not impose this restriction. See Appendix A.2 for details.

The data include the year of commitment,[30] financial value, recipient country, type of finance, sector, funding agency within the Chinese government, and a short description for each project. In addition, I identify the names of Chinese contractors implementing each project by manually searching the source documents provided by the databases underlying the dataset. The majority of projects in the sample are supplied by Chinese construction

[28] Developing countries include non-high income, non-OECD countries as classified by the World Bank.

[29] In contrast to most Western donor countries, China is not in the OECD-Development Assistance Committee and does not distinguish between ODA and other forms of official finance (Bräutigam, 2011*a*). The latter includes export credits and loans at commercial interest rates (e.g., LIBOR). Even though such instruments do not meet the strict definition of Western foreign aid, they constitute a subsidy from the recipient country's perspective since China guarantees the debt and recipients would typically not be able to access credit at the same conditions on international financial markets. This is in line with other scholars studying Chinese aid (e.g., Copper (2016)). Note that a considerable fraction of US "aid" is also in the form of loans (Bräutigam, 2011*a*).

[30] This is standard in the literature (e.g., Dreher et al. (2021)). Information about the year of disbursement is unavailable for a large share of projects in the data.

companies. I exclude projects not involving any Chinese contractor from the sample.[31] The resulting dataset includes 1,034 projects in 102 countries committed between 2005 and 2015. The average project is worth $276 million (real 2014 USD). Tables A.6 and A.5 list the 10 largest ODA- and OOF-like projects and their contractors.

As Figure 1 shows, Chinese aid has grown from less than $5 billion yearly in 2005 to almost $30 billion yearly in 2015 (excluding aid not supplied by Chinese firms), now matching US and exceeding World Bank finance in scale. Chinese aid is significantly more volatile than US and World Bank finance. Figure 2 shows that Chinese contractors supply aid projects all over the world. The largest share of aid projects goes to Africa (45%) and Asia (35%), with the remaining projects going to Latin America (11%), the Middle East (5%) and Eastern Europe (4%). Angola, Ethiopia and Pakistan are the largest recipients. The majority of aid is in the energy and transport sectors (Figure A.3). Nearly all aid projects implemented by Chinese firms are in the form of hard infrastructure, including pipelines, power plants, transmission lines, railroads, highways, ports, government buildings, sports stadiums, telecommunication networks, schools and hospitals. The Export-Import Bank of China funds the majority of aid projects. Figures A.4 to A.11 provide a detailed breakdown of Chinese aid by type of finance and year, sector, funding agency as well as geographical region.

Firm-level data. There exist no comprehensive, granular data about which firms implement which Chinese aid projects. While some of the existing databases provide names of contractors implementing a project, the names are not harmonized and typically only identify the firm group (conglomerate) rather than the subsidiary that actually implements the project. This has so far prevented researchers from linking data on aid projects to firms and conducting systematic analysis at the firm level. In addition, to study the allocation of aid projects at the firm level, not only the *actual* contractors of projects but also the pool of *potential* contractors are of interest.

To address these challenges, I construct a firm-year level dataset by 1.) identifying the set of potential aid contractors for each year at the subsidiary level from archival administrative records, 2.) linking contractors to administrative datasets such as firm-level customs and tax data, and 3.) linking contractors to aid projects. Appendix B.2 provides details on the linking and the data. The result is an unbalanced yearly panel of the universe of potential domestic aid contractors. The main variables of interest are the number and financial value of aid contracts supplied by a firm in a year. I calculate the financial value of a contract assigned to a firm in a year as the financial value of the entire project committed in that

[31] While I do not systematically collect information about projects not involving Chinese contractors, a comparison using *AidData's Global Chinese Official Finance Dataset* indicates that Chinese firms implement around 80% of the financial value of Chinese aid projects. Aid projects not in the sample are typically small and are supplied by foreign firms or not supplied by any known firm (including in-kind donations, stipends and debt relief).

year, divided by the number of Chinese firms involved in the project.[32]

The panel includes 1,290 firms of which 496 are administered by the central SASAC. Of those, 122 SASAC firms and 103 other firms ever supply Chinese aid projects during the sample period I study. Table A.7 provides descriptive statistics. The average SASAC firm (non-SASAC firm) is awarded 0.15 (0.05) aid contracts or contracts worth $46 million ($3 million) per year.[33] On average, SASAC (non-SASAC) firms have 1,408 (1,152) employees, $100 million ($56 million) fixed assets, $540 million ($313 million) yearly operating income, and $128 million ($81 million) exports. The firms in the sample are located in 150 different prefectures spread across China (see Figures A.12 and A.13).

Unrest data. I use labor unrest as a proxy for domestic social instability, in line with the literature on the political economy of China (see, e.g., Qin, Strömberg and Wu (2017), Campante, Chor and Li (2019)). The Chinese government sees labor unrest events as potential threats to national stability as they may spread across the country and ultimately threaten the Chinese Communist Party's legitimacy as the country's *de facto* leader if left unchecked (Gerschewski, 2013, Qin, Strömberg and Wu, 2019). Despite the autocratic nature of the regime, labor strikes and other collective action by workers are common in China and often involve protests against the government.[34]

There are no disaggregated data about strikes or worker protests from official sources in China. I address this challenge by combining data on labor unrest across China from two unofficial sources: *China Strikes*, collected by a researcher at the University of British Columbia covering 2004–2011,[35] and the *China Labour Bulletin* (CLB), a non-profit organization based in Hong Kong, covering 2012-2014.[36] These data are crowd-sourced from worker reports as well as traditional media and online sources, including social media. The data have been used to study trends in worker actions within China by foreign media and renowned economists such as Campante, Chor and Li (2019) and Qin, Strömberg and Wu (2019). They contain detailed information about each unrest event, including the date, location (prefecture), sector and a short description. Appendix B.5 provides examples of unrest events in the sample and further details on the data. I use the yearly number of strike events per million inhabitants in a prefecture as the main measure of local unrest.

[32] I do not observe the confidential details of the actual contracts underlying the projects, such as the exact payment made to each firm or subcontracting.

[33] Note that since many firms get no contracts in some years or no contracts ever, conditional on getting at least one contract, the value of yearly contracts awarded to a SASAC firm (non-SASAC firm) is $585 million ($125 million).

[34] See https://clb.org.hk/content/what-you-need-know-about-workers-china for background information about labor relations and workers' rights in China. See Wright (2019) for a review of scholarly on unrest in China.

[35] https://chinastrikes.crowdmap.com/

[36] https://maps.clb.org.hk/

There are 7,646 recorded unrest events during the period I study. Figure 3 shows a choropleth map of the average yearly number of unrest events per one million inhabitants for each prefecture. Figure A.15 plots all unrest events across China in 2004–2014. Unrest events take place in almost all of China's densely populated regions and are concentrated in manufacturing hubs such as the Pearl River Delta. Figure A.14 points to an increase in the number of reported unrest events over time. The majority of protests took place in the manufacturing (30%), construction (29%), public transport (20%) and service (11%) sectors.

One potential concern with the unrest data is reporting bias. Both China Strikes and CLB acknowledge that they cannot record all unrest events and reporting may differ across prefectures. In addition, the increasing availability of the internet and other media may have led to more reporting of unrest events over time. For the purposes of this paper, such concerns are likely not major, for several reasons: First, I include firm / prefecture and year fixed effects in the analysis to deal with classical measurement error. Second, selective reporting would only lead to bias in the direction of my results if it were systematically correlated with the allocation of foreign aid projects, which is unlikely. Third, as Campante, Chor and Li (2019) show, the CLB data are highly correlated with official records on the number of labor dispute cases submitted for mediation or arbitration, as reported in the China Labor Statistical Yearbooks published by the Ministry of Human Resources and Social Security (MOHRSS).

Other micro data. I use micro data from several other sources to construct variables for complementary analyses and robustness checks, including firms' political connections and prefecture-level characteristics. I describe these variables in Appendix B.6. Table A.8 shows prefecture-level descriptive statistics.

3.3. Results

Figure 4 shows the effects of local unrest on the number of aid contracts allocated to a SASAC firm, conditional on firm and year fixed effects. As expected, the figure shows that aid is allocated in response to local unrest lagged by one calendar year, but not to unrest at other leads and lags. This specification is restrictive as it limits the sample to a subsample of years. To exploit the full data from all years and to maximize statistical power, I henceforth keep only unrest lagged by one year in the specification.[37] The estimated coefficient on unrest lagged by one year is unaffected by whether other leads and lags are included or not, as Table A.12 shows.

[37] Results including all leads and lags for other specifications are available on request.

Table 1 Column (1) shows the first main result: Conditional on firm and year fixed effects, one additional lagged unrest event per million inhabitants in a prefecture leads to 0.103 more foreign aid contracts allocated to each eligible local SASAC firm on average. A one standard deviation increase in unrest increases the number of contracts allocated to SASAC firms by 22% of its mean.[38] Cumulatively, this is a large effect: For example, a back-of-the-envelope calculation shows that a one standard deviation increase in yearly unrest in Beijing leads to the additional allocation of 5.5 aid contracts with a total value of $1.7 billion to SASAC firms in the prefecture, which is as much as around 40% of the amount Beijing spends on public security in a year (Zenz, 2018).[39]

Columns (2) and (3) of Table 1 repeat this analysis for ODA-like (more concessional) and OOF-like (less concessional) finance only. The effects are similar for both kinds of aid. Column (4) uses the log of one plus the total financial value of contracts allocated to a firm in a year as the outcome variable.[40] A one standard deviation increase in unrest (0.331 events per million inhabitants) is estimated to increase the value of contracts by 0.21 log points (24%). Again, the estimates for ODA-like and OOF-like projects are qualitatively similar and not statistically distinguishable from each other. All coefficients in Table 1 are statistically significant at the 1% or 5% level, except Column (5) which is statistically significant at the 10% level. Figures A.16 and A.17 show binned scatter plots for the corresponding results.

Tables A.14 and A.15 control for a host of prefecture-year level variables that are potentially correlated with local labor unrest and aid allocation, including lagged local GDP per capita, exports per capita, employment, wages and population, as well as province-year fixed effects and unrest in neighboring prefectures. A.16 controls for firm-year level variables, including firms' lagged number of employees, fixed assets, operating income, exports and materials inventory. The rationales for controlling for these variables and more details are provided in Appendix D.2. The coefficient of interest is highly robust to the inclusion of these controls.

As a placebo check, Table 2 replicates the main table for non-SASAC firms (firms not administered by the central government, including private firms) in the sample. Although these firms are licensed to bid on aid contracts,[41] there is no economically or statistically significant relationship between local unrest and contract allocation for any measure of aid. This is consistent with political motives of the central government, rather than economic

[38] A one standard deviation in unrest is 0.33 events per million inhabitants. The mean number of aid contracts allocated to a local SASAC firm in a year is 0.154. $0.330 \cdot 0.103 \div 0.154 = 0.221$.

[39] The average aid contract supplied by a SASAC firm in Beijing is worth $304 million. There are 161 SASAC firms in Beijing. $0.330 \cdot 0.103 \cdot 161 \cdot 0.304 = 1.7$.

[40] Estimates for the inverse hyperbolic sine instead of the log function are very similar (results available on request).

[41] 65% of aid contracts in the sample are awarded to SASAC firms and 35% to other firms.

factors, driving aid allocation in response to unrest.[42] A potential concern with this analysis is that SASAC firms differ from other firms in characteristics other than their political role, which could explain the difference in the effect of unrest on aid allocation for the two types of firms. Appendix D.3 addresses this concern. The results are robust to several alternative explanations.

3.4. Corroborating Evidence

The results presented so far are consistent with SASAC firms supplying more foreign aid projects to increase employment following an increase in the intensity of local unrest. I now provide corroborating evidence for this mechanism.

Unrest by sector. Table 3 shows that only labor unrest in industrial sectors (construction, manufacturing, mining), which supply the majority of Chinese aid projects, has a large and statistically significant effect on contract allocation to SASAC firms. Reassuringly, unrest in service sectors (mainly strikes by school teachers and taxi drivers) has no significant effect on contract allocation.

Political motives. I have argued that SASAC firms, but not other firms, actively help the government maintain social stability. I systematically analyze the text of firm statements to provide support for this argument. I proceed as follows: For each firm and year, I count how frequently each of several keywords related to maintaining social stability are mentioned in the firm's annual report relative to the total word count in the report.[43] I then re-estimate the baseline Equation 1 using the keyword count as the outcome variable. To construct an outcome variable with a simple interpretation, I summarize the counts of the keywords by taking their first principal component and, alternatively, the sum of all keyword counts.[44]

Figure 5 shows the results. Each red dot is the standardized coefficient from a regression of the frequency of the term listed on the left hand side of the figure on lagged local unrest, controlling for firm and year fixed effects, for SASAC firms. The gray dots are the standardized coefficients for the same exercise for other firms. The figure shows that SASAC firms, but not other firms, mention keywords related to maintaining social stability significantly more frequently in response to an increase in local unrest. One potential concern

[42] Figures A.18 and A.19 show binned scatter plots of the corresponding results for the number and total financial value of contracts allocated to non-SASAC firms. Figure **??** shows the effect of different leads and lags on the number of aid contracts allocated to non-SASAC firms. As expected, none of the coefficients are economically or statistically significant.

[43] I use the keywords from Campante, Chor and Li (2019) to avoid cherry-picking. I exclude keywords that appear less than a total of 10 times over all firms and years.

[44] Annual reports are available for the subset of firms in my sample that are publicly listed. For unlisted firms that are part of larger conglomerates, I use the annual reports of listed firms in the same conglomerate and prefecture. Details on data construction are available on request. Since this likely leads to correlated measures across firms within conglomerates, I two-way cluster standard errors at the prefecture and conglomerate level.

with this exercise is that SASAC firms are more likely to use language that is more similar to the language the government uses in general (specifically following local unrest). To address this concern, I repeat the analysis above using a set of placebo keywords typically used in government speeches but not in firms' annual reports. Figure A.20 shows that the coefficients for these keywords are small and insignificant for both types of firms.

Employment. Table 4 examines whether firms actually increase employment in response to local labor unrest. Since the sample includes only observations with information on employment from the tax survey data, Columns (1) and (2) for comparison replicate the main specification for this subsample. As in Tables 1 and 2, SASAC but not other firms contract aid projects in response to local unrest. Columns (3) and (4) show the effect of unrest on a firm's number of employees (in logs). Consistent with the proposed mechanism, unrest increases employment at SASAC but not other firms: one additional labor unrest per million inhabitants in the prefecture increases SASAC employment by around 3%. In contrast, unrest has a negative effect on employment by non-SASAC firms (although the coefficient is statistically insignificant at conventional levels).

The role of local governments. One tool available to governments to secure stability is fiscal policy. For example, local governments in China may use public expenditure to shore up public support and prevent further unrest.[45] However, this is costly and may have decreasing returns. Hence, when existing public expenditure becomes too high, contracting aid projects becomes comparably more attractive to secure stability on the margin. I test this mechanism by estimating heterogeneous effects of unrest on aid allocation depending on whether the local prefecture government has constraints on fiscal policy. To measure such constraints, I construct a dummy variable that equals 1 (0) if the prefecture government's lagged public income / expenditure ratio is below (above) the sample mean.[46] I then interact local unrest with this dummy. Table 5 shows the results: consistent with the proposed mechanism, unrest has a significantly higher effect on aid allocation if the local government is constrained in its ability to stimulate demand directly using fiscal policy. The interaction coefficient is statistically significant at the 5% level.

3.5. Alternative Specifications and Other Robustness

Appendix D.4 discusses a number of robustness checks. First, I estimate the effects of unrest on aid allocation and employment at the prefecture- instead of firm-level to show that the

[45] For example, in response to the global Great Recession in 2008/2009, the central government encouraged local governments to massively expand their balance sheets to encourage domestic infrastructure construction. As a consequence, local governments increased expenditures and indebted themselves to record levels, raising questions about their ability to stimulate demand using fiscal policy in the future (Copper, 2016, Cong et al., 2019). Local governments in China are responsible for 85% of general government budgetary spending (Wingender, 2018).

[46] Using data from the China City Statistical Yearbooks.

results are driven by a re-allocation of aid projects across prefectures rather than across firms within a prefecture. Second, I repeat the main analysis using a non-linear specification to address potential concerns related to functional form. Third, I show that the results are robust to excluding specific observations, including outliers, prefectures and time periods. Finally, I report alternative standard errors.

4. The Effects of Chinese Infrastructure Aid on Recipient Countries

The previous section showed that the allocation of aid projects to Chinese contractors is influenced by the Chinese government's political need to address domestic unrest. Motivated by this evidence, I now examine whether such aid can promote economic development in recipient countries.

4.1. Empirical Strategy

The main challenges for estimating the causal effects of aid on recipients are the issues of reverse causality and joint determination. In this subsection, I motivate and describe my empirical strategy to address these challenges.

To help understand the identification challenge, first consider the simple case where country i's outcome is regressed on the level of aid received by China s years prior:

$$\mathrm{Y}_{i,t+s} \;=\; \beta\, \mathrm{aid}_{i,t} \;+\; \alpha_i \;+\; \delta_t \;+\; \epsilon_{i,t}, \tag{2}$$

where $\mathrm{Y}_{i,t+s}$ is an outcome of country i in year $t+s$ (e.g., GDP per capita) and $\mathrm{aid}_{i,t}$ is the number of Chinese aid projects received by country i in year t. α_i and δ_t denote the vectors of country and year fixed effects. The sample includes a panel of 137 non-OECD, non-high income countries between 2003 and 2018.[47]

The coefficient of interest, β, is the effect of an additional Chinese aid project on the recipient country outcome s years later. However, the coefficient will capture reverse causal effects if China allocates aid based on recipient outcomes. For example, if China allocates more aid to countries on a negative growth path, then the estimate $\hat{\beta}$ of this OLS regression would be biased downwards. Alternatively, aid and the outcome in the recipient country could be jointly determined by a third factor, such as a change in the political regime of the recipient country, which could bias $\hat{\beta}$ in either direction. To address these difficulties, I construct an

[47] Table A.24 shows the set of developing countries in the sample (non-high income, non-OECD countries, as defined by the World Bank). Due to inconsistencies in the data, I exclude from the sample mini-states, Libya, as well as the Democratic Republic of the Congo and Republic of Congo. The latter is necessary since the two states seem to be often confused in the primary sources underlying my dataset, leading to implausible values.

instrumental variable for the endogenous measure of Chinese aid. The instrument exploits the time and spatial variation in labor unrest in Chinese prefectures, in combination with time-invariant variation in connections between recipient countries and SASAC firms in these prefectures. The logic is as follows. Each country differs in its propensity to receive aid from SASAC firms in a specific Chinese prefecture. As explained in the Background section, SASAC firms are more likely to supply aid projects in countries with which they are politically or economically connected (e.g., because of other aid projects). When there is labor unrest in a prefecture, the Chinese central government will allocate more aid contracts to SASAC firms in that prefecture to boost employment there. The firms are more likely to direct the aid to countries with which they are connected than to countries with which they are unconnected. Causal identification assumes that the decision to allocate aid to firms in a given Chinese prefecture, when there is local labor unrest in the prefecture, is driven by the desire to quell unrest and not driven by the desire to direct aid to connected countries. Similarly, I assume that domestic labor unrest in a prefecture is unrelated to economic outcomes in connected countries (relative to country and year fixed effects). I discuss potential threats to identification and tests to validate the identifying assumptions further below. The second and first stages of the 2SLS specification are:

$$\mathrm{Y}_{i,t+s} \;=\; \beta\, \widehat{\mathrm{aid}}_{i,t} \;+\; \mathbf{X}_{i,t}\Gamma \;+\; \alpha_i \;+\; \delta_t \;+\; \epsilon_{i,t} \tag{3}$$

$$\mathrm{aid}_{i,t} \;=\; \gamma\, \mathrm{Z}_{i,t-1} \;+\; \mathbf{X}_{i,t}\Theta \;+\; \alpha_i \;+\; \delta_t \;+\; \mu_{i,t}, \tag{4}$$

where the instrument $\mathrm{Z}_{i,t-1} = \sum_p(\omega_{i,p} \cdot \mathrm{unrest}^*_{p,t-1})$ is the sum of lagged local unrest shocks in Chinese prefectures in year *t-1*, multiplied by country i's propensity of receiving aid projects by SASAC firms in each of the prefectures ("weighted unrest"). I calculate $\omega_{i,p}$ as the time-invariant fraction of years between 2005 and 2015 that country i receives any aid by SASAC firms in prefecture p (formally, $\omega_{i,p} = \sum_{t=2005}^{T=2015} \frac{\mathbb{1}[aid_{i,p,t}>0]}{11}$). $\mathrm{unrest}^*_{p,t-1}$ denotes the lagged number of unrest events per million inhabitants in prefecture p in year *t-1*, relative to average unrest across all prefectures in year *t-1*. $\mathbf{X}_{i,t}$ is a vector of country-year level control variables, which includes recipient country population in the baseline specification and other variables which I introduce in the Robustness section. All other variables are as before. The baseline specification clusters standard errors at the country level.

The empirical strategy exploits the country and time variation in the receipt of Chinese aid caused by the timing and spatial variation in local unrest in Chinese prefectures to estimate causal effects of Chinese aid on recipients. The specifications control for year fixed effects that capture changes over time that affect all countries similarly, as well as country fixed effects that capture time-invariant differences across countries, including countries' cross-sectional

propensities to receive aid from Chinese prefectures, $\sum_p \omega_{i,p}$.[48] The cross-sectional variation in aid flows to specific countries could be correlated with those countries' outcomes, and changes in aggregate Chinese aid flows over time may be correlated with global economic trends. However, conditional on country and year fixed effects, the timing of prefecture-level unrest shocks is exogenous to recipient countries. Hence, the interaction between shocks to local unrest in China and recipient countries' exposure to this variation generates a valid IV for the amount of Chinese aid received by a country in a year. The IV is exogenous if the unrest shocks are exogenous conditional on controls.

The exclusion restriction requires that, conditional on the controls, weighted unrest among Chinese prefectures only affects recipient countries through the provision of aid. The main concern is that variation in local unrest in Chinese prefectures is correlated with other factors that differentially affect outcomes of frequent recipients of aid from specific prefectures. A first concern is that an increase in local unrest in a prefecture is correlated with the economic conditions of countries that receive aid more frequently from that prefecture. For example, if a prefecture-specific, negative export demand shock is associated with an increase in unrest in a prefecture and simultaneously with a decrease in GDP of countries connected to this prefecture, $\hat{\beta}$ is biased downward. In this case, I would underestimate the true effect of Chinese aid on recipient country GDP. To address this concern, I control for lagged weighted exports per capita in Chinese prefectures connected to a country. A second concern is that local unrest in a prefecture leads not only to the allocation of aid projects but also to an increase in aid-unrelated Chinese exports or FDI to countries connected to the prefecture. I address such concerns by controlling for Chinese exports and FDI received by each country. In addition, I conduct a number of other robustness and falsification tests, which I describe further below.

For interpreting the 2SLS estimates, note that they reflect the average effect for observations that comply with the instrument, i.e., a local average treatment effect (LATE) (Angrist, Imbens and Rubin, 1996). In my setting, compliers are observations that receive more aid because of an increase in unrest in China. This is precisely the effect of interest to address the question how aid driven by domestic political motives affects recipients.

[48] Note that the year fixed effects do not capture the differential effect of changes in average unrest across China over time on countries that receive aid frequently compared to countries that receive aid infrequently. There are two potential issues. First, the measured increase in average unrest across all prefectures over time may not reflect an actual increase in average unrest over time since reporting of unrest may have increased across all prefectures over time. This weakens the predictive power of the instrument in the first stage since the instrument would predict frequent aid recipients to receive more aid over time, while they should in reality not get more aid only because reporting of unrest increased over time. Second, if the outcomes of frequent and infrequent recipients of aid followed different time trends, differential trends in outcomes may be spuriously correlated with a trend in measured unrest over time stemming from increased average reporting of unrest over time. Demeaning yearly unrest in each prefecture with yearly average unrest across all prefectures addresses these issues by removing shifts in average unrest across all prefectures over time.

4.2. Data

I calculate the total number of aid projects and financial amounts received by each country in a year using the project-level data described in Section 3.2. I use data from the World Development Indicators (World Bank, 2020) to measure various recipient country-level outcomes and controls, including GDP per capita growth and levels as well as its components (including capital formation, government consumption, household consumption, exports and imports). Data on employment come from the ILOSTAT database (International Labour Organization, 2020). Country-level trade data is from the Atlas of Economic Complexity (The Growth Lab at Harvard University, 2019). I also calculate the number of official diplomatic visits by China for each country using data from China Vitae.[49] I winsorize the outcome variables at the 5th and 95th percentiles to limit the influence of extreme outliers. Results without winsorizing are similar (available on request). For robustness checks, I use data on bilateral Chinese foreign direct investment (FDI) from the Chinese Ministry of Commerce and the Global Investment Tracker by the American Enterprise Institute (American Enterprise Institute, 2019). In addition, I construct measures of bilateral trade at the country-prefecture-year level using the Chinese customs data described in Section 3.2. Descriptive statistics for all country-level variables are provided in Table A.9. Appendix C.5 shows cross-sectional correlations between recipient country characteristics and the amount of Chinese aid received to examine potential factors related to China's global aid allocation.

4.3. First Stage Estimates

Figure 6 shows that there is a strong positive correlation between the instrument and aid receipt. A one standard deviation increase in lagged weighted unrest increases the number of aid projects (supplied by SASAC firms) received by a country by 0.25 on average (25% of a standard deviation or 77% of the sample mean), conditional on country and year fixed effects. The effect is statistically significant at the 1% level. The Kleibergen-Paap F-statistic is comfortably above the common rule of thumb of $F > 10$ for the relevance assumption to be satisfied. In other words, a country gets significantly more aid following years during which the Chinese prefectures it regularly receives aid from experience more local unrest, conditional on country and year fixed effects. Multiplied by the number of countries, the estimates mean that a one standard deviation in the instrument leads to the re-allocation of around 34 aid projects worth $9.5 billion in total. Taken together, this implies that the

[49] China Vitae is a US NGO "committed to raise the quality and quantity of English language, biographical information on China's top leadership; and to create a centralized repository of such information available to a worldwide audience." URL: https://www.chinavitae.com/index.php

Chinese government's political need to moderate domestic unrest drives a significant part of China's global aid allocation.[50]

Table A.25 shows the first stage for other measures of aid. The relationship between the instrument and aid is large and statistically significant at the 1% or 5% level for all measures of aid, although the F-statistic is somewhat smaller for measures of aid other than the total number of contracts. The number of aid projects is my preferred measure as it has fewer missings and is less likely to suffer from measurement error than the other measures. I use the total number of contracts as the baseline measure of aid for the results I describe below.

4.4. 2SLS Estimates

Tables 6 and 7 show the main results. In both tables, Panel A shows the IV estimates of the causal effect of an additional Chinese aid project on the outcome variable indicated in the column header three years after commitment, with aid instrumented by lagged weighted labor unrest. The average time it takes for Chinese infrastructure aid projects to be completed after commitment is 2-3 years (for the subsample of the projects in my data for which this information is available). Tables A.28 and A.29 show the IV estimates for other leads and lags.

Columns (1) and (2) of Panel A in Table 6 show the IV estimates of the effect of Chinese aid on GDP per capita in levels and growth, conditional on country and year fixed effects. One additional Chinese aid project on average increases recipient country GDP per capita in levels by around $123 (2.5% of the sample mean) and GDP growth by 0.5 percentage points three years after commitment. The coefficients are statistically significant at the 5% and 10% levels respectively. These estimates are large but very similar in magnitude to recent estimates of the effect of arguably economically motivated Chinese aid by Dreher et al. (2021). Recall that the aid projects in my sample are in the form of large-scale infrastructure, including railroads, pipelines and ports. While these projects may be relatively small for China, they tend to be sizeable relative to recipient countries' economies, and potentially have large returns given the infrastructure gap in many developing countries (G20, 2021). Consistent with large-scale infrastructure construction driving the results, I find sizeable and statistically significant effects on capital formation and government consumption as well, as Table 7 Columns (2) and (3) show.[51] Table 7 Columns (5) and (6) show effects on imports and exports of the recipient country, although these coefficients are imprecisely estimated.

[50] The within-R squared of the first stage offers a different way of assessing the importance of this motive. Residual variation in weighted unrest explains 4.5% of the variation in aid.

[51] 7 includes a smaller sample than 6 due to missings in the data. For comparison, I report the effect on GDP per capita for this smaller sample in Column (1) of Table 7.

Panel B shows reduced form estimates which equal IV estimates multiplied by the first stage. Panel C shows OLS estimates, which I do not interpret as causal. The un-instrumented effects of aid on levels and growth of GDP are considerably smaller than the IV estimates. This difference could be explained by China allocating more aid to countries on a lower growth path (e.g., because such countries have higher demand for aid), or because aid projects allocated in response to domestic unrest are larger or have a higher impact on recipient country income.

As Table 6 Column (4) shows, aid driven by China's domestic political motive also intensifies diplomatic relations between China and the recipient country. Three years after the commitment of one additional project, recipient countries on average get one additional official visit by Chinese top political leaders. That a domestic policy goal can potentially influence foreign relations through the mechanism described in this paper is an interesting finding which deserves further study in future research.

The increases in GDP and benefits to governments do not necessarily translate into an improvement of the economic well-being of the recipient population *ex ante*. Part of the increases may account for the direct value of the infrastructure projects. One may also be concerned that the economic benefits of the projects are captured by political elites (e.g., Werker, Ahmed and Cohen (2009), Dreher et al. (2019)) or hurt the local population via negative spillovers on local labor markets (e.g., Zhao (2014), Wegenast et al. (2019)), especially since the aid discussed here is driven by the interests of the Chinese government and firms. However, such evidence for Chinese aid is largely anecdotal.

To investigate this possibility, I examine variables related to household income. I find large positive and statistically significant effects on household consumption: As Table 7 Column (4) shows, an additional aid project increases household consumption by 68 dollars per capita on average (2.2% of the sample mean) three years after commitment. One potential channel is employment. As Table 6 Column (3) shows, an additional aid project decreases the recipient country's unemployment rate by 0.38 percentage points (5% of the sample mean). This finding is consistent with a recent study that shows positive short- and medium-term effects of Chinese infrastructure projects on local employment in Africa using difference-in-difference analysis (Guo and Jiang, 2021). As they argue, Chinese infrastructure increases employment both directly for low-skill workers in the short run (e.g., for back-work during project construction), as well as indirectly for skilled labor in adjacent industries in the medium and long run (after the projects are completed). While my country-level data do not allow me to directly examine such mechanisms, my results imply that the Chinese government's domestic policy goals do not undermine the benefits of its aid to recipient populations. This is an important finding.

Nevertheless, important questions remain open about potential unaccounted costs of large infrastructure projects, such as conflict or environmental destruction, as well as

distributional and long-term effects of Chinese aid. See the Conclusion for more discussion.

4.5. Robustness and Falsification Tests

Including controls. The exclusion restriction would be violated if unrest in Chinese prefectures were spuriously correlated with other variables that also differentially affect outcomes in recipient countries. The main confounders are trade and foreign direct investment (FDI). To address such concerns, Table A.27 controls for lagged weighted exports and FDI from China to recipients. The results are highly robust to the inclusion of these controls. In other robustness checks, I control for a number of variables at the country-year level that may be correlated with recipient countries' outcomes, such as population, aid received by OECD-DAC donors, as well as total exports, imports and FDI. The results are robust (Tables on request).

Falsification tests. Table A.26 presents the results of first stage falsification tests. Column (1) replicates the baseline first stage for comparison. In Columns (2) to (4), I replicate the first stage for different leads and lags of the instrument. As expected, weighted unrest in China only predicts the number of aid projects received by a country in the next calendar year. In columns (5) to (7), I regress Chinese imports, Chinese FDI and OECD-DAC aid received by a country on the instrument. Reassuringly, lagged weighted unrest does not strongly predict any of these variables. These results provide further support for the validity of the instrument.

Other leads and lags. The main 2SLS specification estimates the effect of Chinese foreign aid on outcomes three years later. Tables A.28 and A.29 show the results for other leads and lags of aid. Each row displays the coefficient from a separate regression. As expected, the effect of aid projects on GDP per capita (and other outcomes) continuously increases over time as the projects get completed and start operating. Reassuringly, aid in the future, i.e., measured 1 or 2 years after the outcome, has no effect.

Different types of aid. Table A.30 shows the effect of Chinese aid instrumented by weighted labor unrest on GDP for different measures of aid (financial value instead of number of contracts and ODA-like / OOF-like official finance only). I find large effects on GDP for all measures, although the estimates are less precisely estimated due to a weaker first stage than in the baseline. Seemingly unrelated regressions (SUR) reveal that the effects of ODA-like and OOF-like on GDP are not statistically distinguishable from each other, i.e., the type of financing does not influence aid projects' efficacy.

Alternative instruments. Tables A.31 and A.32 replicate Tables 6 and 7 but using only aid up to t-1 to construct ω.[52] The estimates are qualitatively similar to the baseline specification,

[52] Formally, $\omega_{i,p,t} = \sum_{s=2005}^{t-1} \frac{\mathbb{1}[aid_{i,p,s}>0]}{t-2005}$). In this specification, the uninteracted term $\sum_p \omega_{i,p,t}$ is not absorbed by the country fixed effects, so I control for it separately.

albeit less powered.

Excluding outliers. Even though this would not change the internal validity of the findings, the interpretation would be different if the coefficients were driven by a few exceptional observations. To investigate this possibility, Table A.33 replicates Table 6 but dropping the three observations with the highest DFBETA (a measure of influence). The results remain qualitatively similar.

5. Conclusion

Foreign aid is one of the most important policy tools with which countries can transfer resources to poorer countries. Yet, the benefits of trillions of dollars in foreign aid for recipient populations remain unclear and highly debated. Existing studies hypothesize that donors' economic and political goals drive foreign aid, partly blaming them for aid's historical failure to foster broad-based economic development. However, we know surprisingly little about the exact processes through which donors' objectives influence aid allocation and efficacy. The rise of China as one of the largest donors in the last two decades has fueled new debates, with much speculation but little rigorous evidence about its motives and impact. This paper makes progress on these questions by diving deep into the opaque process underlying China's aid allocation. I show that a significant fraction of Chinese foreign aid is driven by the Chinese government's political need to moderate domestic unrest. However, this does not undermine the benefits of Chinese aid to recipients. These findings imply that foreign aid allocated according to domestic needs of the donor country need not have deleterious effects on recipient households as suggested by some existing studies.

These findings have important policy implications. Selfless aid is politically unrealistic. However, as this paper shows, the fact that Chinese aid benefits not only recipients but also China itself does not by itself mandate intervention. In fact, measures such as restricting the use of export credit as a form of state subsidy, as currently mandated by the global credit governance regime of the OECD, may end up hurting citizens of poor countries if it leads to a decrease in aid.[53] Policymakers should therefore perhaps focus on other aspects to improve aid efficacy, including the modality and conditions of aid delivery. For example, the majority of Chinese aid is bilateral and uncoordinated with other donors. This has raised concerns that China's aid may undermine Western donors' efforts to improve governance in aid recipient countries and lead to inefficiencies. Efforts to integrate China into multilateral

[53] China, which does not adhere to the OECD's credit governance regime, has been increasingly using export credits to support the international expansion of its firms, including in the context of its foreign aid program (Hopewell, 2021).

aid governance regimes could improve the global efficacy of aid by increasing transparency and coordination among donors.

The results presented in this paper suggest that Chinese foreign aid projects may create a "win-win" situation for the donor and recipient, consistent with the Chinese government's own narrative (State Council, 2014). However, the economic benefits must be weighed against unaccounted costs, such as the support of autocratic regimes, corruption, conflict, environmental degradation and sovereign debt. In addition, the large positive effects on average may mask substantial heterogeneity in who benefits and loses from Chinese aid. It also remains to be seen how sustainable the gains from Chinese aid projects are in the long term. That projects are allocated in response to domestic short-term shocks in China suggests that aid flows are unpredictable from recipients' perspective and may not be allocated optimally to foster long-term growth.

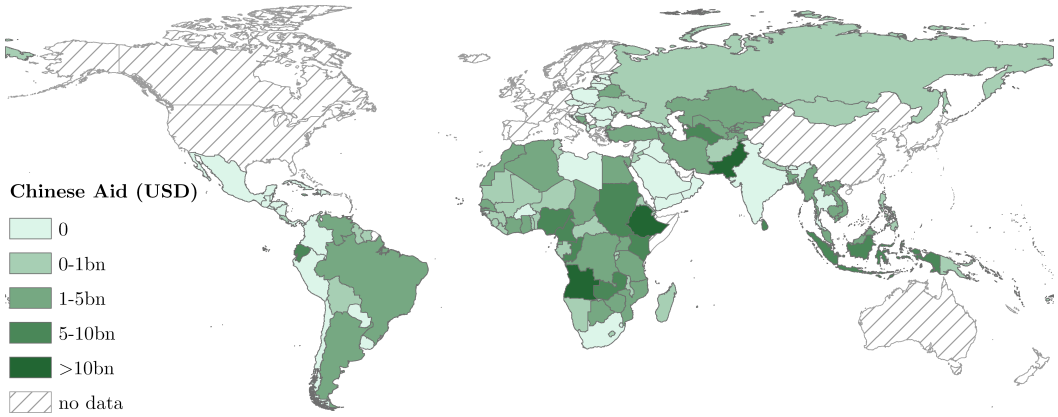
Understanding the causes and consequences of development finance will remain a first order issue for decades to come. In 2013, China announced the *Belt and Road Initiative* (BRI), a massive bid to enhance regional connectivity around the developing world. China plans to spend over a trillion dollars on infrastructure projects abroad in the next years, with many projects similar to those studied in this paper. Some observers have estimated that the BRI will boost world GDP by 2040 by USD 7.1 *trillion per annum* (Cebr, 2019). However, the BRI's goals and impacts are currently poorly understood. In response, the US, Japan and Australia have in the meantime announced their own initiative to promote infrastructure investment in developing countries, the "Blue Dot Network".

The results of this study suggest several important avenues for future research. The first is to examine other donor goals and their implications for aid allocation. The second is to examine the long-term and distributional consequences of Chinese aid as well as other outcomes that are potentially affected by it. Finally, more work needs to be done to better understand the channels through which Chinese aid shapes economic development on the ground. As this paper demonstrates, the use of fine-grained micro data to dive deep into the political processes behind foreign aid is a promising approach to make progress on this important research agenda. Much more research is needed to understand the causes and consequences of foreign aid.

Figure 1: China's vs. Other Donors' Aid Over Time

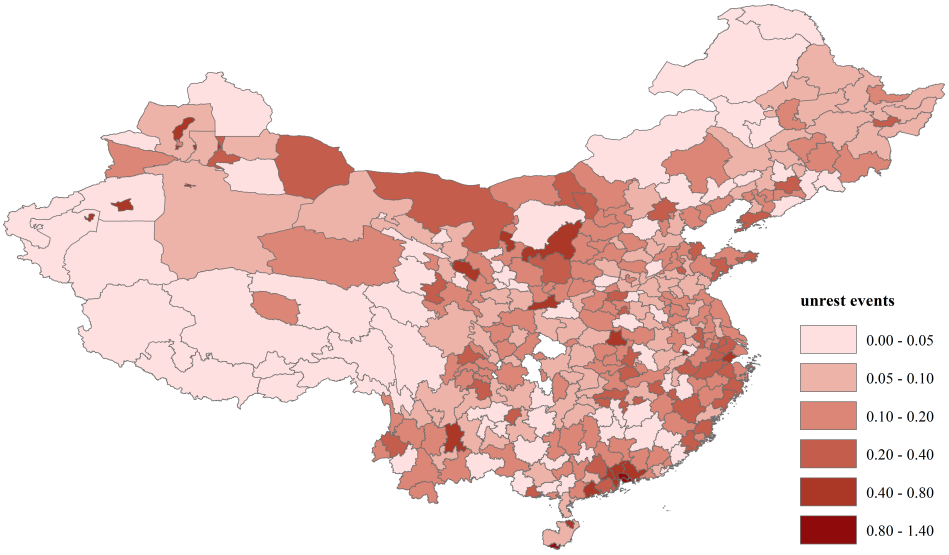
Note: The solid line shows the total financial value of Chinese aid projects implemented by Chinese firms in the sample by year. The dotted line shows the total financial value of official finance by the United States to developing countries by year (data from OECD DAC). The dashed line shows the total financial value of World Bank projects in developing countries outside of China by year (data from the World Bank Major Contracts Database). Financial amounts are in constant 2014 USD.

Figure 2: Map of Chinese Aid In Developing Countries, All Projects



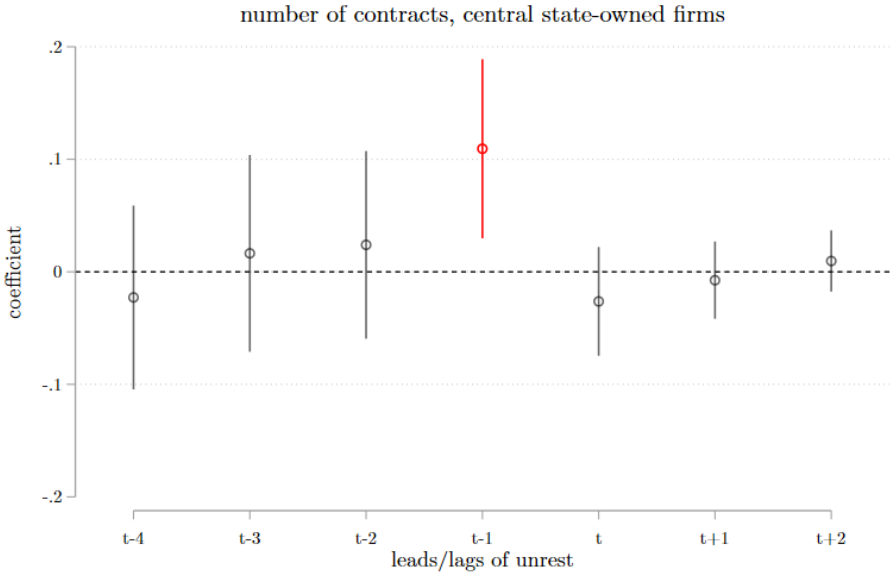
Note: This figure shows the total value of Chinese aid projects in non-high income, non-OECD countries implemented by Chinese firms in the sample. Financial amounts are in constant 2014 USD.

Figure 3: Average Yearly Number of Unrest Events per Million Inhabitants, 2004–2014



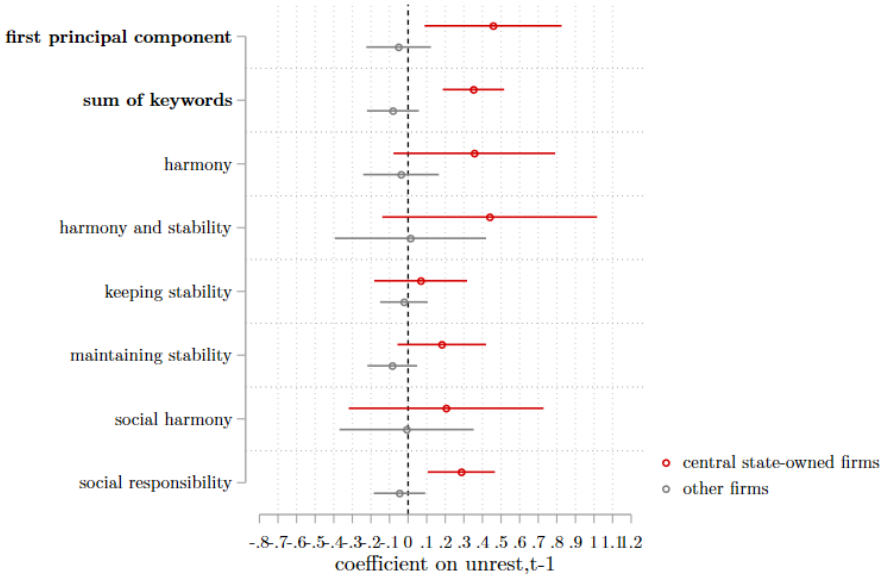
Note: This Choropleth map shows the average yearly number of unrest events per million inhabitants per prefecture. Data for 2004–2011 is from the China Strikes Crowdmap and data for 2012–2014 is from the China Labour Bulletin (CLB).

Figure 4: Local Unrest and Aid Contract Allocation to Central State-Owned Firms, Leads and Lags



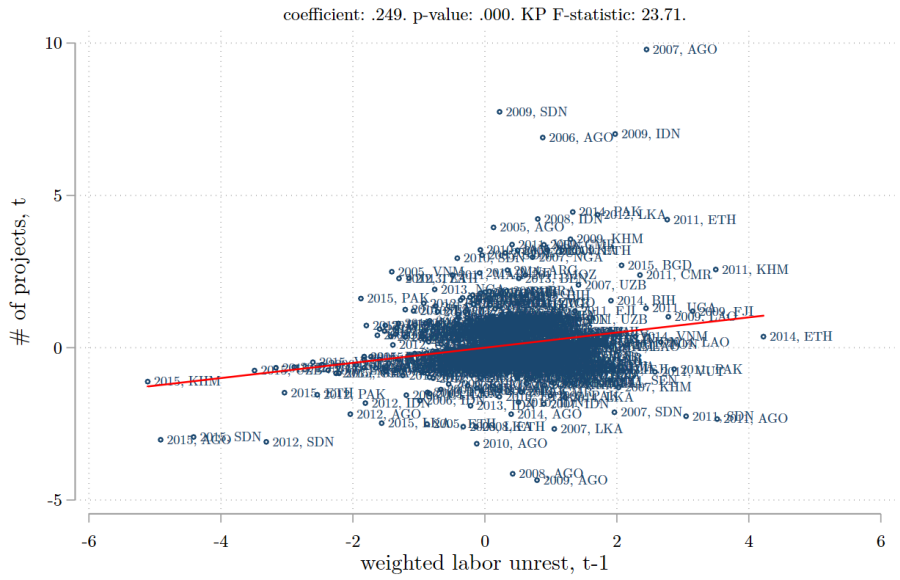
Note: The unit of observation is a firm-year. The figure shows the coefficients from an OLS regression of the number of aid contracts allocated to a SASAC firm on the number of unrest events per million inhabitants in the firm's prefecture at all leads and lags shown on the x-axis, controlling for firm and year fixed effects. Vertical lines show 90% confidence intervals.

Figure 5: Frequency of Social Stability Keywords in Annual Reports in Response to Unrest



Note: Each dot shows the coefficient from a separate OLS regression of the frequency of the phrase labeled on the y-axis in a firm's annual report at t, divided by the total number of words in the report, on unrest at t-1, controlling for firm and year fixed effects. Horizontal bars show 95% confidence intervals. All variables are standardized to have mean 0 and standard deviation 1. Standard errors are two-way clustered at the conglomerate and prefecture level. SASAC firms: N=444. Non-SASAC firms: N=852.

Figure 6: First Stage Residual Plot



Note: The red line is the line of fit from an OLS regression of the residualized number of aid projects received by a country in a year on residualized lagged weighted unrest (residualizing country and year fixed effects). Weighted unrest is calculated as the sum (over all Chinese prefectures) of demeaned unrest in a Chinese prefecture multiplied with the probability that the country receives aid projects implemented by SASAC firms in that prefecture.

Table 1: Local Unrest and Aid Contract Allocation to Central State-Owned Firms, Main Results

	# contracts	# ODA contracts	# OOF contracts	contract value	ODA value	OOF value
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*0.154*	*0.043*	*0.0400*	*1.401*	*0.502*	*0.540*
	(0.688)	*(0.269)*	*(0.280)*	*(5.017)*	*(2.993)*	*(3.204)*
unrest,t-1	0.103**	0.035**	0.048***	0.649**	0.345*	0.575***
	(0.041)	(0.015)	(0.018)	(0.281)	(0.189)	(0.210)
# observations	3532	3432	3432	3500	3413	3427
# firms	474	472	472	474	471	472
# prefectures	81	81	81	81	81	81
unrest mean (SD)	0.271	0.270	0.270	0.271	0.270	0.270
	(0.330)	(0.331)	(0.331)	(0.331)	(0.332)	(0.332)
adjusted R2	0.575	0.334	0.178	0.420	0.220	0.173

Note: The unit of observation is a firm-year. Sample includes SASAC firms only. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Contract value: log(1 + total value of aid contracts implemented by firm). Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Local Unrest and Aid Contract Allocation Other Firms

	# contracts	# ODA contracts	# OOF contracts	contract value	ODA value	OOF value
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*0.048*	*0.028*	*0.007*	*0.419*	*0.254*	*0.112*
	(0.332)	*(0.245)*	*(0.103)*	*(2.690)*	*(2.072)*	*(1.428)*
unrest,t-1	0.009	0.004	0.002	0.047	0.044	0.029
	(0.006)	(0.004)	(0.002)	(0.059)	(0.044)	(0.039)
# observations	5109	5077	5077	5067	5047	5075
# firms	735	735	735	735	735	735
# prefectures	123	123	123	123	123	123
unrest mean (SD)	0.259	0.259	0.259	0.260	0.260	0.259
	(0.431)	(0.432)	(0.432)	(0.432)	(0.433)	(0.432)
adjusted R2	0.450	0.379	0.157	0.333	0.324	0.105

Note: The unit of observation is a firm-year. Sample includes non-SASAC firms only. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Contract value: log(1 + total value of aid contracts implemented by firm). Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Contract Allocation and Local Unrest By Sector

	# contracts	# ODA contracts	# OOF contracts	contract value	ODA value	OOF value
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*0.154*	*0.043*	*0.040*	*1.401*	*0.502*	*0.540*
	(0.688)	*(0.269)*	*(0.280)*	*(5.017)*	*(2.993)*	*(3.204)*
unrest, industrial sectors, t-1	0.116**	0.044**	0.051**	0.886**	0.464**	0.602**
	(0.046)	(0.017)	(0.020)	(0.365)	(0.209)	(0.247)
unrest, service sectors,t-1	0.022	-0.009	0.024	-0.528	-0.040	0.303
	(0.084)	(0.041)	(0.038)	(0.835)	(0.625)	(0.550)
# observations	3532	3432	3432	3500	3413	3427
# firms	474	472	472	474	471	472
# prefectures	81	81	81	81	81	81
unrest, industrial mean (SD)	0.191	0.190	0.190	0.191	0.190	0.190
	(0.267)	(0.268)	(0.268)	(0.268)	(0.268)	(0.268)
unrest,service mean (SD)	0.071	0.0707	0.0707	0.071	0.0707	0.0707
	(0.107)	(0.107)	(0.107)	(0.107)	(0.107)	(0.107)
adjusted R2	0.575	0.334	0.177	0.420	0.220	0.172

Note: The unit of observation is a firm-year. Sample includes SASAC firms only. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Contract value: log(1 + total value of aid contracts implemented by firm). Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. Industrial sectors include construction, mining and manufacturing. Service sectors include education, public transport and other services. Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Local Unrest and Firm Employment

	# contracts		# employees (log)	
	central state-owned firms	other firms	central state-owned firms	other firms
	(1)	(2)	(3)	(4)
dependent variable mean (SD)	*0.224*	*0.0305*	*6.127*	*6.083*
	(0.842)	*(0.218)*	*(1.565)*	*(1.400)*
unrest,t-1	0.073**	0.012	0.031**	-0.032
	(0.035)	(0.009)	(0.012)	(0.023)
# observations	1363	1804	1363	1804
# firms	298	425	298	425
# prefectures	62	92	62	92
unrest mean (SD)	0.270	0.273	0.270	0.273
	(0.309)	(0.459)	(0.309)	(0.459)
adjusted R2	0.637	0.396	0.959	0.947

Note: The unit of observation is a firm-year. The sample includes firms with >10 employees that were selected to participate in the 2007-2015 tax surveys by the State Tax Administration. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. # of employees: log(number of workers employed by the firm). Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Contract Allocation and Local Unrest By Fiscal Capacity of Local Government

	# contracts	# ODA contracts	# OOF contracts	contract value	ODA value	OOF value
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*0.159*	*0.045*	*0.041*	*1.444*	*0.520*	*0.559*
	(0.698)	*(0.274)*	*(0.285)*	*(5.088)*	*(3.044)*	*(3.259)*
local gov constrained, t-1	-0.065	-0.029	-0.026	-0.566	-0.295	-0.417
	(0.044)	(0.026)	(0.020)	(0.402)	(0.197)	(0.320)
unrest,t-1	0.067*	0.015	0.030*	0.125	0.064	0.248
	(0.038)	(0.012)	(0.016)	(0.254)	(0.145)	(0.167)
unrest,t-1 × local gov cons., t-1	0.067**	0.035**	0.033**	0.883**	0.460*	0.590**
	(0.034)	(0.017)	(0.015)	(0.396)	(0.243)	(0.285)
# observations	3413	3314	3314	3381	3295	3309
# firms	467	465	465	467	464	465
# prefectures	78	78	78	78	78	78
unrest mean (SD)	0.272	0.270	0.270	0.272	0.270	0.270
	(0.327)	(0.328)	(0.328)	(0.329)	(0.329)	(0.329)
adjusted R2	0.571	0.331	0.174	0.416	0.217	0.170

Note: The unit of observation is a firm-year. Sample includes SASAC firms only. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Contract value: log(1 + total value of aid contracts implemented by firm). Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. Constrained (unconstrained) local government: yearly prefecture public income / expenditure ratio < (>) 0.8 (sample mean). Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effects of Chinese Aid on Recipient Countries

	GDP per capita, t+3	GDP growth (%), t+3	unemployment (%), t+3	# diplomatic visits, t+3
	(1)	(2)	(3)	(4)
dependent variable mean (SD)	*5130*	*2.209*	*7.598*	*1.393*
	(4597)	*(2.941)*	*(5.358)*	*(2.666)*
		Panel A: IV estimates		
instrumented aid, t	123.359**	0.510*	-0.381**	1.059**
	(50.612)	(0.300)	(0.179)	(0.433)
		Panel B: reduced form estimates		
weighted unrest, t-1	31.267**	0.129	-0.096**	0.267***
	(12.659)	(0.079)	(0.040)	(0.095)
		Panel C: OLS estimates		
aid, t	0.302	0.160**	-0.021	0.164*
	(11.920)	(0.079)	(0.030)	(0.086)
# observations	1,474	1,479	1,423	1,500
# countries	135	136	130	137
# of aid projects mean (SD)	0.333	0.334	0.345	0.330
	(1.024)	(1.024)	(1.041)	(1.017)
KP F-Statistic from first stage	24.41	24.36	23.88	24.30

Note: The unit of observation is a country-year. Panel A shows the regression coefficients from IV estimates of the outcome variable in the Column head at t+3 on the number of Chinese aid projects, implemented by SASAC firms, at t, where the number of aid projects is instrumented by weighted labor unrest at t-1. Panel B shows reduced form estimates of the outcome variable in the Column head at t+3 on weighted labor unrest at t-1. Panel C shows OLS estimates of the outcome variable in the Column head at t+3 on aid at t. All country-year level regressions include country and year fixed effects. Weighted unrest is calculated as the sum (over all Chinese prefectures) of demeaned unrest in a Chinese prefecture multiplied with the probability that the country receives aid projects implemented by SASAC firms in that prefecture. Weighted unrest is standardized to have mean 0 and standard deviation 1. Outcome variables are winsorized at the 5th and 95th percentiles to limit the influence of extreme outliers. Standard errors are clustered at the country level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effect of Chinese Aid on Recipient Countries, Components of GDP

	GDP per capita, t+3	capital formation p.c., t+3	government consumption p.c., t+3	household consumption p.c., t+3	imports per capita, t+3	exports per capita, t+3
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*5097*	*1275*	*855.2*	*3140*	*2608*	*2386*
	(4669)	*(1176)*	*(888)*	*(2610)*	*(3359)*	*(4059)*
Panel A: IV estimates						
instrumented aid, t	164.867***	82.034**	40.414***	68.228*	75.333	-52.100
	(61.594)	(32.660)	(12.168)	(39.585)	(71.979)	(181.907)
Panel B: reduced form estimates						
weighted unrest, t-1	41.876***	20.836**	10.265***	17.330*	19.134	-13.233
	(15.637)	(8.381)	(3.106)	(9.260)	(18.511)	(45.134)
Panel C: OLS estimates						
aid, t	3.760	9.656*	0.725	0.725	15.501	17.754
	(12.552)	(5.710)	(1.543)	(4.834)	(10.888)	(25.805)
# observations	1,189	1,189	1,189	1,189	1,189	1,189
# countries	112	112	112	112	112	112
# of aid projects mean (SD)	0.348	0.348	0.348	0.348	0.348	0.348
	(1.071)	(1.071)	(1.071)	(1.071)	(1.071)	(1.071)
KP F-Statistic from first stage	16.74	16.74	16.74	16.74	16.74	16.74

Note: Sample includes observations that are non-missing for all outcome variables. The unit of observation is a country-year. Panel A shows the regression coefficients from IV estimates of the outcome variable in the Column head at t+3 on the number of Chinese aid projects, implemented by SASAC firms, at t, where the number of aid projects is instrumented by weighted labor unrest at t-1. Panel B shows reduced form estimates of the outcome variable in the Column head at t+3 on weighted labor unrest at t-1. Panel C shows OLS estimates of the outcome variable in the Column head at t+3 on aid at t. All country-year level regressions include country and year fixed effects. Weighted unrest is calculated as the sum (over all Chinese prefectures) of demeaned unrest in a Chinese prefecture multiplied with the probability that the country receives aid projects implemented by SASAC firms in that prefecture. Weighted unrest is standardized to have mean 0 and standard deviation 1. Outcome variables are winsorized at the 5th and 95th percentiles to limit the influence of extreme outliers. Standard errors are clustered at the country level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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APPENDIX

Contents

Appendix A. Additional Background

A.1. Differences Between OECD-DAC and Chinese Official Finance (Aid)

Table A.1 provides an overview of differences between development finance provided by OECD-DAC donors and China that are commonly pointed out by scholars of foreign aid (see, e.g., Bräutigam (2011*a*)). The list is neither definite nor exhaustive.

Table A.1: Differences Between OECD-DAC and Chinese Official Finance

Characteristic	**OECD**	**China**
Type of Financing	primarily ODA, including grants and concessional loans	ODA and OOF, including loans at commercial rates and export credits
Type of Assistance	emphasis on social sectors and services	emphasis on infrastructure and production
Procurement	financing and procurement typically untied	financing typically conditional on $> 50\%$ of goods sourced from China
Conditionality	economic and political conditions common (e.g., governance, human rights)	no official conditions related to domestic governance of recipient country
Reporting	official flows reported to Creditor Reporting System	no official data
Guiding Principles	(i) ownership, (ii) harmonization, (iii) alignment, (iv) results, (v) mutual accountability.	(i) equality and mutual benefit, (ii) respect for sovereignty with no conditions attached, (iii) promote self-reliance, not dependency, (iv) quick results, (v) use best-quality equipment of Chinese manufacture, (vi) emphasize technology transfer, (vii) Chinese experts will live at the standard of local experts.

Note: OECD-DAC stands for The Organisation for Economic Co-operation and Development's Development Assistance Committee. ODA stands for Official Development Assistance. OOF stands for Other Official Finance and includes other financing instruments such as loans at commercial rates and export credits. Sources: Bräutigam (2011*a*), Bräutigam (2011*b*), State Council (2011), State Council (2014), OECD (2021).

A.2. Definitions and Types of Official Finance (Aid)

This paper uses the term "foreign aid" to include any bilateral official finance between donor and recipient developing country government entities (in line with, e.g., Copper (2016)).

Official finance can be categorized into two broad categories, ODA-like and OOF-like (ODA stands for Official Development Finance, and OOF stands for Other Official Flows [Finance]). The former is concessional in nature, aimed at fostering development in recipient countries, and meets the conventional notion of Western "foreign aid" (including grants and concessional loans). The latter may also be aimed at fostering development, but is less concessional in nature (e.g., loans at commercial rates and export credit) (Bräutigam, 2011*a*). It is much more often employed by the Chinese government, with the stated intent of fostering development in recipient countries, than by Western donors (AidData, 2021). In contrast to most Western donor countries, China is not in the OECD-Development Assistance Committee and does not distinguish between ODA and other forms of official finance (Bräutigam, 2011*a*). The latter includes export credits and loans at commercial interest rates (e.g., LIBOR). Even though such instruments do not meet the strict definition of Western foreign aid, they constitute a subsidy from the recipient country's perspective since China guarantees the debt and recipients would typically not be able to access credit at the same conditions on international financial markets. This is in line with other scholars studying Chinese aid (e.g., Copper (2016)). Note that a significant fraction of US "aid" is also in the form of loans (Bräutigam, 2011*a*).

The Organisation for Economic Co-operation and Development's Development Assistance Committee (OECD-DAC) defines ODA as follows, for ODA before 2017 (OECD, 2021):

> *Those flows to countries and territories on the DAC List of ODA Recipients and to multilateral institutions which are:*
>
> *(i) provided by official agencies, including state and local governments, or by their executive agencies; and (ii) each transaction of which: (a) is administered with the promotion of the economic development and welfare of developing countries as its main objective; and (b) is concessional in character and conveys a grant element of at least 25 per cent (calculated at a rate of discount of 10 per cent).*

The OECD-DAC defines OOF as follows (OECD, 2021):

> *Other official flows (OOF) are defined as official sector transactions that do not meet official development assistance (ODA) criteria. OOF include: grants to developing countries for representational or essentially commercial purposes; official bilateral transactions intended to promote development, but having a grant element of less than 25%; and, official bilateral transactions, whatever their grant element, that are primarily export-facilitating in purpose.*

In the paper, I follow the procedures outlined in *AidData's Tracking Underreported Financial Flows (TUFF) Methodology, Version 1.3* (Strange et al., 2017), which adopts the OECD-DAC definitions, to classify Chinese official finance into ODA-like and OOF-like.

Table A.2 gives an overview on the different types of finance extended by Chinese government entities. Note that only the Ministry of Commerce is authorized to provide grants and interest-free loans, and only the China Export-Import Bank is authorized to extend concessional loans (i.e., loans at an interest rate below a competitive international rate such as the LIBOR).

Table A.2: Types of Chinese Finance

Type	**Creditor**	**Approximate Terms**
Grants / turnkey projects, in-kind donations, technical assistance	Ministry of Commerce	no repayment
Interest-free loans	Ministry of Commerce	0% interest rate, 20 year maturity, 5 year grace period
Concessional loans, preferential export buyer's credits	China Export-Import Bank	2-3% interest rate, 20 year maturity, 5 year grace period
Commercial loans, export credits, other financing	China Development Bank, China Export-Import Bank, state-owned commercial banks, other government entities	LIBOR plus spread, 13 year maturity, 0-5 years grace period

Note: Drawing on information from Bräutigam (2011*b*), Zhang and Smith (2017) and Horn, Reinhart and Trebesch (2019).

A.3. Chinese Government Entities and the Aid Allocation Process

Figure A.1 gives a stylized overview on the, for the purposes of this paper, most important Chinese government entities. As seen in Figure A.3, the two most important providers of Chinese official finance are the central government's two main policy banks, the China Export-Import Bank (CEXIM) and the China Development Bank (CDB).[54] They are owned and supervised by the State Council, the chief administrative authority of the People's Republic of China (synonymous with central government). In the case of CDB, the ownership is through the Ministry of Finance and Central Huijin Investment, with the latter acting as the main shareholder of China's state-owned commercial banks on behalf of the State Council. The Chinese central government guarantees the policy banks' debt, allowing them to raise capital on national and international financial markets at favorable conditions (Zhang and Smith, 2017).

CEXIM is the only creditor authorized to issue concessional loans (see Table A.2). It cooperates with the Department of Foreign Aid of the Ministry of Commerce, which is the main entity responsible for ODA-like foreign aid projects (i.e., grants, interest-free loans and concessional loans). CEXIM raises the principal of the loans on capital markets and the Ministry of Commerce subsidizes the interest rate. In addition, like CDB, CEXIM also extends commercial loans, for example for large-scale infrastructure projects and for the purposes of export promotion.[55]

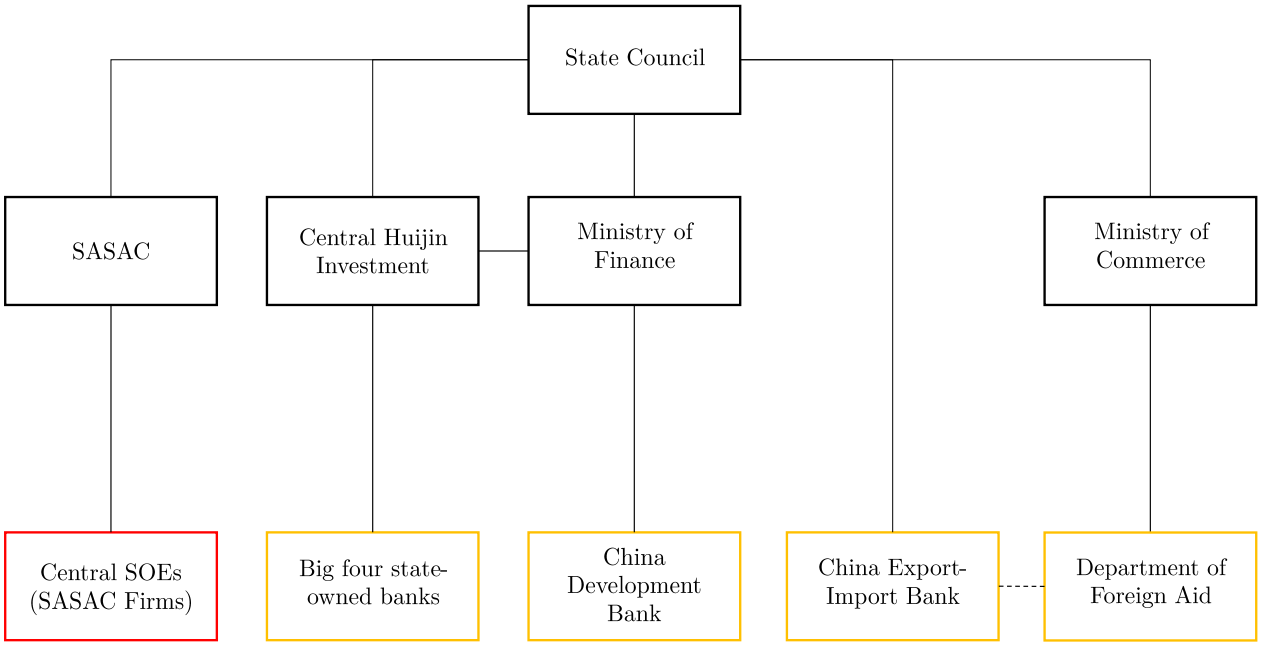
The large state-owned commercial banks have also started extending overseas finance to developing countries. They include the Bank of China, the Agricultural Bank of China, the Industrial and Commercial Bank of China and the China Construction Bank.[56] These banks have grown rapidly over the last years and constitute the world's largest banks measured by total assets (Horn, Reinhart and Trebesch, 2019). Finally, some other central state-owned enterprises, owned and administered by the central State-owned Assets Supervision and Administration Commission (SASAC), themselves extend financing to developing country governments. However, their share of overall Chinese official finance is small (see Figure A.3).

[54] The third policy bank is SINOSURE, which provides export insurance. It is not relevant for the purposes of this paper.

[55] I include such loans to developing countries in my main measure of foreign aid even though they cannot be classified as ODA. See the discussion in Appendix A.2.

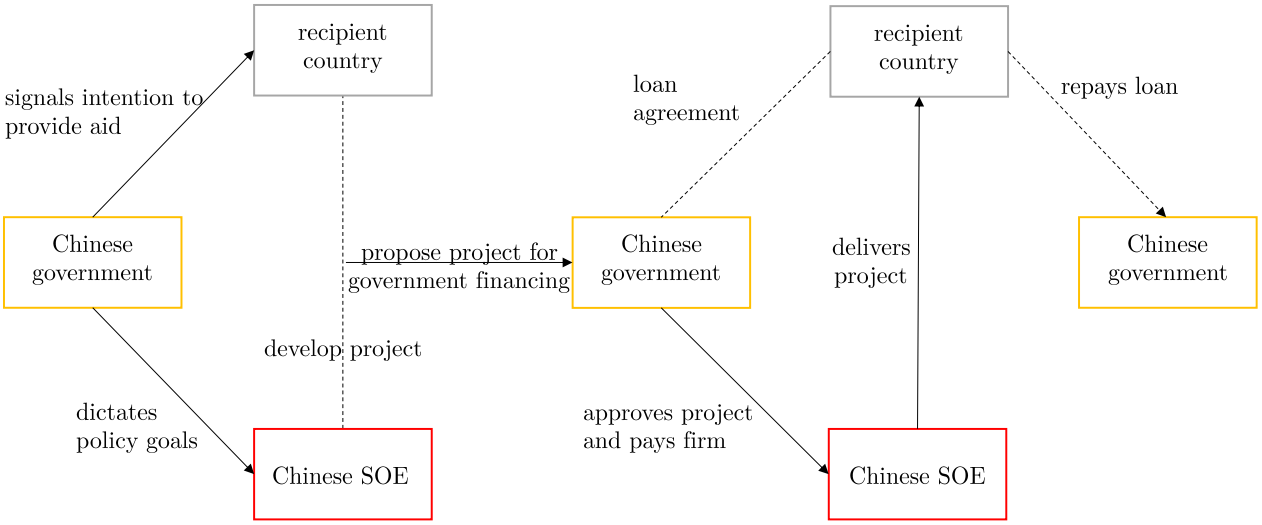
[56] A further large state-owned commercial bank is the China Communications Bank.

Figure A.1: Organization Chart of Relevant Government Entities (Simplified)



Note: This figure shows a strongly simplified overview of China's main official finance agencies (in red) and other central government entities. The China Export-Import Bank and the China Development Bank are state-owned policy banks under the direct supervision of the State Council. The big four state-owned banks include Bank of China, Agricultural Bank of China, Industrial and Commercial Bank of China, and China Construction Bank. Black lines indicate ownership. Based on information from government websites, Zhang and Smith (2017), Horn, Reinhart and Trebesch (2019) and Brautigam and Hwang (2020).

Figure A.2: The Chinese Aid Project Allocation Process (Simplified)



Note: Own illustration based on Gu, Chen and Zhang (2014) and Zhang and Smith (2017) and Brautigam and Hwang (2020).

Appendix B. Details on the Micro-level Data

B.1. Project-level Aid Data

The Chinese government does not publish comprehensive, disaggregated data on the overseas projects it finances. Several academic initiatives have attempted to fill this gap in the last few years by collecting unofficial project-level data, drawing on sources such as news reports, government publications by Chinese embassies and recipient country ministries, and case studies undertaken by scholars and NGOs. These datasets typically specialize on projects in one geographic area, sector or time period, or lack information on the involved contractors. I combine data from several such sources to construct a project-level dataset encompassing the universe of publicly known Chinese aid projects, involving Chinese contractors, in 2005–2015.

Table A.3: Data Sources on Chinese Foreign Aid, 2005–2015

Institution	Dataset / Source	Geographic Coverage	Time Coverage	Type
AidData at William & Mary	Geocoded Global Chinese Official Finance Dataset Version 1.1.1	Global	2000–2014	Loans, grants
AidData at William & Mary	China's Public Diplomacy in East Asia and Pacific Version 1.0	East Asia & Oceania	2000–2016	Loans, grants
Johns Hopkins CARI	China-Africa Loan Database	Africa	2000–2017	Loans
Boston University GDPC	Global Energy Finance Database	Global	2000–2018	Energy finance
Inter-American Dialogue	China-Latin America Finance Database	Latin America	2005–2018	Loans
US Export-Import Bank	Competitiveness Reports	Global	2013–2018	Export credit
Gelpern et al. (2021)	How China Lends Version 1.0	Global	1999–2020	Financing agreements
Mueller (2021)	My merged database	Global	2005–2015	Loans, grants

Note: This table summarizes the data sources underlying the project-level dataset on Chinese foreign aid used in this paper. References: AidData's Geocoded Global Chinese Official Finance Dataset Version 1.1.1 (AidData Research and Evaluation Unit, 2017, Bluhm et al., 2018, Dreher et al., 2021), AidData's China's Public Diplomacy in East Asia and Pacific Version 1.0 (Custer et al., 2018), SAIS CARI China-Africa Loan Database (Brautigam et al., 2019), Boston University's Global Energy Finance Database (Gallagher, 2021), Inter-American Dialogue's China-Latin America Finance Database (Gallagher and Myers, 2021), Export-Import Bank of the United State's Competitiveness Reports 2014-2016 (Export-Import Bank of the United States, 2021).

Table A.3 lists the public databases I use to construct my project-level dataset. An excellent overview on these sources and their methodologies is given by Horn, Reinhart and Trebesch (2019). I restrict the sample to 2005–2015 given both the availability of data on foreign aid and other important data used in the paper, such as administrative firm-level data and data on unrest. In addition to these secondary sources, I also draw on various primary sources, including government and company websites, news articles and academic papers.

The starting point of my dataset is AidData's Geocoded Global Chinese Official Finance Database (Dreher et al., 2021). The project-level dataset systematically collects publicly

available information on all known Chinese official finance, based on scraping of thousands of primary sources, in 2000–2014. The method underlying this data-collection effort is described in detail in Strange et al. (2017). I follow the conventions of Dreher et al. (2021) by excluding umbrella agreements from the sample. Importantly for the purposes of this paper, the database also records information on the implementing contractors, albeit this information is only recorded at the conglomerate level for many projects. I identify all projects involving Chinese contractors from this database and collect further information on the contractors from the primary sources indicated by AidData, following the procedure described in Section 3.2.

I use several other, more specialized databases to extend the temporal coverage, to find additional foreign aid projects involving Chinese contractors, and to verify the information provided by AidData. First, AidData's China's Public Diplomacy in East Asia and Pacific database provides data on Chinese foreign aid projects in Asia and Oceania during 2000–2016 (Custer et al., 2018). It employs the same methodology as AidData's Geocoded Global Chinese Official Finance Dataset. In addition to extending the temporal coverage, it helps fill in gaps with regard to information on Chinese contractors.

Second, the China-Africa Loan Database by the Johns Hopkins China-Africa Research Initiative (SAIS-CARI) (Brautigam et al., 2019) provides data on Chinese loans to 55 countries in Africa in 2000–2017. There is considerable overlap with the AidData database. With the help of several Chinese-speaking research assistants, I carefully check the data for potential duplicates and conduct further research using primary sources in the few cases where the data are contradictory.

Third, I use data on global fossil fuel, nuclear power and renewable energy sector projects since 2000 from the Global Energy Finance Database at Boston University (Gallagher, 2021). Fourth, I draw on the China-Latin America Finance Database by the Inter-American Dialogue (Gallagher and Myers, 2021), which provides data on official loans by the Chinese government to governments in Latin America in 2005–2018. Finally, I complement my dataset with data from the Competitiveness Reports by the US Export-Import Bank since 2013, which focuses mainly on projects financed by the China Export-Import Bank (Export-Import Bank of the United States, 2021). Again, I cross-check and verify the projects and recorded data using primary sources. I harmonize the variables across datasets, following the methodology by Strange et al. (2017). Further details and replication files will be available on request.

The resulting project-level database covers the publicly known universe of Chinese aid projects in developing countries implemented by Chinese contractors. Descriptive evidence is provided in Section 3.2.

B.2. Constructing and Linking the Firm Panel

To construct the firm-year panel, I first identify the set of potential Chinese aid contractors from archival administrative records by the Chinese Ministry of Commerce.[57] The list contains the names and addresses of all firms and subsidiaries licensed to supply overseas construction projects in both Chinese and English. I next determine during which years each firm was active (i.e., eligible to supply aid projects) by manually linking firms to the official transaction-level Customs Trade Statistics (CCTS) by the Chinese Customs Office, using firm names and addresses (see Appendix B.3 for details). For each firm, I keep all years between the first and last year of positive firm exports.[58] The result is an unbalanced firm-year panel of the universe of potential domestic Chinese aid contractors.

I link the panel to various other administrative datasets. First, I use the Chinese credit registry[59] to find each firm's ultimate controller to determine whether the firm is owned by the central government. I classify a firm as a central SOE if it was ever administered by the central SASAC.[60] Second, to get firm characteristics such as assets, revenue and number of employees, I link my sample to firm-level data from the National Tax Survey Database (NTSD) using names and addresses. Since the tax survey data is a stratified random sample of all Chinese firms, I can only link a subset of my sample. I follow standard procedure in the literature and remove observations with less than ten employees, negative fixed assets or revenue, as well as outliers. See Liu and Mao (2019) for a detailed description of the data and Appendix B.4 for further details. Third, for complementary analyses, I use data from the China Stock Market and Accounting Research Database (CSMAR), which contains comprehensive data on firms listed on Chinese stock exchanges. I describe the variables constructed from this dataset as I introduce them in Section 3.

Finally, I link the project-level data described in Section 3.2 to the firm-year panel. I expand on the existing literature by systematically collecting and harmonizing information about the Chinese contractors involved in Chinese aid projects. While some of the existing unofficial databases (see Appendix B.1) provide names of contractors implementing a project, the names are not harmonized and typically only identify the firm group (conglomerate) rather than the subsidiary that actually implements the project. This has so far prevented researchers from linking data on projects to firms and conducting systematic micro-analysis

[57] URL: http://xzsx.mofcom.gov.cn:80/xzsp/advSearch.jhtml , accessed using the *Internet Archive Wayback Machine* in June 2020 (URL: https://archive.org/web/).

[58] Firms should not be included in the panel in years before they entered or after they exited (e.g., due to mergers) because they were not eligible to supply aid projects in those years. In addition, there may be duplicates in the MOFCOM list, for example due to name changes of firms over time. The customs data provides a unique 10-digit firm identifier that is constant over time.

[59] Accessed in July 2020 through https://aiqicha.baidu.com/ .

[60] Companies supervised by the SASAC have continuously been reduced through mergers and privatization since its foundation in 2003. However, virtually all SASAC-administered firms in my sample remained under the SASAC throughout 2003-2015.

of the role of firms in Chinese foreign aid. To address this challenge, whenever possible I identify the actual implementing contractor (i.e., the subsidiary) from the original sources underlying the unofficial databases on Chinese aid.

I manually find the best match between the MOFCOM list of potential contractors and the firm names mentioned in the projects data, firm export patterns, and, if necessary, additional research using other sources such as company websites. Two Chinese-speaking research assistants independently verified the linking. 96% of all projects can be linked unambiguously. I exclude the 4% of projects that cannot be linked from my sample. The resulting main variables are the number and financial value of aid contracts committed to a firm in a year. The number of contracts is the preferred outcome variable as it is less likely to suffer from potential measurement error than the contract value. In addition, information on the financial value is missing for a significant fraction of projects. I calculate the financial value of a contract assigned to a firm in a year as the financial value of the entire project committed in that year, divided by the number of Chinese firms involved in the project. I do not observe the confidential details of the actual contracts underlying the projects, such as the exact payment made to each firm or subcontracting.

B.3. Customs Data

The Chinese Customs Trade Statistics (CCTS) by the Chinese Customs Office provides information on the universe of Chinese exports and import transactions during 2003–2015. For each transaction, the database includes a unique firm identifier, firm name, firm address (including the firm's home prefecture), import/export value and quantity, the product code at the HS 8-digit level, and the country of origin/destination. I aggregate the data to the firm-year level. I use this data to determine during which years a firm was active and thus eligible to contract aid projects (see Appendix B.2). In addition, I use the data to help identify the implementing subsidiary in cases where the sources on aid projects are ambiguous, for example because they only provide the name of the conglomerate implementing the aid project, using patterns in the data such as destination countries of exports (see Appendix B.1). Finally, I use the data to construct controls and other variables at the country-prefecture level for the analysis in Section 4.

B.4. Tax Survey Data

I complement my firm panel with data from the National Tax Survey Database (NTSD) from 2007–2015. The firm-year level dataset contains rich information on firms' financials, tax payments, employment and other characteristics. The survey is conducted annually

by the State Administration of Taxation of China and the Ministry of Finance of China (SAT-MOF). It surveys all large firms as well as a stratified random sample of smaller firms. The database is used by the Chinese government to evaluate the impacts of tax policies such as the "Golden Tax Project". As Liu and Mao (2019) argue, various checks and balances make misreporting by firms unlikely and the data are verified by local tax agencies. The NTSD is unique in that it is the only firm-level database that contains comprehensive information on Chinese firms' financial values and other variables such as employment, covers all sectors, and firms of all sizes.[61]

Despite the checks and balances designed to ensure the accuracy of the tax data, several data cleaning steps are necessary to reduce noise in the data stemming from potential misentries. First, in line with Liu and Mao (2019), I set as missing entries with fewer than 10 employees and non-positive values in the main variables used in the working sample (fixed assets, operating income and exports). Second, I trim the top and bottom percentiles of all entries in the data. Third, for each variable, I set as missing entries that exceed a one standard deviation from a firm's sample mean, affecting approximately the top and bottom decile of entries within each variable. This last step is necessary since some entries are implausibly different for some years compared to a firm's entries in other years, likely due to data entry errors. Finally, I drop firms with the same name that change their tax identification number over time to ensure comparability of the data over time. After imposing these restrictions, I link the NTSD data with my main sample using firm names and home prefectures. The resulting working sample contains data from the tax survey for 298 SASAC and 425 other firms.

B.5. Unrest Data and Examples

I combine data on labor unrest in China from two sources, the China Strikes Crowdmap for 2004–2011 (https://chinastrikes.crowdmap.com/feeds?page=1762&l=ps&l=fa) and the China Labour Bulletin for 2012–2014 (https://clb.org.hk/). The unrest events are geo-coded by the original authors. However, the location data in the CLB data provided to me sometimes lacked information or appeared to be coded incorrectly (e.g., the Chinese provinces Shanxi and Shaanxi were confused). I completed the missing location data using regular expressions and manual checks, which I verified and shared with the CLB team. In addition, I drop labor unrest events involving foreign companies. I classify labor unrest events into two broad sectors, industry and services, based on variables and keywords contained in the event

[61] The other large firm-level dataset used by researchers is the Annual Survey of Industrial Firms (ASIF) conducted by the National Bureau of Statistics of China. In contrast to the NTSD, ASIF contains only large firms in the manufacturing sector and may suffer from reporting bias (Brandt, Van Biesebroeck and Zhang, 2014).

descriptions in the original datasets. The industrial sector includes: construction ("construction"), manufacturing ("manufacturing", "factory", "processing", "plant", "shipbuilding", "steel", "chemical", "plastic", "glass", "paper"), mining and processing ("mining", "mine", "oil", "iron", "coal") and transport ("train", "bus", "air", "logistics"). The service sector includes: education ("school", "teacher",) and other services ("service", "taxi", "office", "restaurant", "retailer", "telecommunication", "bank", "hospital", "doctor", "department store", "pharma", "tourism", "sport", "shop", "beauty"). Using these keywords, all unrest events can be classified in one of the two broad sectors.

Table A.4 provides several representative examples of unrest events recorded in the final dataset.

Table A.4: Examples of Unrest Events

Year	Prefecture, Province	Description
2005	Dongying, Shandong	Shengli oil field workers protest over restructuring
2005	Shenzhen, Guangdong	State-owned construction company workers protest
2008	Shanghai, Shanghai	Huanxin / Yixin electronics factory workers protest over wage arrears
2009	Zhengzhou, Henan	Linzhou Iron and Steel Company protest
2010	Jingzhou, Hubei	Teachers protest by kneeling in front of the government building in Gong'an County
2011	Yongzhou, Hunan	Yuejin machinery factory workers strike
2011	Wenshan, Yunnan	Railway construction workers protest against violence
2012	Honghe, Yunnan	Miners march toward city government buildings, protesting factory move
2013	Wuhan, Hebei	Gas company workers stage strikes against merger
2013	Yulin, Shaanxi	Taxi drivers strike, demanding the government to crack down on illegal counterparts
2014	Chifeng, Inner Mongolia	Over 1000 steel workers demand six months of wages in arrears at local government

Note: Data for 2004–2011 is from the China Strikes Crowdmap and data for 2012–2014 is from the China Labour Bulletin (CLB). Examples selected among unrest events estimated to involve > 1000 participants. Descriptions are abbreviated from the original data by the author for ease of exposition.

B.6. Other Micro Data

Contract-level data on World Bank projects come from the *World Bank Major Contract Awards Database* (The World Bank, 2020). I calculate the number and financial value of World Bank projects implemented by each firm and year in my sample, analogous to Chinese aid projects.

I construct measures of firms' political connections by linking their top executives to the biographies of high-ranking CCP members, provided by Shih, Adolph and Liu (2012) and Jiang (2018). A firm is coded as politically connected if a current member of the Central

Committee, the body of the CCP's 376 top leaders, is or used to be a top executive or board member of the firm.[62]

Finally, I collect a set of socioeconomic variables at the prefecture level from the *China City Statistical Yearbooks*, including local population, GDP, urban employment, wages, and local government revenues and expenditures.[63] Data on prefecture-level exports come from the official customs data described in Section 3.2. I provide prefecture-level descriptive statistics in Table A.8.

[62] Data on firm executives and board members is from the CSMAR database and includes the subset of publicly listed firms. For unlisted firms that are part of larger conglomerates, I use the annual reports of listed firms in the same conglomerate.

[63] The China Statistical Yearbooks data are based on official statistics by the Chinese government.

Appendix C. Additional Descriptive Statistics and Data Figures

C.1. Project-level Data

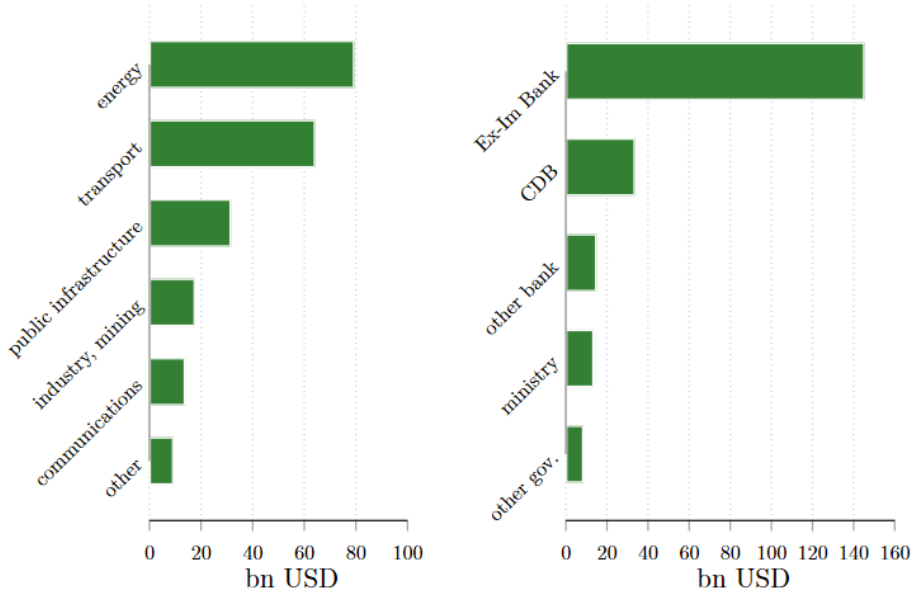
Table A.5: The 10 Largest ODA-Like Contracts by Amount (constant 2014 USD million)

Country	Year	Amount	Short Description	Contractor
Kenya	2014	1600	Mombasa-Nairobi Standard Gauge Railway	China Road and Bridge Co.
Cameroon	2009	1052	Water Distribution Project	China National Machinery Import and Export Co.
Tanzania	2012	973	Mnazi Bay to Dar Es Salaam Gas Pipeline	China Petroleum Technology and Development Co.
Nigeria	2006	920	Modernization of Nigeria Railway	China Civil Engineering Construction Co.
Ivory Coast	2013	890	Abidjan Port	China Harbor Engineering Co.
Chad	2011	777	N'Djamena International Airport	China CAMC Engineering Co.
Mozambique	2011	757	Maputo-Catembe Bridge	China Road and Bridge Co.
Sudan	2014	700	New Khartoum International Airport	China Harbor Engineering Co.
Senegal	2015	699	Thies-Touba Toll Highway	China Road and Bridge Co.
Nigeria	2010	629	Railway Modernization Project	China Civil Engineering Construction Co.

Table A.6: The 10 Largest OOF-Like Contracts by Amount (constant 2014 USD million)

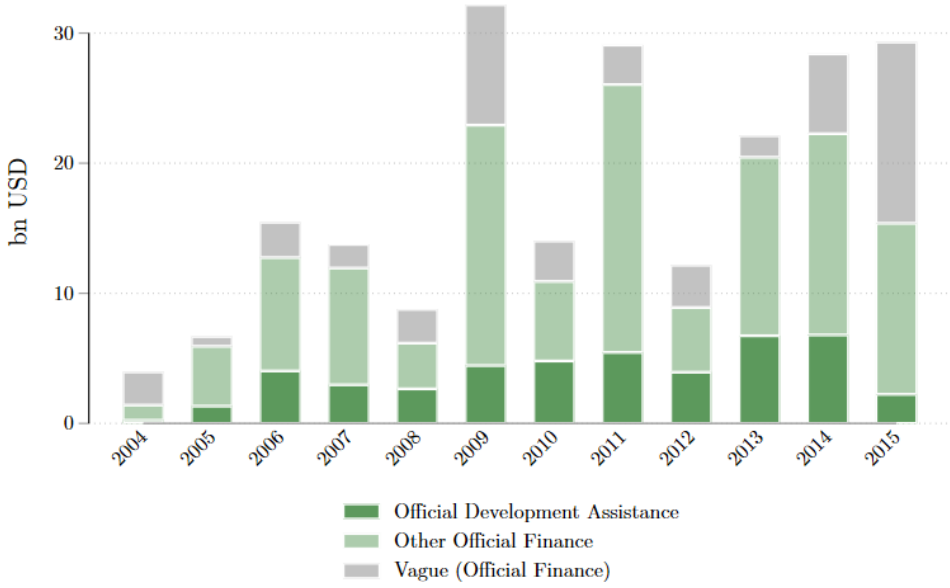
Country	Year	Amount	Short Description	Contractor
Turkmenistan	2009	4551	South Yolotan Osman Field Development	Chuanqing Exploratory Drilling Engineering Co.
Venezuela	2011	4440	Housing Projects	China CITIC Construction Co.
Myanmar	2009	3257	Sino-Myanmar Pipeline	China Petroleum Engineering and Construction Co.
Angola	2010	3144	Kilamba Kiaxi New Town	China CITIC Construction Co.
Belarus	2013	3050	China-Belarus Industrial Park	China CAMC Engineering Co.
Egypt	2015	2898	New Administrative Capital Infrastructure	China State Construction Engineering Co.
Bangladesh	2015	2615	Padma Rail Link	China Railway First Group Co.
Uzbekistan	2011	2422	Central Asia-China Gas Pipeline	China Petroleum Engineering and Construction Co.
Iran	2014	2143	Railway Electrification	China National Machinery Import and Export Co.
Argentina	2014	2090	Belgrano Cargas Train Line	China Machinery Engineering Corporation

Figure A.3: Financial Value of Projects, by Sector and Funding Agency



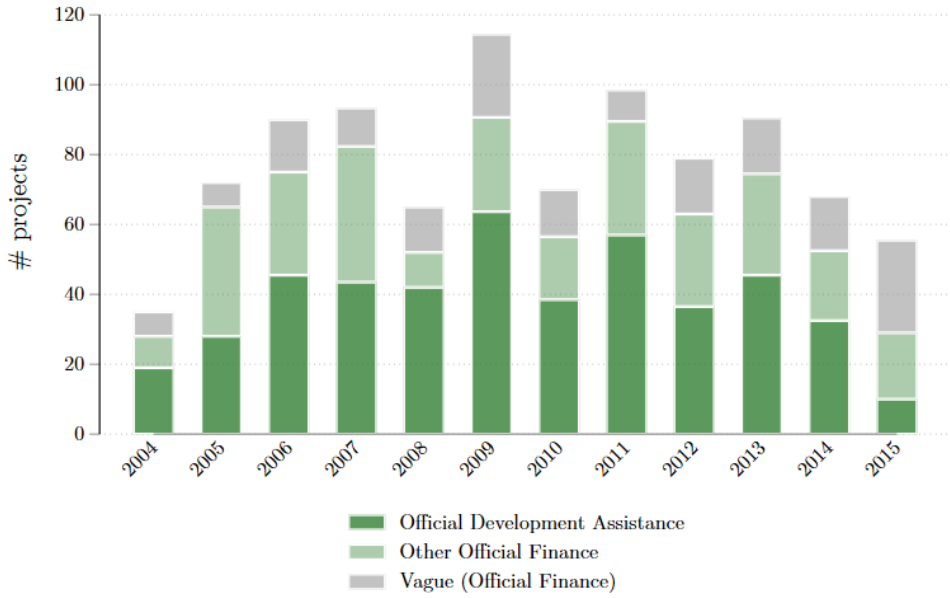
Note: This figure shows the total financial value of all Chinese aid projects implemented by Chinese firms in the sample by sector and funding agency (Export-Import Bank of China (Ex-Im Bank), China Development Bank (CDB), other state-owned banks, central government ministries and other government entities). Financial amounts are in constant 2014 USD.

Figure A.4: Financial Value of Projects, by Year and Type of Finance



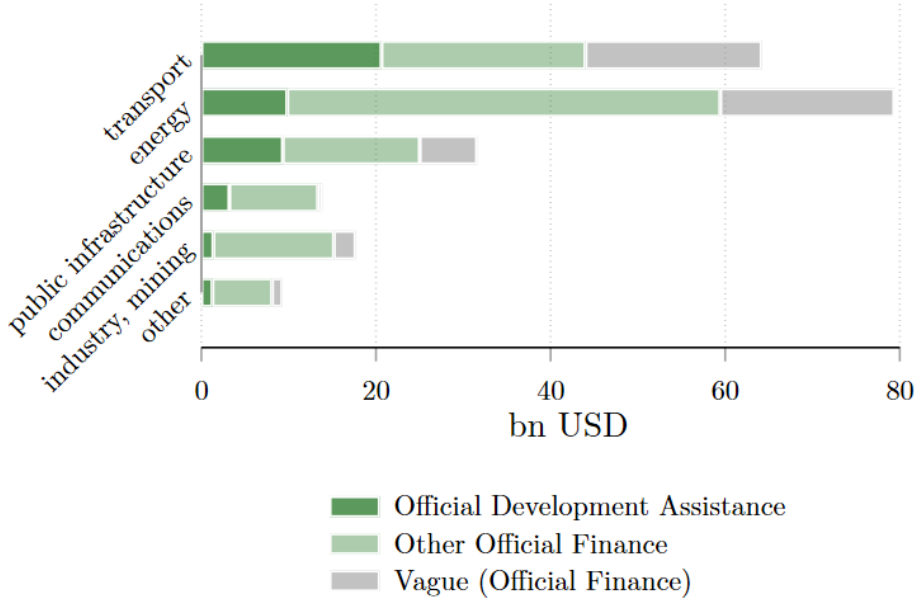
Note: This figure shows the total financial value of all Chinese aid projects implemented by firms in the sample by year and type of finance. Financial amounts are in constant 2014 USD.

Figure A.5: Number of Projects, by Year and Type of Finance



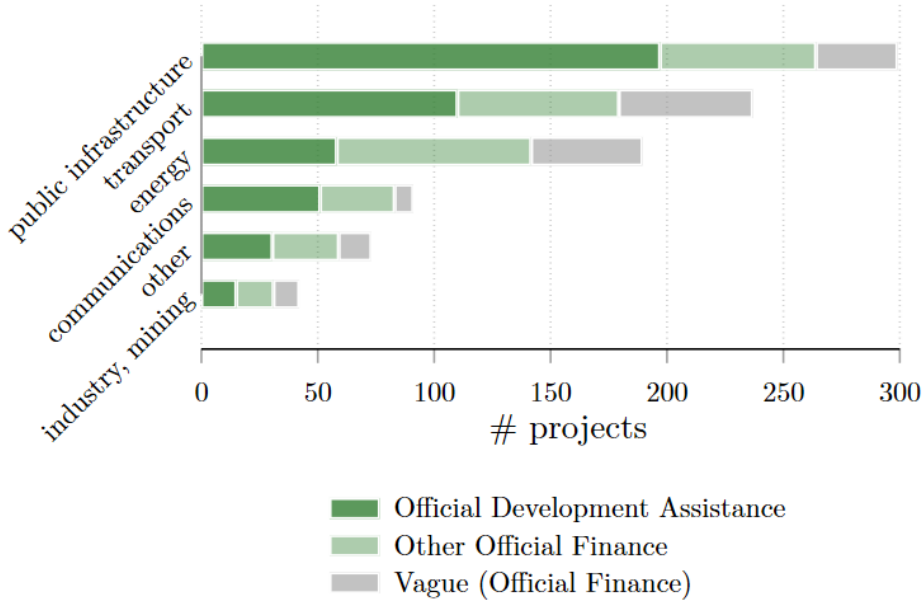
Note: This figure shows the total number of all Chinese aid projects implemented by firms in the sample by year and type of finance.

Figure A.6: Financial Value of Projects, by Sector and Type of Finance



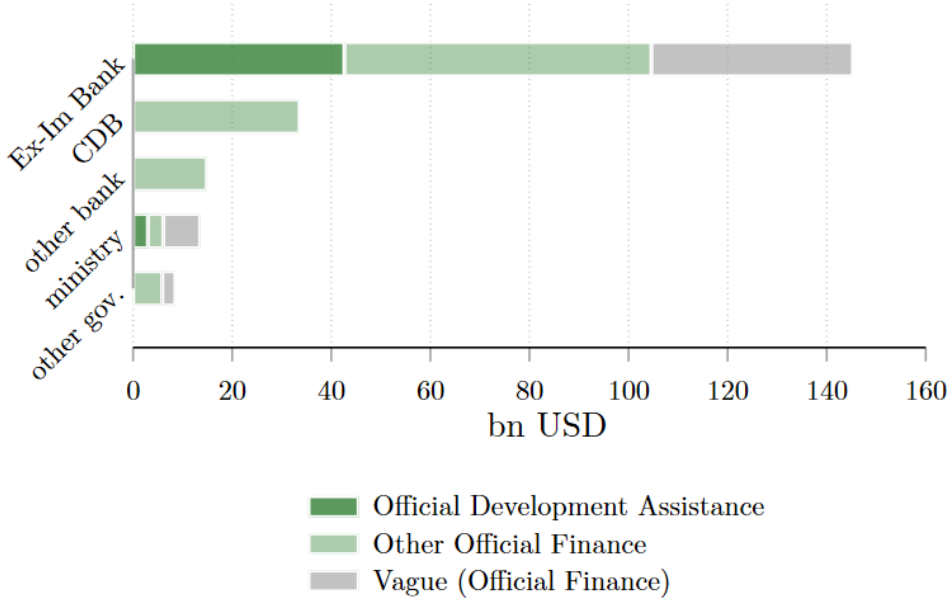
Note: This figure shows the total financial value of all Chinese aid projects implemented by firms in the sample by the sector of the project and type of finance. Financial amounts are in constant 2014 USD.

Figure A.7: Number of Projects, by Sector and Type of Finance



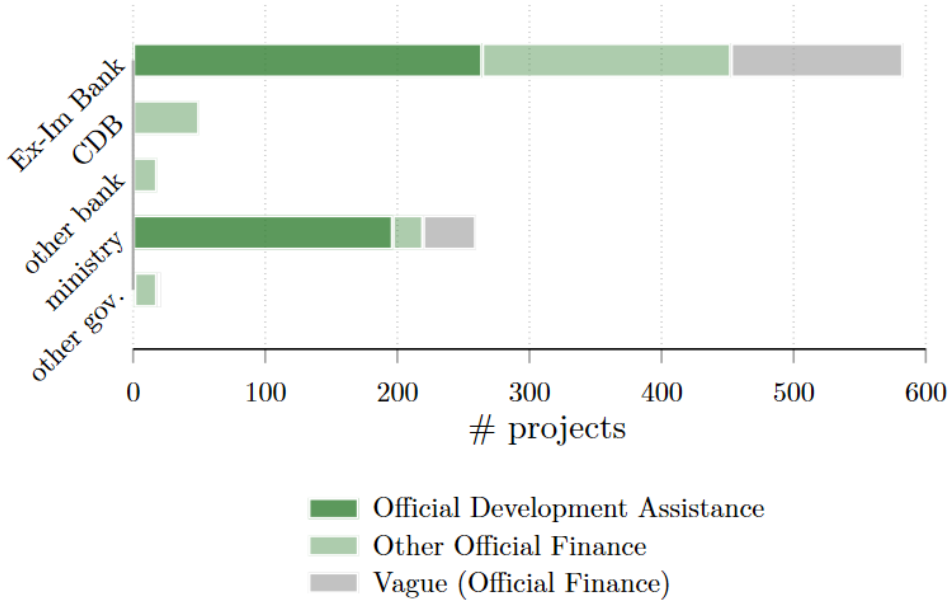
Note: This figure shows the total number of Chinese aid projects implemented by firms in the sample by the sector of the project and type of finance.

Figure A.8: Financial Value of Projects, by Funding Agency and Type of Finance



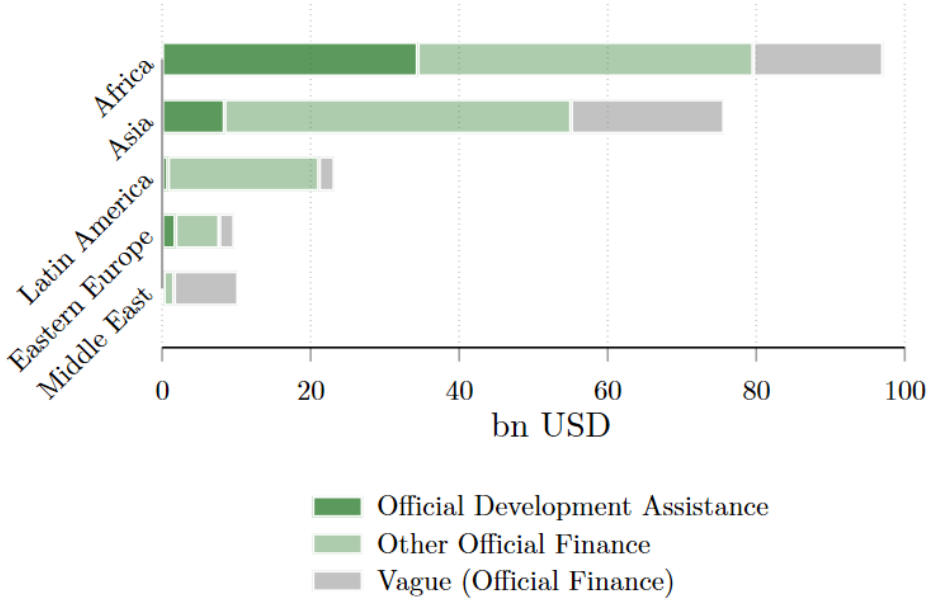
Note: This figure shows the total financial value of all Chinese aid projects implemented by firms in the sample by the funding agency of the project and type of finance. Financial amounts are in constant 2014 USD.

Figure A.9: Number of Projects, by Funding Agency and Type of Finance



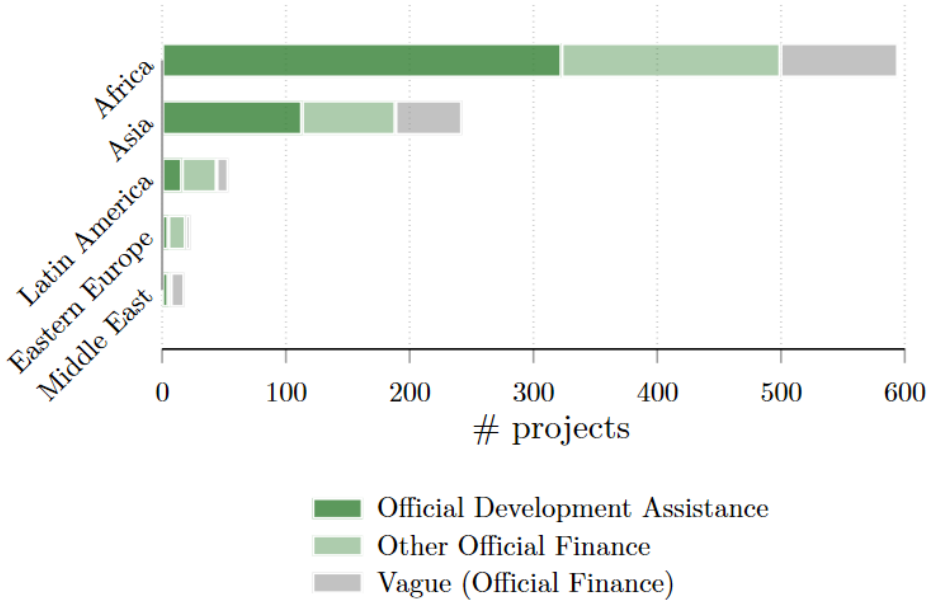
Note: This figure shows the total number of Chinese aid projects implemented by firms in the sample by the funding agency of the project and type of finance.

Figure A.10: Financial Value of Projects, by Region and Type of Finance



Note: This figure shows the total financial value of all Chinese aid projects implemented by firms in the sample by recipient region and type of finance. Financial amounts are in constant 2014 USD.

Figure A.11: Number of Projects, by Region and Type of Finance



Note: This figure shows the total number of Chinese aid projects implemented by firms in the sample by recipient region and type of finance.

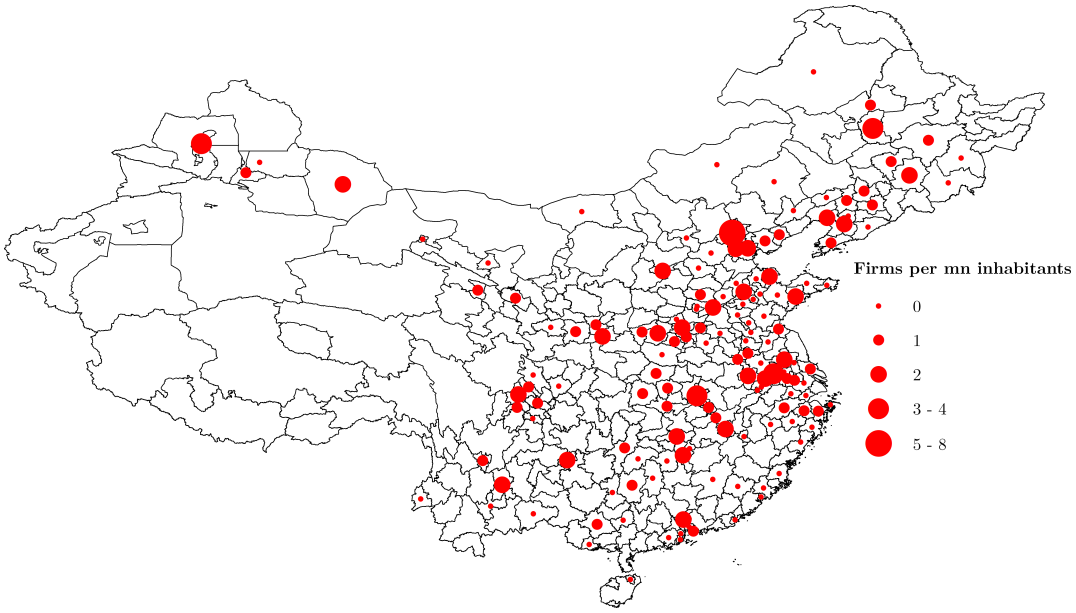
C.2. Firm-level Data

Table A.7: Descriptive Statistics, Firm-Level Variables

	N	Mean	SD	Min	Max
Panel A. Central State-Owned Firms					
# yearly Chinese aid projects	3,532	0.154	0.688	0	13
yearly Chinese aid contract value (mn)	3,532	45.57	280.5	0	4,570
# employees*	1,095	1,408	2,350	18	20,224
fixed assets, mn*	1,000	100	210	0	2,082
operating income, mn*	1,060	540	741	0	4,648
exports, mn*	1,214	128	211	0	1,329
Panel B. Other Firms					
# yearly Chinese aid projects	5,109	0.048	0.332	0	9
yearly Chinese aid contract value (mn)	5,109	3.238	37.81	0	1,227
# employees*	1,444	1,152	1,966	18	19,655
fixed assets, mn*	1,326	56	141	0	1,932
operating income, mn*	1,445	313	523	1	4,612
exports, mn*	1,757	81	165	0	1,225

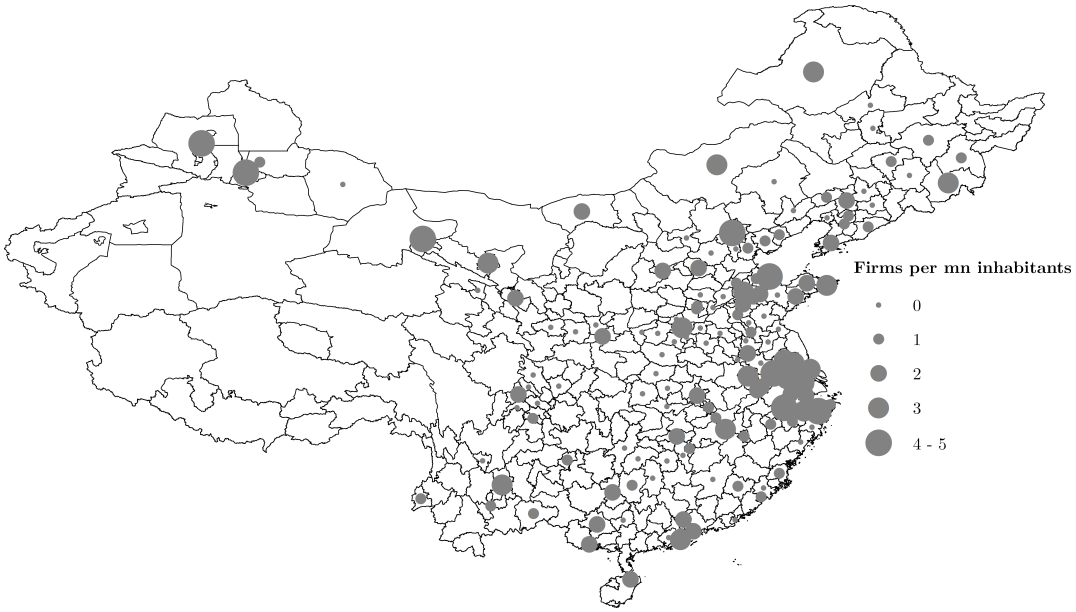
Note: Financial amounts in real 2014 USD. *Variables are from the firms in the 2007-2015 tax survey, which includes a random subsample of Chinese firms. See Appendix B.4 for details.

Figure A.12: Distribution of Central State-Owned Firms Across China



Note: This map shows the locations of SASAC firms in the sample across Chinese prefectures. Each dot is proportional in size to the number of firms per million inhabitants in each prefecture.

Figure A.13: Distribution of Other Firms Across China



Note: This map shows the locations of non-SASAC firms in the sample across Chinese prefectures. Each dot is proportional in size to the number of firms per million inhabitants in each prefecture.

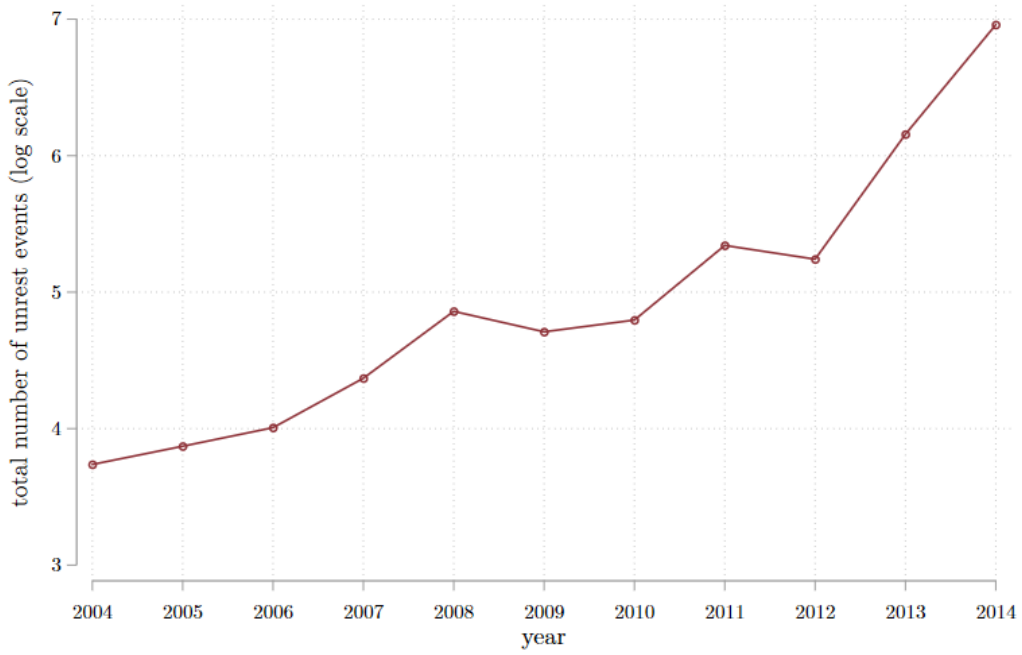
C.3. Prefecture-level Data

Table A.8: Descriptive Statistics: Prefecture-Level Variables

VARIABLE	N	mean	SD	min	max
# labor unrest events per capita	1,339	0.19	0.43	0.00	4.32
population (million)	1,339	5.99	45.43	0.25	31.30
urban labor force employment rate, total (%)	1,251	96.90	1.90	59.02	99.99
urban labor force employment rate, SOEs (%)	1,251	54.68	11.84	17.77	91.61
urban labor force employment rate, private (%)	1,251	42.22	12.10	4.78	81.31
local government expenditures (billion USD)	1,270	62.22	94.31	1.18	1,007.84
local government income (billion USD)	1,270	45.63	82.11	0.96	898.44
GDP per capita (USD)	1,269	7,784	4,521	1,028	31,049
average wage of employees in urban areas (USD)	1,259	6,751	2,224	2,131	1,841
exports per capita (USD)	1,271	3,633	11,080	3.03	129,577
imports per capita (USD)	1,271	2,434	6,532	0.61	69,798

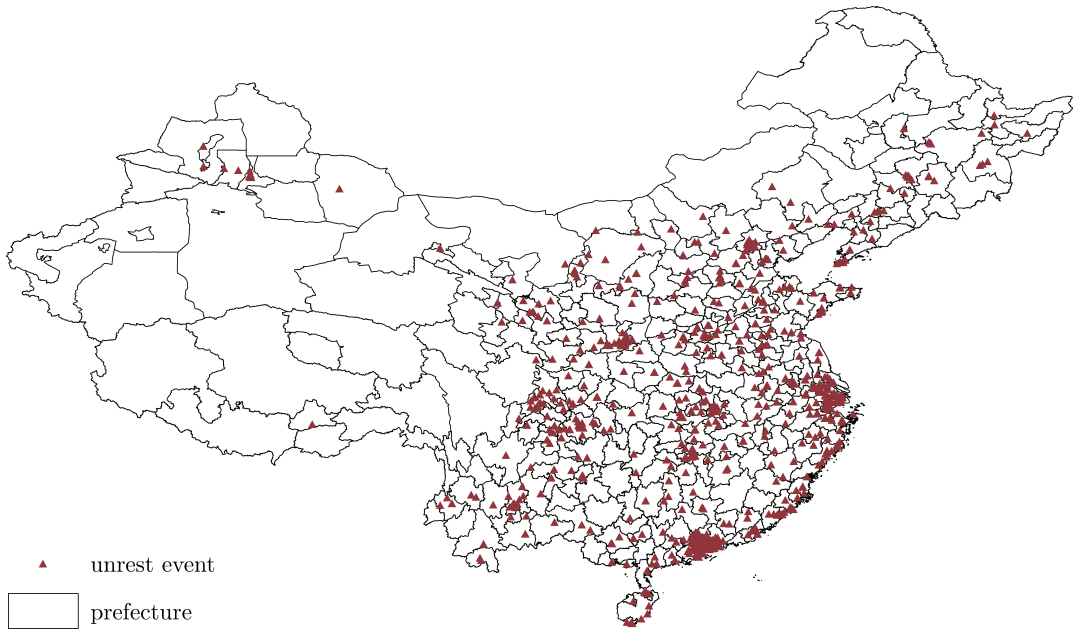
Note: This table shows descriptive statistics for prefecture-level variables for prefectures in the sample, 2004–2014. Unrest data for 2004–2011 is from the China Strikes Crowdmap and for 2012–2014 from the China Labour Bulletin (CLB). Data on exports is from the Chinese Customs Trade Statistics (CCTS). All other variables are based on data from the China City Statistical Yearbooks. Financial values are in 2014 real USD.

Figure A.14: Total Yearly Number of Unrest Events Over Time



Note: This figure shows the log total number of labor unrest events in the sample by year. Data for 2004–2011 is from the China Strikes Crowdmap and data for 2012–2014 is from the China Labour Bulletin (CLB).

Figure A.15: Distribution of Unrest Events Across China, 2004–2014



Note: This figure shows the locations of all labor unrest events in the sample, 2004–2014. Data for 2004–2011 is from the China Strikes Crowdmap and data for 2012–2014 is from the China Labour Bulletin (CLB). The black lines indicate prefecture borders.

C.4. Country-level Data

Table A.9: Descriptive Statistics: Country-Level Variables

VARIABLE	N	mean	SD	min	max
# aid projects	1,496	0.32	1.01	0.00	14.00
# ODA projects	1,496	0.12	0.43	0.00	5.00
# OOF projects	1,496	0.12	0.62	0.00	11.00
aid value (log)	1,496	3.05	7.04	0.00	22.62
ODA value (log)	1,496	1.46	4.99	0.00	20.92
OOF value (log)	1,496	1.32	4.93	0.00	22.56
lagged weighted labor unrest	1,496	0.00	1.00	-4.54	6.89
imports from China (bn USD)	1,496	4.06	8.44	0.00	65.94
FDI from China (bn USD)	1,496	0.23	1.01	0.00	17.47
GDP growth (%)	1,474	4.31	4.46	-36.39	34.47
GDP per capita (USD)	1,470	4,979	4,741	221	21,561
government consumption per capita (USD)	1,197	845	958	16	5,415
household consumption per capita (USD)	1,197	2,973	2,545	190	10,264
capital formation per capita (USD)	1,196	1,252	1,306	-2,744	6,577
imports per capita (USD)	1,492	4,414	18,967	41	294,719
exports per capita (USD)	1,492	2,330	3,920	5	27,150
imports from China per capita (USD)	1,492	430	2,759	1	60,070
exports to China per capita (USD)	1,492	139	419	0	5,068
unemployment rate	1,419	8.00	6.25	0.21	37.25
# diplomatic visits	1,496	1.70	4.39	0.00	52.00

Note: Descriptive statistics for sample used in Table 6. Financial amounts are in 2014 constant USD. Data sources are described in Section 4.2.

Table A.10: Descriptive Statistics: Correlates with Chinese Aid Allocation to Other Countries

VARIABLE	N	mean	SD	min	max
UN General Assembly voting alignment	1,213	0.96	0.07	0.60	1.00
diplomatic relations with Taiwan (dummy)	1,213	0.12	0.33	0.00	1.00
trade with China (log)	1,213	20.49	2.16	12.04	25.32
petroleum exporter (dummy)	1,213	0.50	0.50	0.00	1.00
government debt (% of GDP)	1,213	49.58	40.79	1.98	487.45
democracy (polity score)	1,213	13.06	5.73	0.00	20.00
GDP per capita (log)	1,213	7.51	1.12	4.81	9.62
population (log)	1,213	16.18	1.52	13.03	20.97
English is official language (dummy)	1,213	0.26	0.44	0.00	1.00

Note: Descriptive statistics for sample used in Table A.11. Data from Dreher et al. (2021).

C.5. Correlates of Chinese Aid With Recipient Country Characteristics

In this section, I analyze the factors associated with the global allocation of Chinese aid. Table A.11 shows correlations between recipient country characteristics (data from Dreher et al. (2021)) and the amount of Chinese aid received by countries (aid implemented by Chinese SASAC firms in my sample during 2005–2015). Table A.10 shows descriptive statistics for the country characteristics included in the analysis. Each column in Table A.11 shows the coefficient of an OLS regression of the measure of Chinese aid indicated in the column head on the listed lagged country characteristics, controlling for year fixed effects. Standard errors are clustered at the country level.

Consistent with Dreher et al. (2021), I observe that, conditional on other characteristics, countries are more likely to receive Chinese aid if their voting in the UN General Assembly is more aligned with China and less likely to receive Chinese aid if they have diplomatic relations with Taiwan. This result is consistent with Alesina and Dollar (2000) who show that Western donors reward recipient countries for political alliances. Countries with deeper trade relationships with China, and countries whose official language is English, receive more aid. This is consistent with China using its aid to create commercial opportunities for the Chinese export economy. Poorer countries receive more concessional Chinese finance (ODA-like aid), perhaps due to a greater need for aid. Other country characteristics, including whether a country is more populous, more democratic or a petroleum exporter, have no large and statistically significant correlation with Chinese aid. The latter two coefficients are especially interesting since they go against the conventional wisdom and claims in the press that China uses aid to prop up autocratic regimes or to secure access to natural resources (e.g., Naim (2007)).

Nevertheless, these correlations are consistent with the notion that China may choose the countries it gives aid to based on economic and foreign policy goals. However, there may be alternative explanations for these patterns, and there may also be motives that affect the *timing* of Chinese aid to recipient countries. In Section 4, I go beyond descriptive evidence and show how a *domestic* political objective of the Chinese government influences the timing and size of its aid to other countries.

Table A.11: Correlates with Chinese Aid Allocation to Other Countries

	# contracts	# ODA contracts	# OOF contracts	contract value	ODA value	OOF value
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*0.689*	*0.315*	*0.213*	*5.339*	*3.161*	*2.082*
	(1.554)	*(0.765)*	*(0.839)*	*(8.578)*	*(6.907)*	*(6.053)*
UN General Assembly voting alignment	1.270**	0.495*	0.419	8.822**	5.710*	2.226
	(0.559)	(0.293)	(0.268)	(3.567)	(2.957)	(2.431)
diplomatic relations with Taiwan	-0.617***	-0.336***	-0.170***	-5.345***	-3.547***	-1.782***
	(0.158)	(0.099)	(0.059)	(0.897)	(0.762)	(0.469)
trade with China (log)	0.177**	0.041	0.066	0.686*	0.195	0.362
	(0.089)	(0.034)	(0.053)	(0.388)	(0.283)	(0.259)
petroleum exporter	-0.178	-0.139	-0.056	-1.608	-1.184	-0.858
	(0.240)	(0.124)	(0.122)	(1.150)	(0.873)	(0.945)
government debt (% of GDP)	-0.001	-0.002*	0.000	-0.011	-0.014**	-0.000
	(0.002)	(0.001)	(0.001)	(0.008)	(0.006)	(0.006)
democracy (polity score)	-0.013	0.000	-0.008	-0.117*	0.016	-0.086
	(0.013)	(0.006)	(0.006)	(0.067)	(0.047)	(0.053)
GDP per capita (log)	-0.316***	-0.177***	-0.054	-1.863***	-1.592***	-0.362
	(0.107)	(0.043)	(0.059)	(0.539)	(0.354)	(0.358)
population (log)	-0.031	-0.013	0.014	0.089	-0.015	0.371
	(0.103)	(0.038)	(0.058)	(0.496)	(0.344)	(0.333)
English is official language	0.267	0.249**	0.053	1.949*	2.713***	0.905
	(0.197)	(0.105)	(0.088)	(0.985)	(0.739)	(0.736)
# observations	1213	1213	1213	1213	1213	1213
# countries	122	122	122	122	122	122
adjusted R2	0.105	0.132	0.032	0.163	0.154	0.063

Note: The unit of observation is a country-year. Each column shows an OLS regression of the measure of aid indicated in the column head on the listed country characteristics, conditional on year fixed effects. Independent variables are lagged by one period. # aid projects: total number of aid projects, implemented by Chinese SASAC firms, received by country. Aid value: log(1 + total value of aid projects, implemented by Chinese SASAC firms, received by a country). Country-level data other than Chinese aid from Dreher et al. (2021). Standard errors are clustered at the country level. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

Appendix D. Additional Results: Firm- and Prefecture-level

D.1. Leads and Lags of Unrest

Table A.12: Contract Allocation and Local Unrest, Alternative Leads and Lags

	# contracts							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
dependent variable mean (SD)	*0.163*	*0.154*	*0.158*	*0.157*	*0.154*	*0.154*	*0.162*	*0.168*
	(0.672)	*(0.642)*	*(0.669)*	*(0.687)*	*(0.688)*	*(0.688)*	*(0.716)*	*(0.736)*
unrest, t-4	-0.023	0.005						
	(0.049)	(0.040)						
unrest, t-3	0.016		0.057					
	(0.053)		(0.035)					
unrest, t-2	0.024			0.027				
	(0.050)			(0.034)				
unrest, t-1	0.109**				0.103**			
	(0.048)				(0.041)			
unrest, t	-0.026					0.021		
	(0.029)					(0.013)		
unrest, t+1	-0.007						0.003	
	(0.021)						(0.009)	
unrest, t+2	0.010							0.015
	(0.016)							(0.015)
# observations	2175	2767	3044	3299	3532	3532	3152	2767
# firms	423	445	458	468	474	474	452	429
# prefectures	80	81	81	81	81	81	80	77
unrest mean (SD)	0.195	0.162	0.171	0.200	0.271	0.534	0.586	0.640
	(0.200)	(0.186)	(0.187)	(0.230)	(0.330)	(0.862)	(0.900)	(0.947)
adjusted R2	0.595	0.573	0.587	0.568	0.575	0.574	0.599	0.608

Note: The unit of observation is a firm-year. Sample includes SASAC firms only. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.2. Robustness to Controls

A threat to the causal interpretation of the observed effect of unrest on contract allocation is omitted variables which vary across both firms and time and are correlated with both local unrest and aid allocation. I address such concerns here by controlling for a host of potential correlates at the prefecture-year and firm-year level.

Local economic factors such as local GDP, exports, employment and wages may be negatively correlated with the occurrence of labor unrest, which is often caused by mass layoffs and wage arrears (Campante, Chor and Li, 2019). As Table A.13 shows, this may indeed be the case (although the correlations are relatively small and statistically insignificant). At the same time, these factors affect the marginal costs of aid contractors. For example, if wages in a prefecture decrease due to an economic shock, this may lead to collective action by the affected workers. At the same time, the lower wages may in equilibrium also decrease the labor costs of firms, causing firms to bid on aid contracts more aggressively. If this channel were important, this could be a non-political explanation for the observed relationship between local unrest and aid contract allocation to SASAC firms.

Table A.13: Prefecture-level Correlates with Unrest

	labor unrest			
	independent variable (standardized):			
	GDP per capita	exports per capita	employment rate (%)	average wage
	(1)	(2)	(3)	(4)
variable in column header, t	-0.112	0.046	-0.001	-0.135
	(0.111)	(0.075)	(0.007)	(0.108)
# observations	570	570	570	570
# prefectures	76	76	76	76
unrest mean (SD)	0.217	0.217	0.217	0.217
	(0.398)	(0.398)	(0.398)	(0.398)
adjusted R2	0.545	0.544	0.544	0.549

Note: The unit of observation is a prefecture-year. All prefecture-year level regressions include prefecture and year fixed effects and control for prefecture population. Unrest: number of unrest events per 1mn inhabitants. GDP and exports are in logs. All variables are standardized to have mean 0 and standard deviation 1 to facilitate interpretation. Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

To address this concern, Table A.14, which is otherwise identical to the baseline specification as in Table 1, controls for local economic factors (including lagged GDP per capita, exports per capita, average wages, employment and population at the prefecture-year level). The results are highly robust to the inclusion of these controls. Table A.15 controls for more prefecture-level variables. Column (1) for comparison replicates the baseline specification with the number of contracts as the outcome variable. Column (2) controls

for year fixed effects interacted with the prefecture's distance to important cities (Beijing, Shenzhen and Ürümqi)[64] to control for location-specific trends potentially correlated with variation in unrest over time. Column (3) includes year fixed effects that are allowed to vary by province to capture subnational macroeconomic shocks that may be correlated with unrest and contract allocation (a province is one administrative unit higher than a prefecture). Although this takes out some variation and halves the coefficient estimate (0.047) compared to the baseline (0.103), the coefficient remains statistically significant at the 5% level. Column (4) controls for average lagged unrest in neighboring prefectures. The coefficient of interest is unchanged compared to the baseline. Unrest in neighboring prefectures has no effect on contract allocation, strengthening the notion that aid projects are used to react to *local* unrest.

Table A.14: Contract Allocation and Local Unrest, Controlling for Prefecture Characteristics

	# contracts	# ODA contracts	# OOF contracts	contract value	ODA value	OOF value
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*0.156*	*0.044*	*0.040*	*1.418*	*0.506*	*0.549*
	(0.696)	*(0.272)*	*(0.282)*	*(5.045)*	*(3.005)*	*(3.228)*
unrest,t-1	0.098***	0.035***	0.045**	0.639**	0.362**	0.534**
	(0.032)	(0.012)	(0.018)	(0.266)	(0.147)	(0.220)
# observations	3434	3335	3335	3403	3316	3331
# firms	473	471	471	473	470	471
# prefectures	80	80	80	80	80	80
unrest mean (SD)	0.267	0.265	0.265	0.267	0.265	0.265
	(0.318)	(0.319)	(0.319)	(0.319)	(0.320)	(0.319)
adjusted R2	0.575	0.336	0.181	0.422	0.227	0.169

Note: The unit of observation is a firm-year. Sample includes SASAC firms only. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Contract value: log(1 + total value of aid contracts implemented by firm). Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. The prefecture-year level control variables are based on data from the *China City Statistical Yearbooks* and are standardized to have mean 0 and standard deviation 1. They include log GDP per capita, log exports per capita, urban employment rate, average wage and log population (all lagged). Standard errors are clustered at the prefecture level. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

There may also be potential omitted variables at the firm-year level correlated with local unrest and contract allocation. For example, Dreher et al. (2021) argue that China gives foreign aid to other countries to reduce overproduction of construction materials. At the

[64] Beijing is the seat of the central government and home to many SASAC firms. Shenzhen is a major manufacturing hub with a high incidence of labor unrest. Ürümqi is the capital of the politically unstable Xinjiang Uyghur Autonomous Region.

Table A.15: Contract Allocation and Local Unrest, Controlling for Spatial Characteristics

	# contracts			
	controlling for:			
	baseline	year × distances to major cities	province × year FE	unrest in neighboring prefectures
	(1)	(2)	(3)	(4)
dependent variable mean (SD)	*0.154*	*0.154*	*0.154*	*0.154*
	(0.688)	*(0.688)*	*(0.690)*	*(0.688)*
unrest,t-1	0.103**	0.067**	0.047**	0.101***
	(0.041)	(0.031)	(0.021)	(0.035)
neighboring prefectures unrest,t-1				0.006
				(0.039)
# observations	3532	3532	3505	3532
# firms	474	474	471	474
# prefectures	81	81	79	81
unrest mean (SD)	0.271	0.271	0.271	0.271
	(0.330)	(0.330)	(0.325)	(0.330)
adjusted R2	0.575	0.573	0.556	0.575

Note: The unit of observation is a firm-year. Sample includes SASAC firms only. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. Major cities in the specification in Column (2) include Beijing, Shenzhen and Ürümqi. Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

same time, firms may be more likely to have overcapacity in terms of inputs during periods of unrest in their home prefectures. Table A.16 addresses such concerns by replicating the baseline analysis for the subset of firms linked to the tax survey data, controlling for firms' lagged number of employees, fixed assets, operating income, exports and materials inventory. The coefficient of interest is robust to the inclusion of these controls.

Table A.16: Contract Allocation and Local Unrest, Controlling for Firm Characteristics

	# contracts				
	controlling for:				
	# employees, t-1	fixed assets, t-1	operating income, t-1	exports, t-1	inventory, t-1
	(1)	(2)	(3)	(4)	(5)
dependent variable mean (SD)	*0.248*	*0.198*	*0.205*	*0.200*	*0.231*
	(0.855)	*(0.762)*	*(0.770)*	*(0.708)*	*(0.833)*
unrest,t-1	0.114**	0.112**	0.084*	0.072**	0.133*
	(0.051)	(0.049)	(0.048)	(0.035)	(0.079)
# observations	1213	1139	1173	1214	861
# firms	276	272	276	244	222
# prefectures	61	59	60	58	51
unrest mean (SD)	0.303	0.297	0.300	0.320	0.205
	(0.345)	(0.339)	(0.332)	(0.362)	(0.212)
adjusted R2	0.606	0.575	0.569	0.596	0.639

Note: The unit of observation is a firm-year. The sample includes SASAC firms with >10 employees that were selected to participate in the 2007-2015 tax surveys by the State Tax Administration. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. All firm-level controls are in logs. Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.3. Differences Between Central State-Owned and Other Firms

A potential concern with the placebo check using non-SASAC firms is that SASAC firms differ from other firms in characteristics other than their political role. Table A.17 addresses such concerns. It uses the subsample of firms that are publicly listed or subsidiaries of publicly listed firms, for which we have more detailed information on firm characteristics than for the full sample. Column (1) of Table A.17 regresses the number of contracts allocated to a firm in a year on the lagged number of local unrest events as well as the interaction between the lagged number of local unrest events and a time-invariant dummy that equals 1 if the firm is administered by the SASAC. In addition to the usual firm fixed effects, it also controls for year fixed effects that are allowed to differ between SASAC and non-SASAC firms. The interpretation of the interaction coefficient is analogous to the difference between the coefficients of Column (1) of Tables 1 and 2. The logic of this specification is in the spirit of a difference-in-difference specification. Column (1) of Table A.17 shows that the interaction coefficient for this subsample of firms is very similar to the difference between the coefficients in Column (1) of Tables 1 and 2 which use the full sample.

In Columns (2) to (5) of Table A.17, I control for the interaction between unrest and fixed firm characteristics that are potentially correlated with the SASAC dummy.[65] In Column (2), this is a dummy that equals 1 if the firm is in a sector in which the central government has a monopoly (Chen (2008)). These are sectors which also supply a significant fraction of China's aid (e.g., oil exploration and processing). Hence, the ineligibility of non-SASAC firms for certain aid projects due to their sector could also explain why I find no effect of local unrest on these firms. However, Column (2) of Table A.17 shows that this is not the case. If anything, the estimated difference in the effect of unrest for SASAC and non-SASAC firms is larger when controlling for the interaction between unrest and the sector dummy. In Column (3), I control for the interaction of unrest and a dummy that equals 1 if the firm has political connections to high-ranking members of the central government. If SASAC firms are more politically connected to high-ranking government officials than other firms, this could also explain why aid projects are more likely to be allocated to SASAC firms during periods of unrest, even if no political motive is present. However, Column (3) shows that this cannot explain the differential effect of unrest for SASAC and non-SASAC firms. Column (4) controls for the interaction between unrest and the total number of aid contracts a firm supplies in 2005–2015. As Column (4) shows, Although SASAC firms on average implement more aid projects than other firms, again this cannot explain the differential effect of unrest for the two types of firms. Finally, Column (5) controls for the interactions between unrest and the three firm characteristics in Columns (2) to (4) in the same specification. The main result remains robust.

[65] The uninteracted variables are absorbed by the firm fixed effects.

Table A.17: Contract Allocation and Local Unrest, Controlling for Differences Between Central State-Owned and Other Firms

	# contracts				
	(1)	(2)	(3)	(4)	(5)
dependent variable mean (SD)	*0.117*	*0.117*	*0.117*	*0.117*	*0.117*
	(0.585)	*(0.585)*	*(0.585)*	*(0.585)*	*(0.585)*
unrest,t-1	0.006	0.012	-0.022	0.007	-0.012
	(0.010)	(0.010)	(0.020)	(0.009)	(0.016)
unrest, t-1 × central state-owned firm	0.122**	0.162**	0.196**	0.128*	0.214**
	(0.055)	(0.071)	(0.082)	(0.065)	(0.101)
unrest, t-1 × monopoly sector		-0.090*			-0.057
		(0.052)			(0.039)
unrest, t-1 × political connections			-0.051**		-0.041*
			(0.025)		(0.022)
unrest, t-1 × total # contracts				-0.017	-0.018
				(0.053)	(0.052)
# observations	3909	3909	3909	3909	3909
# firms	563	563	563	563	563
# prefectures	106	106	106	106	106
unrest mean (SD)	0.273	0.273	0.273	0.273	0.273
	(0.379)	(0.379)	(0.379)	(0.379)	(0.379)
adjusted R2	0.573	0.573	0.573	0.573	0.574

Note: The unit of observation is a firm-year. Sample includes only listed firms and their subsidiaries. All firm-year level regressions include firm, year and year × SASAC fixed effects. # contracts: total number of aid contracts implemented by firm. Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. Data on firm characteristics is from the CSMAR listed firms database. *Monopoly sector* is a dummy that equals 1 if the listed firm is in one of the state monopoly sectors (Chen, 2008): 1) oil and gas exploration, 2) petroleum processing, 3) coking and nuclear fuel processing, 4) tobacco, 5) electricity, heat, gas and water production and supply, 6) railway transport, 7) postal service, 8) telecommunications and other information transmission services, 9) financial services, 10) non-ferrous and ferrous metals exploration and processing. *Political connections* is a dummy that equals 1 if at least one of the firm's top executives or members of the board is ever a full or alternate member of the Central Committee during or after being affiliated with the firm, and 0 otherwise. *Total contracts* is equal to the total value of aid contracts the firm implements during all years it is in the sample. Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.4. Alternative Specifications and Other Robustness

D.4.1. Contract Allocation and Employment at the Prefecture Level

Table A.18 repeats the analysis at the prefecture- instead of the firm level. Columns (1) and (2) show that unrest significantly increases the total number of aid contracts allocated to SASAC firms in a prefecture but has no effect on aid contracts allocated to other firms. Column (3) pools both types of firms and shows that lagged unrest has a positive net effect on the total number of contracts allocated to firms in the prefecture: One additional unrest event per million inhabitants is estimated to increase the number of contracts per million inhabitants by around 23% of the mean ($0.012 \div 0.053 = 0.226$). These results are consistent with a re-allocation of contracts across prefectures rather than a re-allocation from non-SASAC to SASAC firms within a prefecture. Recall that the main specification includes year fixed effects, so the effect of unrest on contracts is always relative to China's total amount of aid in a year – in other words, a re-allocation of contracts within the country.

Columns (4) to (6) repeat this exercise for employment. The closest available measure at the prefecture level is the employment rate among the urban labor force from the China City Statistical Yearbooks data, distinguishing between employment in SOE-like and private firms.[66] Consistent with the mechanism, while state-owned enterprises collectively increase employment by 1.11 percentage points ($0.414 \times 0.027 = 0.011$) following a one standard deviation increase in unrest in their prefecture, private firms decrease employment by the same amount (Columns (5) and (6)). This is consistent with Wen (2020) who shows that state-owned firms, but not private firms, hire ethnic minority males in response to local political unrest related to ethnic grievances. The result could be explained by unrest co-moving with local economic downturns which decrease labor demand by private firms. However, such downturns do not affect the allocation of aid contracts to private firms. Finally, Column (6) shows that the net effect of lagged unrest on the employment rate is zero, precisely because state-owned firms stabilize employment (partly using foreign aid projects).

[66] SOE-like firms in this data include not only SASAC firms but also a few other types of firms, such as collective firms and firms administered by lower-ranked local governments.

Table A.18: Contract Allocation and Local Unrest, Prefecture-Level

	# contracts per mn inhabitants			urban labor force employment rate (%)		
	including contracts from:			in:		
	state-owned firms	other firms	any firm	state-owned firms	other firms	any firm
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*0.028*	*0.024*	*0.053*	*0.539*	*0.431*	*0.970*
	(0.178)	*(0.109)*	*(0.218)*	*(0.118)*	*(0.121)*	*(0.018)*
unrest,t-1	0.014**	-0.001	0.012*	0.027**	-0.028**	-0.001
	(0.006)	(0.003)	(0.007)	(0.011)	(0.011)	(0.001)
# observations	1339	1339	1339	1248	1248	1248
# prefectures	149	149	149	142	142	142
unrest mean (SD)	*0.192*	*0.192*	*0.192*	*0.193*	*0.193*	*0.193*
	(0.426)	*(0.426)*	*(0.426)*	*(0.414)*	*(0.414)*	*(0.414)*
adjusted R2	0.795	0.314	0.692	0.678	0.679	0.502

Note: The unit of observation is a prefecture-year. All prefecture-year level regressions include prefecture and year fixed effects. # contracts: total number of aid contracts implemented by firms in the prefecture. Contract value: log(1 + total value of aid contracts implemented by firms in the prefecture). Unrest: number of unrest events per 1mn inhabitants. Number employed: log(number of employed individuals in prefecture). Standard errors are clustered at the prefecture level. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

D.4.2. Alternative Functional Forms

The main specification is estimated using OLS. On the one hand, linear regression estimates are transparent and have a natural interpretation. On the other hand, estimates may be inconsistent since the main outcome variables have many zeroes and the number of contracts is a count variable (Angrist and Pischke, 2008). The many zeroes might be an issue especially in the log-linear model, with log(1 + financial value of contracts) as the outcome variable, because hetero-skedasticity may lead to inconsistent point estimates (Silva and Tenreyro, 2006). I assess the importance of such concerns by replicating the main results using a non-linear Poisson Pseudo Maximum Likelihood (PPML) specification. PPML is widely used in the trade literature (Silva and Tenreyro, 2006) and increasingly also in the empirical aid literature (see, e.g., Fuchs and Vadlamannati (2013)) to address precisely this issue. Table A.19 Columns (3) and (4) report the estimates of the marginal effects of lagged unrest on the number and financial value of contracts allocated to a firm in a year. Note that the number of observations is smaller than in the main sample because, unlike with linear two-way fixed effects models, firms in the sample that never actually contract aid projects do not contribute to the non-linear PPML estimates (Correia, Guimarães and Zylkin, 2020). For comparison, in

Columns (1) and (2) I report the linear estimates for this sample.[67] The estimated marginal effects are very similar for the linear and the non-linear specification and highly statistically significant. This suggests that using the linear specification is unproblematic.

Table A.19: Contract Allocation and Local Unrest, PPML Specification

	OLS		Pseudo Maximum Likelihood	
	# contracts	contract value	# contracts	contract value
	(1)	(2)	(3)	(4)
dependent variable mean (SD)	*0.645*	*6.384*	*0.645*	*6.384*
	(1.292)	*(9.108)*	*(1.292)*	*(9.108)*
unrest,t-1	0.734***	6.213***	0.844***	6.284***
	(0.119)	(1.857)	(0.145)	(1.873)
# observations	843	768	843	768
# firms	92	87	92	87
# prefectures	23	23	23	23
unrest mean (SD)	0.274	0.276	0.274	0.276
	(0.239)	(0.244)	(0.239)	(0.244)
Adjusted R2	0.518	0.227		
Pseudo R2			0.349	0.242
Wild Bootstrap p-value	0.001	0.026	0.001	0.026

Note: The unit of observation is a firm-year. Sample includes SASAC firms only. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Contract value: log(1 + total value of aid contracts implemented by firm). Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. Standard errors are clustered at the prefecture level. Statistical significance is represented by * p < 0.10, ** p < 0.05, *** p < 0.01.

D.4.3. Omitting Outliers, Prefectures and Time Periods

Although not threatening the internal validity of the results, another potential concern is that a few observations, locations or time periods drive the results. I address these concerns by systematically dropping important observations to see if the results remain similar without them. In Table A.20, I drop the observations in the top percentile in the sample in terms

[67] Note that the coefficients are larger than in the main results Table 1. This is expected given that only firms that ever actually contract aid projects are included in the sample. In addition, note that the number of prefectures in this sample drops to 23 prefectures. Since standard errors are clustered at the prefecture level, this may be a problem due to a small number of clusters (Cameron, Gelbach and Miller, 2008). I address this issue by reporting Wild Bootstrap p-values on the bottom of Table A.19. All coefficients remain statistically significant at the 5% or 1% level.

of Cook's distance, a measure of outliers. The coefficient estimates are very similar to the baseline estimates in Table 1.[68]

Table A.20: Contract Allocation and Local Unrest, Omitting Outliers

	# contracts	# ODA contracts	# OOF contracts	contract value	ODA value	OOF value
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*0.136*	*0.042*	*0.035*	*1.337*	*0.477*	*0.512*
	(0.577)	*(0.265)*	*(0.227)*	*(4.905)*	*(2.920)*	*(3.121)*
unrest,t-1	0.089**	0.033**	0.040**	0.636**	0.317*	0.572***
	(0.035)	(0.014)	(0.016)	(0.267)	(0.174)	(0.207)
# observations	3518	3424	3424	3487	3405	3419
# firms	474	470	470	474	469	470
# prefectures	81	80	80	81	80	80
unrest mean (SD)	0.271	0.270	0.270	0.271	0.270	0.270
	(0.330)	(0.332)	(0.332)	(0.332)	(0.332)	(0.332)
adjusted R2	0.588	0.336	0.147	0.405	0.213	0.164

Note: The unit of observation is a firm-year. Sample includes SASAC firms only. Observations with a Cook's Distance in the top percentile of the sample (outliers) are dropped. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Contract value: log(1 + total value of aid contracts implemented by firm). Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In Table A.21, I regress local unrest on the number of contracts allocated, dropping important prefectures one by one. I drop Beijing, Shanghai and Tianjin since they are among the largest cities in China. I drop Shenzhen and Xi'An since many SASAC firms supplying aid projects are located there. I drop Ürümqi since it is the capital of the politically unstable Xinjiang Uyghur Autonomous Region. The results are also robust to dropping other cities (results on request). Excluding Beijing from the sample in Column (1) leads to a lower coefficient on unrest (0.052) compared to the full sample (0.103). However, note that the mean of the dependent variable is also smaller because many SASAC firms that supply aid projects are located in Beijing. In terms of relative magnitudes, the effect is comparable to the baseline. Similarly, the effects are robust to dropping other important Chinese prefectures, as Columns (2) to (5) show.

[68] The results are also robust to using alternative measures of observations' influence on the estimation results, such as DFBETA (results on request).

Table A.21: Contract Allocation and Local Unrest, Dropping Major Prefectures

	# contracts					
	excluding:					
	Beijing	Shanghai	Tianjin	Shenzhen	Xi'An	Ürümqi
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*0.059*	*0.162*	*0.157*	*0.155*	*0.153*	*0.154*
	(0.336)	*(0.707)*	*(0.696)*	*(0.691)*	*(0.691)*	*(0.689)*
unrest,t-1	0.052**	0.100**	0.105**	0.127***	0.095**	0.104**
	(0.024)	(0.039)	(0.041)	(0.044)	(0.039)	(0.041)
# observations	2230	3326	3450	3501	3429	3525
# firms	313	444	461	470	459	473
# prefectures	80	80	80	80	80	80
unrest mean (SD)	0.269	0.275	0.275	0.265	0.265	0.271
	(0.398)	(0.332)	(0.332)	(0.305)	(0.325)	(0.330)
adjusted R2	0.357	0.576	0.575	0.575	0.586	0.575

Note: The unit of observation is a firm-year. Sample includes SASAC firms only. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In Table A.22, I cut the sample in four time periods, including the global financial crisis in 2007–2009 and Xi Jinping's term starting in 2013, and drop each of these periods from the sample one by one. The estimated coefficients are remarkably similar for the four different samples.

Table A.22: Contract Allocation and Local Unrest, Dropping Time Periods

	dependent variable: # contracts			
	excluding:			
	2004-2006	2007-2009	2010-2012	2013-2015
	(1)	(2)	(3)	(4)
dependent variable mean (SD)	*0.158*	*0.141*	*0.151*	*0.167*
	(0.669)	*(0.657)*	*(0.684)*	*(0.750)*
unrest,t-1	0.097**	0.104**	0.102**	0.103**
	(0.040)	(0.044)	(0.040)	(0.044)
# observations	3044	2656	2464	2381
# firms	458	464	462	403
# prefectures	81	80	81	75
unrest mean (SD)	0.299	0.306	0.302	0.169
	(0.346)	(0.358)	(0.369)	(0.184)
adjusted R2	0.588	0.546	0.563	0.613

Note: The unit of observation is a firm-year. Sample includes SASAC firms only. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. Standard errors are clustered at the prefecture level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.4.4. Alternative Standard Errors

The main results Table 1 reports robust standard errors clustered at the prefecture level since this is the unit of variation of the explanatory variable (Bertrand, Duflo and Mullainathan, 2004). Clustered standard errors account for correlation in outcomes within prefectures as well as serial correlation in local unrest over time. However, assignment of the "treatment" may also be correlated within groups along different dimensions, for example if unrest is correlated across prefectures. Table A.23 reports alternative standard errors for the preferred specification to address this possibility. Column (2) reports heteroskedasticity-robust Huber-White standard errors that do not account for clustering for comparison. Column (3) reports standard errors two-way clustered at the prefecture and conglomerate level (Cameron, Gelbach and Miller, 2011).[69] Column (4) two-way clusters at the prefecture and year level. Column (5) clusters at the province level (one administrative unit above the prefecture) to account for potential correlation in outcomes and unrest across prefectures. Column (6) reports two-way Conley (2010) standard errors allowing for spatial auto-correlation up to

[69] A conglomerate is a group of firms that contains multiple subsidiaries and affiliated companies. As a reminder, a firm the sample is a subsidiary. The 474 SASAC firms in the main sample belong to 76 conglomerates.

5000km and temporal auto-correlation up to 10 years using a Bartlett kernel. The standard errors in Columns (2) to (6) are very similar to or smaller than the baseline standard errors clustered at the prefecture level shown in Column (1).

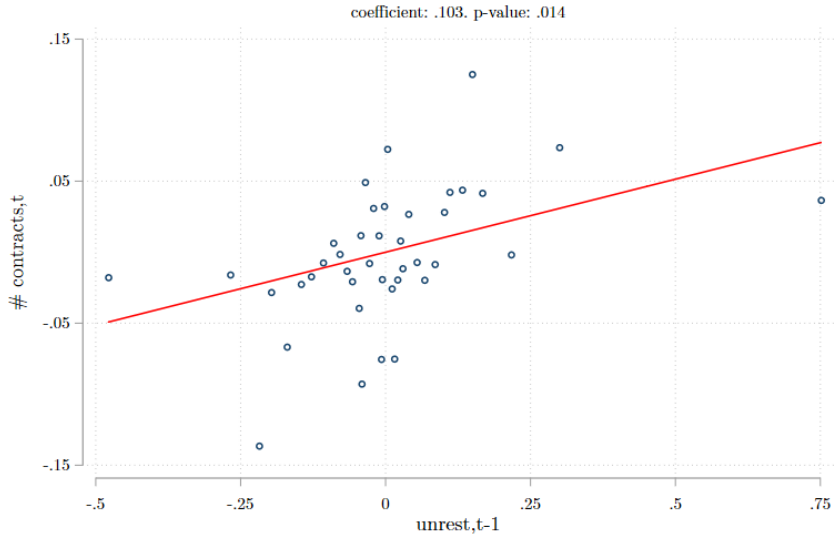
Table A.23: Contract Allocation and Local Unrest, Alternative Standard Errors

	# contracts					
	standard errors:					
	clustering at prefecture (baseline)	Huber-White (robust)	clustering at prefecture + conglomerate	clustering at prefecture + year	clustering at province	Conley (2008)
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*0.154*	*0.154*	*0.154*	*0.154*	*0.154*	*0.154*
	(0.688)	*(0.688)*	*(0.688)*	*(0.688)*	*(0.688)*	*(0.688)*
unrest,t-1	0.103**	0.103***	0.103**	0.103**	0.103**	0.103***
	(0.041)	(0.025)	(0.045)	(0.043)	(0.045)	(0.032)
# observations	3532	3532	3532	3532	3532	3532
# firms	474	474	474	474	474	474
# prefectures	81	81	81	81	81	81
unrest mean (SD)	0.271	0.271	0.271	0.271	0.271	0.271
	(0.330)	(0.330)	(0.330)	(0.330)	(0.330)	(0.330)
adjusted R2	0.575	0.575	0.575	0.575	0.575	0.575

Note: The unit of observation is a firm-year. Sample includes SASAC firms only. All firm-year level regressions include firm and year fixed effects. # contracts: total number of aid contracts implemented by firm. Unrest: number of unrest events per 1mn inhabitants in firm's prefecture. Standard errors as indicated in the Column heads (clustered at the prefecture level for the baseline). Two-way Conley standard errors (Conley, 2008) account for spatial auto-correlation up to 5000km (Bartlett kernel) and serial correlation up to 10 years. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

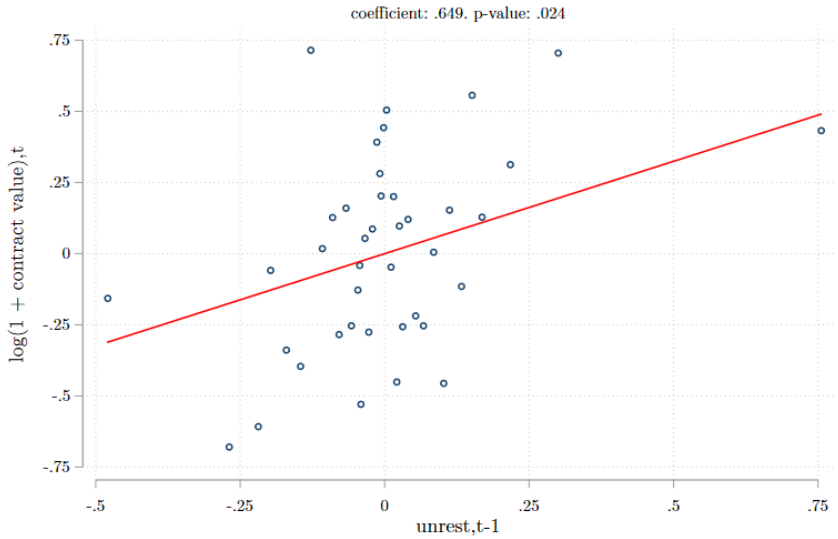
D.5. Additional Figures

Figure A.16: Local Unrest and Aid Contract Allocation to Central State-Owned Firms (# Contracts)



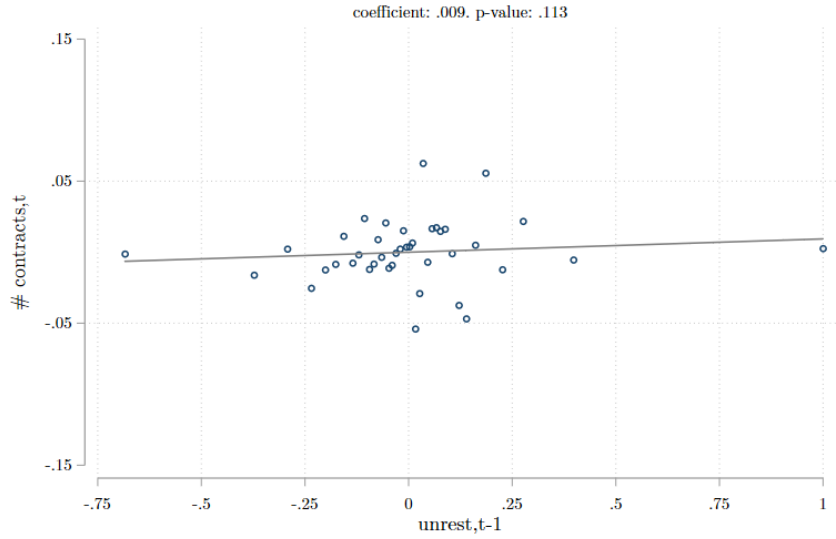
Note: The unit of observation is a firm-year. The red line shows the line of fit from an OLS regression of the residualized number of aid contracts implemented by a SASAC firm on residualized lagged number of unrest events per 1mn inhabitants in the firm's prefecture (residualizing firm and year fixed effects). The blue dots show mean residualized number of contracts for each of 40 bins.

Figure A.17: Local Unrest and Aid Contract Allocation to Central State-Owned Firms (Contract Value)



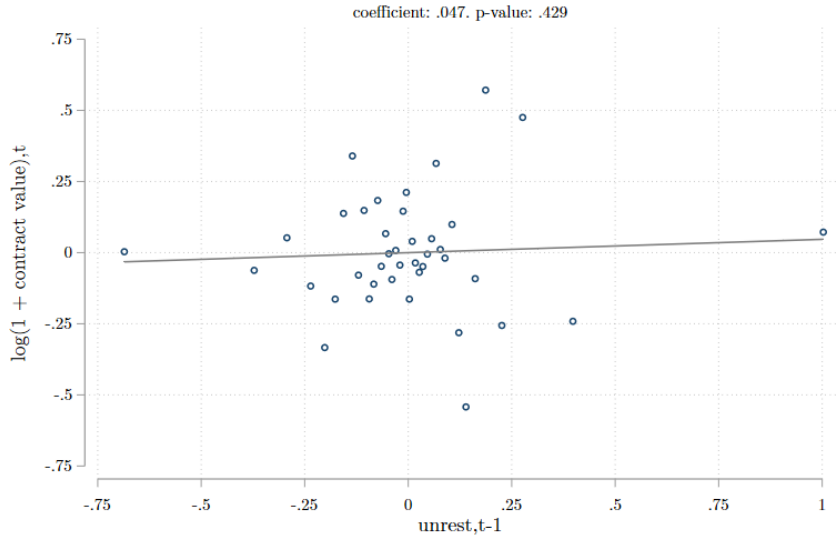
Note: The unit of observation is a firm-year. The red line shows the line of fit from an OLS regression of the residualized log financial value of aid contracts implemented by a SASAC firm on residualized lagged number of unrest events per 1mn inhabitants in the firm's prefecture (residualizing firm and year fixed effects). The blue dots show mean residualized number of contracts for each of 40 bins.

Figure A.18: Local Unrest and Aid Contract Allocation to Other Firms (# Contracts)



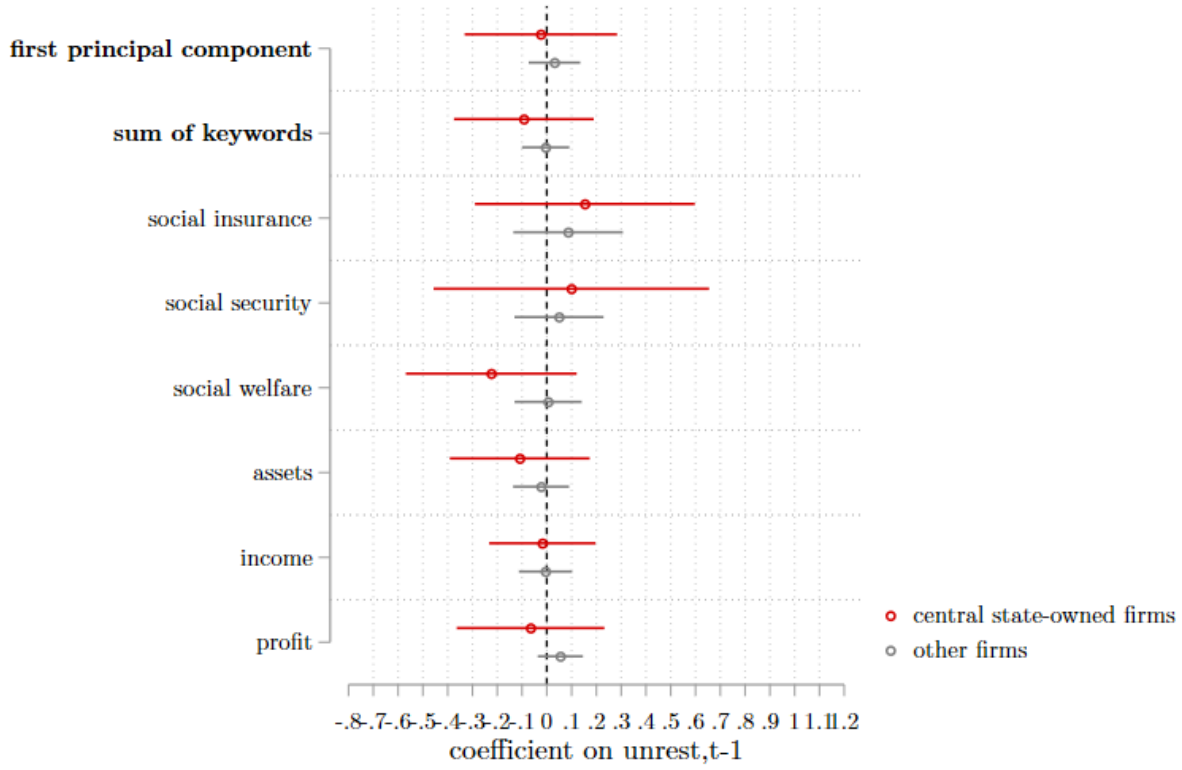
Note: The unit of observation is a firm-year. The red line shows the line of fit from an OLS regression of the residualized number of aid contracts implemented by a non-SASAC firm on residualized lagged number of unrest events per 1mn inhabitants in the firm's prefecture (residualizing firm and year fixed effects). The blue dots show mean residualized number of contracts for each of 40 bins.

Figure A.19: Local Unrest and Aid Contract Allocation to Other Firms (Contract Value)



Note: The unit of observation is a firm-year. The red line shows the line of fit from an OLS regression of the residualized log financial value of aid contracts implemented by a non-SASAC firm on residualized lagged number of unrest events per 1mn inhabitants in the firm's prefecture (residualizing firm and year fixed effects). The blue dots show mean residualized number of contracts for each of 40 bins.

Figure A.20: Frequency of Placebo Keywords in Annual Reports in Response to Unrest



Note: Each dot shows the coefficient from a separate OLS regression of the frequency of the phrase labeled on the y-axis in a firm's annual report at t, divided by the total number of words in the report, on unrest at t-1 as well as firm and year fixed effects. All variables are standardized to have mean 0 and standard deviation 1. Standard errors are two-way clustered at the conglomerate and prefecture level. SASAC firms: N=444. Non-SASAC firms: N=852.

Appendix E. Additional Results and Robustness: Country-level

Table A.24: List of Countries

Panel A. Countries that ever get Chinese aid projects implemented by central state-owned firms		
Afghanistan	Gabon	Nepal
Algeria	Ghana	Niger
Angola	Grenada	Nigeria
Argentina	Guinea	Pakistan
Bangladesh	Guinea-Bissau	Philippines
Belarus	Guyana	Russia
Benin	Indonesia	Rwanda
Bolivia	Iran	Senegal
Bosnia and Herzegovina	Jamaica	Seychelles
Botswana	Kazakhstan	Sierra Leone
Brazil	Kenya	Sri Lanka
Burundi	Kyrgyz Republic	Sudan
Cambodia	Laos	Tajikistan
Cameroon	Liberia	Tanzania
Central African Republic	Macedonia	Togo
Chad	Madagascar	Tonga
Congo	Malawi	Tunisia
Costa Rica	Malaysia	Turkey
Cote d'Ivoire	Maldives	Turkmenistan
Democratic Republic of Congo	Mali	Uganda
Djibouti	Mauritania	Ukraine
Dominica	Mauritius	Uzbekistan
Ecuador	Micronesia	Vanuatu
Egypt	Montenegro	Venezuela
Equatorial Guinea	Morocco	Vietnam
Eritrea	Mozambique	Yemen
Ethiopia	Myanmar	Zambia
Fiji	Namibia	Zimbabwe
Panel B. Other non-high income, non-OECD countries		
Albania	Honduras	Peru
Armenia	Hungary	Poland
Azerbaijan	India	Romania
Belize	Iraq	Saint Kitts and Nevis
Bhutan	Jordan	Saint Lucia
Bulgaria	Kiribati	Saint Vincent and the Grenadines
Burkina Faso	Latvia	Samoa
Cape Verde	Lebanon	Sao Tome and Principe
Chile	Lesotho	Saudi Arabia
Colombia	Libya	Slovak Republic
Comoros	Lithuania	Solomon Islands
Croatia	Marshall Islands	South Africa
Cuba	Mexico	Suriname
Czech Republic	Moldova	Swaziland
Dominican Republic	Mongolia	Syria
El Salvador	Nicaragua	Thailand
Estonia	Oman	Trinidad and Tobago
Gambia	Palau	Tuvalu
Georgia	Panama	Uruguay
Guatemala	Papua New Guinea	
Haiti	Paraguay	

Note: Panel A lists all countries that ever get Chinese aid projects implemented by SASAC firms. Panel B lists all other non-high income, non-OECD countries (as defined by the World Development Indicators) that are included in the sample.

Table A.25: First Stage: Unrest and Chinese Aid at the Country Level

	# contracts	# ODA contracts	# OOF contracts	contract value	ODA value	OOF value
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*0.326*	*0.122*	*0.125*	*3.079*	*1.474*	*1.343*
	(1.012)	*(0.430)*	*(0.615)*	*(7.071)*	*(5.010)*	*(4.966)*
weighted unrest,t-1	0.249***	0.068***	0.117***	0.835***	0.535**	0.922***
(standardized)	(0.051)	(0.025)	(0.027)	(0.254)	(0.266)	(0.232)
# observations	1518	1518	1518	1518	1518	1518
# countries	138	138	138	138	138	138
adjusted R2	0.348	0.218	0.281	0.289	0.185	0.258
F-statistic	23.71	7.185	18.25	10.77	4.039	15.73

Note: The unit of observation is a country-year. All country-year level regressions include country and year fixed effects. # aid projects: total number of aid projects, implemented by Chinese SASAC firms, received by country. Aid value: log(1 + total value of aid projects, implemented by Chinese SASAC firms, received by country). Weighted unrest is calculated as the sum (over all Chinese prefectures) of demeaned unrest in a Chinese prefecture multiplied with the probability that the country receives aid projects implemented by SASAC firms in that prefecture. Weighted unrest is standardized to have mean 0 and standard deviation 1. Standard errors clustered are at the country level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.26: First Stage Placebo Checks

	# aid projects				imports from China	FDI from China	DAC aid
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
dependent variable mean	*0.326*	*0.339*	*0.342*	*0.344*	*4.041*	*0.251*	*0.629*
(SD)	*(1.012)*	*(1.020)*	*(1.048)*	*(1.070)*	*(8.407)*	*(0.806)*	*(1.670)*
weighted unrest,t-1	0.249***				0.066	-0.003	0.005
(standardized)	(0.051)				(0.132)	(0.023)	(0.019)
weighted unrest,t-2		-0.083					
(standardized)		(0.058)					
weighted unrest,t			0.064				
(standardized)			(0.058)				
weighted unrest,t+1				0.038			
(standardized)				(0.046)			
# observations	1518	1380	1380	1242	1507	1518	1372
# countries	138	138	138	138	137	138	127
adjusted R2	0.348	0.304	0.338	0.348	0.890	0.390	0.382
F-statistic	23.71	2.028	1.236	0.684	0.248	0.0156	0.0724

Note: The unit of observation is a country-year. All country-year level regressions include country and year fixed effects. # aid projects: total number of aid projects, implemented by Chinese SASAC firms, received by country. Imports from China: total imports from China received by a country in billion USD (WDI data). FDI from China: total foreign direct investment from Chinese firms received by a country in billion USD (AEI Global Investment Tracker data). DAC aid: Total foreign aid received by OECD Development Assistance Committee donors received by a country in billion USD (WDI data). All financial amounts are in 2014 real USD. Weighted unrest is calculated as the sum (over all Chinese prefectures) of demeaned unrest in a Chinese prefecture multiplied with the probability that the country receives aid projects implemented by SASAC firms in that prefecture. Weighted unrest is standardized to have mean 0 and standard deviation 1. Standard errors clustered are at the country level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.27: Effect of Chinese Aid on Recipient Countries, Robustness to Controls

	GDP per capita, t+3	GDP growth (%), t+3	unemployment (%), t+3	# diplomatic visits, t+3
	(1)	(2)	(3)	(4)
dependent variable mean (SD)	*5130*	*2.209*	*7.598*	*1.393*
	(4597)	*(2.941)*	*(5.358)*	*(2.666)*
		Panel A: IV estimates		
instrumented aid, t	120.722**	0.507*	-0.375**	1.048**
	(50.959)	(0.297)	(0.179)	(0.425)
		Panel B: reduced form estimates		
weighted unrest, t-1	30.794**	0.129	-0.095**	0.266***
	(12.950)	(0.080)	(0.040)	(0.094)
		Panel C: OLS estimates		
aid, t	0.664	0.160**	-0.022	0.166*
	(12.596)	(0.079)	(0.032)	(0.086)
# observations	1,474	1,479	1,423	1,500
# countries	135	136	130	137
# of aid projects mean (SD)	0.333	0.334	0.345	0.330
	(1.024)	(1.024)	(1.041)	(1.017)
KP F-Statistic from first stage	23.15	23.10	22.73	23.11

Note: The unit of observation is a country-year. Panel A shows the regression coefficients from IV estimates of the outcome variable in the Column head at t+3 on the number of Chinese aid projects, implemented by SASAC firms, at t, where the number of aid projects is instrumented by weighted labor unrest at t-1. Panel B shows reduced form estimates of the outcome variable in the Column head at t+3 on weighted labor unrest at t-1. Panel C shows OLS estimates of the outcome variable in the Column head at t+3 on aid at t. All country-year level regressions include country and year fixed effects, imports from China per capita at t, and weighted inward foreign direct investment (FDI) per capita at t. Weighted unrest is calculated as the sum (over all Chinese prefectures) of demeaned unrest in a Chinese prefecture multiplied with the probability that the country receives aid projects implemented by SASAC firms in that prefecture. Weighted unrest is standardized to have mean 0 and standard deviation 1. Outcome variables are winsorized at the 5th and 95th percentiles to limit the influence of extreme outliers. Standard errors are clustered at the country level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.28: Effect of Chinese Aid on Recipient Countries, Other Leads and Lags (IV Estimates)

	GDP per capita, t+3	GDP growth (%), t+3	unemployment (%), t+3	# diplomatic visits, t+3
	(1)	(2)	(3)	(4)
dependent variable mean (SD)	*5130*	*2.209*	*7.598*	*1.393*
	(4597)	*(2.941)*	*(5.358)*	*(2.666)*
instrumented aid, t	123.359**	0.510*	-0.381**	1.059**
	(50.612)	(0.300)	(0.179)	(0.433)
instrumented aid, t+1	118.902**	0.566*	-0.400**	-0.263
	(48.780)	(0.308)	(0.163)	(0.301)
instrumented aid, t+2	78.629*	1.039**	-0.165	0.069
	(42.735)	(0.404)	(0.152)	(0.390)
instrumented aid, t+3	27.078	0.793*	-0.143	-0.038
	(43.416)	(0.442)	(0.152)	(0.324)
instrumented aid, t+4	-8.070	-0.013	0.027	-0.300
	(58.195)	(0.336)	(0.162)	(0.254)
instrumented aid, t+5	-7.118	0.453	-0.049	-0.184
	(66.590)	(0.375)	(0.263)	(0.305)
# observations	1,474	1,479	1,423	1,500
# countries	135	136	130	137
# of aid projects mean (SD)	0.333	0.334	0.345	0.330
	(1.024)	(1.024)	(1.041)	(1.017)
KP F-Statistic from first stage	24.41	24.36	23.88	24.30

Note: The unit of observation is a country-year. Each row shows the regression coefficient from a separate IV regression of the outcome indicated in the column head at t+3 on the number of Chinese aid projects, implemented by SASAC firms, at the indicated lead/lag, where the number of aid projects is instrumented by weighted labor unrest at the indicated lead/lag minus 1. All country-year level regressions include country and year fixed effects. Weighted unrest is calculated as the sum (over all Chinese prefectures) of demeaned unrest in a Chinese prefecture multiplied with the probability that the country receives aid projects implemented by SASAC firms in that prefecture. Weighted unrest is standardized to have mean 0 and standard deviation 1. Standard errors are clustered at the country level. Outcome variables are winsorized at the 5th and 95th percentiles to limit the influence of extreme outliers. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.29: Effect of Chinese Aid on Recipient Countries, Components of GDP, Other Leads and Lags (IV Estimates)

	GDP per capita, t+3	capital formation p.c., t+3	government consumption p.c., t+3	household consumption p.c., t+3	imports per capita, t+3	exports per capita, t+3
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*5097*	*1275*	*855.2*	*3140*	*2608*	*2386*
	(4669)	*(1176)*	*(888)*	*(2610)*	*(3359)*	*(4059)*
instrumented aid, t	164.867***	82.034**	40.414***	68.228*	75.333	-52.100
	(61.594)	(32.660)	(12.168)	(39.585)	(71.979)	(181.907)
instrumented aid, t+1	145.088**	95.599***	39.046***	61.933	180.630**	127.108
	(56.713)	(31.954)	(12.600)	(41.126)	(79.746)	(149.081)
instrumented aid, t+2	94.372*	35.484*	29.891***	25.711	25.888	77.739
	(49.046)	(20.638)	(11.121)	(38.887)	(74.027)	(143.621)
instrumented aid, t+3	38.394	21.029	20.451*	7.607	-94.734	-32.038
	(50.529)	(23.267)	(10.705)	(46.387)	(77.519)	(94.402)
instrumented aid, t+4	4.050	-18.776	11.763	-1.306	-53.718	-117.201
	(66.269)	(31.482)	(11.490)	(51.570)	(44.291)	(83.163)
instrumented aid, t+5	3.757	13.704	0.964	-0.751	-15.335	-106.308
	(75.141)	(40.905)	(12.173)	(58.200)	(51.006)	(91.060)
# observations	1,189	1,189	1,189	1,189	1,189	1,189
# countries	112	112	112	112	112	112
# of aid projects mean (SD)	0.348	0.348	0.348	0.348	0.348	0.348
	(1.071)	(1.071)	(1.071)	(1.071)	(1.071)	(1.071)
KP F-Statistic from first stage	16.74	16.74	16.74	16.74	16.74	16.74

Note: Sample includes observations that are non-missing for all outcome variables. The unit of observation is a country-year. Each row shows the regression coefficient from a separate IV regression of the outcome indicated in the column head at t+3 on the number of Chinese aid projects, implemented by SASAC firms, at the indicated lead/lag, where the number of aid projects is instrumented by weighted labor unrest at the indicated lead/lag minus 1. All country-year level regressions include country and year fixed effects. Weighted unrest is calculated as the sum (over all Chinese prefectures) of demeaned unrest in a Chinese prefecture multiplied with the probability that the country receives aid projects implemented by SASAC firms in that prefecture. Weighted unrest is standardized to have mean 0 and standard deviation 1. Standard errors are clustered at the country level. Outcome variables are winsorized at the 5th and 95th percentiles to limit the influence of extreme outliers. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.30: Effect of Chinese Aid on Recipient Countries, Different Measures of Aid

	GDP per capita, t+3					
	measure of aid:					
	# aid projects	# ODA projects	# OOF projects	aid value	ODA value	OOF value
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*5130*	*5130*	*5130*	*5130*	*5130*	*5130*
	(4597)	*(4597)*	*(4597)*	*(4597)*	*(4597)*	*(4597)*
Panel A: IV estimates						
instrumented aid, t	123.359**	448.660*	263.369**	21.082*	31.783	19.108*
	(50.612)	(266.080)	(105.776)	(11.560)	(21.345)	(10.716)
Panel B: reduced form estimates						
weighted unrest, t-1	31.267**	31.267**	31.267**	31.267**	31.267**	31.267**
	(12.659)	(12.659)	(12.659)	(12.659)	(12.659)	(12.659)
Panel C: OLS estimates						
aid, t	0.302	-18.794	4.722	-0.730	-0.730	-1.342
	(11.920)	(18.796)	(25.348)	(0.968)	(1.098)	(1.337)
# observations	1,474	1,474	1,474	1,474	1,474	1,474
# countries	135	135	135	135	135	135
aid mean (SD)	0.333	0.125	0.128	5.390	2.639	2.312
	(1.024)	(0.435)	(0.623)	(12.220)	(8.840)	(8.491)
KP F-Statistic from first stage	24.41	7.480	18.75	11.12	4.313	16.72

Note: The unit of observation is a country-year. Panel A shows the regression coefficients from IV estimates of GDP per capita at t+3 on the measure of Chinese aid indicated in the Column head, implemented by SASAC firms, at t, where aid is instrumented by weighted labor unrest at t-1. Panel B shows reduced form estimates of GDP per capita at t+3 on weighted labor unrest at t-1. Panel C shows OLS estimates of GDP per capita at t+3 on aid at t. All country-year level regressions include country and year fixed effects. Weighted unrest is calculated as the sum (over all Chinese prefectures) of demeaned unrest in a Chinese prefecture multiplied with the probability that the country receives aid projects implemented by SASAC firms in that prefecture. Weighted unrest is standardized to have mean 0 and standard deviation 1. Outcome variables are winsorized at the 5th and 95th percentiles to limit the influence of extreme outliers. Standard errors are clustered at the country level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.31: Effect of Chinese Aid on Recipient Countries, Past Weights (IV Estimates)

	GDP per capita, t+3	GDP growth (%), t+3	unemployment (%), t+3	# diplomatic visits, t+3
	(1)	(2)	(3)	(4)
dependent variable mean (SD)	*5130*	*2.209*	*7.598*	*1.393*
	(4597)	*(2.941)*	*(5.358)*	*(2.666)*
		Panel A: IV estimates		
instrumented aid, t	186.959**	0.871**	-0.384	0.799
	(80.017)	(0.374)	(0.285)	(0.670)
		Panel B: reduced form estimates		
weighted unrest, t-1	26.770**	0.126**	-0.055	0.115
	(11.123)	(0.057)	(0.034)	(0.096)
		Panel C: OLS estimates		
aid, t	-4.000	0.155*	-0.021	0.175**
	(12.202)	(0.079)	(0.033)	(0.086)
# observations	1,474	1,479	1,423	1,500
# countries	135	136	130	137
# of aid projects mean (SD)	0.333	0.334	0.345	0.330
	(1.024)	(1.024)	(1.041)	(1.017)
KP F-Statistic from first stage	10.61	10.79	10.41	10.70

Note: The unit of observation is a country-year. Panel A shows the regression coefficients from IV estimates of the outcome variable in the Column head at t+3 on the number of Chinese aid projects, implemented by SASAC firms, at t, where the number of aid projects is instrumented by weighted labor unrest at t-1. Panel B shows reduced form estimates of the outcome variable in the Column head at t+3 on weighted labor unrest at t-1. Panel C shows OLS estimates of the outcome variable in the Column head at t+3 on aid at t. All country-year level regressions include country and year fixed effects and control for the uninteracted sum of weights. Weighted unrest is calculated as the sum (over all Chinese prefectures) of demeaned unrest in a Chinese prefecture multiplied with the probability that the country receives aid projects implemented by SASAC firms in that prefecture (using only years up to t-1). Weighted unrest is standardized to have mean 0 and standard deviation 1. Outcome variables are winsorized at the 5th and 95th percentiles to limit the influence of extreme outliers. Standard errors are clustered at the country level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.32: Effect of Chinese Aid on Recipient Countries, Components of GDP, Past Weights (IV Estimates)

	GDP per capita, t+3	capital formation p.c., t+3	government consumption p.c., t+3	household consumption p.c., t+3	imports per capita, t+3	exports per capita, t+3
	(1)	(2)	(3)	(4)	(5)	(6)
dependent variable mean (SD)	*5097*	*1275*	*855.2*	*3140*	*2608*	*2386*
	(4669)	*(1176)*	*(888)*	*(2610)*	*(3359)*	*(4059)*
Panel A: IV estimates						
instrumented aid, t	223.964**	130.708**	50.998**	112.034*	177.563	-22.618
	(95.434)	(62.825)	(19.983)	(67.361)	(124.125)	(227.353)
Panel B: reduced form estimates						
weighted unrest, t-1	29.968**	17.490**	6.824**	14.991*	23.759*	-3.027
	(12.812)	(7.412)	(2.671)	(7.685)	(13.840)	(30.104)
Panel C: OLS estimates						
aid, t	-2.643	9.636	-0.398	-2.956	18.737	24.576
	(13.297)	(5.980)	(1.608)	(5.096)	(11.414)	(29.211)
# observations	1,189	1,189	1,189	1,189	1,189	1,189
# countries	112	112	112	112	112	112
# of aid projects mean (SD)	0.348	0.348	0.348	0.348	0.348	0.348
	(1.071)	(1.071)	(1.071)	(1.071)	(1.071)	(1.071)
KP F-Statistic from first stage	5.727	5.727	5.727	5.727	5.727	5.727

Note: The unit of observation is a country-year. Panel A shows the regression coefficients from IV estimates of the outcome variable in the Column head at t+3 on the number of Chinese aid projects, implemented by SASAC firms, at t, where the number of aid projects is instrumented by weighted labor unrest at t-1. Panel B shows reduced form estimates of the outcome variable in the Column head at t+3 on weighted labor unrest at t-1. Panel C shows OLS estimates of the outcome variable in the Column head at t+3 on aid at t. All country-year level regressions include country and year fixed effects and control for the uninteracted sum of weights. Weighted unrest is calculated as the sum (over all Chinese prefectures) of demeaned unrest in a Chinese prefecture multiplied with the probability that the country receives aid projects implemented by SASAC firms in that prefecture (using only years up to t-1). Weighted unrest is standardized to have mean 0 and standard deviation 1. Outcome variables are winsorized at the 5th and 95th percentiles to limit the influence of extreme outliers. Standard errors are clustered at the country level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.33: Effects of Chinese Aid on Recipient Countries, Excluding Outliers

	GDP per capita, t+3	GDP growth (%), t+3	unemployment (%), t+3	# diplomatic visits, t+3
	(1)	(2)	(3)	(4)
dependent variable mean (SD)	*5120*	*2.212*	*7.602*	*1.393*
	(4589)	*(2.939)*	*(5.362)*	*(2.668)*
		Panel A: IV estimates		
instrumented aid, t	91.670*	0.437	-0.384**	1.242**
	(48.737)	(0.310)	(0.187)	(0.480)
		Panel B: reduced form estimates		
weighted unrest, t-1	21.908**	0.105	-0.091**	0.296***
	(10.592)	(0.072)	(0.041)	(0.104)
		Panel C: OLS estimates		
aid, t	-2.722	0.149*	-0.020	0.176**
	(11.308)	(0.084)	(0.031)	(0.087)
# observations	1,471	1,476	1,420	1,497
# countries	135	136	130	137
# of aid projects mean (SD)	0.333	0.334	0.345	0.330
	(1.025)	(1.025)	(1.042)	(1.018)
KP F-Statistic from first stage	26.35	26.28	25.79	26.20

Note: The three observations with the highest DFBETA from Table 6 are excluded from the sample. The unit of observation is a country-year. Panel A shows the regression coefficients from IV estimates of the outcome variable in the Column head at t+3 on the number of Chinese aid projects, implemented by SASAC firms, at t, where the number of aid projects is instrumented by weighted labor unrest at t-1. Panel B shows reduced form estimates of the outcome variable in the Column head at t+3 on weighted labor unrest at t-1. Panel C shows OLS estimates of the outcome variable in the Column head at t+3 on aid at t. All country-year level regressions include country and year fixed effects. Weighted unrest is calculated as the sum (over all Chinese prefectures) of demeaned unrest in a Chinese prefecture multiplied with the probability that the country receives aid projects implemented by SASAC firms in that prefecture. Weighted unrest is standardized to have mean 0 and standard deviation 1. Outcome variables are winsorized at the 5th and 95th percentiles to limit the influence of extreme outliers. Standard errors are clustered at the country level. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.